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INDETERMINACY OF TRANSLATION AND

THEORIES OF TRUTH

Dissertation

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Degree Doctor of Philosophy in the Graduate School
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by

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* * * *

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CHAPTER I

INTRODUCTION

Quine's claim that translation is indeterminate, first developed in detail in Word and Object,[1] has attracted a good bit of attention and there are signs that it will attract a great deal more attention. In spite of this, however, there seem to me to be a number of very fundamental matters that need elucidation. In this work I shall then consider a few of these matters. In addition to this I shall offer a few remarks purporting to make plausible the claim that the indeterminacy of translation thesis (hereafter ITT) is incompatible with three recent accounts of truth in natural languages. In considering these matters it is necessary, in my opinion, to undertake a good bit of exegetical work on various of Quine's work, most especially Word and Object.

As stated, there is a good bit of exegetical material related to the ITT herein, but it may be useful to very briefly set out a couple statements of the ITT.[2] There are several passages in Word and Object that are frequently taken as suitable brief statements of the ITT. One remark

which occurs early in Word and Object is often taken as a

statement of the ITT:

> manuals for translating one language into another
> can be set up in divergent ways, all compatible
> with the totality of speech dispositions, yet
> incompatible with each other. (Word and Object,
> p. 27)

Even this purported statement of the ITT raises numerous

questions. Are we restricted to speech dispositions as

the only thing to which translation manuals must conform

In what ways are differing translation manuals different

from one another? Is the ITT to be understood purely as a

case of epistemological indeterminacy or is it a metaphysi-

cal thesis? One might also ask how this differs from both

familiar inductive uncertainties and the claim that all

theories are undetermined by the data. That Quine intends

the ITT to be taken as more than a mere epistemological

thesis can be seen in the following:

> Thus the analytic hypotheses, and the grand
> synthetic one that they add up to, are only in
> an incomplete sense hypotheses....The point is
> not that we cannot be sure what analytic hypo-
> thesis is right, but that there is not, even,
> as there was in the case of 'Gavagai', an ob-
> jective matter to be right or wrong about.
> (Word and Object, p. 73)

The only real support offered in Word and Object for the

sort of metaphysical view set out in the second of these

quotations is the remark on translation manuals set out in

the first of the quotations. This leaves us wondering how

we get from the claim concerning translation manuals to the

metaphysical claim embodied in the ITT.

In Chapter II I, then, offer some remarks which attempt to make plausible the claim that the ITT is incompatible with three recent accounts of truth in natural languages. In this matter I consider material by Sommers, Davidson, and Sellars.

In Chapter III I attempt to show why one cannot appeal to any purported knowledge of psychological states as evidence for translational hypotheses. Here I consider the arguments of, among others, Charles Landesman, to the effect that one can appeal to such data in support of particular translational hypotheses.

In Chapter IV I consider a much debated question. Does the ITT depend on any sort of verificationist principles and if so is this incompatible with Quine's rejection of the so called "verificationist theory of meaning"? I also in this chapter offer an explanation as to why persons seem reluctant to accept any sort of inference from epistemological matters to metaphysical claims of indeterminacy.

In Chapter V I attempt to explain why, contrary to the opinions of several persons, it is not correct to say that if translation is indeterminate then Quine has no ground for saying that physics is not indeterminate. In this chapter I then attempt to explain what the relevant

difference between translation and physics consists in.

There is a good bit of exegetical material related to the ITT in subsequent chapters but it is probably worthwhile to briefly set out the grounds for the doctrine I shall be using. (It should be noted that there are other ways of arguing for the ITT). Quine's basic procedure in Chapter II of <u>Word and Object</u> is to set up "though experiments" in which he assumes that we know what prompts assent and what prompts dissent to a particular expression by a speaker of a language and claims that differing English expressions can all equally well translate the native expression.

Quine's famous example involves the native expression 'Gavagai' assented to in the presence of rabbits and denied in the absence of rabbits. The claim is then that all the evidence there could in principle be for translating 'Gavagai' accords equally well with the English expressions 'Rabbit', 'Rabbit Stage', etc. We might set up the argument as follows

1. The native expression 'Gavagai' can be translated variously in English in a way in accord with all of the native's dispositions to speech behavior.

2. Dispositions to speech behavior is all of the evidence for translational hypotheses.

3. If a translation is undetermined by all of the

relevant evidence it is indeterminate.

Therefore the translation of "Gavagai' is indeterminate.

Of course, if this argument works for Gavagai it can be

extended to other terms.

In Chapter VI I consider aspects of Quine's theory

of truth and evidence. In Chapter VII and VIII I try to

make plausible the sort of verificationism I attribute

to Quine in Chapter IV. Doing this will involve, among

other things, a consideration of arguments of Rorty and

Davidson to the effect that "We cannot make good sense

of the idea that there are seriously different conceptual

schemes".[3]

My general purpose is simply to set out a few of the

very large number of issues one becomes involved in when

considering the soundness and importance of the ITT.

Further, I hope to do a small bit toward showing that a

very great deal is at stake in the debate over the ITT.

Footnotes

[1]The bulk of Quine's published writings on the in-
determinacy of translation can be found in Word and Object,
(Cambridge: MIT Press, 1960) and Ontological Relativity and
Other Essays (N.Y.: Columbia University Press, 1969).
Further references to these works will be included in the
text.

[2]There are as of this writing over one hundred papers
on the ITT, many of which offer their own accounts of the
ITT and Quine's argument for it. In my opinion, one of the
best accounts of the ITT is Dagfinn Follesdal, "Indeter-
minacy of Translation and Underdetermination of the Theory
of Nature", Dialectica, XXVII, (1973), pp. 289-301.

[3]This remark of Donald Davidson's occured in the
printed discussion session following his paper "Psychology
as Philosophy", in Philosophy of Psychology, ed. by S. C.
Brown (N.Y.: Humanities Press, 1974), p. 66.

CHAPTER II

THE INDETERMINACY OF TRANSLATION AND THREE
RECENT THEORIES OF TRUTH

As stated in the introductory chapter, one of my ob-
jectives here is to show that there are important conse-
quences of the ITT for those who would give an account of
truth in natural languages. I shall herein be trying to
show that the manner in which truth is traditionally ex-
plained along correspondence lines is incompatible with
the ITT.[1] I shall be trying to argue that the account of
truth held by a number of philosophers depends on the in-
correctness of the ITT.

One would find this a simpler task if there was a
universally accepted account of truth in natural languages.
If there were, one would have only to examine this account
and in so doing establish if it would be effected in one
way or another by the ITT. There is not, however, any
such universally accepted theory. There is even little
general agreement concerning the criteria of adequacy for
such theories.[2] The only general area where there seems
anything even approximating general agreement concerns the
work of Tarski in terms of formal languages and truth

definitions, but this is quite another matter from explaining truth in natural languages. (Tarski, of course, thought it was impossible to give a definition of truth for natural languages).

In the light of this I shall attempt to make out the claim made in the opening paragraph by considering the accounts of truth given by Wilfrid Sellars in "Truth and Correspondence" and Science and Metaphysics[4], Donald Davidson in "True to the Facts"[5] and Fred Sommers in "On Concepts of Truth in Natural Languages"[6]. It may be of use to explain briefly why I have chosen these three persons and what I think it shows if I am correct in my claim concerning them. Aside from the obvious reason of using here what one considers among the most interesting approaches of which one is aware, there are several reasons for employing these three sources.

I wish to look at the theories and views of Davidson in view of the fact that he is one of the prime movers in the recent attempts of various and sundry philosophers to apply the methods of Tarski to natural languages. Secondly, there is a strong and frequently acknowledged influence of the work of Quine on Davidson. In the case of Sellars, aside from what I find to be the intrinsic interest in attempting to see the connections between various aspects of his philosophical theories, I wish to

consider him in that he takes quite seriously the venerable

idea that language in some way pictures reality and is to

date the most comprehensive articulator of a theory of

the language-world relationship that has had several

advocates in philosophy. I rather expect little disagree-

ment on the import of considering the views Sellars and

Davidson but it may be that some would require a further

justification for treating views of Sommers, expressed

as they are in his own rather baroque language. One of

the more common attitudes toward truth involves the

definition or explanation of truth in terms of states of

affairs. For example, Chisholm has explained truth as

follows:

> Our answer, then, to the question 'What is truth'
> is this: A belief or assertion is true provided,
> first, that it is a belief or assertion with
> respect to a certain state of affairs that that
> state of affairs exists, and provided, secondly,
> that that state of affairs does exist.[7]

Truth is in some way a matter of the correspondence of

statements (sentences) to reality. To indicate further

the import of Sommers' account which deals with states of

affairs, we can see in the following that Quine takes

the ITT to do away with any notion of state of affairs:

> ...then the indeterminacy of translation of
> theoretical sentences is the natural conclusion.
> And most sentences, apart from observational
> sentences, are theoretical. This conclusion,
> conversely, once it is embraced, seals the fate

of any general notion of propositional meaning
or, for that matter, state of affairs. ("OR",
p. 81)

I wish to consider Sommers' paper because he provides a

fairly well articulated account of truth in terms of

states of affairs. I am not saying that these three

approaches exhaust all that truth could in principle be

and using the incompatibility of these three theories with

the ITT to claim that all correspondence theories of truth

are incompatible with the ITT. I am rather claiming that

if we see that this is the case in these cases we will

thereby have a good idea of how one might show the same

thing with respect to other accounts that might or have

been offered.[8] I am not, by any means, assuming that

there is some essence to truth which can hold only if

there is determinate translation. Rather I am simply

considering whether or not these three accounts of truth

(and thereby others like them in relevant respects) depend

on the incorrectness of the ITT. I stress this only

because I have found it to be a perhaps natural tempta-

tion to assume that one could set out in an a priori man-

ner what would or would not be a notion of truth (or good,

meaning, etc.). In avoiding such a priorism, I consider

only what people have actually thought to be the proper

account of truth in natural languages, with the additional

rider that the types of accounts set out here are

roughly like many philosophers would in fact give were they
inclined to articulate a theory of truth along correspon-
dence lines. (It is perhaps worth mentioning that it
appears that so called "coherence theories" might be less
likely to depend on the falsity of the ITT. I do not
consider them primarily in that they are less plausible for
familiar reasons and that it is rather hard to find a
well articulated account of the coherence theory of truth
which forthrightly takes coherence as anything other than
a criteria for warranted assertion. Indeed, the two most
recent and sustained expositions of the coherence theory
of truth, that of Rescher and Lehrer, both explicitly hold
that coherence can serve only a justificatory function.[9])
In other words, I wish to avoid any unseemly talk of the
meaning or essence of truth and stick to what people have
said truth is. I do this for two reasons: one is the
fact that many persons, including myself, find such notions
quite unclear in themselves and the second is that I hope
to avoid by this practice some of the begged questions
that can result if one assumes that one knows what is and
is not a proper part of an explanation of truth in natural
languages. Having done what I can here to avoid what I
take to be moderately likely ways of misconstruing my
words, I wish to turn to a consideration of the relevant
aspects of Sellars's philosophy.

The primary text from Sellars that I shall consider is Science and Metaphysics, Chapter V "The Conceptual and the Real: 3. Picturing". This is the most extended account Sellars has to date published concerning his account of the relationship between language and the world. Sellars' definition of truth is:

> for a proposition to be true is for it to be assertable, that is, in accordance with the relevant semantical rules, and on the basis of such additional, though unspecified, information as these rules may require. The phrase, 'semantical rule' is used in the sense described in the preceding chapter, but which will be further explicated as the argument proceeds. 'True' then means semantically assertible.[10]

Precisely what Sellars takes semantical rules to be is not, at least to myself and some of the commentators on Sellars[11], easily established. According to Harman's reading of the relevant texts, they may be either principles of evidence or principles in the theory of truth, or perhaps even a mixture of the two. According to Harman, 'semantical rule' and 'semantical regularity' are used interchangeably in Science and Metaphysics.

Having set out the preliminary definition of truth one is best served here by setting out Sellars' summation of the relevant chapter of Science and Metaphysics, given in the preface to that work.

> The fifth chapter is, as already indicated, the heart of the enterprise. In it I attempt to spell out the specific differences of

matter-of-factual truth. Levels of 'factual'
discourse are distinguished and shown to pre-
suppose a basic level in which conceptual items
as items in rerum natura 'represent' or
'picture' (in a sense to be distinguished from
the semantical concepts of reference and predi-
cation) the way things are. The distinctions
drawn enable a definition of 'reality' and
'(ideal) truth' in terms of adequate represen-
tation.[13]

Sellars, in chapter V of Science and Metaphysics, attempts

to clarify the definition set out above. I shall then

attempt to consider that clarification as a point of

departure.

The explication of truth as S-assertability
raised the question: assertable by whom?
With respect to the concept 'true statement'(in L),
the obvious but superficial answer is: 'by
users of L'.[14]

Sellars's explication of truth is language relative and

it is in the explication of this that the considerations

concerning the ITT become relevant. Sellars assumes that

expressions in a particular language are to be translated

into expressions into another language via the correlation

of the expressions through the intermediary of a common

conceptual scheme shared by the two languages. As the

following quotation will illustrate, a statement is true

if it has as a translation an expression in an underlying

conceptual scheme which is true.

'___' (in L) is true   '___' (in L) are·......· and
·......· are true
i.e. in terms of what is misleadingly called the
'absolute' sense of truth, the most penetrating
answer is: s-assertible by us. For truth in
the 'absolute' sense is, in its own way, language

relative, relative to our language. Thus the

    •.....• are true
on the right-hand side of the above schema has the
sense of
    •....• are S-assertible by us.
where we are users of the language in terms of
which specific propositional expressions are
introduced.[16]

We can see then that the notion of translation is involved

in the correlation of sentences in L with dot-quote expres-

sions in setting out the notion of truth. Sellars is quite

explicit in the claim that statements in language other

than our own cannot be characterized as true without in

effect translating them into our language. Further,

according to Sellars (as shall shortly be illustrated) the

translation of, for example, French into English requires

that there is a common underlying "game" which English

and French are both instances of. Thus the characteriza-

tion of statements in our own language as true it self

involves the translation of these expressions "into an

underlying game" (hence the use of dot quotes to serve

the function of standing for expressions in the underlying

game. Sellars uses the analogy of chess pieces being

defined in terms of their roles, not in terms of their

physical construction.)

    Thus, to characterize a statement in a foreign
language, for example, French as true is, in effect,
to treat this language as a 'dialect' of a language
game which we play, i.e. to treat speakers of
French as speakers of our language, as players of

a common game. Since the term 'language' as it
is ordinarily used refers to specific linguistic
material (sign designs and surface grammar)
which differentiate, e.g., French from German,
we need another term for the common game played
by users of such differing resources. I shall
use the expression 'conceptual structure' to
serve this purpose. Thus the above scheme, made
explicit, becomes

'----' (in L) is true   '----' (in L) are ·...·
and ·...· are S-assertible
propositions that belong
to our conceptual
structure.[17]

Sellars' explanation of truth then requires a notion

of translation. That this notion of translation must be

a determinate one can, I think, be grasped by the considera-

tion of Quine's rabbits. We utter the expression "That

rabbit is the same one that we pointed out earlier", some-

one else utters an expression similar enough in all relevant

respects to pass as a phonetically similar utterance. By

passing for a moment the fact that expressions in our own

language can be characterized as true or false only via their

dot quote translations, we can see that unless the trans-

lation of the other's dialect into our own is determinate

then truth with respect to the others dialect will be

indeterminate. Consider, if we translate homophonically

his expression may be true whereas if we translate 'rabbit'

via 'rabbit stage' (by passing the problem of the trans-

lation of 'the same one') the other's expression may well

be false. When we translate our expression into the

underlying conceptual scheme we run up into the same thing,

some translations will get us false sentences and some
true. But if the ITT is correct, there is no saying which
of these translations is correct as there is no right or
wrong in question. Thus, there would be no fact of the
matter as to whether or not the expressions in question
are true.

Differing natural languages then are such that truth
is defined for them in terms of their translation into some
underlying conceptual scheme. Sellars further goes on to
say that the truth of the underlying expressions is itself
determined by whether or not these expressions would have
correlates (successors) in an ideal Piercean end of
science but we need not go into that here.[18]

We can see then, that if Sellars' account of truth is
to work (needless to say I have given only the broadest
picture of it, intending only to try to establish the
claim made on page one) then translation must be determin-
ate, else truth will be indeterminate. I wish now to turn
to a consideration of the views of Sommers.

As earlier stated, Sommers in "On Concepts of Truth
in Natural Languages" attempts to explain truth in terms of
states of affairs. Further, as also previously mentioned,
according to Quine the ITT brings with it the death of
states of affairs. Hence all that is needed here is a
consideration of the manner in which states of affairs

function in Sommers' theory and a consideration of why the

ITT rules out states of affairs. Much of Sommers' paper is

taken up with his attempt to specify "rules of natural

syntax" which allow him to avoid the liar paradox. This

material is not of concern here (fortunately in that

Sommers has his own rather peculiar grammar which would

have to be gone into in order to go into this material.)

Perhaps, however, it would be useful to give Sommers' gene-

ral statement of his intentions and of his account of the

role a solution to the liar paradox plays in explaining

truth for natural languages:

> To do justice to the idea that truth is to be
> defined in terms of existing states of affairs is
> not more desirable than doing justice to our in-
> tuitions concerning our use of 'true' in natural
> languages. Of course an adequate account of
> truth cannot be contradictory. But a solution to
> the paradoxes ought to possess the more homely
> virtues too. An acceptable solution must possess
> the following two features:
> (i) Barriers to the Liar paradox are dis-
>   covered in natural syntax.
> (ii) The natural barriers which keep out the Liar
>   do not also exclude meaningful and harmless
>   linguistic reference.
> We first define truth in a way that does justice
> to our intuitions. We then apply the definition
> to paradoxical sentences predicating 'true' and
> 'false' and find that the formation of the Liar
> paradox is prohibited by the rules of natural
> syntax.

Again, readers interested in seeing how Sommers uses his

rules of "natural syntax" to accomplish objectives i and ii

above can read his paper to find out; my concern here is

not with the adequacy of his solution to the liar paradox
or even the wider question of the adequacy of his defini-
tion of truth but simply with showing that his account of
truth in natural languages depends on the falsity of the
ITT. This can shortly be shown by illustrating that his
account depends on states of affairs and that the notion
of states of affairs falls if the ITT   . I shall
first establish the first part of the above claim, which
can be done rather quickly. The second part will take a
bit longer.

The relationship of translation to Sommers' notion of
truth and states of affairs can be gathered from the
following remarks:

> In formulating the theory of truth in terms of
> correspondence, the first step is to keep facts
> and states apart. For when one assimilates
> states to facts, the correspondence relation for
> truth is mystifying and finally incoherent. When
> states of affairs are distinguished from facts,
> the correspondence relation, construed as holding
> between truths on the one side and reality (exist-
> ing states) on the other side, may be elucidated
> in the following way:
>
> (i) A sentence is said to correspond to the
>   state of affairs it specifies. If that
>   state exists the sentence is said to
>   correspond with reality. In that case
>   it is true.
> (ii) A statement corresponds to the state of
>   affairs specified by any sentence that may
>   be used to make the statement. If that
>   state exists, the statement is said to
>   correspond to reality. In that case it is
>   a true statement. (280)

The second step in the correspondence theory on Sommers'
account involves distinguishing between specifying states
of affairs and saying something about them (see p. 280).
The final step for the correspondence theory is to

> formulate definitions for truth. In any corres-
> pondence formula defining 'x is true' the
> definiens is a state sentence asserting the
> existence of the state to which x corresponds.
> (280)

The crucial considerations for the purposes at hand in-
volves the relationship between statements, sentences, and
states of affairs. The claim will be then that the state
of affairs a sentence specifies must be determinate if
Sommers' theory is to work but if the ITT is correct then
what state a sentence specifies is indeterminate.

There are, it turns out, two relations here which
must be determinate if Sommers' theory is to work (leaving
aside for a moment the considerations concerning states
of affairs and the ITT). Firstly, if it is indeterminate
as to what statements sentences correspond to (make) then
the truth of sentences cannot be accounted for, as Sommers
wishes, by the statements being such that "a statement
corresponds to the state of affairs specified by any
sentence that may be used to make the statement". If there
is no fact of the matter as to what statements a given
sentence makes then the truth of statements cannot be
accounted for in terms of states of affairs specified by
sentences as there is no fact of the matter concerning

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what "statements" a given sentence makes. No account can, hence, be given of states of affairs making statements true unless there is a determinate matter as to what statement a sentence is used to make. If the ITT is correct, it is not a determinate matter as to what statement is made by means of the use of a sentence. That Sommers assumes the falsity of the ITT can be seen by considering what he says about statements:

> But even in its statement making use a sentence
> is English or Latin and so forth; the statement
> it makes is not. (Sommers, 267)

Or "sentences from different languages are used to make the same statement". (281) According to most accounts of the ITT it was precisely this sort of philosophical use of the theory of meaning that Quine developed the ITT to refute. As to the second we may turn to a consideration of "True to the Facts". First, however, I wish to consider the claim by Quine that the ITT spells the death of states of affairs, this being a thesis of very far ranging import given the wide use of talk of states of affairs, besides its immediate import here.

To quote a relevant passage from "Epistemology Naturalized,"

> If we recognize with Pierce 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

> large blocks of theory, then the indeterminacy
> of translation of theretical sentences is the
> natural conclusion. And most sentences, apart
> from observation sentences, are theoretical. This
> conclusion, conversely, once it is embraced, seals
> the fate of any general notion of propositional
> meaning or, for that matter, states of affairs
> ("EN", p. 81)

The indeterminacy of translation thesis is considered at
length in subsequent chapters, so I shall here merely
briefly sketch the connection between the ITT and states
of affairs. If translation is indeterminate, then, for
example, there will be no fact of the matter as to whether
or not (to take the famous case from Chapter II of Word and
Object) 'Gavagai' refers to rabbits, rabbit stages, instances
of the great rabbit, etc. All these possible designata are,
Quine claims, compatible with all of the user's speech
dispositions. When, then, we speak of the state of affairs
in which there is a gavagai at space/time point x we are
still quite in the dark as to what the purported state
of affairs in question is. Appeal to our normal practices
as a way out here will not get us anywhere as if the ITT
is correct there is no fact of the matter as to what these
practices are.[19] This will, I take it, make clear at
least the outlines of why states of affairs go with the
ITT.

I wish then to turn to a consideration of the view
set out by Donald Davidson in "True to the Facts". Again,
I shall be attempting to show that the theory of truth

suggested by Davidson in "True to the Facts" is incompatible

with the ITT. (I should point out that "True to the Facts"

is one of a series of papers by Davidson on truth, a

series in which there appears some reason to think that

Davidson is altering his views as he goes on,[20] so I am

concerned only with this one paper and do not mean my

remarks to apply to Davidson's ongoing program in the

philosophy of language.) In doing this I shall be

neglecting aspects of "True to the Facts" not centrally

related to the relevance of translation to Davidson's

views.

Davidson's aims are set out in the following:

> In this paper I defend a version of the cor-
> respondence theory. I think truth can be explained
> by appeal to a relation between language and the
> world, and that analysis of that relation yields
> insight into how, by uttering sentences, we
> sometimes manage to say what is true. My project
> is less ambitious: I shall be satisfied if I can
> find a natural interpretation of the relation of
> correspondence that helps explain truth. ("True to
> the Facts", 748-749)

Davidson begins by considering Frank Ramsey's idea that

phrases like "it is true" are all such that they have

equivalents that do without expressions like 'true'.

Davidson concludes his consideration of this suggestion

by saying of the "eliminative theory of truth" that it

has neither been shown that the theory is correct nor has

it been shown that it is in principle impossible to develop

a theory that would allow for the replacement of all occurences of 'true'. This question is, of course, independent of the correspondence theory's soundness, even if we could show that it is impossible to carry out the required elimination.

Davidson then goes over various considerations concerning the use of facts to explain truth:

> Such an account would enable us to make sense of sentences with this form:
> (6) The statement that p corresponds to the fact that q. The step to truth would be simple: a statement is true if there is a fact to which it corresponds. (752)

(A number, perhaps most, of those who use the strategy of facts would rephrase this with an "if and only if" clause. After considering familiar objections (largely from Frege) dealing with the problems of distinguishing facts without producing an "ontological collapse" which leaves us with only one fact or with specifying what facts are without reference to truth, Davidson begins his account of his own view:

> Talk about facts reduces to predication of truth in contexts we have considered: this might be called the redundancy theory of facts. Predications of truth, on the other hand, have not proved so easy to eliminate. If there is no comfort for redundancy theories of truth in this, neither is there encouragement for correspondence theories.
> I think there is a fairly simple explanation for our frustrations: we have so far left language out of account. Statements are true or false because of the words used in making them, and it is the words that have interesting, detailed conventional connections with the world. Any

serious theory of truth must therefore deal with
these connections, and it is here if anywhere that
the notion of correspondence can find some pur-
chase. ("True to the Facts", p. 754)

We have by way of further introductory remarks

Truth (in a given natural language) is not a
property of sentences; it is a relation between
sentences, speakers, and dates. To view it thus
is not to turn away from language to speechless
eternal entities like propositions, statements, and
assertions, but to relate language with occasions
of truth in a way that invites the construction
of a theory. ("True to the Facts," p. 754)

After considering some here irrelevant material concerning

performatives, speech acts and such Davidson sets out a

generalization that (when fully developed) has as its role

the provision of a test of adequacy for theories of truth

for natural languages:

(7) Sentence s is true (as English) for speaker u
at time t if and only if p. (An alternative scheme
apparently attributing truth to statements could
be substituted.) Even if we restrict the descrip-
tions we substitute for 's' to some stylized
vocabulary of syntax, we may assume that there is
a true sentence of the form (7) for each English
sentence. The totality of such sentences uniquely
determines the extension of the three place
predicate of (7) (the relativized truth predicate).
("True to the Facts", p. 756)

'p' is to be replaced by a sentence that states the con-

ditions under which the sentence in question is true

whereas 's' is to be replaced by a structural description

of the sentence. Davidson then goes on to argue that

theories of truth involving the notion of "satisfaction"

are a form of correspondence theory. After further

elaborating his theory (which might perhaps with more justice be called his argument for the elaboration of a theory) Davidson sets out two problems which he considered to be unsolved as of the writing of "True to the Facts". The first of these problems concerns the extent to which the methods of formal semantics are applicable to natural languages. The most serious problem here, on Davidson's views, are the difficulties of expanding such methods to include things like attributive adjectives, adverb modification, talk of propositional attitudes, causality, and obligation, etc. The second problem is the logical form of sentences of indirect discourse and remarks of the form

(8) It is true that it is raining

and

(9) The statement that it is raining is true

without reference to timeless entities such as propositions or meanings. Davidson reminds us that he is supposing we have a theory "of truth-in-English with truth treated as a relation between a sentence, a speaker, and a time". ("True to the Facts", p. 762) Davidson is then desirous of finding natural counterparts of these things for sentences such as (8). Davidson suggests something like the following might do for (8):

> A long winded version of (8) might, then, go like
> this. First (reversing the order for clarity) I
> say 'it's raining'. Then I say 'That speech act

embodied a sentence which, spoken by me now, is
true'. On this analysis, an utterance of (8) or
(9) consists of two logically (semantically)
independent speech acts, one of which contains
a demonstrative reference to the other. ("True to
the Facts", p. 762)

Let us then turn to Davidson's remarks on indirect dis-

course. Davidson offers as a test case: (10) Peter's

statement that Paul is hirsute is true. It is here that

the notion of translation begins to become explicit in

Davidson's account of truth in natural languages.

> Following the suggestion made for (8) and (9), the
> analysis of (10) should be 'Paul is hirsute. That
> is true, and Peter said (stated) it'. The 'that',
> as before, refers to the act of speaking, and now
> the 'it' picks up the same reference. What is
> needed to complete the account is a paratactic
> analysis of indirect discourse that interprets
> an utterance of u of 'Peter said that Paul is
> hirsute' as composed of an utterance 'Paul is
> hirsute' and another utterance ('Peter said that')
> that relates Peter in a certain way to u's
> utterance of 'Paul is hirsute'. The relation in
> question can, perhaps, be made intelligible by
> appeal to the notion of samesaying: if u says
> what is true when he says 'Peter said that', it
> is because, by saying 'Paul is hirsute', he has
> made Peter and himself samesayers. ("True to the
> Facts", p. 762)

It is with the relationship of samesaying that we should

begin to become suspicious; (Not that there are not pos-

sible accounts of samesaying that can be compatible with

the ITT as I might say the same thing when I say "Gerald

Ford likes to go skiing" as when I say "The President of

the U.S. likes to go skiing", but as Harman has stressed

in "Quine on Meaning and Existence"[21] this sort of same-

saying is not what most philosophers have had in mind.)

Davidson then attempts to show both that this account need
not employ entities like propositions and to show that
it _does_ employ a notion of translation. Consider a sen-
tence that does not avail itself of paraphrase into a _some_
utterance makes us samesayers form: (11) Peter said some-
thing true. In this case, we may not know the appropriate
thing that would serve to make us samesayers. Likewise
we cannot employ 'Some utterance of Peter's embodied a
sentence true under the circumstances' as (11) does not
tell us what language Peter spoke and the notion of truth
Davidson is dealing with is, as mentioned earlier, relative
to specific known languages (i.e., in order to avoid such
things as given combinations of letters or sounds occuring
in two different languages in a way that causes a variation
in truth value of utterances containing said combinations
of letters.) Not knowing what his language is, we cannot
make sense of 'true in his language' ("True to the Facts",
p. 763) Davidson then suggests the following:

> What we can hope to make sense of, I think, is
> the idea of a sentence in another tongue being
> the translation of a sentence of English. Given
> this idea, it becomes natural to see (11) as
> meaning something like 'Peter uttered a sentence
> of English true under the circumstances....
> The conclusion I would tentatively draw is
> this. We can get away from what seems to be
> talk of the (absolute) truth of timeless
> statements if we accept truth as relativized
> to occasions of speech, and a strong notion of
> translation. ("True to the Facts", p. 763)

Let us consider whether the notion of translation employed
here need be determinate (i.e., such that there must be a
fact of the matter concerning what sentences in our
language are translated by Peter's expression.) If we can
show that the relationship between the sentence Peter
uttered and the one in our language which it is supposed
to translate must be such that there is a fact of the
matter involved then it will have been shown that the
translation relation employed here must be determinate.
I think we can see what this "strong notion of translation"
is by further considering what is involved in the matter
quoted above concerning a sentence in another tongue being
a translation of a sentence in English. (Also if we are
going to be strict about this, we should refer to the par-
ticular English of a particular person.) Now, what we
need to do here is to show that the relationship between
the other sentence and our own must be such that it is
stronger than the fact that they are simply <u>both true and
both false</u>, when speakers and times are held fixed. If
there are English sentences that fulfill both condition
and such that they are not able to serve the purpose here
then we will have made out our case. Peter utters the
sentence "My dog is moribund" and I, not hearing the
remark of Peter but knowing him to hever speak falsely
say, correctly, of course, "Peter said something true".

Now, even though Peter and I might both be native speakers

of the same language, it does not from this follow that

there is no notion of translation at work here but rather

the translation is probably a homophonic one where we say

roughly "If Peter had uttered a sentence in my idiolect

which is a translation of the sentence he uttered, then

he said (would have) something true." If we do not assume

determinacy of translation here, then the only requirement

on my translation of Peter's remark (again, the strong

notion of translation comes into play when we speak of

the sentence of English that would be true under the

circumstances) is that it be something true during the

circumstances under which Peter made his remark. Among

the qualified candidates here would be 'it rained in Ohio

in 1945' or 'I will one day in the future see a dog'.

Both of these remarks will doubtless fit the requirement

of sameness of truth value. The difficulty is that the

truth of either of these two sentences or the circumstances

under which either of these sentences are true has no

connection with the circumstances that made Peter's remark

true. It has to be certain English sentences that we

attribute to the other. If we then do not assume

determinacy of translation here we cannot explain the

truth of remarks like (11) in terms of sentences in our

own idiolect. If there is no fact of the matter as to

what sentence to count as the translation of a remark of
another person then there can be no construction of a
theory of truth in terms of relationships of the circum-
stances in which sentences in our own language are true
and circumstances in which the sentences of the other are
true. If the translations in question are indeterminate
then the circumstances under which the translated remark
is true are such that there is no fact of the matter
concerning the relationship of the circumstances under
which our sentences are true and the circumstances under
which the sentence of the other is true. The notion of
translation Davidson needs to make the theory set out in
"True to the Facts" is then a determinate one.[22]

Perhaps it might be efficacious here to say a bit
more concerning why sameness of truth value is not enough
here. It should be noted that we do not here merely wish
to attribute a truth value to the utterance of the other
but explain why the sentence in question is true. We
explain the truth of the sentence of the other in terms
of the other's utterance translation in our idiolect
being true under circumstances like those under which the
other's sentence was uttered.

Recall Davidson is dealing with a concept of truth
which is relative to languages. (cf. 763, "True to the
Facts") What sort of translation is Davidson speaking of

when he says (in the passage quoted on   ) that what we
can make sense of is "the idea of a sentence in another
tongue being the <u>translation</u> of a sentence of English."
Perhaps if we consider a bit more what Davidson says
about schema (7) (Sentence S is true [as English] for
speaker u at time t if and only if P) we can see why
Davidson says he needs a strong notion of translation.
Firstly, we need to explain why he says that he cannot use
the sort of formula Tarski used. This is because the
language Tarski dealt with contained no indexical expres-
sions. Further, Davidson construes the relation of $\underline{S}$ and $\underline{P}$
on Tarski's account (where the Metalanguage is not con-
strued as containing the object language) as one in P
"translates that sentence in some straightforward sense".
("True to the Facts", p. 757) For languages that contain
indexicals Davidson holds that what is taken as the
replacement of 'p' "must be systematically related to the
sentence described by the replacement of 's' by the rules
that govern the use of indexicals in English". (p. 757,
"True to the Facts") Clearly such cases as considered
earlier (those involving the moribund dog and the weather
in Ohio, though they are material equivalents (or so we
here assume) are not so related. The case is, I take it,
the same when one deals with a speaker of another language.
The sentence his remark is taken as a translation of must

be related by the rules that govern the use of indexical expressions. In an early part of "True to the Facts" Davidson stated that he wants to be able to construct a theory that will allow us to

> explain what it is to make a true statement in terms of conventional relations between words and things that hold when the words are used by particular agents on particular occasion (Davidson, p. 755)

The sentence that makes us samesayers (in the case of 11) should have the same conventional relations to things as the statement of Peter. If Peter and our idiolect were pooled and truth conditions given for the samesaying utterances in question they would be the same. (i.e., in the Scheme 7 the replacement for p could be the same in both cases.) Thus in "Peter uttered a sentence that translates a sentence of English true under the circumstances" Davidson needs not a reference to "a" as any sentence true under the circumstances but a sentence with roughly the same conventional relations to things as the sentence uttered by Peter. It is, I take it, the replacement of p in the schema 7 (cf. 756) that induces Davidson to say that he needs a strong notion of translation.

One should note here that there are alternative accounts of sentences like (11) that makes no use of the notion of samesayer and thus involve no notion of translation into our idiolect of the words of others. Perhaps

aspects of the program Davidson sets forth in "True to the Facts" can be done without bringing in the notion of translation. Of course to pursue these departures from the view of Davidson is, though of course it might well be of importance, not to the point here. It is one thing to say that Davidson is wrong in his account of what is involved in giving a theory of truth and another to say that if he were right then translation must be determinate.

More could be said on all three of the theories considered in this chapter. Hopefully, enough has been said to substantiate the claim that the ITT should be of interest to those concerned with giving an account of truth in natural languages. I wish now to turn to a consideration in the next three chapters of objections to the ITT.

Footnotes

[1]Field, Harty, "Quine and the Correspondence Theory", Philosophical Review, LXXXIII (1974), pp. 351-387.

[2]Rescher, Nicholas, The Coherence Theory of Truth (N.Y.: Oxford Press, 1973) contains a general survey of the agreement that does seem to exist.

[3]Tarski, Alfred, "The Concept of Truth in Formalized Languages", in Logic, Semantics, and Metamathematics (N.Y.: Oxford Press, 1956), pp. 158-279.

[4]Sellars, Wilfrid, "Truth and Correspondence", in Science, Perception, and Reality (N.Y.: Humanities Press, 1963), pp. 197-226 and Sellars, Wilfrid, Science and Metaphysics (N.Y.: Humanities Press, 1968).

[5]Davidson, Donald, "True to the Facts", Journal of Philosophy, LXVI (1969), pp. 748-764. Further references to this article will be included in the text.

[6]Sommers, Fred, "On Concepts of Truth in Natural Languages," Review of Metaphysics, XXXIII (1970), pp. 259-289. Further references to this article will be included in the text.

[7]Chisholm, Roderick, Theory of Knowledge (Englewood Cliffs, N.J.: Prentice Hall, Inc., 1966), p. 103.

[8]See Putnam, Hilary, "The Refutation of Conventionalism", Nous, VIII (1974), pp. 25-40 for an account of related misunderstandings of Quine.

[9]See Rescher for details.

[10]Science and Metaphysics, p. 101.

[11]Harman, Gilbert, "Sellars' Semantics", Philosophical Review, LXXIX (1970), pp. 404-419.

[12]Ibid., p. 418.

[13]Science and Metaphysics, IX.

[14]Science and Metaphysics, p. 131.

[15]For further elaboration on the matter of "dot quotes" see Sellars, Wilfrid, "Abstract Entities", in _Philosophical Perspectives_, (Springfield, Ill.: Charles Thomas, Inc., 1967), pp. 229-269.

[16]_Science and Metaphysics_, p. 132.

[17]_Science and Metaphysics_, p. 132.

[18]For explanation on this matter see _Science and Metaphysics_, pp. 133-136.

[19]For example, Chisholm in _The Theory of Knowledge_ uses mentalistically leaden language in explaining states of affairs, see pp. 77-78.

[20]For a consideration of the ways in which Davidson may have altered his program see Alan Reeves, "On Truth and Meaning", Nous, VIII, (1974), pp. 343-351. Reeves' criticisms of Davidson seem to me to be off the point in that it appears Reeves assumes a faulty notion of what it is to know a language; secondly he thinks that it is only T-sentences that are supposed to "give the meaning of sentences" rather than the proof and the apparatus that allows us to prove the sentences, and thirdly he rejects the ITT on very dubious ground. Reeves, by the way, argues that Davidson in "Truth and Meaning" and the other papers associated with program of that paper (see references in Reeves) assumes the ITT to be correct.

[21]For elaboration on this see Harman's "Quine on Meaning and Existence".

[22]Putnam (in the printed discussion session of the Conn. conference on Translation and Intentionality, 1973, ("Discussion Session", _Synthese_ CVIII (1974)) takes the program pushed by Davidson at that time to involve quite a strong notion of translation: "No. 2, the T-sentences should have the same sense, they should provide a translation. In other words, the P on the right side of the T-sentence has some kind of translation relation to the left side".

[23]See Field's "Quine and the Correspondence Theory" for some interesting very general reflections on the relationship of the ITT and correspondence theories of truth.

CHAPTER III

INDETERMINACY OF TRANSLATION AND OUR
KNOWLEDGE OF THE MENTAL

A large number of objections to Quine's indeterminacy of translation thesis have been advanced since the publication of Word and Object. A large number, perhaps most, of these criticisms turn on misunderstandings of Quine. Most of these misunderstandings involve either failing to see the extent of the indeterminacy argued for by Quine or claiming that in order to argue for the ITT one has to assume principles that are either false or which are inconsistent with other doctrines held by Quine. There is a large class of objections to the ITT which turn on claims to the effect that some evidence based on our knowledge concerning either our own mental states or the mental states of others allows us to avoid the ITT. I shall in this chapter attempt to show the groundlessness of these objections.

I shall also relate certain arguments based on recent work by John Wallace and D. C. Dennett to the ITT and issues concerning the indeterminacy of the mental. With respect to Wallace I shall attempt to show that the

acceptance of the ITT is a plausible reason to accept the indeterminacy of the mental.

I shall try to show further, that one can use certain arguments developed from Dennett's <u>Content and Consciousness</u> to make plausible the claim that the mental is indeterminate, without bringing in the ITT. This is of importance here in order to show that there are plausible enough reasons for the claim that the mental is indeterminate to warrant the claim that one must offer arguments against the indeterminacy of the mental in order to be able to cite purported knowledge of mental states as evidence for determinate translation. My aim is to show that the ITT and the indeterminacy of the mental are such closely related matters that one cannot assume determinacy in one realm and then appeal to it to argue for determinacy in the other realm.

There are a pair of objections that can, I think, be dismissed rather quickly, namely, those of M. C. Bradley and P. Wilson.[1] Bradley in "How to Never Know What You Mean" argues that if the ITT is correct then we have no knowledge of our own mental states and, as we have such knowledge, the ITT must be incorrect.[2] However, the second premise of this argument simply comes to the dogmatic assertion of the falsity of something Quine holds to be correct. (In fact, it is a conclusion Quine draws more or

less explicitly.) Likewise Wilson's rather obscure claim in "Quine on Translation" that good translation must "preserve belief" and that this requirement assures determinate translation overlooks the fact that Quine is arguing against just such a simple minded notion as the claim that there is a right or wrong in the attribution of beliefs to others. Wilson simply assumes the determinacy of belief ascriptions thus rendering his remarks question begging.

Similar objections have been raised by L. J. Cohen in his book <u>Diversity of Meaning</u>. Cohen claims that Quine employs a "comparatively sterile" way of testing stimulus meaning hypotheses. Cohen suggests if we employ what he calls a "purpose and function" theory of meaning we can test analytic hypotheses."[3] If we employ a certain analytic hypothesis and are able to get along in the native culture, carry out our purposes and obtain the objects of our intentions, we will have confirmed our analytic hypotheses by showing that they work. (This is, I think, the sort of objection that might well occur to a "naive Wittgensteinian".) This procedure, however, simply assumes that talk of purpose and intention is itself not indeterminate. Quine's rather opaque views on the mental are hardly transparent, but it is fairly clear that he does not regard such purported mental states as intentions

and purposes as part of the determinate furniture of the universe. (This metaphor is to some extent cashed in Chapter V.) Quine might well also assert in reply to Cohen that we could "get along" in the native culture with a number of nonequivalent analytic hypotheses. With this in mind, let us have a brief look at Quine's views on mental states and intentional words:

> For, using the intentional words 'believe' and 'ascribe' one could say that a speaker's term is to be construed as 'rabbit' if and only if the speaker is disposed to ascribe it to all and only the objects that he believes to be rabbits. Evidently then the relativity to nonunique systems of analytic hypotheses invests not only translational synonymy but intentional idioms generally. Brentano's thesis of the irreducibility of intentional idioms is of a piece with the thesis of indeterminacy of transla-tion....to accept intentional usage at face value is, as we saw, to postulate translational relations as somehow objectively valid though indeterminate in principle relative to the totality of speech dispositions. (Word and Object, pp. 220-221)

Critics of Quine are then involved in vicious question begging if they assume determinate intentional or belief ascription usage. I belabor these perhaps pedestrian objections to the ITT because a large number of those who reject the ITT have in fact simply assumed the determinacy of intentional idioms.

I wish now to turn to a consideration of certain objections raised by Landesman in a paper that has already had some influence, "Skepticism About Meaning: Quine's Thesis on Indeterminacy".[4] Landesman, among other things,

claims that certain introspective or psychological data can be used to override the ITT. He says "it is an interesting question whether or not the existence of translational indeterminacy depends essentially upon the restriction to behavioral evidence".[5] Landesman asks us to consider the sort of evidence introspection could provide us for translational hypotheses. Landesman introduces this consideration by making a distinction between what he calls "spatial pointing" and "mental pointing". Landesman wishes to argue that if one can use mental pointing in testing analytic hypothesis one may be able to escape the ITT. Those unfamiliar with Word and Object, Chapter II can perhaps garner a fairly clear understanding of what an analytic hypothesis is from the following:

> We have had our linguist observing native utterances and their circumstances passively, to begin with, and then selectively querying native sentences for assent and dissent under varying circumstances. Let us sum up the possible yields of such methods. (1) Observation sentences can be translated. There is uncertainty, but the situation is the normal inductive one. (2) Truth functions can be translated. (3) Stimulus analytic sentences can be recognized. So can sentences of the opposite type, the 'stimulus-contradictory' sentences, which command irreversible dissent. (4) Questions of intrasubjective stimulus synonymy of native occasion sentences even of non-observational kind can be settled if raised, but the sentences cannot be translated.
> And how does the linguist pass these bounds? In broad outline as follows. He segments heard utterances into conveniently short recurrent parts, and thus compiles a list of native 'words'. Various of these he hypothetically equates to English words and phrases, in such a way as to conform to (1)-(4). Such are his analytic hypotheses, as I call them. (Word and Object, p. 68)

Landesman quotes the following passage from <u>Word and Object</u>

for subsequent criticism: "Point to a rabbit and you have

pointed to an integral part of a rabbit, to a rabbit

fusion, and where rabbithood is manifested".[6] Landesman

claims that this is true only in an unusual sense of

pointing:

> it is the sense of 'pointing' in which we say that
> the branch on a certain tree is pointing at the sun.
> When a person points at something, we can follow the
> line of his finger until we strike an opaque object;
> the point at which the object meets the line Quine
> calls the ostended point. Let us stipulate that an
> object is being spatially pointed at if it is in the
> vicinity of the ostended object.[7]

Let us pause here to see if the Quinian native spatially

pointing is just like a tree pointing at the sun. Perhaps

we are being led astray at the start by this claim. If we

attempt to translate the speech of the native we already

no doubt have good reasons for thinking that he is a per-

son and hence we know that if we can translate his

sounds and gestures into our language we will most likely

be much helped in our dealings with him. We, of course,

take the physical posturing of the native as something

that might help us on this project. It is also the case

that we have not in the recent past found such procedures

helpful in our dealings with trees.[8] The difference in

our attitude toward the "spatial pointing" of the native

and the tree is simply a reflection of the differences in

a priori attitude we take in our encounter with trees and

persons. As stated, this <u>may</u> not seem to be a matter of large import but insofar as it seems to foist a stupidity on the Quinian radical translator it should be caviled at.

Landesman's position on mental pointing is seen in the following:

> Consider the case now of two persons who at the same time are both spatially pointing at a bright green chair. One says 'Look at that chair' and the other says 'Look at that bright green color'. When each is asked what he is pointing at the first answers 'the chair' and the second 'the color'. Though they are spatially pointing at the same thing, they are, we shall say, <u>mentally pointing</u> at different things. Roughly speaking a person is mentally pointing at an object x provided he thinks x is manifested in the vicinity of the ostended point and he is spatially pointing at something with the intention of calling someone's attention to x.[9]

First, Landesman's account here is inadequate as there seems to be no reason for not treating as a case of mental pointing cases in which someone points out something to someone else, where all involved are aware that everyone's attention is already directed at the object in question. (In other words, I can mentally point at a tree without having the intention of thereby calling your attention to it. Perhaps ceremonial introductions and such might serve to get us illustrative counterexamples here.) Thus Landesman's definition is at least too narrow to fit all of the cases that intuitively seem to be cases of mental pointing. (I rather expect that it could also

be shown to be too broad, if one were to make a steadfast
effort to think up a counterexample.)

Landesman now moves, after setting out his definition
of mental pointing to argue that if we can appeal to
evidence gained from our knowledge of the objects of mental
pointing we can get evidence that might allow us to over-
come the ITT. (1) If behavioral evidence relative to
which the indeterminacy exists is all the evidence there
is, then, since different mental pointings are behaviorally
indistinguishable, it is incorrect to cite them as helping
to determine the meaning of, say 'Gavagai'. (2) If we had
some reason to think that the native was mentally pointing
at a rabbit and not a rabbit stage or fusion, we would
have some reason to think that 'Gavagai' means 'rabbit'.
(Conclusion) Mental pointing may serve to show that behav-
ioral evidence is not all the evidence there is. Landesman
states the conclusion in this way; "It seems to me that
some indeterminacies depend essentially upon the restriction
of the evidential base to observable behavior untainted by
hypotheses about underlying mental states."[9] Landesman
cites as evidence that Quine holds the antecedent of (1)
the following passage from Word & Object:

> All the objective data he (the linguist) 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.[10]

It should be fairly clear that if we consider the account of mental pointing quoted on page   above we can see that mental pointing is not going to help us overcome the ITT. This is because we have no good reason to hold that the person in question is such that there is a determinate matter as to whether or not he "thinks x is manifested in the vicinity of the ostended point", hence it is of no purpose to cite mental pointings in criticism of the ITT.

The antecedent of (1) is undisputedly a Quinian thesis and the claim that different mental pointing are behaviorally indistinguishable is not one I here object to.[11] The bone of contention, as Landesman would agree, is whether or not we have good reasons to think that the native was mentally pointing at a rabbit and not a rabbit stage. How does one obtain, and what are, good reasons for thinking that the native was mentally pointing at a rabbit and not a rabbit stage? These must be good reasons available to us without the adoption of analytic hypotheses. Further they must be available before we have made sense of the native's pattern of inferences and beliefs and before we have settled on an account of his ontological commitments. Mental pointing, if it is to serve the function Landesman desires, must be evidence for these matters, not evidenced by them. Landesman suggests that we let the linguist become bilingual, so that

he comes to think in native terms. (One should note
again that Landesman assumes that "thinking in native
terms" is a determinate matter.) "Now he glimpses a
rabbit; he utters 'Gavagai', and presumably he knows
whether or not he means rabbit".[12] This is much like
the Bradley objection.) Landesman is not, of course,
unaware of Quine's remarks on bilinguals:

> When as a bilingual he finally turns to his
> project of a jungle-to-English manual, he will
> have to project analytic hypotheses much as if
> his English personality were the linguist and
> his jungle personality were the informant: the
> differences are just that he can introspect his
> experiments instead of staging them. (Word and
> Object, p. 71)

Landesman next claims that Quine's rejection of introspec-
tion as a source of evidence for translation depends upon
a pernicious verificationism inconsistent with other views
of Quine and implausible in itself. As I attempt to show
in the next chapter, this claim is at least a serious
exegitical error but I wish now to look at Landesman's
charge as it relates to our purported knowledge of the
mental. Landesman claims that knowledge of the mental as
this relates to our knowledge of the object of our mental
pointings is compatible, assuming one is bilingual, with
both Quine's remarks on bilinguals and the ITT. Landesman
argues in criticism of the rejection of introspective
evidence by arguing that Quine needs an argument like the

following:

1. The evidence the bilingual has as to the meaning of 'Gavagai' does not entail that the native means 'rabbit' and not 'rabbit stage'.

2. If the evidence does not entail that the natives mean one thing and not another by 'Gavagai' then it is logically possible that they mean 'rabbit' or 'rabbit stage', etc.

3. If it is logically possible that the native means any of several things then there is no fact of the matter as to what the native means.

Conclusion. There is no fact of the matter as to what the native means. Landesman then procedes to argue in criticism of the argument he says Quine has to offer.[13] One should note that Landesman is not saying that Quine offers this argument but rather that Quine needs an argument like this to make good on the ITT. Briefly put, Landesman is claiming that Quine is depending on the following thesis: if it is logically consistent with our evidence that two different hypotheses are true then neither is in fact true or false. Otherwise put, where no conclusive evidence is available or is in principle obtainable concerning some subject matter, there is no fact of the matter in question.

Recall that what is needed to make Landesman's case is introspective evidence available to us independently of

analytic hypotheses. Let us then consider what Quine says about bilinguals, in order that we may assess Landesman's argument. First, Landesman's bilingual has to learn the language just as the natives do. In order to accomplish this he has to, assuming that he is not already in such a condition, turn into the possessor of an infantile mentality and then learn the language from the ground up. This is, needless to say, not something the radical translator is going to be able to accomplish, thus on its most interesting construal no introspecting bilingual will ever be found. Let us, for the sake of argument, consider what sort of "introspective knowledge" that a child raised from birth as a bilingual would have. If the child were simply taught languages x and y he is not thereby taught the correlations of expressions in the two languages. If he is taught that two expressions in the languages translate each other then he is depending on the analytic hypotheses of his informant. If he simply figures out for himself that two expressions are translations of each other then he has constructed for himself analytic hypotheses and his introspective claims will reflect this. Quite the same sort of reply can be given to those who bring up such recherche cases as "teaching" someone a language via brain surgery, rays from outer space, etc. A person who learns two languages from

birth is no better off with respect to introspective

evidence available independent of analytic hypotheses than

is any other bilingual. The linguist becomes bilingual

by helping himself with all sorts of analytic hypotheses

not available to the infant. The introspector then has

no reason to think that he is going to get the kind of

evidence that Landesman needs.

> Now, of course, the truth is that he would not
> have strictly simulated the infantile situation
> in learning the native language, but would have
> helped himself all along the way; thus the elements
> of the situation in practice may be pretty inex-
> tricably scrambled. What with this circumstance
> and the fugitive nature of the introspective
> method, we would have been better off theorizing
> about meaning from the more primitive paradigm:
> that of the linguist who deals observably with the
> native as live collaborator rather than first
> ingesting him. (Word and Object, p. 71)

The knowledge of the bilingual is then just the result

of his having adopted analytic hypotheses. The weakness

of these introspected results is not just the normal

underdetermination of induction but rather it is that the

analytic hypotheses and their introspective "confirmation"

cannot go wrong.

> But in any event the translation of a vast range
> of native sentences, though covered by the semantic
> correlation, can never be confirmed or supported at
> all except in a cantilever fashion: it is simply
> what comes out of the analytic hypotheses when they
> are applied beyond the zone that supports them.[14]
>
> Most of the semantic correlation is supported only
> by analytic hypotheses in their extension beyond
> the zone where independent evidence for them is

> possible. That those unverifiable translations
> procede without mishaps must not be taken as
> pragmatic evidence of good lexiography, for
> mishap is impossible. (Word and Object, p. 71)

One can perhaps get the feel of why this introspective

data can provide no confirmation for analytic hypotheses

by considering the following fable. One comes to a

certain point in a quest where one arrives at a place

where he must choose one of several alternative routes.

He picks one and has no trouble walking down it and he

concludes that this is pragmatic evidence that he has

taken the right path. Further, it might be that there

are certain places any journey has to pass and the chosen

path takes him past all these points. The problem with

his conclusion is that there may be several paths that

allow one to do this, thus there is no fact of the matter

as to which path is right even though the paths are

distinct. The radical translator's roads to introspective

knowledge are cleared by his prior adoption of analytic

hypotheses, hence the unimpeded translation is no more

evidence than the unimpeded walking was.

Once analytic hypotheses are adopted it does indeed

clear the road to introspective knowledge. Even if one

fails to observe the constraints on analytic hypotheses

(set out in the citation from Word and Object on pages

above) and adopts some seemingly outrageous set of

hypotheses upon adoption this to will be "confirmed" by any introspecting bilingual who employed it. (To pursue the fable a bit further, so ignoring the restraints on analytic hypotheses would be like ignoring any of the various paths and striking off through the wilderness.) We can see then that the bilinguals engaging in an act of mental pointing are engaging in an act whose object is itself indeterminate. (One should note that it is of course possible for one to become bilingual without the adoption of any analytic hypotheses, perhaps a child who grows up in a household where two languages are continually spoken might at some point in his development be able to use both languages without having any ideas concerning the translation of one into the other. Such cases will not, of course, be of help to the critic of the ITT.)

Landesman has a second strategy which can be stated fairly simply. He divides the ITT into three separate "parts" and argues that Quine has refuted "intuitive semantics"[16] only if certain entailment relations between the three parts of the ITT obtain. Landesman argues both that the relations do not hold and that the claim that they do is inconsistent with certain of Quine's basic doctrines. I shall first present the argument in bare skeletal form and then consider in more detail the intermediate steps in the arguments as well as the

considerations that Landesman offers in defense of the
premises of the argument. Landesman's three parts of the
ITT are:

I. Many different analytic hypotheses are compatible with
all behavioral evidence.

II. The behavioral evidence relative to which translation
is indeterminate is all the evidence there is.

III. There is no objective matter for translation to be
right or wrong about--glossed by Landesman as asserting
that propositional and mentalistic theories of meaning
are false.[17] Landesman's argument is as follows:

    1. I does not entail III.

    2. II does not entail III.

    3. I and II together do not entail III.

    4. III is itself the very conclusion Quine wishes to
establish, hence it cannot be cited.

Conclusion. Even if true none of the three parts of the
ITT can either individually or collectively be cited as
conclusive reasons for rejecting mentalistic theories of
meaning or intuitive semantics. One should note that all
of this turns upon the claim that I, II, or I and II
must entail III if they are to function in making the ITT
plausible. To state the perhaps obvious, if Landesman
is merely saying that I and II respectively do not make
it necessary that the ITT is true this would be completely

trivial and no threat to the ITT. A more interesting question is whether I and II entail III and further if they do not is this in itself a threat to the holder of the ITT?

Let us consider first Landesman's argument for premise 1. Landesman holds that there being an indeterminacy of translation relative to all the behavioral evidence there is "leaves open the possibility that the indeterminacy is one of knowledge and not reality."[18] Landesman says that it may well be the case that non-behavioral evidence will give rise to determinate translation. Hence it is logically possible that I can be true and III false. With respect to premiss 2, if all the evidence that there in principle could be does not entail that there is determinate translation then it is still logically possible that there could be determinate translation. This is again a rather trivial and probably noncontroversial claim. Likewise even I & II together do not entail III. Landesman says that the only way the argument he sets up for Quine can work is if we assume the truth of a verificationist principle such as "where all evidence there could possibly be fails to determine which of the two theories is correct which then it makes no sense to speak of correct or incorrect".[19]

There appear to me to be several things wrong with

Landesman's argument. Firstly, given the emphasis Landesman puts upon this, he apparently thinks that the only evidential relation that could hold between I and/or II and III is that of entailment. However, this assumption is itself incompatible with Quine's views on justification of theories. Landesman considers no other possibility. A ten hour search of a small room for a hundred-pound bull dog which comes up with no evidence for his existence does not entail that there is no bull dog. As parallels Landesman's remark in premise four it is not proper for the person who claims that there is no bull dog in the room to cite this as evidence that there is no bull dog. (There are no doubt some persons who would regard this as a misuse of the word 'evidence'.) Likewise neither an inability to find any evidence on which to base determinate translation nor the fact that translation is indeterminate with respect to all the legitimate evidence there could possibly be itself entails that translation is indeterminate. However, in the case of the bull dog, the person who said that the reasonable thing to think was that there was no bull dog would be rightly unimpressed with the person who pointed out that all the evidence there could in principle be does not entail the nonexistence of the bull dog. (Though I leave to others the making up of a story that might illustrate

this claim.) It might be remarked that there is a relevant disanalogy here, namely we expect to be able to see bull dogs whereas we do not expect to be able to literally see that a translational hypothesis is the correct one. This is true, of course, the problem with citing it here is that we do expect the totality of observations in space-time to give us evidence as to what the correct translational hypothesis is, assuming that there is one. Thus, it is not, it appears to me, relevant to point out this disanalogy here.

I wish now to briefly consider certain arguments of John Wallace and D. C. Dennett which, if correct, go a long way toward showing that critics of the ITT cannot simply assume the mental as determinate or cannot even assume that the two issues are independent of one another. If this can be rendered plausible then it shows that my earlier claim that this was the case does not simply rest on the ad hominem that Quine holds that the mental is indeterminate and that therefore one cannot assume that the mental is determinate in offering criticisms of the ITT.

Wallace in "A Query on Radical Translation" argues that if translation is indeterminate then nerve pattern equivalence of both and inter and intrapersonal sort are

indeterminate.[20] Wallace attempts to make out this claim

by showing that certain crucial notions employed by Quine

in stating when certain stimulations and physical patterns

are the same area of a translational character (and notion

to be shortly set out) and are hence by Quine's standards

indeterminate.

Wallace is not very clear about what exactly a

relation of "transalational character" is, simply offering

as a paradigm of such a relation translation.[21] Transla-

tion is, on Wallace's account a six-place relation:

> expression S1 in the mouth of person P1 at time T1
> translates as expression S2 in the mouth of person
> P2 at time T2 (S1, P1, T1, S2, P2, T2). The rela-
> tion cannot be analyzed, at least not obviously
> (without using propositions), as a logical
> compound of relations with fewer argument places.[22]

A relation of translation character is evidently then a

six place relation in which two of the places are for

persons and two for times and two for events in persons

where the claim that one of the events is the same as

(same type of mental event, expresses same meaning) the

other is not decidable on the basis of mere similarity

of physical patterns. (One should note that the only two

cases Wallace offers, maybe they are the only cases, fit

this account of the matter.)

Quine's approach to translation uses an intrapersonal

notion of same sentence: "expression S1 in the mouth of

person P1 at time T1 is the same as expression S2 in the

mouth of person P1 at T2".[23] As evidence for this claim

Wallace cites the following definition of stimulus meaning:

> a stimulus * belongs to the affirmative stimulus
> meaning of a sentence S for a given speaker if and
> only if there is a stimulating *' such that if the
> speaker were given *', then were asked S, then were
> given *, and then were asked S again, he would
> dissent the first time and assent the second.[24]

If this is to be a relation of translation character, it

must not be the case that "asked S again" refers to a

sameness of physical pattern. Wallace then cites the

fact that we may be dealing with the kind of cycling-

alphabet case considered by Wittgenstein in Zettel to show

that this is so:

> We could imagine a language in which the meanings
> of expressions changed according to definite rules,
> e.g.: in the morning the expression A means this,
> in the morning the expression A means this, in the
> afternoon it means that. Or a language in which the
> individual words altered every day; each day the
> letter of the previous day would be replaced by the
> next one in the alphabet (and Z by A.)[25]

The point here being that one could well adopt translation

manuals where "asked S again" cannot be explicated by

reference to similarity of physical patterns; in fact the

sort of translation manuals we would adopt to deal with

persons with such cycling alphabets would rule such a

practice out. Observation sentences are defined by employ-

ing, among other things, the notion of same stimulation

where the relevant notion of same stimuli depends on

interpersonal comparison of patterns of stimulation.

Wallace cites as textual evidence a passage from "Epistemology Naturalized":

> ...an observation sentence is one on which all speakers of the same language give the same verdict when given the same concurrent stimulation.[25]

There are also several passages in <u>Word and Object</u> that one could cite as support for Wallace's claim:

> occasion sentences whose stimulus meanings vary none under the influence of collateral information may naturally be called observation sentences. (<u>Word and Object</u>, p. 42)

> For, in behavioral terms, an occasion sentence may be said to be the more observational the more nearly its stimulus meaning for different speakers may be said to coincide. (<u>Word and Object</u>, p. 43)

> We have defined observationality for occasion sentences as degree of constancy of stimulus meaning from speaker to speaker. (<u>Word and Object</u>, p. 43)

(I cite the above passages as the primary text I am herein dealing with from the Quinian corpus is <u>Word and Object</u> and also because the paper Wallace refers to was published eight years after <u>Word and Object</u>.) Occasion sentences are

> sentences such as 'Gavagai', 'Red', 'It hurts', 'His face is dirty', which command assent or dissent only if queried after an appropriate stimulation (<u>Word and Object</u>, pp. 35-36)

Interpersonal correlation of nerve patterns is then a relation of translational character as two of the places are for persons (who need not, of course, be different persons), two of the places are for times, and two of the places are for events in (or actions by in the case of

correlation of spoken expressions) persons which cannot
be correlated on the basis of similarity of physical
patterns. The correlation of neural events involves
relatively long term particulars, persons, and relatively
short lived occurrences in or brought about by persons.
This relationship is, as stated, a translation relation
and is hence indeterminate if translation is indeterminate.
Simply put, if translation is indeterminate and if the
interpersonal correlation of nerve pattern is a transla-
tional relation then the interpersonal correlation of
nerve pattern equivalences is indeterminate. A bit more
should be said as to why it is that translation relations
give rise to indeterminacy. Recall that the indeterminacy
claimed to hold in the case of the indeterminacy of nerve
pattern equivalences is between an event in a person and
an event in another person (or the same person at
different times). An indeterminacy arises if there is
more than one item that could according to available
evidence count as equivalent to another. Now, on the
grounds of the reasons offered earlier, correlation of
nerve pattern equivalence is such that this is the case.
The reason that translational relations can (not must)
give rise to indeterminacy of correlation is that they call
for treated as having the proper sort of equivalence items
that cannot be identified by mere sameness of physical

patterns and further require that vast amounts of items
(nerve patterns, words) be correlated as wholes.

To illustrate this a bit, let us isolate a given bit
of nerve pattern in a person X and indicate why the
equation of that stimuli with the nerve patterns of another
person Y (or X at another time) is indeterminate. The
idea at work here is that nerve pattern equivalence is
part of a systematic equation of items, i.e., we do not
and could not simply look at X and Y at one isolated
point and determine then and there that such and such a
nerve pattern in one of them was to be equated with such
and such a nerve pattern in the other (i.e., because we
need to look at them in terms of cause and effect).
Further, there are differing systems of equations of
nerve patterns that could be set up which would count
different nerve patterns as equivalent to one another.

Wallace's most succinct statement at his commitment
to the connections between the ITT and the indeterminacy
of the mental occur in the following passage.

> It might be objected that to demand analyses of
> linguistic community, nerve-pattern equivalence
> and intrapersonal translation, which are indeter-
> minate if translation is, is pointless; to do so
> makes the picture extremely complicated, and the
> upshot can only be to make interpersonal transla-
> tion even less determinate.[26]

Wallace, by the way, thinks one has to do all three of
these things in order to develop a theory of radical

translation.

Dennett, for the purposes at hand, can be seen
as offering arguments to show that the mental is indeter-
minate independently of the ITT. The relevance of the
material from Content and Consciousness[27] can perhaps
be most quickly displayed by looking at an example Dennett
considers. We take our unfed and presumably hungry bull
dog Mary and give her a beefsteak and Mary gathers some
straw together in the shape of a bird's nest, places the
steak in the middle of the nest, and procedes to sit on
the beefsteak. Assume also that we have extensive data
on Mary's neural states. Afferent state A is the normal
outcome of afferent analysis[28] when Mary discriminates
food but this time it leads to bizarre action. Given
that her behavior is so bizarre no candidate for the
content of A is supported. Mary's action is appropriate
to many different ascriptions of content to A, perhaps
limited only by our imagination and endurance in thinking
them up. Dennett suggests as possible contents of A:
the beefsteak is an egg and Mary is a hen; this is beef
but if you pretend it is an egg you will get twice as
much tomorrow; it's worth starving to throw confusion into
these psychologists; and sitting on beef improves its
flavor. Dennett from the intuitive base provided by the
above (I will shortly attempt to explain why it does

not matter that this involves perhaps very odd behavior,
Dennett employs an odd case largely to make his argument
vivid seeming) case can be for the purposes at hand be
seen as offering the following argument.[29]

1. Any behavior will be appropriate to a wide variety of
beliefs and desires.

2. Only the afferent source can determine the correct
hypothesis as to the afferent state A (in Mary).

3. The afferent source will favor one hypotheses over others
only in the event that the behavior is appropriate to
only the condition of the source.

4. In the case of Mary (and other cases where the matter
is merely less marked) the requisite appropriateness is
not present, hence no content can be ascribed as a deter-
minate matter.

Conclusion. No intentional description is determined as
correct by the afferent analysis as all cases can be
treated as the Mary case given enough imagination in the
ascription of beliefs. (A word more on this can be given
by setting out a case quite different from the Mary case,
one in which the behavior in question is quite normal.
If in this case we can also come up with different contents
for the agent's afferent state then it will be clear that
nothing untoward was done by employing the Mary case.
We take our unfed and presumably hungry neighbor Jane to

a hamburger stand and give her a hamburger and Jane discriminates the hamburger as food, Jane eats the hamburger. Now, Jane's behavior is appropriate to many ascriptions of content to her A: that this is a hamburger which will satisfy my hunger, that eating hamburgers is a sign of one's social sophistication, that eating a hamburger here will induce the person buying me the hamburger to give me $100, that it would be bad manners to refuse to eat a hamburger someone bought for us, etc. Some of these involve attributing perhaps "unusual" beliefs, etc. to Jane, some of them involve only quite prosaic suggestions. Further, we regard some of the attributions as perhaps outrageous, but it is because it does not fit in with all the other such content ascriptions we have made. Thus, when one begins the process of afferent analysis, no such notion of bizarreness will be at hand to rule out content ascriptions. Until we have completed afferent analysis we cannot give a determinate content ascription to Mary's neural states and we cannot complete afferent analysis until we know what her beliefs are. The attempt to ascribe determinate content to mental states is thus blocked by a vicious circle. It is useful here to consider an objection that one might raise at this point. It might well be objected that the above argument can establish no more than showing that we could never have known what the

determinate content of mental states are (because of the

vicious circle and the number of alternative contents)

but it still might be the case that in fact one ascription

of content is determinately correct. Now, the reply to

this is that we can, for the time being, concede the point,

for all we need here is the claim that we could not appeal

to such knowledge on support of translation hypotheses.

The objection, even if it is correct, is then besides the

point.

In this chapter I have attempted to show that one can-

not appeal to any purported knowledge of the mental in

criticism of the ITT. Further, in order to remove the

appearance of mutual question begging I have tried to show

that there are good reasons for thinking that the mental

is indeterminate without appeal to the ITT. I turn now

to a consideration of another group of objections to the

ITT.

Footnotes

[1] Bradley, M. C., "How Never to Know What You Mean",
LXVII (1970); "The object of the preceding has been to
argue that the reductio ad absurdem of his position that
Quine considers, only to dismiss, is after all what it
purports to be. But I would like to say that I think
the way out of the impasse is. In pressing the regress
I used such expressions as 'understand the sense and the
reference', 'intend sentences in a certain way', 'grasp
of the language'. It seems to me that the plain deliver-
ances of introspection is that we do perform mental acts
answering to these expressions, that they guarantee that
the language we use is not intentionally or extensionally
inscrutable, and that therefore meaning (a term used in a
certain way) is, contrary to Dewey, a psyche experience,
that recent criticisms notwithstanding, a private language
is possible and that we have in that possibility just
another route into the Other-Minds problem." (p. 124)
     Wilson, Patrick, "Quine on Translation", Inquiry VIII
(1965), 198-211: "But if he [the Martial linguist] can
say in Martian what he is doing, then, whether or not he
has fixed on translation of syncategormata, he can tell
what various words are true of, at least when used by him;
and that he has evidence that what he refers to, and thus
what words are true of is one way rather than another only
shows that there is an inside track to knowledge as well
as an outside track." (202-203) "Were this to happen
(two bilinguals differ on a translation-my introjection)
it would be agreed that one of the translations must be
incorrect, for translation must 'perserve belief', must
result in a form of words which expresses intuitively the
'same belief '; hence at least a belief." (p. 207)

[2] Kripke expresses (though not in a way that can be
taken as clearly endorsing it) such an attitude in the
discussion sections of the Conn. Conference on Translation
and Indeterminacy, reprinted in Synthese, XXVII (1974),
481.

[3] L. J. Cohen. The Diversity of Meaning (London:
Mentheun, 1962), 67-74. My criticsms of Wilson and Cohen
are influenced by Robert Kirk, "Translation and Indeter-
minacy ", Mind LXXVIII (1969), 321-341. Incidentally,
Kirk acknowledges that the criticism he offered in the
above paper fails in a more recent paper; "undetermination

of Theory and Indeterminacy of Translation", Analysis, XXXIII (1973), 195-201.

[4]Landesman, Charles. "Skepticism About Meaning: Quine's Thesis of Indeterminacy", Australasian Journal of Philosophy, XLVIII (1970), 321-337. See also Harry Beatty's "Behaviorism, Mentalism, and Quine's Indeterminacy Thesis", Philosophical Studies, XXVI (1974), 97-110 in which Beatty argues that the restriction to behavioral evidence is not at all needed in the argument for the ITT.

[5]Landesman, 331.

[6]Landesman, 333, Landesman quotes from Word and Object, 52-53.

[7]Landesman, 333.

[8]If W. Sellars' account of the development of what he calls the "manifest image" is correct then there was a time when we found such a procedure useful. It is not outrageous to suggest, indeed it has been suggested, that there will come a time when we no longer need such procedures with persons, see W. Sellars, "Philosophy and the Scientific Image of Man", in Science, Perception and Reality, (New York, 1963), 1-41, esp. 6-14.

[9]Landesman, 333-334.

[10]Landesman, 326; Word and Object, 28.

[11]See Beatty article for related material.

[12]Landesman, 334.

[13]Landesman, 334.

[14]Quine, W. V. "Meaning and Translation", in The Structure of Language: Readings in the Philosophy of Language, ed. by J. Fodor and J. Katz (Englewood Cliffs, N.J.: Prentice-Hall, 1964), pp. 460-479.

[15]After all, should Dorothy conclude that she took the right fork in the road merely from the fact that the road she took led her to her destination?

[16]See Landesman for background.

[17]Landesman, 1., 323-324; 2., 326; 3., 331.

[18]Landesman, 333.

[19]Landesman, 333.

[20]Wallace, John, "A Query on Radical Translation", Journal of Philosophy, LXVIII (1971), 143-151.

[21]Wallace, 144.

[22]Wallace, 144.

[23]Wallace, 146.

[24]Wallace, 146, Word and Object, 32.

[25]Wallace, 146-147.

[26]Wallace, 149.

[27]Dennett, D. C. Content and Consciousness (New York: Humanities, 1969). See Content and Consciousness for background information.

[28]A fairly clear idea of Dennett's notion of afferent analysis can be found in the following passage from Chapter IV of Content and Consciousness. "For a start it is clear that for any system to be called Intentional it must be capable of discriminating and reacting to fairly complex features of its environment (e.g., external physical objects and not just changing conditions--temperature, contact, pressure--on its outer surface), and for any system to do this it must be capable of interpreting its peripheral stimulation. That is, it must be capable of producing within itself states or events that normally co-occur with generalized conditions of objects within the system's perceptual field. I do not think that this is a formal requirement for any Intentional system so much as one that is designed to satisfy our intuitions; no system that lacked this capacity could engage its environment in ways interesting and sophisticated enough to make it plausible to say that it had beliefs, desires, intentions-- even if in the end we could find no logically necessary trait for, say, belief, that the system lacked.

[29]Dennett, 82-90.

CHAPTER IV

VERIFICATION AND THE ITT

A very large amount of the resistance to the ITT in
current thinking on the matter is based on the claim that
Quine's argument for the ITT depends on some sort of
verificationist principle. Further, and these two things
are more than accidentally related, a large part of the
misunderstanding of Quine's philosophy involves the facile
assumption that Quine rejected all forms of verification-
ism in "Two Dogmas of Empiricism"[1] and would hence be in-
consistent were he to employ any version of a principle
of the following sort: "Where there is no way in principle
to establish claims about Xs, there is no fact of the
matter about Xs". There are varying verificationist
principles attributed to Quine in the literature (and
doubtless a much larger spoken opinion) as well as a number
of papers where Quine is charged with being a verificationist
of some unspecified sort.

There are two tasks one must undertake in order to
evaluate this criticism. One must first see if Quine does
indeed reject all forms of verificationism, there being
forms of verificationism (as we shall see) that one could

hold ahd agree perfectly with everything in "Two Dogmas of Empiricism". I shall herein then be attempting to support the claim that Quine is in fact a verificationist. This matter will be considered in some detail and support will be offered from a variety of texts. In view of the confusion on this matter exhibited in the literature and because of the large place Quine's work occupies in contemporary philosophy this matter, it seems to me, is well worth much more attention than is normally given to exegetical claims about contemporary philosophers.[2] Secondly, given that we are in fact able to show that Quine does hold verificationist principles we need to consider whether or not the principle Quine uses is plausible. I shall herein attempt a preliminary defense of the sort of verificationism I attribute to Quine which draws on a consideration of Quine's views on language learning, evidence, and theories. (In Chapter VIII a more extensive defense of a verificationist principle is given.) As parallels my strategy in Chapter III, I shall try to do two things. I shall attempt to show that the rejection of the ITT on the grounds that it involves verificationist assumptions begs the question here as Quine's pragmatism, central to his whole orientation, is very much tied up with verificationist assumptions. To assume the unsoundness of the version of empiricism set out at the

close of "Two Dogmas of Empiricism". Secondly, I shall attempt to show that this matter is not a case of mutual question begging but that Quine has offered, though in an oblique way, reasons for thinking verificationist principles plausible.

As stated, it is here necessary to go back to the beginning with a consideration of what Quine did reject in "Two Dogmas of Empiricism". (The widespread assumption that all forms of verificationism were repudiated in "Two Dogmas of Empiricism" makes it here essential, apart from other reasons, to return to such supposedly well worn ground.) Quine was concerned in "Two Dogmas of Empiricism" to refute two dogmas. One was a belief that there is fundamental cleavage between analytic truths and synthetic truths. (See "TD", p. 20) "The other dogma is reductionism: the belief that each meaningful statement is equivalent to some logical construct upon terms which refer to immediate experience". ("TD", p. 20) Clearly one could deny this empiricist dogma and maintain that where there is no way in principle to determine the truth concerning a subject there is no fact of the matter.

Quine begins his consideration of verificationist theory of meaning in order to show that an account of synonymy cannot be given by saying statements are

"synonymous if and only if they are alike in point of

method of empirical confirmation and information". ("TD",

p. 37)

The so called "verification theory of meaning is

explained as follows:

> The verification theory of meaning, which has been con-
> spicuous in the literature from Peirce onward, is
> that the meaning of a statement is the method of em-
> pirically confirming or infirming it. An analytic
> statement is that limiting case that is confirmed no
> matter what. ("TD", p. 37)

Some persons have held, as Quine says, that if the

verification theory of meaning set out above can be

accepted the notions of analyticity and synonymy can be

saved. Quine begins his consideration of the verifica-

tionist theory of meaning by asking what these methods of

confirmation and information could be. ("TD", p. 38)

A naive empiricist view claims that every statement

can be translated as a whole onto a sense datum language.

Such radical reductionism "sets itself the task of

specifying a sense datum language and showing how to

translate the rest of significant discourse, statement by

statement, into it". ("TD", p. 39) The problem is that

these reductionists, Carnap being the only one who

seriously tried to work the program out, were embarked on

a project that was destined to fail.

> Carnap did not seem to recognize, however, that
> his treatment of physical objects fell short of

reduction not merely through sketchiness, but in
principle. Statements of the form "Quality q is
at the point instant x;y;z;t" were, according to
his canons, to be apportioned truth values in such a
way as to maximize and minimize certain overall
features, and with the growth of experience the
truth values were to be progressively revised in
the same spirit. I think this is a good schemati-
zation (deliberately oversimplified, to be sure)
of what science really does; but it provides no
indication, not even the sketchiest, of how a
statement of the form "Quality q is at x; y; z; t"
could ever be translated into Carnap's initial
language of sense data and logic. The connective
'is at' remains an undefined connective; the canons
counsel us in its use but not its elimination.
(TD, p. 40)

This radical reductionism is not held by many today but

it does linger on to influence the thought of empiricists.

"The dogma of reductionism survives in the supposition

that every statement, taken in isolation from its fellows,

can admit of confirmation or infirmation at all."

("TD", p. 41) Quine, as is well known, claims that our

"statements about the external world face the tribunal of

sense experience not individually but only as a corporate

body". (TD, p. 41) Quine does not really provide an

explicit argument for this claim in "Two Dogmas of Empiri-

cism" but rather says that when our experience conflicts

with our beliefs there are many different ways we could

modify our views to account for this. "But the total

field is so undetermined by its boundary conditions,

experience, that there is much latitude of choice as to

what statements to reevaluate in the light of any single

contrary experience". ("TD", pp. 42-43) The general claim

then is that if the verificationist theory of meaning

were correct then on all those occasions when we have an

experience that conflicts with our beliefs we would be

required to modify those particular beliefs (or drop from

our theory those sentences) which were confirmed or in-

firmed by the experience in question. The contrary claim

is then that given the large number of ways we could

modify our beliefs (or alter the sentences of our theory)

there are no specific alterations required as the verifi-

cationist theory of meaning would have it. But very

shortly the point becomes:

1. For any belief, if the verification theory of
   meaning were correct then recalcitrant experience
   would require specific alterations of our views.

2. Recalcitrant experience does not require specific
   alterations of our views,

hence the verification theory of meaning is false.

Premise 1 above is supported by the claim that if the

meaning of a statement is given in terms of a sensory

stimulation the presence or absence of a given stimula-

tion can effect only those statements whose meanings are

given either fully or partially in terms of the afore-

mentioned stimuli. It may be helpful to consider a

simple example. Let us take as our belief "Columbus, Ohio

is 75 miles from Cleveland". Even if we could do it

it would be a horrendous project to spell out in detail
what sensory stimulation would "confirm" this but we can
get the verificationist position across if we allow a
kind of "operationalist" notion of confirming or infirming
experience to do duty here. The meaning of the above
belief is then defined in terms of the amount of time it
takes to go by a certain method from one city to the
other. If the amount of time is not as the operationalist
account of the distance would have it, then that calls
for the revision of the belief in question. However, as
premiss 2 above has it, there are many different ways one
could revise our beliefs to handle this recalcitrant
experience. Premiss two is supported by a consideration
of all the different ways one could modify our beliefs
in the light of recalcitrant experience.   In the case
at hand, we might modify our belief in the accuracy of
the instrument that measured how long it took to get from
Columbus to Cleveland. We might decide we had been
wrong in our estimate of the speed at which we traveled,
etc. The real requirements are that the periphery state-
ments (those about physical objects, etc.) be in accord
with experience while the rest of our beliefs used in
the prediction of the future should strive for simplicity
of laws as well as, of course, predictive accuracy.

Any object which our theory commits us to is a "posit" designed to simplify and structure our theory of our experience. Insofar as the positing of an object is neither confirmed or infirmed by the totality of science then there is no fact of the matter concerning the existence of the posited objects. (To point out that most of our posits are confirmed or infirmed by the totality of science would be simply to note that our practice does not often bring us to posit entities that are such that there is no fact of the matter concerning them). Of course it is the case that if it can be shown that there is no point to positing something, then we would say that there probably is no such thing. There is no fact of the matter about something when no account that we might give of it accords any better than any other account that might be given of it. (It is important to see here that this is the situation Quine claims we are in in the case of translation. As in any case of interpretation of the words of another, I here try to assume that Quine is not saying things for which he has no support, so one should look for an interpretation of his remarks on verificationism that would, if correct, allow us to infer from the unverificability of analytic hypotheses to the ITT. Though the highly cryptic closing of "Two Dogmas of Empiricism" is radically underdetermined as to best

nonmetaphorical interpretation (was Quine trying to con-
vince us even back then of the ITT by offering us a bit
of speech behavior which we could all interpret differently?)
I suggest, though I admit the textual evidence is hardly
overwhelming, that Quine can be read as endorsing (and
certainly should not be read as rejecting) a principle
such as "Where there is is no way in principle to estab-
lish which of different theories about Xs are true there
is no fact of the matter". (In the next chapter I shall
attempt to show why this does not make physics indeter-
minate). I wish now to turn to a consideration of the
part verificationism principles play in Quine's work
after "Two Dogmas of Empiricism".

"Epistemology Naturalized",[3] concerned with large
scale reflections on the epistemological enterprise,
contains some of Quine's least opaque remarks on transla-
tion and verification. In "Epistemology Naturalized"
Quine again rejects the view that single statements have
empirical evidence on their own. Only large blocks of
statements taken together are evidenced for by experience.
If the sentences of a language

> have their meaning only taken together as a
> body, then we can justify their translation
> into Arunta only together as a body ("EN",
> p. 80)

Different translations of language x into language y will

be as "correct as any other, so long as the net empirical
implications of the theory as a whole are preserved in
translation". ("EN", p. 80) However, as the familiar
claim goes many different ways of translating x into y
would deliver the same empirical implications. As long
as this is preserved then "there would be no ground for
saying which of two glaringly unlike translations of
individuals sentences is right". ("EN", p. 80) Now, and
this is a point at which I think controversy will arise,
Quine refers to the above situation as an indeterminacy.
("For the uncritical mentalist, no such indeterminacy
threatens", p. 80 "EN".) We may tentatively conclude
(more evidence will shortly be offered) that Quine, rather
than rejecting all forms of verificationism in fact
accepts such an assumption. He differs from earlier
verificationists in that he takes as the unit of empirical
significance the whole of science, not individual sentences.
Quine is also unlike earlier holders of verificationist
principles in that he indulges in no a priori attempts
to specify the verification conditions for scientific
claims (or "common sense" claims for that matter, sug-
gesting no dichotomy here, of course). The principle
simply tells us that if we cannot <u>confirm</u> one set of
analytic hypotheses over others, then there is no fact of
the matter in the case of translation. This view is in

strong contrast to earlier verificationists (here I have in mind the Logical Positivists but I avoid use of the name to prevent someone from thinking that if you held the sort of verificationist principles the positivists did you would inevitably end up with many of the other views popularly associated with the positivist viewpoint) who attempted to specify what would count as verifying particular sorts of claims. Such an attempt to specify in advance the business of science is no doubt an unwarranted form of rationalism and it is to be expected that anyone who attempts to specify precise verification conditions to be used by scientists will run aground in the attempt. One can then rightly opt out of the demand that we specify what counts as the verification of sentences in our total scientific theories for this feat can only be done with the advance of science. On the sort of verification principle Quine uses, we need only say that given whatever such a verification consists of, where it does not decide what the fact of the matter is there is no fact of the matter.

> 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
> large blocks of theory, then the indeterminacy
> of translation of theoretical sentences is the
> natural conclusion. And most sentences, apart
> from observation sentences, are theoretical.
> ("EN", p. 81)

Quine then moves to consider the objection that the
conclusion about translation set out above ought to per-
suade us to abandon the verification theory of meaning.
("EN", p. 81) Quine's strategy is to claim that all that
matters in learning a language is the keying of language
to external stimuli.[4]

> 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.
> A child learns his first words and sentences by
> hearing and using them in the presence of
> appropriate stimuli. These must be external stim-
> uli, for they must act both on the child and on
> the speaker from whom he is learning. Language is
> socially inculcated and controlled; the inculca-
> tion and control turn strictly on the keying of
> sentences to shared stimulation. Internal factors
> may vary ad libitum without prejudice to communi-
> cation as long as the keying of language to
> external stimuli is undisturbed. Surely one has
> no choice but to be an empiricist as far as one's
> theory of linguistic meaning is concerned ("EN",
> p. 81)

As Quine says that "the crucial considerations behind my
arguments for the indeterminacy of translation was that
a statement about the world does not always or usually
have a separate fund of empirical consequences to call its
own". ("EN", p. 82)

I wish now to turn to a consideration of Quine's use
of verificationist principles in Word and Object[5] and
The Roots of Reference.[6]

There will be those who will be quick to claim that
Quine has confusedly assimilated epistemological

underdetermination with ontological indeterminacy. As

claimed, however, this simply begs the question. However

I believe a plausible defense of such a principle can be

made along Quinian lines and, further, I think it is pos-

sible to advance strong evidence in support of the claim

that Quine would endorse such a defense.

In Word and Object the issue of verificationism is

less explicit than it is elsewhere. However, it is

fairly clear that some such assumption is operating in

that work (though it is harder to document textually).

In making out the claim that Word and Object makes use

of verificationist assumptions one must, I think, look at

the various passages toward the end of Chapter II where

Quine first says that analytic hypotheses are unverifiable

and subsequently states that translation is indeterminate.

The primary difficulty from an exegetical point of view

is that there is no "hence" present in the text and thus

we must conjecture that he is making use of some such

verificationist assumption. (It may well be the case

that the "hence" is not there merely because Quine did

not feel the need to say what was "so obvious".) Consider

> Most of the semantic correlation is supported only
> by analytic hypotheses, in their extension beyond
> the zone where independent evidence for transla-
> tion is possible. That those unverifiable transla-
> tions proceed without mishap must not be taken as
> pragmatic evidence of good lexiography, for mishap
> is impossible. (Word and Object, p. 71)

Perhaps the closest thing to a transparent statement of

such an assumption is seen in the following:

> Yet one has only to reflect on the nature of
> possible data and methods to appreciate the in-
> determinacy. Sentences translatable outright,
> translatable by independent evidence of stimu-
> latory occasions, are sparse and woefully under-
> determine the analytic hypotheses on which the
> translation of all further sentences depends. To
> project such hypotheses beyond the independently
> translatable sentences is in effect to impute
> our sense of linguistic analogy unverifiably to
> the native mind. (Word and Object, p. 72)

Another passage that I take as evidence, given that Quine

had earlier in the same chapter of Word and Object, Chapter

Six, "Flight From Intension", argued against accepting

intentional usage at face value, can also be cited. (I

read the passage as saying it would be absurd to postulate

determinate translation when no analytic hypothesis is

verifiable as the correct one.)

> To accept intentional usage at face value is, as
> we saw, to postulate translation relations as
> somehow objectively valid though indeterminate in
> principle relative to the totality of speech dis-
> positions. Such postulation promises little gain in
> scientific insight if there is no better ground for
> it than that the supposed translation relations are
> presupposed by the vernacular of semantics and
> intentions (Word and Object, p. 221)

One should note that this material is not saying that

translation is indeterminate but rather that if taking

intentional usage at face value is all there is to the

verification of analytic hypotheses then translation

would be unverifiable. This matter of taking intentional

usage at face value is opaque at best, however, what it roughly means is assuming that what Chisholm calls Bentano's thesis holds ("there is no breaking out of the intentional vocabulary by explaining its members in other terms") shows that intentional usage is both indispensable and such that no explanation for it can be given in physical terms. Thus even if there is a fact of the matter as to what my intention is, this in itself will not give us determinate translation. (See Word and Object, pp. 219-221 for references and remarks on this matter. The consideration of Dennett's hungry dog in Chapter III also related directly to this matter.) Should there be other relevant evidence, then the unverifiability of analytic hypotheses with respect to intentional usage taken at face value would not in itself warrant the ITT. All of this is quite, to repeat at the risk of growing tiresome, compatible with Quine's famous rejection in "Two Dogmas of Empiricism" (and elsewhere) of the verification theory of meaning. It remains to consider what Quine says concerning verificationism in his latest work The Roots of Reference.

The verification theory of meaning is mentioned only once in The Roots of Reference. It is very interesting for our purposes in that Quine locates the fault of the

verification theory of meaning <u>not</u> in the claim that where

there is no way to verify something there is no fact of

the matter but rather in the claim that individual sen-

tences are verifiable in isolation. Indeed, it appears

that he is endorsing the first part of the verification

theory of meaning:

> The two roles of observation, their role in the
> support of theory and their role in the learning
> of language, are inseparable. Observations are
> relevant as evidence for the support of theory
> because of those very associations, between ob-
> servable events and theoretical vlcabulary, whereby
> we learn the theoretical vocabulary in the first
> place. Hence, of course, the commonplaces of the
> verification theory of meaning. The meaning of a
> sentence lies in the observations that would support
> or refute it. To learn a language is to learn the
> meaning of its sentences, and hence to learn what
> would count as evidence for and against them. The
> evidence relation and the semantic relation to theory
> are coextensive.
> But the old champions of a verification theory of
> meaning went wrong in speaking too blithely of the
> meaning of individual sentences. Most sentences do
> not admit separately of observational evidence.
> Sentences interlock. An observation may refute some
> chunk of theory comprising a cluster of sentences,
> and still leave us free to choose which of the
> component sentences to abandon. The evidence
> relation is thus intricate and indirect. (<u>The Roots</u>
> <u>of Reference</u>, p. 38)

Again, one would like to be able to find an explicit state-

ment and defense of a verificationist principle in <u>The</u>

<u>Roots of Reference</u> but such a thing is not to be found.

(The principle of interpretation used on this bit of

text from <u>The Roots of Reference</u> is that is one is offering

a critique of a position that contains two tenets, A and B,

and one says "where the position went wrong" was B, then
one is led to assume that the critic takes no exception
to A. This, like all such principles of interpretations
is not without counterexamples, of course, but unless we
have a reason to assume the principle is not being ob-
served we are obligated to follow it.[7]) Having offered
some documentation of my exegetical claim I wish now to
turn to a consideration of why Quine thinks one can infer
from unverifiability to indeterminacy. As stated, I shall
be herein considering what sort of defense of the principle
can be constructed from Quine's writings, returning to a
consideration of the plausibility of this sort of posi-
tion at length in Chapter VIII.

Quine claims in "On the Reasons for Indeterminacy of
Translation"[8] that he anticipates little disagreement with
the claim that all hypotheses are underdetermined by the
available evidence. However, once we adopt a scientific
theory, albeit underdetermined or not, then it to a large
extent determines what sentences we will accept. For
example, perhaps the evidence for molecules might be
underdetermined molecular theory but once one adopts such
a theory one can no longer decide not to hold that mole-
cules exist (unless one gives up the theory, of course).
One might claim here that this suggests that we stipulate
what nature is, not create it. This remark overlooks the

fact that

> The lore of our fathers is a fabric of sentences.
> In our hands it develops and changes, through more
> or less arbitrary and deliberate revisions and
> additions of our own, more or less directly con-
> ditioned by the continuing stimulation of our sense
> organs. It is a pale grey lore, black with fact
> and white with convention. But I have found no
> substantial reasons for concluding that there are
> any quite black threads in it, or any white ones.[9]

The general pragmaticism of Quine in "Two Dogmas of

Empiricism" and elsewhere holds that existence claims

make sense only insofar as they help us in organizing the

laws we employ to explain and predict our experience.

> As an empiricist I continue to think of the concep-
> tual scheme of science as a tool, ultimately, for
> the prediction of future experience. ("TD", p. 44)

Our theories are then devices for predicting experience

(one should note in passing that this claim is compatible

with either a realist or an instrumentalist approach to

the status of objects of theories) and are such that their

objects are posits, well motivated only when they are

efficacious in simplifying this task.[10] Insofar as two

different posits serve equally well toward this purpose

then either one is as rational as the other, insofar as

there is any correctness to speak of. When two incompatible

posits (for example, if we were to posit that the meaning

of a given expression in language y were given by the

expression 'rabbit' this would be incompatible with

positing that it was given by 'rabbit stage') serve

equally well, then there is *no* fact of the matter concerning which of the posits is correct. Such then is the general drift of Quine's pragmatism as it applies to the issues considered in this chapter. I wish now to present a general outline of a position opposed to Quinian pragmatism, a position I shall call "Realistic Antiverificationist" (hereafter RAV). I shall not put any names or particular wources on this position as given the generality of the RAV position as herein described probably no one would say that I have described exactly his position. More importantly RAVism is rather than a well articulated theory, a murky set of assumptions that lead persons to reject any sort of inference from epistemological concerns to metaphysical concerns as well as the general pragmatism of Quine (and earlier American pragmatists as well, of course).

Roughly, the RAVist position involves a radical split between justification and truth, between epistemological claims and metaphysical claims, between posits and the world. The RAVist claims that from the fact that we can verify a particular claim it does not follow that there is no fact of the matter to it because this unverifiability may simply be a reflection of our own epistemological inadequacies. The RAVist sees no absurdity in the claim that we might be completely wrong in our accounts of what the world is like, in fact wrong in ways that can never be

discovered and never rectified. Of course, the RAVism need not hold that this is probably true, but he does hold that the fact that it is possible blocks verificationist inference from epistemological matters to claims of metaphysical indeterminacy. (In Chapter VII, the objections of Rorty and Davidson to the claim that we could be radically wrong are used to critically consider the RAVist remarks on the possibility of gross error, I am here mostly interested in setting out the RAVist picture and the outlines of a Quinian critique of it.) The RAVist might agree that the best we could do is to posit entities but holds that the objects that actually are in the world exist even if there is no evidence for them and would even if there were equally good (or even better) evidence for schemes that repudiate their existence. Once we have a picture of this radical split between justification and truth in mind then one might be inclined to say that there is a qualitative difference between the objects that actually exist and those that are simply posits. The qualitative difference being that posits are helpful in predicting future experience in the light of past whereas the objects of the world exist independently of the question as to what sort of evidence there might be for them. Posits, on the RAVist view, are objects used to help predict future experience and the objects of the

world (what is real) exist irrespective of what sort of
evidence there may or may not be for them. Of course,
most RAVists would hold that we do know that our current
theories coincide with the real world but he does claim
that it is possible that something may work wonderfully
well in organizing experience yet still not exist. Like-
wise, according to the RAVist, something can have no
effect on any predictions that we could in principle make
and still exist. One might even claim that there can be
facts of the matter about which no physical science can
give us an account. The RAVist might even attempt to
render his position "intuitive" by claiming that behind
his position lies the firm "common sense" idea that the
world is what it is independently of what we think or of
what we could know about it. After all "just because we
cannot figure out something it does not mean that it could
not be true". I am willing to concede, for the sake of
argument, that RAVism is the position of common sense.
However, common sense is far from always right. The
RAVist should also be taken as believing that there is
a definite way to sort out metaphysical from epistemologi-
cal claims, that something can be shown to be one or the
other (or neither, but not a "mixture"). One could perhaps
develop a kind of gradation of RAVism depending on how
difficult one thought it was to make such distinctions,

ranging from those who boldly kick stones and claim the

distinction completely unproblematic to those who give

historically sophisticated accounts of the development of

scientific theories in such a way that a distinction is

made between those aspects of theories that are the result

of tacitly adopted conventions and those that are the

result of data, etc. Quine states the sort of reflections

that might lead one to RAVism in the following:

> This reflection strengthens our natural suspicion:
> that the benefits conferred by the molecular doc-
> trine give the physicist good reason to prize it,
> but afforded no evidence of its truth. Though the
> doctrine succeed to perfection in its indirect
> bearing on observable reality, the question of its
> truth has to do rather with its direct claim on
> unobservable reality. Might the molecular doctrine
> be ever so useful in organizing and extending our
> knowledge of the behavior of observable things, and
> yet be factually false.[11]

Quine then wishes to use some reflections on language

learning to criticize the RAVist split between justifica-

tion and truth.

> Words are human artifacts, meaningless save as
> our associating them with experience endows them
> with meaning. The word 'swarm' is initially
> meaningful to us through association with such
> experiences as that of a hovering swarm of gnats, or
> a swarm of dust motes in the shaft of sunlight.
> When we extend the words to desks and the like,
> we are engaged in drawing an analogy between swarms
> ordinarily so-called, on the one hand, and desks,
> etc., on the other.[12]

The connection between this and the critique of RAVism

is the RAVist expressions 'real' and 'world' and such

admit of use in terms of something radically disassociated

from experience (it is not, of course, to the point for

the RAVist to again assure us that he does not think we

are radically wrong about what is real, etc.). Assuming

that we grant Quine's claim that sentences do not have

evidence when construed in isolation, then the RAVist has

no theory in which to couch his claims about a world which

we could be totally mistaken about. One should also note

that the RAVist cannot simply insist that a well supported

theory is one thing and the world another, for 'world'

is a word in a theory as much as any other.[13]

> Where it makes sense to apply 'true' is to a
> sentence couched in terms of a given theory and
> seen from within the theory, complete with its
> posited reality. (Word and Object, p. 24).

Let us imagine that we can find a unique best theory.

Even with this we still would not have established whether

sentences like "this scheme of translating language x into

language y is simpler than all others" by saying that

these sentences or their translations belong in the unique

best theory for

> there is no sense in equating a sentence of a theory ϕ
> with a sentence S given apart from ϕ. Unless pretty
> directly and firmly conditioned to sensory stimula-
> tion, a sentence S is meaningless except relative to
> its own theory: meaningless intertheoretically.
> (Word and Object, p. 24)

There is also another verificationist defense inherent

in the first chapter of Word and Object. (It is pretty

evident that many critics of the ITT just read Chapter II
of Word and Object, figuring no doubt that the fact that
Chapter I precedes Chapter II is but an arbitrary decision
of no importance.) The basic idea is that if one follows
simplicity in developing theories[14] and if one is given
the option of either adopting a version of the verifica-
tionist principle or holding that even though all the
evidence there could in principle be for some hypothesis
neither informs or confirms the hypothesis there still is
(or might be) a fact of the matter, the simpler course is
to adopt the verificationist principle. To object that
this is not enough to show that the verificationist
principle is necessarily true is quite a harmless charge
for those of us who do not think that anything is a
necessary truth. (The simplicity of adopting a verifica-
tionist principle can be seen, I think, from the following
passage where holding that something is indeterminate if
it fails the verificationist test is correlated with
the "principle of sufficient reason":

> Yet another principle may be said to figure as a
> tacit guide of science is that of sufficient
> reason. A lingering trace of this venerable
> principle seems recognizable, at any rate, in
> the scientist's shunning of gratuitous singulari-
> ties. If he arrives at laws of dynamics that
> favor no one frame of reference over others that
> are in motion with respect to it, he forthwith
> regards the notion of absolute rest and hence
> absolute position as untenable. (Word and Object,
> p. 21)

Analogously, if by the methods at which we arrive at analytic hypotheses (described briefly in Chapter III) we can arrive at more than one set of analytic hypotheses we conclude that there is nothing for them to be right or wrong about.

Verificationism can, thus, be defended purely within the context of Quine's writings, especially his comments on the nature of theories. If one is to reject all forms of verificationist principles one must take an approach fundamentally different from that of Quine to basic questions. Now, I fully acknowledge that Quine's defense of verificationism raises many questions but it is sufficient to require his antiverificationist critics to offer arguments to refute it rather than just assertions to the effect that the argument for the ITT requires verificationist principles thus it must be no good. As was herein stressed, the sort of verificationist principle Quine holds is considerably different from those held by earlier verificationists (thus no comments about tales from the Vienna woods are proper here). More will be said in defense of such a holistic version of verificationism in Chapter VIII, it here being largely my purpose to establish the role verificationist principles play in the development of the ITT.[15] It seems most useful now to turn to a consideration of a much considered and

perplexing matter. Namely, why is physics merely under-determined and not indeterminate? How is physics "better off" than translation?

Footnotes

[1] Quine, W. V., "Two Dogmas of Empiricism", in From a Logical Point of View (Cambridge, 1964), pp. 20-46. Further references to this paper will be included in the text, the initials "TD" being used to refer to the paper.

[2] This sort of criticism is raised in, for example, "Charles Landesman, "Scepticism About Meaning: Quine's Thesis of Indeterminacy", Australasian Journal of Philosophy XLVIII (1970), pp. 32-337; Richard Rorty, "Indeterminacy of Translation and of Truth", Synthese XXIII (1972), pp. 443-462; Jay F. Rosenberg, "Synonymy and the Epistemology of Linguistics", Inquiry X (1967), pp. 405-420. Closely related criticisms are also raied in R. Kirk, "Underdetermination of Theory and Indeterminacy of Theory", Analysis XXXIII (1973), pp. 195-201; Michael D. Root, "Quine's Methodological Reflections", Meta Philosophy V (1974), pp. 36-50.

[3] Quine, W. V., "Epistemology Naturalized", in Ontological Relativity and Other Essays (New York, 1969), pp. 69-90. Further references to this paper will be included in the text, the initials "EN" being used to refer to the paper.

[4] This quote may appear to some to be claiming that verificationism holds in the case of linguistics and not elsewhere, however, in the context of Quine's other remarks on the subject (some of which are herein set out) nothing of import can be inferred from this fact.

[5] Quine, W. V., Word and Object (Cambridge, Mass.: MIT Press, 1960). Further references to this book will be included in the text, the initials W & O being used to refer to this book.

[6] Quine, W. V., The Roots of Reference (La Salle: Open Court, 1973).

[7] The source of these exegetical principles here is H. P. Grice's "The Logic of Conversation", reprinted in The Logic of Grammar, ed. by D. Davidson and G. Harman (Encino, Calif.: 1975), pp. 64-74, but they were doubtless set forth in numerous texts on rhetoric and such a long time ago.

[8] Quine, W. V. "On the Reasons for Indeterminacy of Translation", Journal of Philosophy LXVII (1970), pp. 178-183.

[9] Quine, W. V., "Carnap and Logical Truth", in The Ways of Paradox (New York: Random House, 1966), p. 125.

[10] One should note that as Quine's remark concerning the lore of our fathers does not (though he of course does believe this) depend on claiming that there is no distinguishing one's beliefs from one's theory, it is not to the point to indicate that perhaps someone would say that language and theory are to be sharply distinguished from one another.

[11] Quine, W. V., "Posits and Reality", in The Ways of Paradox, p. 235.

[12] Ibid., pp. 235-236.

[13] cf. Thomas Kuhn in The Structure of Scientific Revolutions (Chicago: University of Chicago, 1970): "Perhaps there is some way of salvaging the notion of 'truth' for application to whole theories, but this will not do. There is, I think, no theory-independent way to reconstruct phrases like 'really there'; the notion of a match between the ontology of a theory and its real counterpart is nature now seems to me illusive in principle", p. 206. (What will not do is the claim that successive theories will grow toward giving a true account of the way nature is as the objects the theories come closer to matching up what is "really there".)

[14] It appears to me to be the case that Quine's very offhand talk of simplicity in Word and Object is itself far too simple to accord with the facts of ontological relativity. Is simplicity a function of the ease with which a theory can be translated into a background language and is one theory simpler than another if it can be more simply translated into a common background language than the other and if so, as translation is claimed by Quine to be indeterminate, does this make simplicity indeterminate? In "On Simple Theories of a Complex World" Quine says that simplicity is "relative to the texture of a conceptual scheme" and also says that the simpler of two theories is more likely to be true. The obvious question then is that if simplicity is relative to conceptual schemes, how are we to use it, as Quine says we do, as a device for deciding which of

competing theories are correct? As stated, the problem is
compounded, not solved by a translation of the competing
theories into a background language for this would make
simplicity indeterminate if translation is indeterminate.
Needless to say, I have no solution to these complicated
matters here, though I do say a bit more on simplicity
in Chapter VI. My tentative suggestion is that the
difficulties these matters bring out are the result of
the fact that Quine himself wants to be a kind of RAVist
in spite of the fact that does hold verificationist
principles. "On Simple Theories of a Complex World" is
reprinted in The Ways of Paradox (New York, 1966), pp.
242-245.

[15]There is starting to be a bit of exegetical dispute
in the literature on the ITT over whether or not Quine
is a verificationist. Bruce Aune in "Quine on Transla-
tion and Reference", Philosophical Studies, XXVII (1975),
pp. 221-236, dogmatically (and with no citation of textual
evidence) declares that he is not. D. Follesdal, also without
citing textual evidence one must admit, claims that Quine
definitely is a verificationist: "Indeterminacy of
Translation and Underdetermination of the Theory of Nature",
in Dialectica, XXVII (1973), pp. 289-301. Michael Dummett
in "The Significance of Quine's Indeterminacy Thesis"
also claims that Quine is a verificationist (Synthese,
XXVII (1974), pp. 351-397) and also offers no textual
evidence. Given this disagreement as this it becomes
necessary to involve oneself in such exegetical considera-
tions as I take up in this chapter. Concerning Aune's
paper there are several things that can be said by way
of criticism. Firstly, Aune has Quine inferring from the
fact that from one's speech dispositions we cannot dis-
cover what a right translation would be to there being
no right translation, the ITT. Now, as the RAVist
rightly claims, with some sorts of verificationist prin-
ciple one cannot go from the fact that speech dispositions
do not establish a unique right translation to the ITT.
Thus on Aune's reconstruction, the argument for the ITT
does not work. Also, on Aune's construal of the matter
there is no reason why physics should not be indeterminate
(In the next chapter I attempt to show that this is a
fatal flaw in any account of Quine's work). Furthermore,
Aune seems to me to fail to appreciate the very important
point that Quine's naturalism (as set forth in "Epistemology
Naturalized" and briefly remarked on in my consideration
of that paper in this chapter) is verificationist, i.e.,
if nature (where nature is nature accessible to us) does
not establish a fact of the matter then there is no fact of
the matter. (My remarks on Aune are based primarily on
pp. 221-226 of his article.)

CHAPTER V

PHYSICS, TRANSLATION, AND THE ITT

A criticism one quite frequently finds raised by
commentators on Quine is that if translation is indeter-
minate then, contrary to Quine's claim, parallel arguments
will show physics to be indeterminate. If there is no
fact of the matter concerning physics, then there is no
fact of the matter concerning the way the world is.[1]
The critics are agreed in holding that indeterminacy
arguments work in either both the case of translation
and physics or in neither case. As a matter of record, a
large number of philosophers appear to think that if it
can be shown that indeterminacy arguments work in both
areas then this in itself would be a reductio ad absurdum
of the arguments in question. There are those, however,
who claim that indeterminacy hold in physics and transla-
tion. However, and this is the point of import, the
critics are agreed in claiming that Quine cannot consis-
tently maintain that there is a fact of the matter in
physics and maintain that there is no fact of the matter
in translation.

In considering these important criticisms it is

necessary to attempt several interrelated things. One
must consider why Quine holds that the parallel argument
concerning physics fails. What, on Quine's view, assures
that there is a fact of the matter concerning physics and
not translation? The obvious prima facie strategy on
the part of one interested in replying to these criticisms
would be to show that consistently within Quine's views
one can distinguish translation and physics in terms of
determinacy. One would here need to give an account of
what the relevant differences were between linguistics
and physics that made one determinate and the other not.
(One should note here that the critics considered herein
are not worrying over whether or not Quine's arguments
for the ITT are themselves sound but over why the parallel
arguments in the case of physics do not work.) Once these
matters have been considered one should sort out various
notions of determinacy and indicate what sort of notion
of fact of the matter is at work in the case of Quine's
account of the factuality of physics. Here I shall venture
to sketch what I take to be the basic realist-antiverifica-
tionist notions of fact of the matter. (It is perhaps
of interest to note that behind much of the antiverifica-
tionist objections to the ITT lies the assumption that
there is a fact of the matter concerning how the world is.
Many of the critics who advance the objections considered

in this chapter would also offer versions of the objections considered in the previous chapter. Certainly if one were on some a priori ground convinced that there was a fact of the matter concerning how the world was one would certainly be undaunted by the fact that one could not verify certain hypotheses concerning various facts. The question of the plausibility and intelligibility of such an a priori assumption is, of course, another matter.)

One should perhaps mention that there is lurking in some minds[2] a kind of excluded opposites argument which holds that if everything is indeterminate then nothing is indeterminate just as there would be no good if there were no evil. The idea being that indeterminacy of translation makes sense only if there is something determinate to contrast translation with. This is, I think, a dubious mode of argument (perhaps it is even generally thought to be dubious) but it has been raised with respect to the ITT. I mention it here for if one can show that there is a determinate notion of fact of the matter available notwithstanding the ITT then we can dispense with this sort of objection here.

In order to demonstrate the pervasiveness of this sort of objection to the ITT I shall begin by offering some salient quotations from critics of the ITT. (Those referred to in footnote 1). Following this I shall give

an account of the relevant aspects of Quine, in terms of
which I will give a somewhat detailed consideration of
the arguments of Richard Rorty and Jay Rosenberg. I shall
then attempt to answer the general objection considered in
this chapter through a development of the aforementioned
material.

Rosenberg in "Synonymy and the Epistemology of
Linguistics" puts the criticism this way:

> Correlative to the thesis that in any case of
> translation there exists a pair of equally good
> analytic hypotheses is the thesis that there
> exists a pair of equally good incompatible
> physical theories in the case of physical pheno-
> mena....The indeterminacy which Quine has found in
> the case of translation is, at its worst, the
> ordinary indeterminacy to which any case of transla-
> tion is subject....Epistemologically, Quinian
> analytic hypotheses and physical theories are on
> a par. (Rosenberg, 412-413)

Rosenberg, in fact, claims not only that translation is
no worse than physics in terms of determinacy but, due
to the purported knowledge of native speakers is, in fact
better off than physics. Rosenberg, in an argument not
all that different from the argument of M. C. Bradley
considered in Chapter III, claims that as the ITT entails
that there is no fact of the matter as to our knowledge
of the structure of English it is reduced to absurdity (see
Rosenberg, 416-417). Rosenberg and Bradley thus both take
as a reductio of Quine's views what Quine takes as a
natural, though doubtless surprising to some, consequence

of his views: "Radical Translation begins at home".
("Ontological Relativity", p. 46)

There are numerous other philosophers who have
offered this same objection against Quine. Among them
are Stich, Rorty, Root, Katz, Chomsky, Kirk, Martin and
Smith, and Hockney (see footnote 1). This list could
be made considerably longer and there doubtless will
be other critics making the same sort of objection in
the future. As stated, I shall consider the arguments
of Rosenberg and Rorty, but we would be amiss not to
explicitly mention and quote Chomsky's famous statement
of this criticism. Chomsky is, and this alone gives his
views special import, the only critic on this matter to
whom Quine has explicitly replied.

> It is to be sure undeniable that if a system
> of 'analytic hypotheses' goes beyond the
> evidence then it is possible to conceive alter-
> natives compatible with the evidence...Thus the
> situation in the case of language, or 'common
> sense knowledge' is, in this respect, no dif-
> ferent from physics...Thus what distinguishes
> the case of physics from the case of language
> is that we are, for some reason, not permitted
> to have a 'tentative theory' in the case of
> language.... (Chomsky, 61-62)

This sort of criticism of the ITT is, insofar as one can
tell through a reading of the critical literature, one of
the main reasons for the resistance of persons to the
thesis. To Førllesdal, evidently, to offer this
criticism is to indicate that one understands Quine's

views very poorly: "The general perplexity over this point is remarkable, since understanding Quine's point seems to be necessary for understanding his thesis of indeterminacy of translation".[3] Hopefully I have made out the claim that the criticism considered in this chapter is one of the major reasons for resistance to the ITT. Before considering Quine's views on the matter one should note that even if the critics are correct about the parallel between linguistics and physics this taken simply by itself does not serve to refute the ITT. One can maintain that this simply shows that there is no fact of the matter concerning physics or the way the world is. So over if all the critics considered here are right they will only have shown that on their understanding of the matter if physics is determinate then translation is determinate. One must, if one is to refute the ITT go on to offer a sound, nonquestion begging argument to the effect that physics must be (or is) determinate. None of the papers considered in this chapter attempt this (nor does anyone else that I am aware of for that matter) further step. However, one can remove the need to deal with such objections if we can show that one can hold that physics may be determinate while translation is indeterminate. What needs doing then is, Quine's critics testify to the need for this, to explain the relevant

differences between translation and physics. With this in mind, I wish to turn to a consideration of Quine's views on the subject at hand in order to make it possible to consider the arguments of Rorty and Rosenberg in support of the criticisms.

The most relevant texts from Quine are section 16 of Word and Object and Quine's reply to Chomsky.[4] The following passage from Word and Object is quoted by several of the critics considered herein and can serve as focal point for our consideration of Quine's views.

> May we conclude that translational synonymy is at its worst no worse off than physics? To be thus reassured is to misjudge the parallel. In being able to speak of the truth of a sentence only within a more inclusive theory one is not much hampered; for one is always working within some more inclusive theory, however tentative. Truth is even overtly relative to language, in that e.g., the form of words 'Brutus killed Caesar' could by coincidence have unrelated uses in two languages; yet this little hampers one's talk of truth, for one works within some language. In short, the parameters of truth stay conveniently fixed most of the time. Not so analytic hypotheses that constitutes the parameter of translation. We are always ready to wonder about the meaning of a foreigner's remark without reference to any one set of analytic hypotheses, indeed even in the absence of any; yet any two sets of analytic hypotheses compatible with all linguistic behavior can give contrary answers, unless the remark is one of the limited sorts that can be translated without reference to analytic hypotheses. (Word and Object, p. 75-76)

Let us consider a case that may help to clarify the matter. We approach a fist sized rock and wonder about its

composition. What ever answer we come up with must be compatible with the ongoing physics of the day, assuming that we do not hold some idiosyncratic set of scientific theories. In fact, even if we hold some idiosyncratic set of physical theories, we will want our account of the composition of the rock to be compatible with those theories. If our account of the composition of the rock violates some thesis of this accepted physics then it is therefore not a correct hypothesis. A necessary condition for a hypothesis concerning the composition of the rock is that the hypothesis not be inconsistent with our basic physical theories. (Needless to say, it is not the affair of philosophers to set out this physical theory, the claim is simply that whatever the basic physical theory is, our hypothesis concerning the rock must be compatible with it.) One should further note that this does not require that our evidence for the physics we accept itself determine a unique truth. Many hypotheses concerning the composition of the rock would be ruled out by this sort of requirement. For instance, if we heated the rock to the burning point of coal and, other things being equal, it did not burn then we would have observational evidence that it was not coal. The constraint of compatibility with the basic physical theories does exercise fairly strong control over what sort of hypotheses concerning the

structure of the rock will be acceptable. (We should note that this is <u>not</u> a prejudice as some meaning theorists may claim, against "queer" or odd entities. Physics, as was observed by Descartes, contains entities that seem quite strange to the man in the street.) What turns out to be crucial here, as it was in the previous chapter, is the claim that we can meaningfully speak of truth

> only within the terms of some theory of conceptual
> scheme (cf. 5). So on the whole we may meaning-
> fully speak of interlinguistic synonymy only
> within the terms of some particular system of
> analytic hypotheses. (<u>Word & Object</u>, p. 75)

Insofar as we have some background theory which exercises constraints concerning hypotheses on a subject matter there is a fact of the matter concerning such things. (This is why there is a fact of the matter concerning stimulus meaning of occasion sentences, and such.) Further, there are no such constraints that effectively serve to rule out competing and incompatible analytic hypotheses.

This point can be made a bit clearer by considering translational hypotheses concerning the stimulus meaning of occasion sentences. Occasion sentences are sentences such as "'Gavagai', 'Red', 'It hurts', 'His face is dirty', which command assent or dissent only if queried after an appropriate prompting stimulation". (<u>Word and Object</u>, pp. 35-36) Hypotheses concerning the stimulus

meaning of occasion sentences are genuine hypotheses

because there are physical matters that act as constraints

upon hypotheses.

> Thus the analytic hypotheses, and the grand
> synthetic one that they add up to, are only in an
> incomplete sense hypotheses. Contrast case of
> translation of the occasion sentence 'Gavagai' by
> similarity of stimulus meaning. This is a genuine
> hypothesis from sample observations though possibly
> wrong. 'Gavagai' and 'There's a rabbit' have
> stimulus meanings for the two speakers, and they
> are roughly the same or significantly different,
> whether we guess right or not. On the other hand
> no such sense is made of the typical analytic
> hypothesis. The point is not that we cannot be
> sure whether the analytic hypothesis is right, but
> that there is not even, as there was in the case
> of 'Gavagai', an objective matter to be right or
> wrong about. (Word and Object, p. 73)

(The stimulus meaning of an occasion sentence is the ordered

pair of the affirmative and negative stimulus meaning. The

affirmative stimulus meaning is the class of all stimula-

tions that would prompt assent to the sentence. The

negative stimulus meaning is the class of stimulations

that would prompt dissent, see Word and Object, pp. 32-33).

Quine suggests that stimulation can "be cashed in terms

of dispositions which are in turn some sort of structural

condition of the disposed organism". (Word and Object,

pp. 33-35, see also section 3 of The Roots of Reference.)

One might construe things as follows: Quine has

argued that philosophy (or anything else for that matter)

cannot provide a first ground on which science ought to

(or must) rest. Our physical theories are not to be

construed as taking some philosophical outlook as a first
philosophy. More needs to be said here about the notion
of <u>first philosophy</u> before the import of Quine's use of
any such notion can be understood. Roughly, a first
philosophy is a set of truths that any adequate theory of
the world must encompass, a portion of our theory that
cannot under any circumstances be revised away without
error. This notion of a first philosophy is a bit broader
in what it allows as possible candidates than traditional
users of the expression might have liked but it is close
enough to traditional usage to cause no confusion. In
other words, this is here a matter of import, a first
philosophy could be an incorrigible sense datum language,
a sancrosanct set of common sense beliefs, or the purported
necessary truths of logic rather than the results of
traditional philosophical speculation. Something is a
first philosophy if it must be included in any adequate
account of the world and is such that any claim that is
inconsistent with it is thereby shown to be incorrect.
Quine has, as is very well known held that there can be no
"pure experience language" (see <u>Word and Object</u>, p. 22),
that the ordinary man's views are far from unrevisable,
and that there are no necessary truths (see, among many
other sources, "Necessary Truth").[5] (I am aware that all
of these views have their defenders but prefer to ignore

them here.) It is interesting to watch the interanimation of Quine's seemingly diverse doctrines. More traditionally conceived, the results of many philosophers' metaphysical speculations would (when they are conceived as necessary truths as they usually were [are]) be first philosophies. (More on this matter very shortly.)

If there can be no first philosophy then there is nothing to which alternative physics can be equally compatible, as there is physics with respect to which alternative translations can be equally compatible. (It should be noted that it is not here claimed that it is not possible to develop alternative physics, physics being underdetermined. There would be indeterminacy and not just underdetermination in physics if there were different physical systems all equally compatible with all the data (or all such that they tied on this matter, maybe none of them are perfect fits) and all equally consistent with a first philosophy. Let us pause here to consider a philosophical claim that if true and if construed in the manner it traditionally was (and still is as I shall note) would be a first philosophy (or part of one) that would play a role for physics relevantly analogous to the one physics here plays for analytic hypotheses. A good example here (and a familiar one) is Descartes' claim that the mind and the body are distinct entities.

Descartes claimed (here I recount traditional readings of
Descartes) that this was not a mere empirical truth but
something that could be known to be necessarily true.
Now, if Descartes were right about this then any theory
of physics would have to be compatible with this first-
philosophical thesis. If more than one set of physical
theories (where "physical theories" is to be construed
broadly enough to include the exotic posits of physics
and whatever things that, though they may not themselves
be properly described as physical matter, are needed to
develop our physical theories, i.e., the abstract entities
of mathematics and perhaps even such things as gravity)
fits the data in terms of which such theories are judged
and are compatible with this Cartesian first philosophical
tenet than physics would be indeterminate. Here mind-body
dualism plays the role for physics that physics plays for
analytic hypotheses. (One should note here that if one is
a dualist but holds that dualism is but a contingent truth
then dualism is not a first philosophical thesis, in
point of fact most dualists were (and are) not so modest
in their claims). Least one think that such first
philosophical claims are no longer made by respectable
philosophers, a reading of the 3rd of Kripke's lectures
"Naming and Necessity" will establish that first
philosophical claims are still made.[6] If Kripke were

right in claiming that the mind-body identity thesis is
not merely false but necessarily false then this would be
a first philosophical thesis which any and all physical
theories must be consistent with. Interestingly enough,
Kripke's critics on this matter have by and large tried
to show that he is wrong in his claim about the necessity
of the falseness of the identity thesis rather than
attempting to show that the identity theory is not false.
Hopefully, enough has been said to clarify the notion of
a first philosophy and its potential relation to physical
theories.

Quine's claim is, of course, that there are incompatible
translational systems all equally compatible with our
physics and all of which fit the relevant data equally
well. (i.e., assent/dissent behavior, truth-functional
sentences, matching up of stimulus meaning of occasion
sentences, etc.). Physics provides the background theory
with respect to which different systems of analytic
hypotheses are indeterminate, however, there is no further
theory available for physics. One should note that it
would be misguided to remark here that we might simply
"write up a first philosophy--for example, that God
Exists". Thus, in the concocted first philosophy we
have a theory with respect to which indeterminacy can
arise in physics. The reason that this is on the wrong

track is that if the hypotheses conjectured is but a contingent truth then is not a first philosophical thesis and if it is claimed to be a necessary truth one must not merely assert it but render the claim plausible. The defender of the "no first philosophy thesis" holds, obviously enough, that it is very unlikely that this can be done. (To hold that it was a necessary truth that it could not be done would be a self-defeating claim here.) Likewise, Quine does not on my reading claim that it is a necessary truth that there can be no "pure experience language".

In making conjectures about our rock the physics of the day provides the "parameters of truth" whereas there is no background theory in the case of translation strong enough to rule out incompatible hypotheses. (The perhaps most tempting attempts to provide a physical background that could support one analytic hypothesis over other lies in materialistic accounts of the mental considered in Chapter III.) I shall return to these matters and attempt to render these matters less obscure after considering Quine's reply to Chomsky.

Quine thinks he has replied to the very objection raised by Rosenberg, Rorty, etc. in his reply to Chomsky's "Quine's Empirical Assumptions" it is worth looking at his answer in some detail. Quine evidently thinks that

no one who had properly understood what he said in Word

and Object would raise the objections that Chomsky raises.

Quine's account of the way in which translation and

physics are alike can be garnered from the following:

> In respect of being underdetermined by all possible translation and theoretical physics are indeed alike. The totality of possible observations of nature made and unmade, is compatible with physical theories that are incompatible with one another. Correspondingly the totality of possible observations of verbal behavior, made and unmade, is compatible with systems of analytic hypotheses that are incompatible with one another. ("Reply to Chomsky", p. 302)

Quine has then stated the criticism of his position at

least as clearly as have any of his critics. Before con-

sidering Quine's account of the difference between transla-

tion and physics it is useful here to consider aspects

of the opening chapter of Word and Object where Quine sets

out his view on, among other things, taking our own views

seriously.

The general import of the claim here is that indeter-

minacy arises when then there is a background theory which

allows the development of two or more incompatible but

equally well confirmed hypotheses   all of which are in

accord with the relevant data and the background theory.

As stated, for translation physics plays such a role,

however there is no parameter of truth in the case of

physics over and above the theory itself and hence indeter-

minacy does not arise in physics. A parameter of truth

then is a kind of limit on acceptable hypotheses, when the
parameter fits incompatible hypotheses then indeterminacy
arises in the subject matter with which the aforementioned
hypotheses deal. This then allows us to understand what
Quine means when he says that we wonder about analytic
hypotheses without benefit of parameters of truth.

To express this in another way, if two mutually in-
compatible hypotheses about a translation fit the data
equally well and both are compatible with the physics we
accept, then translation is indeterminate. The parallel
between translation and physics ends in the absence of
anything to play the role for physics that physics plays
for translation.

> It is rather when we turn back into the midst
> of an actually accepted present theory, at least
> hypothetically accepted, that we can and do speak
> sensibly of this and that sentence as true. Where
> it makes sense to apply 'true' is to a sentence
> couched in terms of a given theory and seen from
> within the theory, complete with its posited
> reality. (Word and Object, p. 24)

A bit of comment on the notion of a given theory needs to
be made here. (Quine's theory of truth is considered in
detail in Chapter VI.) As noted earlier, we cannot simply
make up any sort of theory and couch sentences in terms of
it. We are restricted to theories which fit the canons
of evidence and the data and which make no first philo-
sophical claims. (One should note that given that Quine

has arguments against such matters that do not assume the
ITT, this claim begs no question with respect to that issue.
This is not to say that some of his arguments against
first philosophical claims do not assume the truth of the
ITT.) Given this we can see why the claim that parallel
arguments would show physics indeterminate if translation
is indeterminate are based on very important misunder-
standings. Further, and here we perhaps see the extent of
the misunderstanding of Chomsky/Rosenberg/Rorty/et. al
criticism of the ITT. If physics were indeterminate and
there were no first philosophical claims that could be
made then there would be no posited reality in which any
claim could be made, let alone the claim that transla-
tion is indeterminate. I should point out that this is
not at all a contradiction of my earlier claim (page 7
this chapter) that the critics argument was at best an
ad hominem. On their understanding of the matter, given
that they failed to grasp the importance of holding
that physics is its own parameter of truth this is all the
argument can be. However, not wishing to be uncharitable
here, the critics have raised a question that is very well
worth asking even if they did not really see why the
question they raised had an interest beyond that of an
argument ad hominem. (These remarks may seem a bit harsh
and it of course may well be that the authors of the

articles considered here understood matters better than their writings indicate: all of the articles listed also, it should be noted, make points of interest not connected to the issue considered in this chapter.)

Our understanding of Quine's theory of evidence may be facilitated by considering what sort of theory of evidence Quine is opposing. Richard Schuldenfrei in "Quine in Perspective"[7] has given, in a perhaps unavoidably obscure way, a good start on sorting out this matter. (Schulderfrei's paper can also be read as offering evidence that the basic point of much of Quine's argument for the ITT is circular in that he argues for the ITT by claiming that there are no propositions (where propositions are construed as abstract objects in a meaning theory) while arguing against propositions by appeal to the ITT.) Schuldenfrei says Quine's general notion of evidence should be contrasted with Cartesian notion of evidence which attempts to preserve certainty in all inferences, Millian notions of evidence based on induction by enumeration on the basis of observations, and with Carnap's attitude toward simplicity in "Empiricism, Semantics, and Ontology". (As perhaps may be clear from my previous remarks, I am in nearly complete agreement with Schuldenfrei's remarks concerning the import of the notion of evidence in Quine, though I find it more efficacious to stress the role of

"posits" in Quine's philosophy of science.) What Quine
is denying in the passage quoted on page 112 here (and
in section 6 of <u>Word and Object</u> at greater length) is
that any notion of evidence which explains the warrant
for the claim that a rock has a certain composition in
terms of enumerative induction on the basis of observa-
tion or some such standard empiricist line will get us
anywhere. Without a previously adopted theory we will
not know what to take our observations as evidence for.
Further the passage (as well as much else in Quine) can
be seen as denying that it is possible to compare theories
with reality. All we can do is to compare new theories
in terms of their relative compatibility with our currently
uncontroversial theories and in terms of how they fit
what we agree to take as the relevant observations. (Per-
haps it should rest unsaid that along with the rejection
of any "first philosophy" Quine also rejects the idea of
a "pure sensation language" (sense data). This is impor-
tant here as this is another possible source of a theory
with respect to which alternative physics could be in-
determinate.) Having attempted to clarify the relevant
material, I shall turn to an examination of in what the
factuality of physics consists.

> Have we now so far lowered our sights as to settle
> for a relativistic doctrine of truth--rating the
> statements of each theory as true for that theory,

> and booking no higher criticism? No so. The
> saving consideration is that we continue to take
> seriously our own particular world-theory or
> loose fabric of quasi-theories, whatever it
> may be. Unlike Descartes, we own and use our
> own beliefs of the moment, even in the midst of
> philosophizing, until by what is called scien-
> tific method we change them here and there for
> the better. Within our own total evolving
> doctrine, we can judge truth as earnestly and
> absolutely as can be; subject to correction, but
> this goes without saying. (Word and Object,
> pp. 24-25)

Again, one would like to have more than this to go on but

I think an account of "fact of the matter" can be extracted

from this. Given the ongoing theory we employ is such

that within it we can confirm or infirm a hypothesis con-

cerning something there is a fact of the matter concerning

the matter in question.

There are a couple of objections that are worth

considering at this point. The perhaps natural tendency

to object that there would be facts even if there were no

persons to construct theories is itself a very uncontrover-

sial aspect of the theories we do have. In other words,

the attempted objection here turns on the assumption that

the notion of fact of the matter developed here has the

consequence that if there were no persons to construct

theories about snow then snow would not be white. But,

as stated, it does not have this consequence and thus it

cannot be refuted by claiming that it does. To reiterate,

it does not have this consequence as one of the most

firmly entrenched of our ongoing beliefs is that snow is
white independently of whether or not there is anyone
around to construct a theory in which "snow is white" is
true. A bit more should perhaps be said concerning this
sort of view does not have the consequence that the truth
of "snow is white" is relative-to-a-theory. It is the
fact that we use and can use only the theory we have
(recall the metaphor of the boat) that blocks Quine from
being committed to any sort of pernicious truth-as-relative
to theories doctrine. The best ongoing theory is treated
as if it were the absolute truth, the truth-as-relative
doctrine requires that we find some point of view outside
of our theory to make the claim that truth is relative to
theories. More needs to be said on these matters and,
as stated in Chapter I, more is said in the next chapter
on these matters. I include these remarks here in order
to attempt to avoid certain objections at this point.

One might also object as follows: your notion of
fact of the matter has the absurd consequence that when
you alter your theories the facts alter. The reply to
this object lies in seeing that when we alter our
theories (assuming that we do not do so capriciously) we
would be working in the context of a new theory in terms
of which we would say that we were always ill-advised to
include the now rejected sentences in our theory. To

illustrate a bit further, suppose that we are at some

point in time where we were well advised to think that

dwarfs have poor memories, as Aristotle did.[9] Now, we

regard such views as quite silly. Does this then show

that the fact of the matter concerning the memonic powers

of dwarfs has altered? Of course not, it shows that we

should never have thought that in the first place. Having

considered these objections, about which more could be

said of course, we can return to a consideration of

Quine's answer to Chomsky's question about the difference

between physics and translation. Wherein does the dif-

ference lie?

> Essentially in this: theory in physics is an
> ultimate parameter. There is no legitimate first
> philosophy, higher or firmer than physics, to which
> to appeal over physicists' heads. Even our
> appreciation of the partial arbitrariness or even
> underdetermination of our overall theory is not a
> higher-level intuition; it is integral to our
> under-determined and evolving theory of nature, the
> best one we can muster at any one time.... ("Reply
> to Chomsky", p. 303)

If then a remark is in conflict with our ongoing physical

theory it is, as this theory is the ultimate parameter

of truth, therefore to be rejected. If a claim is such

that there are no parameters operating, then (and this

is perhaps another way of saying what was said earlier)

the claim is indeterminate. Or, if the claim is such

that contrary claims can be equally well confirmed, then

there is no fact of the matter concerning the subject mat-
ter of the claims.

It is now possible to give an account of what Quine
takes being a realist to consist in which helps to bring
out both the importance of the notion of parameters of and
to explain further the crucial differences between trans-
lation and physics.

> ...adopt for a moment my fully realistic attitude
> toward electrons and muons and curved space-time,
> this falling in with current theory of the world
> despite knowing it is in principle methodologi-
> cally underdetermined...The point about the in-
> determinacy of translation is that it withstands
> even all this truth, the whole truth about nature.
> This is what I mean by saying that, where the
> indeterminacy of translation applies, there is
> no real question of right choice; there is no
> fact of the matter even to within the acknowledged
> underdetermination of a theory of nature. ("Reply
> to Chomsky", p. 303)

To be a realist about the entities one's theory posits
then is, in effect, to take these posits as being par-
tially constitutive of the ultimate parameters of truth,
while knowing that the theory is methodologically under-
determined. The question as to what theory of the world
we should "fall in with" does not really arise given the
conservative Neurathian tenants of Quine's epistemology
there will be only one best on going theory. (Harry Beatty
says that the difference between translation and physics
is that even if physics were determinate, translation
would still be underdetermined.)[10] Now, as should be

clear, this way of putting things is not really what we need here, but if one did need to explain the difference between translation and physics in one sentence, this is as good a way to do it as any and much better than many other ways one can hear).

Before proceding to a consideration of the arguments of Rosenberg and Rorty it seems to me useful to set out what Quine thinks the ITT should "do for us" and connect this up with the considerations about the indeterminacy of the mental advanced in Chapter III. I shall also consider a variant of the physics objection which might well be raised, though to my knowledge it has not yet been raised in the literature of the ITT.

In Chapter III there were two arguments offered for the indeterminacy of the mental, one in which the ITT is assumed and one where no such assumption is made. It is of interest to note that quite the same sort of relation holds, on my account of these matters, between the attribution of mental states to persons and physics as holds between translation and physics. If these arguments work then one cannot attempt to say Quine improperly banned from the realm of nature reference to the expression of ideas in sentences which would serve to give us determinate translation. Quine sets out in his reply to Chomsky the connection of these two issues (determinacy of the mental

and the ITT):

> A conviction persists, often unacknowledged, that
> our sentences express ideas, and express these
> ideas rather than those, even when the behavioral
> criteria can never say which. There is a stubborn
> notion that we can tell intuitively which idea some-
> one's sentences express, our sentences, anyway,
> even when the intuition is irreducible to
> behavioral criteria. This is why one thinks that
> one's question 'What did the native say?' has a
> right answer independent of choices among mutually
> incompatible manuals of translation. ("Reply to
> Chomsky", p. 304)

(There may well be some truth in the claim that Quine's

view that "radical translation begins at home" is an

analogue to Wittgenstein's "private language argument".[11]

It is no great surprise to discover that Word and Object

and the Philosophical Investigations have some common

targets, not smallest among them what Quine calls the

"myth of the museum" ("Uncritical semantics is the myth

of a museum in which the exhibits are meanings and the

words are labels" ("Ontological Relativity", p. 27). An

extensive account of the common enemies of the Philosophical

Investigations and Word and Object would, I think, be

instructive and interesting in many respects.)

Instead of objecting that if the ITT holds similar

arguments would show physics to be indeterminate, one

might ask why the parameter of truth could not be the

ongoing physics plus our various set of analytic

hypotheses or perhaps our general semantical theory which

entails these analytic hypotheses. Then, the objection
goes, we would have a parameter of truth which would rule
out competing sets of analytic hypotheses, leaving a
unique best theory. Some might even urge that the
deference here paid to physics is but some sort of
bigotry and one might, somewhat truculently, ask "what is
so great about physics that isn't great about highly theo-
retical linguistics"? There is, I think, a fairly
straightforward answer to this charge which follows the
same lines as the reply to the objections of Rosenberg,
Chomsky, et. al. Recalling that we adopt a fully realis-
tic attitude toward the posits of our theories, we can
see that if we are to be so realistic it must be the case
that within the context of our theory only the posits
we have made and not incompatible posits are compatible
with our physical theory. However, in the case of
analytic hypotheses, or if one likes our general linguistic
theory (beyond the small range of observation sentences
and such) even if we do include ongoing sets of analytic
hypotheses within our parameter of truth we could still
just as well confirm contrary analytic hypotheses without
altering anything else in the general theory we hold.
When we alter our beliefs in the determinate aspects of
our theory, we are forced to make corresponding adjustments
throughout our theory, however, we can alter our analytic

hypotheses by themselves without such corresponding re-
adjustments. Here, one should note that if we do adopt
an attitude toward our analytic hypotheses that makes us
refuse to alter them, then, of course, no alternative
hypotheses will be acceptable to us. However, the dif-
ference between physics and translational hypotheses
reemerges in the differences the alternation of theories
would require. Also, it should be noted that alternative
analytic hypotheses might well be just as convenient as
the ones we refuse to give up. Mere insistence that we
will hold with a certain set of analytic hypotheses come
what may is not enough to get us determinate translation.[12]
One can perhaps rest here with Harty Field's comment con-
cerning the viability of sticking with physicalism (where
the physical is construed broadly, of course): "This is a
methodology that has proved extremely fruitful in science,
and I think that we would be crazy to give it up in
linguistics."[13]

One might also object here that Quine is not really
consistent in his physicalism. After all he includes in
his principles not only nonphysical entities such as sets
but also certain methodological principles (i.e., those
considered in Chapter IV). The more Quine includes in
his views the more apparent plausibility is lent to the
claim that his dislike of propositions and such is but

some sort of bigotry. There, however, seems to be a fairly straightforward rationale for the privileged status of numbers (etc.) and methodological principles. Namely, we need numbers to do physics as well as methodological and epistemological principles. However, we do not need propositions, synonymy relations and such to study molecular reality. Thus there is at least a reason for giving numbers and epistemological principles a status not given to propositions and such. Quine, insofar as I know, does not consider in his writings where his epistemological and methodological principles (there is no weight here being put on any sort of distinction between these two sorts of things) belong in an account of things thus I cannot really say if he would in fact agree with this or not. However, it at least appears to be the case that if this were correct it would go some way toward further explaining the disparity in Quine's attitude toward numbers and such versus his attitude toward determinate translation relations.

I wish now to turn to my attempt to consider in some detail the arguments for the criticism set out earlier. In the interests of brevity, I shall consider only the papers of Rosenberg and Rorty, the conclusion of all the papers considered here being largely the same.

One can, I think, see where Rorty's argument goes wrong by considering his remarks on verificationism in the argument for the ITT. What I shall argue is that Rorty sees in Quine's rejection of the verification theory of meaning of the positivists not a rejection of the idea that sentences have no meaning taken in isolation from whole theories but as a rejection of any and all inferences from unverifiability to nonfactuality. As I attempted to show in the previous chapter (additional textual support can be found in the Martin and Smith paper) what Quine rejects is the first half of the above, not the second. Quine does hold a kind of "holistic verificationism" which infers from the univerifiability of a claim by the total of our theories to the indeterminacy of the claim in question.

To return to Rorty who says

> can we make anything of the notion that there is
> an 'unverifiability' involved in the 'canons' of
> linguistics of a sort not present in the 'laws'
> of chemistry? Not, I think without adopting a
> verificationism which is pretty close to what
> Quine repudiated in "Two Dogmas of Empiricism".
> (Rorty, p. 449)

We can then answer Rorty's argument if we can make something of an unverifiability found in linguistics but not in chemistry. Chemistry (construed here as the study of the composition, structure, properties, and reactions of matter, etc.) is verifiable or infirmable with respect to

particular hypotheses in terms of physical observations
and hence within the ongoing physics there will be arrange-
ments of physical matters tending to confirm or infirm
chemical hypotheses which at the time rules out incom-
patible hypotheses. Again, in the case of translation
all the evidence there could be accords equally well with
incompatible hypotheses and hence confirms no one
hypothesis. Of course, our practical ability to check all
of this out is not at issue here.

Likewise in the case abstract entities we need only
say here the evidence for their existence lies in the
systematic simplicity they accord to the workings of our
predictive theories, etc. (Though one would feel better
if one had a fully worked out nominalistic account of
abstract entities.) As I attempted to show analytic
hypotheses are not evidenced for by any physical matter
(the most plausible area to look was in terms of brain
states) but chemistry, etc., are. This is, I take it
what can be made of the difference between physics and
linguistics in a way that will answer Rorty's question.
For rather stark purposes Rorty's argument can be set
out as follows:

1. If translation is indeterminate and physics deter-
minate then we can make something out of the notion that
there is an unverifiability in the canons of linguistics

not found in physics.

2. It is false that we can make something out of the
notion that there is an unverifiability in the canons of
linguistics not found in physics.

_____

It is not the case that translation is indeterminate and
physics determinate.

As Rorty says, his argument leaves open the question
as to whether both or neither are determinate. My reply
was then to attempt to show that 2 above was false.

Let us consider Rosenberg's reasons for holding
that the disputed parallel between physics and translation
holds. Rosenberg begins his argument for the claim that
linguistics is no more underdetermined than physics by
setting out what he acknowledges is a familiar account
of postulational procedure in the sciences and then
attempts to show that quite the same procedure is followed
in the case of translation. Rosenberg in the synopsis
he provides of his paper is quite clear so perhaps it
will be helpful to quote it in full before considering
his argument.

> In Word and Object, Quine argues from the observa-
> tion that 'there is no justification for collating
> linguistic meanings, unless in terms of man's
> dispositions to respond overtly to socially obser-
> vable stimulations' to the conclusion that 'the
> enterprise of translation is found to be involved
> in a certain systematic indeterminacy'. In this

paper, I propose to show (1) that Quine's thesis, when properly understood, reveals in the situation of translation no peculiar indeterminacy but merely the ordinary indeterminacy present in the case of empirical investigation; (2) that it is plausible that, because the subject of inquiry is language, we are in a better position that we are in other areas of investigation; and that, in any case, Quine's arguments are impotent, for they are either contradictory or incoherent and (3) that Quine is led to his radical conclusions because he confuses a trivial and unexciting indeterminacy, which does obtain, with the striking indeterminacy for which he argues, which does not obtain. (Rosenberg, p. 403)

Perhaps one should mention in fairness to Rosenberg that his paper was one of the earliest published attempts to reply to the ITT and that it was written before the publication of "Ontological Relativity" and "On the Reasons for Indeterminacy of Translation". After setting out his account of postulational science he offers the following by way of analogy:

Let us regard the translator's analytic hypotheses, taken collectively, as a theory of the native language. Corresponding to the observable macro-phenomena which constitutes the (external) subject matter of a physical theory will be the totality of observable speech dispositions of the native speakers. And corresponding to the postulated micro-entities and macro-properties of a physical theory will be the categorical structure, the set of term translations and the referential apparatus, which is projected for the native language by a set of analytic hypotheses. (Rosenberg, p. 411)

We need then to set out what Rosenberg takes the ITT to amount to and what he thinks the parallel between physics and translation is. Again, I shall try to show that Rosenberg has failed to see what the crucial difference

between translation and physics is for Quine. To indulge
in an extended Quinian/Neuratian metaphor, physics is
the matter that makes up the boat we float on and there
will be incompatible translational schemes all compatible
with the structure of the boat, the boat is a craft we
must hold to while fully seeing its imperfections and
making no claims that it is the only boat that we could
have built, to have a first philosophy would be to reach
land where we might build other boats and compare them to
each other but there is no land.... On this metaphor
physics and linguistics are not coequal parts of the boat
but such that if more than one account of the latter is
compatible with the former then there is no fact of the
matter concerning the latter.

Rosenberg says

On this interpretation, Quine's thesis of the
indeterminacy of translation amounts to the
claim that
 For any pair of languages A and B, there are
 equally good incompatible sets of analytic
 hypotheses for the translation of one into
 another. (Rosenberg, p. 412)

Here is what Rosenberg takes the analogous thesis with
respect to physics to be:

 Correlative to the thesis that in any case of
 translation there exists a pair of equally good
 sets of analytic hypotheses which are incompatible
 is the thesis that there exists a pair of
 equally good incompatible theories for a given
 body of phenomena. (Rosenberg, p. 412)

Rosenberg sums up his point

> The important critical point is just this: what-
> ever the degrees of indeterminacy inherent in
> theoretical language in general we are <u>no worse</u>
> <u>off</u> with respect to theories of language than we
> <u>are</u> with respect to any scientific theory.
> (Rosenberg, p. 413)

Briefly put, Rosenberg's analogy breaks down because

nothing plays the role for physics that physics plays for

translational hypotheses.   Rosenberg has then failed to

notice the sense in which Quine does provide a sense of

fact of the matter for physics and not for linguistics.

Footnotes

[1] Among those who have raised the objection to the indeterminacy of translation thesis considered herein are: Noam Chomsky, "Quine's Empirical Assumptions" in Words and Objections: Essays on the Work of W. V. Quine, ed. by Donald Davidson and Jaakko Hintikka (Dordrecht: Reidel, 1969), pp. 53-68; Noam Chomsky and Jerrold J. Katz, "What the Linguist is Talking About", Journal of Philosophy, 71 (June 27, 1974), pp. 347-366; Donald Hockney, "Conceptual Structures", in Conceptual Change, ed. by Pearce and Maynard (Dordrecht: Reidel, 1973), pp. 141-166; Edwin Martin and David Smith, "On the Nature and Relevancy of Indeterminacy", Foundations of Language (forthcoming): Jay Rosenberg, "Synonymy and the Epistemology of Linguistics", Inquiry, 10 (1967), pp. 405-420; Richard Rorty, "Indeterminacy of Translation and of Truth", Synthese, 23, (1972), pp. 443-462; Michael D. Root, "Quine's Methodological Reflections", Metaphilosophy, 5 (January, 1974), pp. 36-50; Stephen P. Stich, "Grammar, Psychology, and Indeterminacy", Journal of Philosophy, 69 (December 7, 1972), pp. 799-818.

[2] This sort of thing can be found, for example, in Thomason, J. M., "Ontological Relativity and the Inscrutability of Reference", Philosophical Studies, XXII (1971), pp. 208-215.

[3] Føllesdal, D., "Indeterminacy of Translation and Under-determination of the Theory of Nature", Dialectica, XXVII (1973), p. 290. It seems to me that Føllesdal's account of the difference between physics and translational hypotheses, is not quite right in that he seems to impute uniqueness to the posits of our theories or suggests that we are not proper in positing something if another incompatible posit could serve equally well. Also, Føllesdal's account of the difference between translation and physics does not explain either why we need to take our ongoing physical theory as the parameter of truth, why a critic of the ITT is not justified in insisting that translational hypotheses be included in the parameter of truth, further Føllesdal seems to suggest that there is some sort of theory independent evidence for our physical theories. Nevertheless, his account of the difference

between physics and translation is an improvement over the sort of view put forth in the papers listed in footnote 1.

[4]Quine, W. V., "Reply to Chomsky", in Words and Objections, ed. by J. Hintikka and D. Davidson (Dordrecht-Holland, 1969), pp. 302-311.

[5]Quine, W. V., "Necessary Truth", in The Ways of Paradox (New York: Random House, 1966), pp. 48-56.

[6]Kripke, Saul, "Naming and Necessity", in Semantics of Natural Language, ed. by D. Davidson and G. Harman (Dardrecht-Holland, 1972), pp. 253-355.

[7]Schuldenfrei, R. "Quine in Perspective", Journal of Philosophy, LXIX (1972), pp. 5-15.

[8]Ibid., pp. 13-14.

[9]Aristotle, De Memoria et Reminiscentia, 453.

[10]Beatty, Harry, "Behaviourism, Mentalism and Quine's Indeterminacy Thesis", Philosophical Studies, XXVI (1974) pp. 97-110.

[11]Ibid., 106.

[12]Putnam, H and Oppenheim, P., Unity of Science as a Working Hypothesis", Minnesota Studies in the Philosophy of Science, Vol. II (Minneapolis: University of Minnesota Press, 1972), pp. 3-27 contains the sort of unity of science notion that I am herein using as a working hypothesis.

[13]Cf. Field, H., "Tarski's Theory of Truth", Journal of Philosophy, LXIX (1972), pp. 347-375.

CHAPTER VI

QUINE'S THEORY OF TRUTH: THE TWO ASPECTS

In the preceding chapters I have attempted to show
that the major criticisms of the ITT in the literature
to date are illfounded. I wish now to turn to a considera-
tion of Quine's own views on truth. Quine presents his
views on truth largely as if they are simple platitudes,
however, his views are often in fact expressed in a way
that is bewilderingly obscure. In considering Quine's
views on truth one finds two quite distinct tendencies,
one being his claim that Tarski has said all there is to
say ("Notes on the Theory of Reference"[1] is as good an
example of this strain of Quine's views as any) and the
other being his claim, considered to some extent in the
previous chapter, that "truth itself is immanent to the
conceptual scheme" ("Replies", p. 334) Or, more obscurely,
"Where it makes sense to apply 'true' is to a sentence
couched in terms of a given theory and seen from within
the theory, complete with its posited reality". (Word and
Object, p. 24) In this chapter I shall be concerned with
several matters. Firstly, I shall attempt to offer an

explanation of what Quine means by saying "truth in im-
manent to conceptual schemes". This will involve another
more or less general survey of the relevant texts.
Secondly, I shall defend that aspect of Quine's views
(or my reconstruction of them) from objections raised by
Donald Hockney in "Conceptual Schemes"[2], one of the few
serious attempts to understand what is an extremely ob-
scure part of Quine, or at least obscure to Hockney and
myself. The obscurity of this material is made even more
important by the central part Quine's views on the theory
dependence of truth play in his philosophy. Quine's
views on truth are not, though perhaps this does not
need saying, just another obscure bit of philosophy but
are central to Quine's outlook. Thus understanding
these matters becomes of paramount importance to anyone
whose general "philosophical outlook" is in accord with
Quine's. I shall also attempt to show, analogous to my
remarks in Chapter III on Sommers, Davidson, and Sellars
that Quine's doctrines of truth as "imminant to conceptual
schemes" are undercut by the ITT.

With respect to the first mentioned aspect of Quine's
views on truth, I shall be setting out certain highly
important arguments developed by Stephen Stich in
"Dissonant Notes on the Theory of Reference"[3] to the effect

that on general Quinian grounds the theory of reference
is no better off than the theory of meaning. This
argument is of high import in that Quine's reason for
not giving up the notion of truth along with notions from
the theory of meaning is essentially that he thinks
Tarski has shown us the way to explain truth in a way
free of the obscurities that caused him to reject the
theory of meaning. I shall also have occasion to mention
some of the conclusions of Field in "Tarski's Theory of
Truth" where he attempts to show that Tarski, counter to
Tarski's claim, did not explain truth without appeal to
notions from the theory of meaning.[4] Stich and Field both
offer persuasive arguments to the effect that on Quinian
grounds truth is no better off than meaning. The attack
on meaning vis a vis the ITT then can also be directed
just as well at the notion of truth. As Stich's paper
is both shorter and earlier, I shall set out only
Stich's argument to the conclusion mentioned above.

One might, in the terminology of Rescher,[5] see Quine's
views on truth as having a criteriological and a defini-
tional aspect. The definitional aspect included those
writings where Quine says that Tarski has said what needs
to be said. The criteriological half involves Quine's
claims that truth is immanent to conceptual schemes. The
claim I shall herein advance is that both the definitional

and the criteriological aspect of Quine's theory of truth
are undercut by the ITT. I shall be arguing that Quine's
account of truth is undercut also by Quine's claim that
our current "conceptual scheme" is but one of many we
could have chosen to employ. This will serve as a
transition to a consideration of whether or not there are
alternative conceptual schemes and further whether or
not the notion of "conceptual scheme" is itself tenable.
In my attempts at criticism of Quine I should indicate
that I share the attitude of Richard Schuldenfrei:

> But the kind of system that Quine is describing,
> connected as it is by plausibility rather than
> certainty, leaves room for attack. One may use
> Quine against himself in the same way Quine uses
> the data he starts with against itself. One may
> show that by taking the bulk of what Quine says,
> but rejecting other things, one can improve his
> system. A simple counterexample will not do, and
> that is where many philosophers have gone wrong.
> The objections raised, if they are to stand, must
> point in the direction of an alternative to
> Quine's system. Objections cannot be proposed
> as though they were facts independent of any
> theory, which any acceptable theory must account
> for.[6]

As good a way as any to launch into these matters is
by a consideration of Hockney's criticism of Quine's
account of conceptual schemes, as set forth in Hockney's
paper "Conceptual Schemes". The method of exposition of
Quine will be similar to that followed in Chapter VI,
"Physics, Linguistics, and the ITT". I will present the
views of a Quinian critic and will then give my own

explication of Quine in response to the critic. The main
drift of Hockney's exposition is that Quine is aiming at
showing that a distinction between a person's language
and a person's theories would be spurious. Hockney refers
to the following passage from Quine:

> We learn thus to use the component words to form
> new sentences whose relative truth conditions are
> derivable. Which of these dependencies of truth
> value are due to meaning, and which belong rather
> to a substantive theory that is widely shared,
> is in my view a wholly unclear question. (Hockney,
> p. 143)

On Quine's view it is a mistake to regard theory as one
matter and language another. Hockney's summary of Quine's
views is put thusly:

> To recapitulate: a person's conceptual structure,
> on Quine's view, consists of: a syntax, a
> semantics--a set of maps from sentences generated
> by the syntax into a specified domain, which
> maps depend on shared beliefs relative to some
> community and the (set theoretical) union of his
> theories--that is, sets of sentences he believes
> to be true, relative to some vocabulary and some
> subject matter. (Hockney, p. 144)

Hockney, interested for his part in the individuation of
conceptual schemes, turns to a consideration of Quine's
views on this matter. Hockney for some reason says that
one would naturally think changes in reference would
serve to differentiate conceptual structures. (See
Hockney, p. 144). However, Quine's thesis of the
relativity and inscrutability of reference makes these
matters not quite so simple. Hockney attributes three

theses on reference to Quine. The first of the theses
says that questions of reference have no meaning except
relative to some background language. The second holds
that even if we are given the totality of sentences
relative to a language, there is still not any sense
in asking the reference of any term except relative to
another background language. The last of the three
theses holds that there does not exist a "privileged
background language" (see Hockney, pp. 144-145. Hockney
quotes various passages from "Ontological Relativity" in
support of these claims. Hockney states well the import
for the matters at hand of these three theses so I shall
take the liberty of quoting in full the relevant material:

> Consider a theory T1. The vocabulary of T1 con-
> sists of signs as quantifiers, truth functions,
> identity, singular terms and general terms.
> Select those sentences which are true according
> to the theory. Let us say that they form a
> set S. These truths will be true relative to a
> model for T1' (that is T1 minus interpretation).
> Now T1' may be given another interpretation:
> some nonempty universe of objects with singular
> terms assigned to one place predicates, and so
> forth. Supposing that each sentence in S comes
> out true under this interpretation, we have a
> model M2 for T1' resulting in a theory T2. So
> far this is all orthodox. Quine's point is not
> simply that model M2 for T1' is available, only
> because there is some background language in
> terms of which it may be specified. It is this
> and more. Namely, that any question about what
> the objects of T1 "really are" is relative 'to
> some choice of a manual of translation' of T1
> into T2. T1 may be interpreted as T2, but
> correctness of such interpretation cannot arise

except relative to the selection of some such
manual. This is always the case. The protest
that T2 may simply be the 'containing theory'
is declared empty. It is still a case of degenerate
translation depending on a homophonic rule.
(Hockney, pp. 145-146)

The ITT and the inscrutability of reference play an

important part in Hockney's account of Quine's account of

conceptual differentiation.[7] After setting out the three

theses mentioned above Hockney turns to a consideration

of Quine's theory of conceptual scheme differentiation.

I turn now to Quine's statement of his theory
of conceptual difference and its ground. He
says: 'We have been beaten into an outward
conformity to an outward standard; and thus it is
when I correlate your sentences with mine by the
simple rule of phonetic correspondence, I find
the public circumstances of your affirmations
and denials agree pretty much with my own. If
I conclude that you share my conceptual scheme,
I am not adding a supplementary conjecture so
much as spurning unfathomable distinctions;
for, what further criterion of sameness of con-
ceptual schemes can be imagined. (The quotation
by Hockney is from "Speaking of Objects")

(It should be noted here that Quine spurns what he spurns

in the above because the spurned item turns upon unveri-

fiable conjectures.) Hockney's criticism of Quine

are that

The facts available to him are utterance strings,
nods, shakes, and stimulus hits. Conceptual dif-
ferences arise if you nod enough when I shake
enough given the same stimulus conditions for
utterances which correspond via an arbitrarily
selected manual. I have no quarrel with the
doctrine that reference is relative. The
trouble lies with indeterminacy and the inscruta-
bility of reference. I will argue that the only
grounds for the doctrine of the inscrutability
of reference are those advanced for the

indeterminacy thesis and that the latter won't
wash.   (Hockney, p. 147)

Let us then consider why Hockney thinks the arguments for

the ITT won't wash.   Hockney's criticism of Quine is

stated in a somewhat obscure way but his objection comes

down to the criticism considered in the previous chapter:

> what is interesting is that this contrast
> between physical theory and translation cannot
> be consistently maintained, and that no conclu-
> sion should yet be drawn about the indeter-
> minacy thesis (Hockney, p. 150)

Hockney does, however, pose a question that can serve for

a starting point for a further consideration of the "truth

is immanent" doctrine.

> Questions about the truth of a sentence within a
> theory compare with questions of the correctness
> of translation given a set of analytic hypotheses.
> Here there is no contrast.   Theory and transla-
> tion fare alike.   (Hockney, p. 152)

I wish then to consider what account can be given of truth

within a theory that does not, in relevant ways, compare

with questions of correctness of translation within given

sets of analytic hypotheses.   In order to consider this

matter it is, as stated, required that we undertake a

more or less general survey of Quine's remarks on the

criteriological aspect of the theory of truth.

I wish to begin by looking at the relevant aspects

of Chapter 1 of <u>Word and Object</u>.   After considering this

work, I shall look at other relevant texts, most

especially "Ontological Relativity".   Upon completing this

matter I shall be arguing that Quine's views on truth as immanent to coneptual schemes are undercut by the ITT. Further, I shall attempt to show that this undercutting is consistent with the remarks on verificationism and the primacy of physics set out earlier.

The general drift of the first section of Chapter 1 of Word and Object, "Beginning with Ordinary Things" is that language learning depends on the availability of public clues:

> Conceptualization on any considerable scale is inseparable from language, and our ordinary language is about as basic as language gets. (Word and Object, p. 3)

As indicated in the previous chapter, the insistence on the public nature of language is quite closely related to the claim that there is "no first philosophy", here it would be sensation talk, on which physics rests and with respect to which it could be indeterminate. Our understanding of words such as 'understood', 'real' and 'evidence' is itself dependent on the acceptance of this public realm of objects.

> On the face of it there is a certain verbal perversity in the idea that talk of familiar physical things is not in a large part understood as it stands, or that the familiar physical things are not real, or that the evidence for their reality needs to be uncovered. For surely the key words 'understand', 'real', and 'evidence' here are too ill-defined to stand up under such punishment.

> We should only be depriving them of the very
> denotations to which they mainly owe such sense
> as they make to us. (Word and Object, p. 3)

The first notion of a "conceptual scheme" offered in Word
and Object is that of a set of sentences about physical
object which is taken for granted by all of us. So far
all of this is simple enough. However, Quine seems to
put forth a claim which suggests that it might be possible
to ascertain the "extent of man's net contributions" to
our conceptual schemes. Quine's point in the material
that I shall quote is that there is a world independent
of our knowledge of it and that, even though the process
of investigation may be difficult, sense can still be
made of considerations concerning what we find in the
world and what we stipulate in our investigations:

> In assimilating this cultural fare we are little
> more aware of a distinction between report and
> invention, substance and style, cues and concep-
> tualization, than we are of a distinction between
> the proteins and the carbohydrates of our
> material intake. (Word and Object, p. 5)

As far as we are to take this analogy seriously, two
things are clear: in our first reaction we do not dis-
tinguish reports from inventions as we do not distinguish
proteins from carbohydrates and there is a difference
between reports and inventions which can be revealed by
investigation just as there is a difference between
proteins and carbohydrates which can be discovered by
investigation.

> Retrospectively we may distinguish the components
> of theory building, as we distinguish the proteins
> from the carbohydrates while subsisting on them.
> We cannot strip away the conceptual trappings
> sentence by sentence and leave a description of
> the objective world; but we can investigate the
> world, and man as a part of it, and thus find out
> what cues he could have of what goes on around
> him. Subtracting his cues from his world view, we
> get man's net contribution as the difference. This
> difference marks the extent of man's conceptual
> sovereignty--the domain within which he can revise
> theory while saving the data. (Word and Object,
> p. 5)

Quine's point in the above is that there is a limit to our

conceptual sovereignty, some hypotheses about the world

can be wrong. To some extent, hypotheses that can be

wrong can be located by setting out the limits of the

domain within which he can revise theory while saving

data. One should note here that the claim that there

are limits to the extent in which we can revise theory is

not incompatible with Quine's claim that any individual

sentence might be such that some day we might wish to

alter the attitude we take toward it.

I wish then, before going on, to explain why I

regard the above two passages as odious and further why

this matter is of importance for considerations concerning

truth and realism. The problem here is what to make of

the distinctions between 'report' and 'invention', etc.

Now, as earlier stated, Quine does seem to claim that

something can be made of these distinctions, obscure

though they may be. However, in the face of Quine's
general claims to the effect that there is no distinguish-
ing questions of theory and language it is hard to see
how <u>anything</u> could be made of these distinctions. Our
very investigation of cues and the purported subtraction
process itself depends on knowing what part of the data
we get as a result of this investigation is the result
of inventions and what part the result of reports. Quine
is not, of course, here claiming that the sentence left
over after the subtraction process are either neatly
divisible into information or report nor is he claiming
that the leftover sentences are "pure information". The
crucial claim is that there is a limit to the extent of
man's "conceptual sovereignty" and with it the claim that
there is something (the world) which imposes the limit.
The import of this matter for RAVism is that central to
the RAVist position is the claim that what is the case
and what our theories say are two different matters and
that there are limits to acceptable hypotheses. What is
of import here is not the clearness of the report/invention
distinction but whether or not there is any sense to the
notion of the limits of man's conceptual sovereignty.
Quine's position here is apparently that there is something
to be made of the notion of such a limit. Whether or not

the process can in fact be carried out is not what is at

issue here (Quine might well claim that it could never be

done) but whether or not the proposed objective of the

investigation makes sense. I shall be saying more of

these matters shortly, but I think that the problems with

the procedure quoted above are illustrative of the general

problems with the criteriological aspect of Quine's

theory of truth.

Section 2 "The Objective Pull: or E Pluribus Unum"

of Chapter 1 of Word and Object does not contain a great

deal of relevance here, being largely devoted to an

account of how children could come to learn terms on the

basis of public cues. Perhaps it is worth setting out

the conclusion of that section here:

> Different persons growing up in the same language
> are like different bushes trimmed and trained to
> take the shape of identical elephants. The ano-
> tomical details of twigs and branches will fulfill
> the elephantine form differently from bush to
> bush, but the overall outward results are alike.
> (Word and Object, p. 8)

The section following section 2 "The Interanimation

of Sentences" is more relevant in the effort to understand

what Quine's doctrine of the immanence of truth to con-

ceptual schemes comes to. In section 3 Quine argues, in

effect, that unless sentences are "associated" with other

sentences and not just nonverbal stimulation we would be

too greatly restricted in our production of new sentences:

"Association of sentences is wanted not just with non-verbal stimulation, but with other sentences, if we are to exploit finished conceptualization and not just repeat them." (Word and Object, p. 10) Let us then consider what this "association" is, other than the production of new sentences from old via the process of analogical substitution, to see if we can from Quine's conjectures on language learning get some insight into the truth as immanent doctrine. One form of association among sentences is in terms of a verbal response to a verbal stimulation in the form of a question. Given the high import of this matter in understanding Quine's remarks on the "parameter of truth" it is important to consider his remarks on the interanimation of sentences via their containing theory.

> Thus someone mixes the contents of two test tubes, observes a green tint, and says "There was copper in it". Here the sentence is elicited by a nonverbal stimulus, but the stimulus depends for its efficacy upon an earlier network of associations of words with words; viz., one's learning of chemical theory. Here we have a good glimpse of our workaday conceptual scheme as a going concern. Here, as at the crude stage of (1) and (2), the sentence is elicited by a non-verbal stimulus; but here, in contrast to that crude stage, the verbal network of an articulate theory has intervened to link the stimulus with the response. (Word and Object, pp. 10-11)

The intervening theory is a set of sentences associated with each other in various ways. There are logical

connections and connections that we deem as "causal".

However, Quine claims that such interconnections must be

due to the conditioning of persons to give sentences as

response to sentences as stimuli. (See Word and Object,

p. 11) The calling of some connections logical and others

causal depends on the connections of the proper sentences

with logical or causal laws of the theory. The laws

themselves are but sentences within a theory.

> The theory as a whole--a chapter of chemistry, in
> this case, plus relevant adjuncts from logic
> and elsewhere--is a fabric of sentences variously
> associated to one another and to non-verbal
> stimuli by the mechanism of conditioned response.
> (Word and Object, p. 11)

The question, then, is exactly what is the link between

theory and the world. The link between theory and the

world appears to be in the relative probability of our

taking certain nerve hits as confirmatory or infirmatory

of various sentences.

> The firmness of association to non-verbal stimuli,
> the power of such association to withstand the
> contrary pull of a body of theory, grades off
> from one sentence to another. Roughly imaginable
> sequences of nerve hits can confirm us in the
> statement that there is a brick house on Elm
> Street, beyond the power of secondary associations
> to add or detract. Even where the conditioning
> to nonverbal stimulation is so firm, however,
> there is no telling to what extent it is original
> and to what extent it is the result of conditioning,
> of old connections of sentences with sentences.
> (Word and Object, pp. 12-13)

The world-theory link is then through nerve hits. (Here

the import of Wallace's claim, considered in Chapter III,

that nerve pattern equivalence is indeterminate if
translation is indeterminate becomes of considerable
importance.) One should note here that Quine makes no
reference to our theories being true, only claiming that
given our theories are a certain way we can claim that
a certain sequence of nerve hits (physical patterns) can
serve as confirmatory of given sentences. That we can do
so is, of course, part of the theories we hold. The
notion of confirmation attributed to Quine does not
require a notion of truth, only an account of the inter-
animation of sentences of our theory and the claim that
physics is the parameter of acceptable hypotheses.

Section 4 of Chapter 1 of Word and Object "Ways of
Learning Words" is largely a continuation of the sort
of remarks found in section 3 so I shall not here consider
it. However, it is with section 5 "On Evidence" and
section 6 "Posits and Truth" that we come to the crux
of the matter. I have had previous occasion to refer to
section 6 in considering the relevant differences between
translation and physics. I wish now to consider sections
5 and 6 insofar as they help us to understand the criterio-
logical aspect of Quine's theory of truth. It is perhaps
worth noting that these sections of Word and Object are
among the most obscure of Quine's writings. However, they
are of prime import in understanding the multifarious

issues related to the ITT so I shall attempt to clarify
them insofar as I can.

.What sort of theory of evidence does Quine have.
(Recall that Schuldenfrei regards this matter as perhaps
the central difference between Quine's views and more
traditional forms of empiricism.) Quine's theory of
evidence, as indicated in the previous chapter can be
seen as a rejection of Cartesian and traditional inductivis-
tic conceptions of evidence. Even in acts of casual
observation, on Quine's account, simplicity considerations
are paramount.

> At any rate, simplicity considerations in some
> sense may be said to determine even the least in-
> quisitive observor's most casual acts of individual
> recognition. For he is continually having to
> decide, if only implicitly, whether to construe
> two particular encounters as repeated encounters
> with two distinct physical objects. And he decides
> in such a way to minimize, to the best of his un-
> conscious ability, such factors as multiplicity of
> objects, swiftness of interim change of quality and
> position, and in general, irregularity of natural
> law. (Word and Object, p. 19)

More self conscious gathering and consideration of evidence
such as found in the sciences is also highly influenced by
simplicity considerations.

> The deliberate scientist goes on in essentially
> the same way, if more adroitly; and a law of least
> action remains prominent among his guiding prin-
> ciples. Working standards of simplicity, however
> difficult of formulation, figure even more ex-
> plicitly. It is part of the scientists' business
> to generalize or extrapolate from sample data, and

> so to arrive at laws covering more phenomena than
> have been checked; and simplicity, by his lights, is
> just what guides his extrapolation. (Word and
> Object, p. 19)

Simplicity is, as the saying goes, not a simple matter,
but it is possible to indicate to some extent the connection
between Quine's views on simplicity and the truth is
immanent doctrine. This can be done by showing that we
cannot get a handle on what it means to say that one of
two sentences is simpler than the other without considering
the effects the sentences have on the total theory we
hold. To take a relevant case with respect to the ITT,
we, for the sake of argument, attempt to ascertain, in
isolation from other sentences we believe, whether it is
simpler to translate the one word sentence 'Gavagai' in
some unknown language as 'rabbit' or 'rabbit stage'.
The perhaps "natural" inclination to say that the first
alternative is simpler than the second alternative is true
only if we judge simplicity in terms of our previously
adopted views concerning the ontological commitment of
the natives, the belief that people tend to have short
names for enduring visible physical objects, etc.[8] If
our antecedently held views were more inclined toward
attributing a set of views to the native in accord with
the second suggestion, then this would be the simpler of
the pair.[9] It is perhaps worth pointing out here that

the claims that we can use simplicity considerations to decide in favor of one set of analytic hypotheses over others are largely undermined by the fact that simplicity considerations can often only be employed in terms of a background theory which is itself indeterminate. It may be useful here to consider a bit further the notion of simplicity, in particular we need to consider whether simplicity is purely relative to analytic hypotheses, for if this is the case then we do not need to consider objections to the ITT which turn on simplicity considerations. Roughly, the defense of the ITT from simplicity objections in the 'Gavagai' case would be that the fact it seems simpler to translate with 'rabbit' rather than 'rabbit stage' is simply because we have already decided on all sorts of other translations in terms of which 'Gavagai' is most simply translated as 'rabbit' however those other translations are themselves indeterminate. To apply simplicity considerations based on the ease with which we can accomodate 'rabbit' over 'rabbit stage' as a translation of 'Gavagai' is not in itself of help. I wish then to consider this matter at a bit greater length.

Suppose that we are, to return to the familiar example, faced with the alternatives of translating the native expression 'Gavagai' either as 'rabbit' or 'rabbit stage'. Assuming that the native's dispositions to verbal

behavior remain the same under either hypothesis we cannot
appeal to simplicity considerations in this area. If we
already know that the natives do not refer to "stage" or
such very often and we do know that they often refer to
visible physical objects then it will be simpler to
assume that 'rabbit' is the correct translation. Let us
suppose that we do not yet know whether the natives often
refer to stages or more often refer to visible objects.
Which hypothesis is then simpler? The basis for simplicity
considerations here is in terms of the compatibility of a
translation with the other translational hypotheses we
accept. If we start out without such translational hypo-
theses, as we of course do in the case of radical transla-
tion, then there is no basis for saying that 'rabbit' is
preferable to 'rabbit stage'. Likewise to argue that the
fact that we have always translated expressions in English
and French in a certain way can itself be taken as evidence
that our practice is either warranted or simpler than other
hypotheses is simply to assume that what our practice
has been in the past is itself determinate. Thus one
should not apply something like the following principle set
out by Gilbert Harman in Thought in the attempt to show
that one translational hypothesis is simpler than others:
"The present suggestion is to turn the matter around. You
are to use the fact that you accept a hypothesis as a sign

plausible on the sort of grounds that would, I think, be quite familiar and acceptable to Quine and serves to show the groundlessness of a certain class of objections to the ITT.

Simplicity, then, is not an "absolute" measure on sentences taken in isolation but is a rough measure of how the adoption of a given sentence will effect the other views we have. It is perhaps useful here, in order to avoid the suggestion that simplicity is here being equated with conservatism, to set out the following:

> Note, though, the important normative difference between simplicity and conservatism. Whenever simplicity and conservatism are known to counsel opposite courses, the verdict of conscious methodology is on the side of simplicity. (Word and Object, pp. 20-21)

The evidence for theories can be considered only holistically.

It might be suggested that even if it is the case that single analytic hypotheses cannot be compared in terms of simplicity, whole sets of translations can be. Perhaps one might claim that if translation scheme one of language x has fewer words than scheme two then one is simpler than two. This, however, seems to me to overlook the fact that there is no comparison of the two languages which does not involve translating both of them into a background language. Thus the simplicity of one over

two is in terms of how simple it appears we can translate

the two languages into a third. Further, the mere fact

that one of the languages contains fewer words and such

than the other is no guarantee the one even would appear

simpler than the other. It may well be the case that

this simplicity is bought only in terms of attributing

unsimple beliefs to the speakers of the language in ques-

tion. If we attempt to use a simplicity criterion that

goes beyond the mere counting of words, such as similarity

to our own beliefs, etc., we run into the matters considered

in Chapter III. Quine may be getting at something like

this line of thought in the following from "Speaking of

Objects".[11]

> English general and singular terms, identity,
> quantification, and the whole bag on ontological
> tricks may be correlated with elements of the
> native language in any of various mutually in-
> compatible ways, each compatible with all possible
> linguistic data, and none preferable to another
> save as favored by a rationalization of the native
> language that is simple and natural to us.

Section 6 of Word and Object, "Posits and Truth" is

the place where Quine most explicitly sets out the "truth

is immanent to conceptual schemes" doctrine. I shall here

try to show, in a manner of course consistent with my

earlier remarks on the difference between physics and

translation, that Quine is not on his own grounds entitled

to the claim that physics is true. I shall set out Quine's

reasons for claiming that "pragmatic" definitions of truth
or definitions of truth in terms of the application of
scientific method fail and then I shall attempt to apply
this sort of reasoning to the criteriological aspects of
Quine's theory of truth.

> And even if we by-pass such troubles by identifying
> truth somewhat fancifully with the ideal result
> of applying scientific method outright to the
> whole future totality of surface irritations, still
> there is the trouble in the imputation of unique-
> ness ('the ideal result'). For, as we urged two
> pages back, we have no reason to suppose that man's
> surface irritations even unto eternity admit of
> any one systematization that is scientifically
> better or simpler than all possible others. It
> seems likelier, if only on account of symmetries
> or dualities, that countless alternative theories
> would be tied for first place. Scientific method
> is the way to the truth but it affords even in
> principle no unique definition of truth. Any so-
> called pragmatic definition of truth is doomed to
> failure equally. (Word and Object, p. 23)

It appears then that Quine claims that a satisfactory
definition of truth must yield a unique result, i.e., must
be such that no inconsistent scheme can meet the same
standards. The two methods considered fail for the reason
that they do not yield a unique result. (The intuition
that "truth is one" is the source, of course, of objections
of very long standing to coherence theories of truth.)
If it can be shown that Quine's criteriological aspect of
the theory of truth do not yield a unique result then we
will have shown that on Quine's own grounds his theory is
not acceptable. Even, then, if we adopt a certain general

theory on "pragmatic grounds" and are able to ascertain
how this adoption effects the various other sentences we
hold we are still not entitled to the claim that the
sentences of the theory in question are true for there
is no reason to think that sentences incompatible with
those we employ might not in the end stand up equally well
on pragmatic grounds. Fitting in with a pragmatically
acceptable theory may be a reason for us to include a
sentence in our held theories but it is something else
again to say that therefore the sentence in question is
true. (If holding a "pragmatic definition of truth" is
a necessary condition for being a pragmaticist then Quine
is not a "pragmaticist".) In Quine's remarks on posits
in Section 6 he makes no reference to truth.

> To call a posit a posit is not to patronize it.
> A posit can be unavoidable except at the cost
> of other no less artificial expedients. Everything
> to which we concede existence is a posit from the
> standpoint of a description of the theory-building
> process, and simultaneously real from the standpoint
> of the theory that is being built. Nor let us look
> down on the standpoint of the theory as make-
> believe; for we can do no better than occupy the
> standpoint of some theory or other, the best we
> can muster at the time. (Word and Object, p. 22)

However, Quine is not so scrupulous when he speaks as
follows:

> It is rather when we turn back into the midst
> of an actually present theory, at least hypotheti-
> cally accepted, that we can and do speak sensibly
> of this and that sentence as true. Where it
> makes sense to apply 'true' is to a sentence

couched in terms of a given theory and seen from
within the theory, complete with its posited
reality. (Word and Object, p. 24)

It may be useful here to provide a brief gloss on "makes

sense" in the above. Here we have, I think, a reflection

of Quine's verificationism in that only in the context of

a given theory do we have some rough idea what would

count for the truth or falsity of a given sentence and

hence only in the context of a theory does it "make sense"

and being confirmable or infirmable (in Quine's theory

relative manner, of course) are then intimately connected

(to back off here from saying they are or are not the same

thing). To continue on with the quotation begun on the

previous page:

> Here there is no occasion to invoke even so much
> as the imaginary condification of scientific
> method. To say that the statement 'Brutus killed
> Caesar' is true or that 'The atomic weight of
> sodium is 23' is true, is in effect simply to say
> that Brutus killed Caesar, or that the atomic
> weight of sodium is 23.

(Quine is here, I take it, construing the two sentences

as eternal sentences relative to English in the current

period.[12])

> That the statements are about posited entities, are
> significant only in relation to a body of theory,
> and are justifiable only by supplementing observa-
> tion with scientific method, no longer matters; for
> the truth attributions are made from the point of
> view of the surrounding body of theory and are in
> the same boat. (Word and Object, p. 24)

The problem with this is that, given that we assume with

Quine, that our current theories are but one of a number

of alternatives (how many doesn't matter as long as it is
more than one) the fact that a sentence is seen from within our current theory and is in accord with it is no more
reason to think it true than we had to think pragmatically
sanctioned sentences were true. Perhaps a bit more should
be said as to why Quine rejected the so-called pragmatic
definition of truth (the relevant material is set out on
pages   and   above). The reason that Quine does not
accept the pragmetic account of truth is that there is
no reason to think that "our surface irritations even unto
eternity admit of any one systematization that is scien-
tifically better or simpler than all possible others".
In other words, if a pair of incompatible theories fit
equally well a certain set of phenomena, then, on Quine's
ground for rejecting Peirce's definition, neither one of
those theories is true. Thus, if it is the case that a
sentence is seen from within our ongoing theory and our
ongoing theory is but one of a number of alternatives
(leaving aside here the complications involving translat-
ing these languages into background languages we take for
granted) then this inclusion within our ongoing theories
is no more reason to regard the sentence as true than,
as stated, there is to think that a sentence in a prag-
matically sanctioned theory is true.

The mere fact that we take our sentences seriously

(this no doubt is a reference to how we act, what our attitude toward the sentences of our theory is) no more makes the sentences included within the theory we take seriously true than does the pragmatist claim that he finds his sentences useful thereby render said sentences true.

What we can say here, without doing any damage to remarks in previous chapters, is that a sentence is to be verified in terms of its compatibility with our held theories. We are warranted in holding a sentence as part of our ongoing theory if it fits in well with that theory and what we take as the relevant observations. However, if we grant that there are other theories we could have used such that they would be equally in accord with all data but give different truth values to the same sentences, then the truth as immanent doctrine is no better off than the pragmatic doctrine of truth for the reasons given earlier. Further, as mentioned earlier, even within our ongoing theory it will no doubt be the case that the theory will not give us a unique result as to what to count as true and what false. I shall shortly say more as to why the truth as immanent doctrine does not yield a unique result but first it is necessary to textually document my claim that Quine does indeed hold that we might use different, incompatible theories while remaining in

accord with the observable data. As clear a statement as any on these matters is found in the opening chapter of Philosophy of Logic:

> Briefly, our theory of nature is underdetermined by all 'possible' observations. This means that there can be a set H of hypotheses, and an alternative set H' incompatible with H, and it can happen that when our total theory T is changed to the extent of putting H' for H in it, the resulting theory T' still fits all possible observations just as well as T did.[12]

Let us attempt to get a further development of the reasons why the truth is immanent doctrine goes afoul by considering certain matters related to the setting out of the theories in question. I think that if we consider a remark of Quine's from "Ontological Relativity" we can see what the problem is here. If the truth of immanent doctrine is to result in a unique set of sentences counted as true then it must not be the case that it is an "arbitrary" matter what sentences we take our theory as calling true and what sentences we take our theory as calling false. If it turns out that there are different incompatible ways of settling this matter then the doctrine will not yield a unique result. As will be shortly indicated, it turns out that there is no saying what the objects of a theory are beyond saying how to translate the theory in question into another theory. If this is the case, then if we assume that translation is indeterminate, differing interpretations of the theory in terms

of which truth is judged (via translation into a background language) will result in different distributions of truth values to sentences in the theories that we take seriously.

The fact that we <u>happen</u> to use a certain translation of our theory into the background language is no more evidence for the truth of that translation than is the fact that we happen to have standard translations of English into German evidence for the truth of such translations. We could construe the theories we take seriously to be quite other than we do and still be compatible with all our dispositions to behave. This is simply, I take, the consequence of taking very seriously the doctrine that "radical translation begins at home". The reference to "Ontological Relativity" mentioned above is the following:

> What our present reflections are leading us to
> appreciate is that the riddle about seeing things
> upside down, or in complementary colors, should be
> taken seriously and its moral widely applied. The
> relativistic thesis to which we have come is this,
> to repeat: it makes no sense to say what the
> objects of a theory are, beyond saying how to re-
> interpret that theory in another.[14]

There are then, to bring the point out briefly, different interpretations of what the theory we take seriously is, accounts which are compatible with all our acceptance and rejecting behavior and which distribute truth values

differently. Thus, again, taking seriously the idea that radical translation is indeterminate and begins at home, the "truth as immanent doctrine" has difficulties much like those of the pragmatist. These sorts of reasons are why Quine rejected the pragmatic theory of truth and the definition of truth via the application of the scientific method. One should note that the type of verification considered in Chapter IV makes no reference to truth but only to the rejection of certain theories of meaning and to the compatibility of our hypotheses with our physical posits.

Given the importance and the intrinsic interest of Quine's doctrines concerning the criteriological aspect of the theory of truth I wish to consider his remarks on relevant matters in "Ontological Relativity". Also by a consideration of "Ontological Relativity" I think I can spell out a bit further why the criteriological aspect of Quine's theory of truth will not do what Quine wishes.

In "Ontological Relativity" Quine clarified these matters considerably by telling is that

> In their elusiveness, at any rate--in their
> emptiness now and again except relative to some
> broader background--both truth and ontology may
> in a suddenly rather clear and tolerant sense
> be said to belong to transcendent metaphysics.
> ("Ontological Relativity", p. 68)

In the second part of "Ontological Relativity" Quine considers the consequences of the ITT for matters related

to my concerns. As indicated in the passage quoted at
the top of page   above, if the ITT is correct then there
is no comparing theories to the world or no fact of the
matter concerning the relationship of the sentences of
our conceptual scheme to the world for there is no saying
what the objects of a theory are except by translating
the theory in question into a background theory. This,
then, makes it appear that it is impossible to make
claims concerning reference and thus in practice impossible
to say what objects our theory posits and thus, of
course, no comparison of theories with the world. Quine
then develops his doctrine of "Ontological Relativity",
in part at least, to allow a kind of sense in which we
can specify the posits of a theory.

> In short, we can reproduce the inscrutability of
> reference at home. It is of no avail to check
> on this fanciful version of our neighbor's meanings
> by asking him, say, whether he really means at a
> certain point to refer to formulas or to their
> Godel numbers; for our question and his answer--
> 'By all means, the numbers'--have lost their
> title to homophonic translation. The problem at
> home differs none from radical translation
> ordinarily so called except in the willfulness of
> this suspension of homophonic translation.
> ("Ontological Relativity", p. 47)

Quine states well the problems these doctrines seem to
produce in terms of our "intuition" as to reference and
the determinacy of our theories and their objects.

> We seem to be maneuvering ourselves into the
> absurd position that there is no difference on
> any terms, interlinguistic or intralinguistic,

objective or subjective, between referring to
rabbits and referring to rabbit parts or stages;
or between referring to formulas and referring to
their Godel number. Surely this is absurd, for
it would be to imply that there is no difference
between the rabbit and each of his parts or
stages, and no difference between a formula and
its Godel number. Reference would seem to be
nonsense not just in radical translation but at
home. ("Ontological Relativity", pp. 47-48)

Quine attempts to soften the force of all this by saying

Fair enough; reference is nonsense except relative
to a coordinate system. In this principle of
relativity lies the solution to our quandary.
    It is meaningless to ask whether in general, our
terms 'rabbit', 'rabbit part', 'number', etc.,
rather than to some ingeniously permuted denota-
tion. It is meaningless to ask this except
relative to some background language. ("Ontological
Relativity", p. 48)

It might be worth pausing here to indicate the relevance

of part of the above for the remarks earlier made with

respect to simplicity and the ITT, i.e., if we can pro-

duce two translations one which accords with our speech

disposition in a simple seeming way and one which accords

with our disposition via ingeniously permuted devices they

are both equally acceptable, the fact that one appears

"simpler" than the other just doesn't mean a thing.

It might well appear that we are launched into a

vicious infinite regress. Quine does agree that we are

in a regress but denies that the regress is vicious.

In practice of course we end the regress of
coordinate systems by something like pointing. And
in practice we end the regress of background

> languages, in discussions of reference, by
> acquiescing in our mother tongue and taking
> its words at face value. ("Ontological
> Relativity", p. 49)

We can now turn to the crucial matters for the concerns

at hand.

> What makes sense is not to say what the objects of
> a theory are, absolutely speaking, but how one
> theory of objects is interpretable in another.
> ("Ontological Relativity", p. 50)

> The relativistic thesis to which we have come
> is this, to repeat: it makes no sense to say
> what the objects of a theory are, beyond saying
> how to interpret that theory in another.
> ("Ontological Relativity", p. 50)

If we are then to judge whether within the scope of our

current conceptual scheme a claim about the composition

of a certain rock is true or false we must know what the

objects of our theory are. Now, if the objects our

theory is committed to (i.e., what sort of rocks?) cannot

be absolutely stated but only stated via the translation

of our current theory into another theory and if that

translation is indeterminate then there may well be

equally adequate (in terms of confirmatory to speech

dispositions) translations that say our theory posits

certain sorts of rocks and likewise there may be accounts

which deny that our theory posits certain kinds of rocks.

When we regress to the "background language" in which we

specify the references there is no reason to think that

there will not be alternative background languages equally

accqptable which specify different references. There is, then, no fact of the matter as to how one theory is to be interpreted in another and given that this is the only sense Quine will allow to the specification of the objects of a theory no way to set out a way of ascertaining the truth of sentences in our theory that is not itself indeterminate. The attempt to specify what our conceptual scheme is committed to (and hence what sentences it takes as true) itself involves the translation of our theory into another. Insofar as the ontology we use in the background theory is an "ultimately inscrutable ontology" we cannot use the background language as the conceptual scheme with respect to which truth is immanent. Nor can the conceptual truth with respect to which truth is immanently judged be the "lower theories" for until we regress to a background language the notion of reference does not make sense.

> Ontology is indeed doubly relative. Specifying the universe of a theory makes sense only relative to some background theory, and only relative to some choice of a manual of translation of the one theory into the other. Commonly of course the background theory will simply be a containing theory, and in this case no question of a manual of translation arises. But this is after all just a degenerate case of translation still—the case where the rule of translation is the homophonic one. ("Ontological Relativity", pp. 54-55)

If, for example, we attempt to ascertain whether some of our statements about rabbits are true or false with respect to our current conceptual scheme we need to know if 'rabbit' refers to stages, a scattered individual, a single animal, or what not. Some sentences when construed as about some of these will be true and false when about others. Now, however, until we regress to a background language there is no saying what 'rabbit' refers to and the translation into the background language is indeterminate, i.e., it could be any of these things.

The point of these considerations from "Ontological Relativity" is that what theory a given sentence is couched in (is immanent in) can be stated only via the translation of this theory into another. This translation is, if the ITT is correct, itself indeterminate and produces incompatible specifications of the conceptual schemes or theory in question. One of the specifications may count a given sentence as true, one may count it as false.

In summary, the following might be said. To say "snow is white" is true involves, obviously the predication of truth to a sentence. This involves the translation of the sentence into a "background language". This translation is, on Quine's views, indeterminate. There will then be at least two translations of this sentence in the

background language. There will be no fact of the matter as to what the sentence is to be translated as in this aforementioned background language. There will thus be no fact of the matter as to whether it is true or false. Even if we count the background language as itself part of our ongoing conceptual scheme this does not end the translation relation in question, of course. Further, there is the question of specifying what our conceptual scheme itself is. This question inevitably arises in that to judge something as true immanent to a conceptual scheme we must know what that conceptual scheme is. The conceptual scheme in question cannot, of course, be specified within itself but must rather be translated into another language. There will then be no fact of the matter as to the translation of this conceptual scheme and thus no saying what this conceptual scheme counts as true and what it counts as false (other than in an arbitrary way).

I wish now to turn to a consideration of the definitional aspect of Quine's theory of truth. Both Field and Stich have recently urged that Quine is wrong in thinking that the theory of reference is better off than the theory of meaning. I shall here be more or less just setting out Stich's claims and connecting them to the general thesis of this chapter. Stich's central concern in "Dissonant Notes on the Theory of Reference" is that

> Quine's optimism about the theory of reference
> is incompatible with his pessimism about the theory
> of meaning. For, on Quine's own account, the prob-
> lems that discourage him about the theory of
> meaning beset the theory of reference as well.
> (Stich, p. 385)

Quine's optimism about the theory of reference "can be traced to Tarski's work on the concept of truth". (Stich, p. 387) According to Stich, Quine has three arguments, each suggested by Tarski's work, each aimed

> at showing the notion of reference 'very much
> less foggy and mysterious than the notions be-
> longing to the theory of meaning' (from "Notes
> on the Theory of Reference", p. 138). The three
> together, Quine feels, endow the terms of the
> theory of reference 'with a high enough degree of
> intelligibility so that we are not likely to be
> averse to using the idiom'. (From "Notes on the
> Theory of Reference", p. 138) (Stich, p. 387)

The first of the three arguments mentioned by Stich which Quine has is that we have three paradigms which at least tell us what it would be to get the desired definition right. These three paradigms, from "Notes on the Theory of Reference" are

(1) '_____" is true in L if and only if _____.
(2) '_____" is true in L of every _____ thing and nothing else.
(3) '_____" names in L _____ and nothing else.[21]

These three paradigms then serve as a criterion for the adequacy of definitions proposed. The second of the arguments referred to by Stich is the following: (I use 1-3 where Quine uses 7-9)

We have general paradigms (7)-(9) which, though
they are not definitions, yet serve to endow
'true-in-L' and 'true-in-L of' and 'names-in-L'
with every bit as much clarity, in any particular
application, as is enjoyed by the particular
expressions of L to which we apply them. Attribu-
tions of truth to 'Snow is white' for example is
every bit as clear to us as the attribution of
whiteness to snow.[22]

The import of (1) for Quine is that it assures any defini-

tion of truth will at least pick out a certain set of

statements. Also "in the absence of a definition, it

serves to clarify any particular attribution of truth to

a statement". (Stich, pp. 389-390) Stich first argues

against the second of the purported virtues

Clearly (4) is a failure as an attempt to capture
the point of (1). But if not (4), what? The
answer becomes clear when we realize that what is
claimed to hold is the result of putting any one
statement of the object language for the blanks
of (1). This resulting statement is part of the
metalanguage (assuming the object language to be
contained within the meta-language). So to say
it is true, we must go one step higher, to the
meta-meta-language (MML). Thus Quine writes:
    In general, if Language L (for example, German)
  is contained in Language L' (for example, German-
  English), so that L' is simply L or else L plus
  some supplementary vocabulary or grammatical
  constructions, and if the portions, at least,
  of English usage which figure in ((1)) above
  (apart from the blanks) are part of L' then the
  result of putting any one statement of L for
  the blanks in ((1)) is true in L. (From "Notes
  on the Theory of Reference")
A plausible rendition of this would be the following
in MML:
(5) for all x and y, if x is a statement of L and
y is the quote name of x in ML, then the result
of substituting y for 'z*' and x for 'z' in 'z*'
is true-in-L if and only if 'z' is true-in-ML.
(Stich, pp. 390-391)

However, and this is the essence of the critical point as regards the purported second virtue of (1)

> But now there is something startling in both Quine's remark and our gloss, for <u>both of them use the notion of truth-in-the metalanguage.</u> Both statements belong to MML and each uses 'true-in-ML'. What the paradigm tells us is that each of a certain class of statements in ML is true. And to do so, it must presuppose we already understand the concept of truth in ML. But of course, to suppose that we understand this is to make the whole effort at explaining truth in L quite un-necessary, since L is simply a part of ML. Thus paradigm (1) is of no use in endowing 'true-in-L' with a tolerable degree of intelligibility unless we already understand 'true-in-ML'. (Stich, p. 391)

(Similar criticism might, I think, be raised with respect to Field's explanation of truth considered earlier.)

The first of the virtues of (1) was that it purportedly provides us with something which may serve to tell us whether a proposed definition is right or wrong. The idea being that any acceptable definition will at least pick out the same statements. Stich's critical aim here will be to try to show that this seeming advantage of the theory of reference over the theory of meaning is illusory. Stich attempts this by a somewhat circuitous route. He asks us to imagine

> A semantic theorist who succeeds in explaining to our satisfaction the 'English' binary connective ' ' for which there is no nontechnical English equival-ent. He might first explain 'analytic-in-English' say by recursively specifying which statements are analytic. He could then go on to explain that a statement formed by writing any statement in English is well formed and is true if and only if the same expression with 'if and only if' re-placing ' ' is analytic-in-English. We do not

assume that he has been resourceful enough to get
us to buy 'analytic-in-L' for any variable 'L'--
only that he has clearly specified the extension
of the predicate 'analytic-in-English'. He
needn't allow this line, however. For present
purposes we need only assume that we understand
' ' as an English connective. Note that an
analogous assumption about 'if and only if' has
been made throughout our discussion of truth.
(Stich, pp. 392-393)

(In Stich's usage ME (meta-English) is a variant of

ordinary English without semantic terms and equipped with

a systematic means of naming expressions. ML is a meta-

language we use to talk about truth in an object language,

L, ML containing L.) The problem with this sort of

approach is seen in the following

But by the quick trick of pooling English and L
he has begun to use ' ' as a connective in the
composite tongue ML. What is more, he is using
it in (10) between a statement of ML descended
from L and one descended from English. Yet for
this usage we have no explanation. He might
remedy this situation by using 'analytic-in-ML'
and explaining (in MML) the usage of ' ' in ML.
The unhappiness of this course should by now be
evident. (Stich, p. 393)

If we do understand the final suggestion, then, the explana-

tion is unnecessary and if we do not the whole procedure

is of no use.

The point of the story is seen in the following:

Recall that ME contains L. The problem is then

What is interesting about this little fable is
that it finds a direct analogue in Quine's treatment
of truth. Granting 'if and only if' in ME it
becomes part of ML and is found in (1) between
an expression deriving from ME and one deriving
from L. Explaining this use of the locution is,

presumably, one of the bothersome details we left to one side in pooling ME and L. But how can it be explained except by recourse to 'true-in ML'? (Lest it be cause for unwarranted optimism, let me observe that even if, as Quine maintains, we can give a pragmatically sound procedure for translating truth functional connectives from one language to another (Word and Object, section 13). This still gives no explanation for the use of these two connectives (from either language) when they occur, as in (1), between a statement in one language and a statement in another. (Stich, pp. 393-394)

So far we have spoken only of two of the three arguments of Quine that Stich refers to. Stich quotes the following from "Notes on the Theory of Reference".

In Tarski's technical construction...we have an explicit general routine for defining truth-in-L for individual languages L which conform to a certain standard pattern and are well specified in point of vocabulary. ("Notes on...", p. 138)

The objection here is that the procedure involves a use of the notion of translation.

Basically, Tarski's technique is to specify the conditions under which an atomic sentential function is satisfied by an infinite sequence of objects, then to state how conditions of satisfaction are combined by the operators, quantifiers and connectives available in the language. ("The Concept of Truth in Formalized Languages", section 3). To begin a Tarski-type definition using English (or better, ME) as our metalanguage we must first list all the atomic predicates and find for each a translation into ME. Then the first clause of our definition will be a set of sentences of the form:
  (11)   A sequence s satisfies Pxi if and only if
         the ith member of s is T_p.
where we replace 'Pxi' by the name (in ME) of the expression formed by appending the ith variable to some predicate and replace 'T_p' by the translation of the predicate named into ME.

For Quine this is an unhappy beginning. In it we
have made use of the concept of translation. The
object language predicate and its ME translation
are to be synonymous. So the specification of the
'explicit general routine' of which Quine speaks
must make use of the concepts of the theory of
meaning. Far from showing the theory of reference
better off than the theory of meaning, the present
line of defense uses a notion of the latter theory
to clarify a notion of the former. (Stich, pp.
394-395)

Perhaps before concluding this chapter it would be of

interest to set out the general conclusions Field and

Stich come to with respect to the impact of their arguments

on the notion of truth.

Stich concludes:

For those--and I am among them--who share Quine's
view on meaning and on radical translation, these
reflections point toward an uncomfortable conclu-
sion. The theories of reference and meaning are
beset with much the same problem. Reference is not
rescued by Tarski's work. So if we are to adjure
using the concepts of meaning we must, in good
conscience, also abstain from the concepts of
reference. If we are to respect our Quinian con-
science we must abandon much philosophical thought
about language and much of modern logic as well.
(Stich, p. 396)

Field, in "Tarski's Theory of Truth" argues that Tarski

only reduced truth to other semantic notions. Given then

that if we are to hold to Quine's views on meaning, we

must follow the course set out by Stich above. Field

suggests as a possible avenue for dealing with these

difficulties the following:

Similarly, insofar as semantic notions like 'true'
are useful, we have every reason to suspect that
they will be reducible to nonsemantic terms, and

it is likely that progress in linguistic theory
will come by looking for such reductions...Of
course, this sort of argument for the prospects
of reducing semantic notions is only as powerful
as our arguments for the utility of semantic terms;
and it is clear that the question of the utility
of the term 'true'--the purposes it serves, and the
extent to which those purposes could be served by
less pretentious notions such as warranted asserti-
bility--needs much closer investigation. (Field,
"Tarski's Theory of Truth", pp. 373-374)

I wish now to turn to a consideration of whether or
not it is correct to say that there are "alternative
conceptual schemes", this being the "problem" with the
criteriological aspect of Quine's theory of truth. In
the process I shall also try to clarify the notion
"conceptual scheme" (via an attack upon it) and will
attempt to further consider the RAVist theory of truth.
I shall be here taking as my textual base Richard Rorty's
paper "The World Well Lost".

Footnotes

[1]Quine, W. V., "Notes on the Theory of Reference", in From a Logical Point of View (Cambridge: Harvard University Press, 1964), pp. 130-138. Further references to this paper will be included in the text.

[2]Hockney, Donald, "Conceptual Structures", in Conceptual Change, ed. by J. Pearce and R. Maynard (Dordrecht: D. Reidel Publishing Co., 1973), pp. 141-166. Further references to this paper will be included in the text.

[3]Stich, Stephen, "Dissonant Notes on the Theory of Reference", Nous IV (1970), pp. 385-397. Further references to this paper will be included in the text.

[4]Field, Hartry, "Tarski's Theory of Truth", Journal of Philosophy, LXIX (1972), pp. 347-375.

[5]See Rescher, N., The Coherence Theory of Truth (New York: Oxford University Press, 1973), pp. 1-5.

[6]Schuldenfrei, Richard, "Quine in Perspective", Journal of Philosophy, LXIX (1972), pp. 5-15.

[7]The basic source on these matters is Quine, W. V., "Ontological Relativity", in Ontological Relativity and Other Essays (New York: Columbia University Press, 1969), pp. 26-68.

[8]See Hintikka, J., "The Semantics of Modal Notions and the Indeterminacy of Ontology", in Semantics for Natural Languages, ed. by D. Davidson and G. Harman (Dordrecht: D. Reidel Press, 1972), pp. 398-414 for related considerations.

[9]Harman, Gilbert, Thought (Princeton, New Jersey: Princeton University Press, 1973), p. 16.

[10]Goodman, Nelson, "Science and Simplicity", in Problems and Projects (New York: Bobbs-Merrill Co., 1972), p. 337.

[11] Quine, W. V., "Speaking of Objects", in <u>Ontological Relativity and Other Essays</u> (New York: Columbia University Press, 1969), pp. 1-26.

[12] Quine, W. V., <u>Philosophy of Logic</u> (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1970), pp. 13-14.

CHAPTER VII

REALISM AND ALTERNATIVE CONCEPTUAL SCHEMES

In the previous chapter I attempted to show that certain apparently problematic matters in the criteriological aspect of Quine's theory of truth (see references in Chapter VI) arise from his claim that the conceptual scheme we employ is but one of numerous possible alternatives we might employ. This claim is sometimes thought to be a consequence (or even the same thing as) the claim that all theories are underdetermined by all possible evidence. It is often urged against "coherence theories of truth" that they fall down due to the fact that there can be mutually inconsistent schemes all of which are equally coherent but which are such that (due to the aforementioned inconsistencies) only one of them could be true. There might, then, be similar objections to the kind of verificationism I attribute to Quine. Namely, differing theories might all be subject to equally good holistic verification but because of inconsistencies between the theories only one of them could be true. Further, the RAVist with his contention that the world is what it is irregardless of what we think it to be (aside, of course,

from that part of the world that is constituted by our

thinking) is committed to the thesis that we <u>might</u> be

massively wrong about the world. Thus it behooves us to

investigate certain recent arguments of Rorty and Davidson

to the effect that there are no alternatives to our concep-

tual scheme and further to the effect that we cannot be

massively wrong in our views about the world. Rorty

identifies these two claims. It is, of course, possible

to quarrel with this assimilation but, as nothing of im-

portance turns on it that is not also established by

other arguments, I shall follow Rorty in this. Further,

the users of the notion of <u>alternative conceptual schemes</u>

(and here I include everyone from Kant and Hegel to Sellars

and Strawson) have not given us a clear enough account of

what is to count as such a scheme (or even of the notion

of <u>a conceptual scheme</u> itself) to be indignant over Rorty's

practice. Thus, along with Rorty I shall assume that if

one conceptual scheme is an alternative to another then

from the point of view of the other the first conceptual

scheme the second will be massively in error, if it is

even intelligible. One can quickly see the relevance

of this two-pronged thesis (which I shall call the "Rorty/

Davidson Thesis" or "RDT") for the sort of verificationist

position I attribute to Quine. If we cannot be

massively wrong, then from the fact that I am well

warranted in using the results of some epistemic procedure
one can infer the metaphysical security (I here use
transparent metaphors which I shall, hopefully, cash
later) of the claims this procedure results in. Likewise
from the fact that something could not be incorporated
into our currently holistically verified scheme without
a massive error being claimed to have been uncovered we
can infer from the epistemologically unestablishable to the
metaphysically indeterminate. The RDT, aside from its
intrinsic interest, is thus a kind of "bridge principle"
from the epistemological to the metaphysical. I wish
then in this chapter to consider the viability of the RDT
(which shall also, of course, be explained in more detail)
and to set out its relevance for the dispute between the
verificationist and the RAVist.

The broader "popular" interest of the RDT arises from
the fact that it is a commonplace of our contemporary
culture that our own way of thinking about the world is
but one of numerous different but equally viable alter-
natives. In fact, this supposed proliferation of concep-
tual schemes is often thought to have normative consequences
in terms of tolerance of views quite different from our own
and encouragement of the development of radical alternatives
to our current ways of thinking.

Davidson himself uses the Rorty/Davidson Thesis in

order to show the untenability of skepticism. (I should note that I do, as it were, take the challenge of the skeptic seriously enough to think he should be given as nonquestion begging a refutation as possible and regard the fact, as is mentioned in passing in the next chapter, that the holistic verificationist can do this better than the RAVist as a strong point in favor of the holistic verificationist.)

By way of textual starting point, I shall consider Rorty's paper "The World Well Lost", an unfortunately (or intriguingly depending on one's taste in such matters) cryptic and obscure text. It is in "The World Well Lost" that the Rorty/Davidson thesis was first published.[1] (Davidson, here I rely on the word of Rorty, developed material related to it in his John Locke Lectures of 1970 at Oxford and Stroud defended a view, though a weaker form of it, like the RDT in his 1969 paper "Conventionalism and the Indeterminacy of Translation").[2] Much fuss could, I know, be made over the obscurity of "The World Well Lost" but I shall herein not be interested in such cavils (though I shall indulge in a couple). Rather, I regard it as of much more importance to attempt to explain and render plausible the RDT rather than raising objections based on infelicities in its textual source, "The World Well Lost". I shall also, by way of consideration of

the "principle of charity" (Neil Wilson's expression made famous by Quine) attempt to develop much more fully than Rorty does the sort of reasons based on the procedure of the Quinian radical translator which support the RDT. In doing this it is necessary to pause to consider just what the principle of charity should be taken to be (there being several incompatible accounts of this in the literature on the ITT, most recently in papers by Richard Grandy, and David Lewis).[3] Further, I shall offer considerations designed to make plausible the claim that the principle of charity in any of its tenable forms can come into play only <u>after</u> we have decided on translational schemes that considerably outrun the considerations of available evidence. If this is the case, then the principle of charity cannot be appealed to (or can at best be appealed to only in a very weak and oblique manner) by the champion of meanings in attempting to get determinate translation.

There are, in fact, two distinct arguments for the RDT. One is a very traditional-looking set of almost a priori reflections on various Kantian notions needed in the formulation of the claim that there are alternative conceptual schemes. The other argument involves considerations derived from Quine's account of radical translation in <u>Word and Object</u>. By a consideration of this second mode of argument we can, by the way, consider

two recent papers of Davidson, "Radical Interpretation"
and "Belief and the Basis of Meaning", in which he does
develop some material related to the RDT. As stated,
I shall get into these matters through a fairly detailed
account of Rorty's paper "The World Well Lost".

"The World Well Lost" is not a paper that will be
entirely clear to persons who have not attempted to re-
construct the claims made in it, so I shall offer a re-
construction of his arguments to the effect that the
notions of "conceptual frameworks" and "alternative con-
ceptual frameworks" are untenable. Rorty offers three
or four different arguments to this effect, some of them
involving perhaps problematic notions and some not. If
nothing else one can see by considering these matters
why people have long talked blithely of "conceptual
schemes" without ever telling us what they mean. I shall
set out arguments in a form based on Rorty's remarks in
"The World Well Lost" and following this shall attempt
the more difficult task of rendering the premises of the
arguments plausible when this appears appropriate. On
occasion, as Rorty employs without commentary or explana-
tion terms from past epistemological traditions (most
notably the Kantian) I shall try to provide some clarifica-
tion of these matters. The formulation of some of these
arguments will be lengthy but I shall give shorter

formulations of arguments to the same effect.

It should be noted that Rorty does not offer anything resembling a straightforward presentation of an argument in "The World Well Lost" but it is possible to develop an argument based on his remarks, hence where appropriate I shall include textual references for particular premises. Given that understanding "The World Well Lost" does not have nearly as widespread interest as does understanding Word and Object I shall be less concerned with matters of textual debate than I was in Chapters IV and V. (There is also not a great deal of published literature that I could debate with anyway, though "The World Well Lost" is the sort of thing which would attract persons who like to debate over cryptic texts and such. In order to avoid the appearance of refusing to take a stand on the accuracy of my exegetical account of "The World Well Lost" I do put forth my account of this paper as a "correct commentary". Now, I am aware that holders of the ITT may need to develop their own theories of textual exegesis and that is why I include this and other equally fulsome paren- thetical comments on principles of exegesis and textual commentary. This matter is related to why I think it is essential for me to include such a large amount of "purely exegetical material" here, for, after all, if we take the ITT seriously we cannot just say "Well, if you want to see

what Quine says or means read <u>Word and Object</u>".)

The first argument I shall consider is for the claim that

>   The notion of <u>alternative</u> conceptual frameworks
>   thus contains the seeds of doubt about the notion
>   of 'conceptual framework', and so of its own
>   destruction. ("The World Well Lost", p. 651)

Rorty claims that in order for the notion of alternative conceptual schemes to be tenable two distinctions of Kant must themselves be tenable:

>   Kant perfected and codified the two distinctions
>   that are necessary to develop the notion of an
>   'alternative conceptual framework'--the distinc-
>   tion between spontaneity and receptivity and the
>   distinction between necessary and contingent
>   truth.   ("The World Well Lost", p. 649)

I shall first consider an argument designed to show that the spontaneity/recepticity distinction is untenable and hence that the notion of "alternative conceptual framework" is untenable. As stated, I shall present the argument (developed largely from pp. 649-651 of "The World Well Lost") in skeleton form and will then attempt to render some of the premises plausible. Where it appears needed I also attempt to clarify what will seem to many certain outmoded terminology. (Premises on which I make additional comments are marked with a "*"). It should be noted that the following argument is only one of several which need be effective for the RDT to be rendered plausible.

*1. if the notion of a "conceptual scheme" is a tenable notion then the distinctions between necessary and contingent truths as well as between spontaneity and receptivity are tenable.

*2. if the receptivity/spontaneity distinction is tenable then the notion of an unsynthesized intuition is tenable.

*3. intuitions are effable or intuitions are not effable.

 4. intuitions are effable (assumption)

*5. if intuitions are effable then intuitions are perceptual judgments.

*6. if an intuition is a perceptual judgment then it is not an unsynthesized intuition.

 7. if intuitions are not unsysynthesized intuitions then the notion of intuition is untenable.

 8. it is not the case that the notion of intuition is tenable. 4-7

 9. intuitions are ineffable (assumption)

*10. if intuitions are ineffable then they are incapable of having an explanatory function.

11. if intuitions are incapable of having any explanatory function then it is not the case that the notion of intuition is tenable.

12. it is not the case that the notion of intuition is tenable. 9-11

13. it is not the case that the notion of intuition is tenable. 4 & 9

*14. if the notion of an intuition is untenable then it is not the case that the notion of an unsynthesized intuition is tenable.

15. it is not the case that the notion of unsynthesized intuition is tenable. 13 & 14

16. it is not the case that the spontaneity/receptivity distinction is tenable. 2 & 15

17. it is not the case that the notion of a conceptual
    scheme is tenable. 1 & 16 (derivation of negation
    of consequent of 1 from 16 is assumed.)

(The above may well be a trivial overly pedantic but

it seems to me no harm in proceding in such a manner. From

now on I will not change "un" into "it is not the case

that".) Assuming the above argument to be valid, it

remains to consider the question of the plausibility of

the premises.

Among the first questions which the above raises is

a request for a gloss on "intuitions", "effable", and an

explanation of why one should believe 1 and 2. I shall

begin by attempting to indicate how one is to take

"intuition". Roughly, an intuition is a pure datum taken

in prior to or without respect to any conceptual scheme,

a "pure given".

> Insofar as a Kantian intuition is effable, it is
> just a preceptual judgment, and thus not merely
> "intuitive". Insofar as it is ineffable, it is
> incapable of having an explanatory function.
> (650)

It is not too hard to give a gloss on "effable" that fits

in with this. To be effable is to admit of description

in a linguistic manner, said description to proceed in

terms of a conceptual scheme. Hence, if something is

effable it is not an intuition. If something cannot be

described then it clearly cannot have any sort of

explanatory role (assuming one makes the uncontroversial

assumption that explanation procedes by verbal means or through nonverbal symbols).

Having considered how one is to take "intuition" and "effable" (I here assume that if Rorty had meant these terms to be taken in some other special technical way he would have said so). I wish to turn to considerations related to the plausibility of premiss 2. Recall that on this account "intuitions" refers to a neutral material that our concepts in some way shape. The faculty of receptivity takes in intuitions. However, if there are no neutral intuitions available then there is no need to posit a distinction between spontaneity and receptivity. Rorty puts it

> There seems no need to postulate an intermediary
> between the physical thrust of the stimulus upon
> the organ and the fullfledged conscious judgment
> that the properly programmed organism forms in
> consequence. Thus there is no need to split the
> organism into a receptive wax tablet on the one
> hand and an 'active' interpreter of what nature
> has imprinted there on the other. 650

It is not difficult to see why Rorty claims that premiss 1 is correct insofar as the notion of a conceptual scheme requires a tenable spontaneity/receptivity distinction. If two conceptual schemes are to be alternatives to one another they must be alternating accounts of something that remains fixed in both, not two accounts of two different things. The passive faculty of the mind that

receives the data and then the active faculty interprets the data, the difference in the interpretive activity accounting for differing conceptual schemes. However, once the notion of neutral data is given up there is nothing for alternative conceptual schemes to be alternative accounts of. Hence Rorty says

> So if the Kantian point that different a priori concepts would, if there could be such things, give different phenomenal worlds gives place either to the straightforward but paradoxical claim that different concepts give us different worlds, or to dropping the notion of 'conceptual framework' altogether. 'Phenomenal' can no longer be given sense, once the Kantian 'intuitions' drop out. For the suggestion that our concepts shape neutral material no longer makes sense once there is nothing to serve as this material.
> ("The World Well Lost", p. 650)

Let us then turn to the second of the ways that one could from the argument given on pages 10-11 get the conclusion of line 17, i.e., by undermining the distinction between necessary and contingent truth. This argument can be dealt with rather shortly, given that this Quinian material is well known and, I think, not subject to the great misunderstanding that other aspect of Quine's work have been. (See references in Chapter IV.) Perhaps it will suffice to set out what Rorty says about this and then pass on to the more novel argument developed by Rorty on the basis of certain ideas of Davidson and Stroud.

> Quine's suggestion that the difference between a priori and empirical truth is merely that between

> the relatively difficult to give up and the
> relatively easy brings in the notion that there
> is no clear distinction between questions of
> meaning and questions of fact. The philosophical
> notion of 'meaning' which Quine is protesting
> is, as he says, the latest version of the 'idea
> idea'....Once the necessary is identified with
> the analytic and the analytic is explicated in
> terms of meaning, an attack on the notion of
> what Harman has called the 'philosophical sense
> of meaning' becomes an attack on the notion of
> 'conceptual framework' in any sense that assumes
> a distinction of kind between this notion and
> that of 'empirical theory'. ("The World Well
> Lost", pp. 651-652)

For those who may be a bit uneasy about the identification
of the necessary and the a priori one may appeal here to
the familiar Quinian claims discussed in previous chapters
concerning the revisability of theories.

Perhaps a bit of an overview of the arguments based
on the untenability of certain Kantian notions may be
useful here. We begin with the fact that both the
necessary/contingent truth distinction and the distinction
between the faculties of receptivity and spontaneity are
needed if the notion of a conceptual scheme is to be
tenable. The spontaneity/receptivity distinction is
required in that a conceptual scheme is virtually always
understood as some sort of interpretive device. Thus, if
the spontaneity/receptivity distinction is untenable the
notion of interpreted data is untenable. The argument
for this claim turns on the sort of considerations that
were earlier introduced concerning intuitions. These

considerations were in turn aimed at showing that either there are no such things as intuitions or that intuitions could serve no explanatory function. The need for the necessary/contingent truth distinction arises from the assumption that it is differences in a priori concepts that produce the "interpretations" that are the product of conceptual schemes. (See "The World Well Lost", p. 651.)

It may also be useful to say a bit more here about three of the perhaps more controversial premises of the argument, namely 5, 10, and 11. As to 5, if intuitions are effable then intuitions are perceptual judgments, the idea behind it, as developed from material in "The World Well Lost" is that if we can give a linguistic account of an intuition, it would take the form of a claim that we see (or whatever) x, x presumably being the cause of the sensation. As Rorty puts it, "Insofar as a Kantian intuition is effable, it is just a perceptual judgment, and thus not <u>merely</u> "intuitive". ("The World Well Lost", p. 650) Premise 10, is "if intuitions are ineffable then they are incapable of having any explanatory function". The idea behind this is the simple one that explanation proceeds via language (the effable) thus what is inexpressible in language is not going to be able to play a role in explanations: "Insofar as it is ineffable, it

is incapable of having an explanatory function". ("The
World Well Lost", p. 650) These two premises and the
reasons behind them appear not to beg any questions with
respect to the RDT. Let us then consider 11, if intuitions
are incapable of having any explanatory function then it
is not the case that the notion of intuition is tenable.
This argument involves the assumption that if a notion
is incapable of having any explanatory function then such
a notion is untenable. Those who felt that merely because
a posit could have no explanatory function it was still
not untenable would, of course, not like this premiss.

So perhaps it is worth saying a bit more about this
point. Given that we have assumed that explanation pro-
ceeds via language (i.e., we do not somehow scrut entites
the scrutation of which is indescribable) if something
is ineffable then it cannot be included within an explana-
tion. The claim that a posit is justified only via
explanatory capacity involves the assumption that posits
are designed for the purpose of prediction and explanation
(an assumption that some would of course centrovert) and
hence it is not correct to posit anything which could not
have an explanatory role, i.e., the ineffable. As to the
claim that if an "intuition" is effable it is a perceptual
judgment a bit more might also be said. Recall that the
function of intuitions for the proponent of alternative

conceptual schemes is to provide the material of which

conceptual schemes are alternative accounts of. Thus, I

describe my purported intuition in terms of seeing a

physical object I am already working in terms of a scheme

which gives primacy to enduring physical objects over

stages, etc. Thus effable intuitions are, in a sense,

no longer neutral and thus no longer intuitions in a

form that would suit the purpose of the prononent of

alternative conceptual schemes.

I wish at this point to consider the principle of

charity set out by Quine in Chapter II of Word and Object

as well as various reformulations of it that can be found

in recent literature. These matters need consideration

at this junction as the principle of charity plays a very

important role in the argument for the RDT based on the

procedures of the radical translator. Davidson's argument

for the RDT, as we shall shortly see, relies very heavily

on a version of the principle of charity.

In Word and Object Quine makes no great effort to

formulate the principle of charity with any great precision.

Quine introduces his discussion of the principle of

charity in section 13 "Translating Logical Connectives"

in explanation of why we would refuse to accept any

translation that attributed to the natives (or any Other)

a denial of the law of contradiction. The principle of

charity is the motivation for our refusal to attribute

what we regard as an absurd logic to the native:

> 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. This maxim is strong enough in all
> of us to swerve us even from the homophonic
> method....The common sense behind the maxim is
> that one's interlocutor's silliness, beyond a
> certain point, is less likely than bad translation--
> or in the domestic case, linguistic divergence.
> (Word and Object, p. 59)

Now, as stated earlier, there are problems with this, some

of which have been brought out in recent papers by D. Lewis

and R. Grandy which we shall shortly consider but there

are a couple of preliminary points that should be made

here. Can we say that, on the basis of the principle of

charity, 'gavagai' should not be translated as 'rabbit

stage', 'part of rabbit fusion', or even 'manifestation

of the spirit of the rabbit god'? If so, then we will be

on our way toward determinate translation. Well, here we

must consider the verities and varieties of silliness as

there are many ways of being silly some of which we do

not want the principle of charity to rule out. Roughly,

to be silly in the sense ruled out by the principle of

charity is to have a belief that we would clearly regard

as clearly false and such that the silliness of this

belief(s) is such that if we attributed it to the native

we would be regarding him as almost unbelievably stupid or

deranged. (We shall shortly see why we need the second clause here and why it should be stated so hyperbolically.) On this account of the principle of charity we cannot rule out 'rabbit stage' or 'rabbit fusion part' as these expressions can be used when 'rabbit' can. A word more needs to be said about 'manifestation of the rabbit god'. One might object that this is just too silly to attribute to the natives. If the natives believe in rabbit gods and such I am perfectly ready to agree that they are silly, but not silly in the right way. It is through a consideration of this matter that we can see why we need to explain the mode of sillingess we are talking about. If we simply to refuse to countenance any translation which attributes a silly religious doctrine to the natives then we will, if we are among those who see little difference in the degrees of silliness of any religious belief, not be able to say that our associates hold religious doctrines at all. We thus need to and do countenance all sorts of silliness in our homophonic translations. We attribute to persons quite like us all sorts of views that we may regard as utter silliness. (These remarks do not assume that religious talk is silly, then merely make the point that even if the radical translator regards all religious talk as silly he still would himself be very silly to on this ground refuse to attribute such a belief to the

natives.) Thus we can agree with the general idea behind
the principle of charity without being able to rule out
any of the possible translates of 'gavagai'. In fact,
it may be that the only sorts of silliness we can rule
out before we have adopted a translational scheme is such
things as radical illogicality and grossly different
perceptual apparatus hypotheses. A bit more will be said
on this later but a further understanding of the ways in
which the principle of charity can and cannot, which is
here really more important, be used by considering the
reformulations of the principle offered by Grandy and
D. Lewis.

Grandy says that Quine uses the principle of charity
in order to assure that we adopt translations which
maximize agreement between ourselves and others.[4] As
Grandy astutely observes in "Reference, Meaning and
Belief" Quine begins "with the correct but insufficiently
general fact that one should attempt to produce agreement
on obvious truths".[5] Grandy then sagaciously notes that
more needs to be said about obviousness if one is to put
the principle of charity into effect. Firstly, obviousness
comes in degrees.[6] Secondly, and this remark is important
enough to set out in detail, Grandy notes something
obvious:

> what is obvious will depend upon facts about a
> situation other than the radiation impinging on
> the surface of the subject. That is one cannot
> simply characterize a sentence as obvious or non-
> obvious except relative to some particular situa-
> tion; 'situation' here is a very vague term, and
> among the features that would have to be spelled
> out would be the focus of attention, expectations,
> instrumentation, et cetera.[7]

I think Grandy is quite right about this observation. Why
it is important is that it indicates further why an appeal
to the principle of charity will not allow us to overcome
the ITT. The notions of attention, expectation and such
cannot be explicated until we have arrived at a transla-
tional system sufficient to attribute a wide spectrum of
beliefs to the native ('native' is being used throughout
in a way that makes everyone, including you, a native).
In order to make the native come out having obvious
beliefs we have to know what would be obvious for him,
and this we cannot know until we have adopted a great
number of analytic hypotheses. (For references see
Chapter III, especially the discussion related to the
material from Content and Consciousness.) Thus, from a
consideration of Grandy's remarks about obviousness we
can see why the champion of meanings cannot expect all
that much help from the principle of charity. (Our
"intuitive" feeling that we can just tell what would be
obvious to someone with whom we are quite familiar is

just a reflection of our contant use of very long ago

adopted analytic hypotheses.) Charity, in a sense, must

begin at home in that what we must do is to avoid trans-

lating great masses of the natives sentences over into

out language as false sentences. We shall come back to

relevance of this to the RDT but I wish first to consider

the principle that Grandy grandly wishes to substitute for

the principle of charity, "the principle of humanity".[8]

I think it is worth considering what Grandy's principle

will not do as a constraint on translational hypotheses

before considering the remarks of David Lewis. The

principle of humanity tells us that

> If a translation tells us that the other person's
> beliefs and desires are connected in a way that
> is too bizarre for us to make sense of, then the
> translation is useless for our purposes. So we
> have, as a pragmatic constraint on translation,
> the condition that the imputed pattern of rela-
> tions among beliefs, desires, and the world be
> as similar to our own as possible.[9]

This proposal is defective in that it can be put into

effect only after radical translation is largely completed.

I do not know what the pattern of beliefs of the native

is until there is adopted a set of analytic hypotheses.

To say that the constraint rules out real bizarre attribu-

tions of patterns of belief to the native is, I think,

besides the point is that what is bizarre is itself

determined by the aforementioned analytic hypotheses.

Bizarreness is then "bizarre to us" and thus will not help
us in dealing with natives. Also, and this is a matter
that Grandy does not consider at all, on Quine's views
there both is no fact of the matter as to what our own
beliefs are and obviously therefore no fact of the matter
as to how "similar" the natives beliefs are to our own.
Again, any "intuitive" notion or feeling of bizarreness
arises from the same source as "intuitive" feelings of
obviousness. (Also, the notion of bizarreness itself
comes in degrees and is relative to our interests, expecta-
tions, etc., and thus is not really much better than the
principle of charity Quine started with.) Further, it
would seem that if we adopt the principle of humanity we
would end up being forced to say people are more like us
than we feel comfortable with, analogous to the diffi-
culties with throwing out "manifestation of the rabbit god"
because it is silly. One of the things we want is to be
able to attribute some bizarreness and silliness to the
natives. (In the interest of fairness here, it is worth
noting that Grandy makes several interesting points in
his article, my seeming lack of charity arises from the
need to put the principle of charity in its proper place.)

I wish now to turn to a consideration of Lewis'
reformulation of the principle of charity in his paper
"Radical Interpretation". Lewis thinks that the principle

of charity needs alteration in view of the fact that there
are circumstances where it is more charitable to translate
the words of the native into sentences that we count as
false. Lewis suggests that we should go so far as to
attribute to the native the same sort of errors we figure
we would make if we were in the circumstances he is in.[10]
For example, if there is some very small object only
visible to us in the barest outline which we feel certain
we could not identify then, other things being equal, we
should not count a native sentence as correctly identify-
ing the objects. Lewis then sets out what he takes to be
an improved version of the principle of charity. (In
order to understand Lewis's definitions one must know what
he uses $\underline{P}$ and $\underline{A_0}$ to refer to. The first refers to the
native ('Karl' is his name) as a physical system whereas
$\underline{A_0}$ refers to the native's attitudes, beliefs, and desires
as expressed in our language.[11]

> Perhaps an improved principle of charity would
> require Karl's beliefs and ours to be related as
> follows: there must exist some common inductive
> method M which would lead to approximately our
> present systems of belief if given our life
> histories, and which would lead to approximately
> the present system of beliefs ascribed to Karl by
> Ao if given Karls' life history of evidence
> according to P.[12]

(Lewis then makes a roughly analogous point about desires
with a common system of underlying values serving where M
serves in the above.) This is a very ingenious suggestion,

but there are problems with it as it now stands. Further,
it seems to me the complications one must introduce in
order to avoid these difficulties are so great as to not
make it possible to apply the principle during the process
of deciding what sets of analytic hypotheses to adopt.
This matter can be conveyed by in effect giving a very
simple example designed to show that we can always set
up an M to get us from the native's experience to his
beliefs and from our experiences (as described in P) to
our beliefs. Let us then specify a person (1) who has
belief B1, namely that all dogs are vicious. His exper-
ience is imagined to be limited to being bitten by every
dog he encounters. There is another person (2) who has
belief B2, namely, no dogs are vicious. 2's experiences
are also quite limited and are limited to being treated
nicely by all the dogs he knows. The trick is then, as
stated, to specify some very simple common inductive
method that will get 1 from his experience to B1 and 2
from his experience (life history of evidence) to B2.
Note also that the principle <u>does not</u> require that 1 and
2 actually do use a common inductive method, only that
there be one. We might have the following simple (this
is all we need given the simplicity of the case) M:
if one is 1 then if one has a life like 1 has had then
believe B1, if one is 2 then if one has had the experience

of 2 believe B2. This is a method that both 1 and 2 could use to arrive at their beliefs. Thus there would be no lack of charity in translating, from the point of view of 1, 2's words in such a way that we attribute to him the belief that no dogs are vicious even though he has been bitten by lots of dogs. This example is kept simple for obvious reasons, but for real persons with lots and lots of beliefs we still can do quite the same thing. Now, what is obviously needed is a restriction of possible candidates for the common inductive method, perhaps by limiting the kind of predicates that are projectible according to the canon. One also, however, has to restrict ourselves to canons that it is reasonable to think that both parties could use. If M is something that would go beyond the capacity of one of the parties, then it cannot be used. But in order to know what may fall within the capacity of the native, one needs to know what he believes, how his pattern of inferences work, how fast he can learn, etc. This sort of thing, then, itself has to wait until we have adopted a set of analytic hypotheses. (Also there are other reasons arising from problems of select-ing what predicates the native can be expected to be able to project, i.e., we have to know what he has projected in the past and, again, we cannot know this until we have arrived at a great many analytic hypotheses.

The notion of projection employed here, that of Goodman,
is considered a bit in Chapter VIII).

The general drift of all of this (and there are
other attempts to improve or clarify the principle of
charity that one might consider) is that (as they depend
for their application on our having an extensive knowledge
of the native beliefs, desires, etc.) on the complicated
and "improved" versions of the principle of charity we
cannot decide what is and what is not charitable until
we have to a very large extent completed the process of
radical translation. (Aside from the fact that when
considered the improved principles themselves appear to
need much work). So we are stuck with the very simple
principle of charity that Quine talks about in section 13
of Word and Object. This principle is, however, too weak
to allow us to avoid the ITT. I wish to return to a
consideration of the second way of arguing for the RDT.

I shall go into some detail on this, but after the
above discussion of the principle of charity we can rather
quickly give the drift of the argument. Let us recall
a couple of assumptions we spoke of earlier. Namely, we
will assume that if one is to be counted as the holder
of an alternative conceptual scheme, then it must be that
we would translate most of one's sentences as false
sentences in our language. Thus we must assume that most

of what you believe is false. But even on the most simple

version of the principle of charity, what could be more

uncharitable than this. In order, then, to identify any

one as having a conceptual scheme alternative to our own

we would then have to grossly violate the principle of

charity.

If we think of "meaning" in terms of the discovery

of speech dispositions then we will be unable to draw any

distinction between a person's using words in a manner

different from our own and a person's having many false

beliefs. If we then arrive at a translational scheme

which requires that most of an individual's sentences be

viewed as false then, assuming that we follow the principle

of charity in some form or other, this will only show us

that we have not succeeded in translating the remarks.

(I suppose that there would be a point, as Rorty stresses

in "The World Well Lost", where we come to think that maybe

the native is not the kind of thing that talks after all

so it was a mistake to try to translate his words from

the beginning. Perhaps the "native" is a window dummy

with a tape recording hidden in him and the tape recorder

becomes scrambled and is uttering weird noises. Thus

after long attempts to establish communication with the

plastic mannequin fail to produce a translation of his

sounds into our language that makes him have mostly true

beliefs, we might thereby come to discover the source of our difficulty.) Let us then consider some objections that Rorty says might be raised in criticism of this argument based on the principle of charity and see what sort of reply can be given to them.

Imagine that we observe humanoid-looking entities emitting sounds. Further imagine that after long endeavors we find ourselves unable to translate the noises of the entities into our own idiom or to correlate the emitted sounds with the entities' behavior or environment. One hypothesis the defender of alternative conceptual schemes might offer here is that the analytic hypothesis we are employing in our attempts at translation employ notions not shared with the entities "because the entities 'carve up the world' differently or have different 'quality spaces' or something of that sort". ("The World Well Lost", p. 653) The problem with this suggestion is that once we begin to consider differing ways of "carving up the world" nothing can prevent us from attributing "untranslatable languages to <u>anything</u> that emits a variety of signals". ("The World Well Lost", p. 653) What needs doing here is then to show that the notion of "people who speak our language but believe nothing we believe is incoherent". ("The World Well Lost", p. 653)

To show that the notion of people who speak our

language but believe nothing we believe is incoherent is to show that no nonlinguistic behavior by any entity would be enough to "underwrite a translation that made all or most of his beliefs false". ("The World Well Lost", p. 654) What is more controversial is the claim that if nothing would be enough to incline us to translate any entities' noises in ways that makes him have mostly false beliefs then the notion of persons who speak our language but believe none of the things we do is incoherent. It is worth setting out in a bit more detail Rorty's account of just what sort of behavior seemingly might underwrite a claim that a person's sentences are best translated in a manner that makes all or most of his sentences false.

> For it might be the case, for example, that the way in which the foreigner dealt with trees while making certain sounds made it clear that we had to translate some of his utterances as 'There are no trees' and so on for everything else with which he had some dealings. Some of his utterances might be translated as: 'I am not a person', 'These are not words', 'One should never use modus ponens if one wishes valid arguments,' 'Even if I were thinking, which I am not, that would not show that I exist'. We might ratify these translations by showing that his nonlinguistic ways of handling himself and others showed that he actually did hold such paradoxical beliefs. The only way to show that this suggestion cannot work, would be actually to tell the whole story about a hypothetical foreigner. It might be that a story could be told to show the coherence of these false beliefs with each other and with his actions, or it might not. To show that Davidson and Stroud were right would be to

show that, indeed, no such story was tellable.
("The World Well Lost", p. 654)

The argument offered against alternative conceptual
schemes has an inconclusiveness that Rorty claims it has
in common with all "interesting verificationist anti-
skeptical arguments". The argument as set out by Rorty
conforms roughly to the following pattern. The skeptic
(here the proponent of alternative conceptual schemes)
suggests that our current beliefs have viable alternatives.
Further these alternatives can never be known to hold but
they still serve to justify the suspension of judgment.
The antiskeptic suggests that the reference to alternative
beliefs about tables, chairs, persons, etc., are couched
or described by the skeptic in such a way that they could
in principle never be verified to hold. The skeptic then
makes a move similar to one that the RAVist makes in
criticism of the holistic verificationism attributed to
Quine in Chapter IV. The proponent of alternative concep-
tual schemes (who Rorty calls a skeptic, thus introducing
unneeded complications into his considerations of the RDT)
suggests that the verificationist

> confuses the ordo essenci with the ordo cognoscendi
> and that it may well be that some alternative is
> true even though we shall never know it. ("The
> World Well Lost", p. 654)

The proponent of the RDT replies (in rough parallel to the
defender of the holistic verificationist principle reply

to the RAVist charge considered in Chapter VIII) that the
matter cannot be considered until the fan of alternative
conceptual schemes sets out an alternative. Here as
Rorty says, the controversy runs the risk of turning into
an argument concerning whom the burden of proof rests on.

As Rorty says the fan of alternative conceptual
schemes gains a dialectical advantage over the defenders
of the RDT (the antiskeptic in "The World Well Lost") by
means of his global approach. The proponent of alternative
conceptual schemes can sketch in the barest outline of a
sequence of events which might result in or produce the
alternative he speaks of without being obligated to spell
it out in detail. Apparently, from the point of view of
the proponent of alternative conceptual schemes, all he
need do is simply "refer us to ordinary scientific and
cultural progress extrapolated just beyond the range of
scientific fiction". ("The World Well Lost", p. 655).
(One should note that the advantage of the fan of alter-
native conceptual schemes is a very slim one. It comes
down to the possibility that there might be these, by
his own admission, only barely (if that) sketchable alter-
native conceptual schemes. The uncharitable proponent
of the RDT could at this point note that the mere possi-
bility of an alternative conceptual scheme counts for
nothing unless the opponent of the RDT can spell out such

an alternative conceptual scheme.)

There is another sort of consideration, albeit one closely related to the argument from the principle of charity, that can be set out here which may help explain why the proponent of alternative conceptual schemes has but a seeming advantage. Here, it will be useful to bring in certain considerations of Davidson concerning what it is to interpret the words of another person and further what it is to be a person. (There are lots of connections between the ITT and our theories of what it is to understand other persons, attribute rationality to them, etc., some of which are brought out by the RDT, some of which are considered in Chapter III. Davidson advanced the claim, here I rely on the word of Rorty (I shall shortly consider Davidson's consideration of the RDT in his "On the Very Idea of a Conceptual Scheme" and his observations of rationality in "Mental Events"), that in order for an entity to be a person it must be the case that

> it have or once have had the potentiality for
> articulating beliefs and desires comparable in
> quantity and complexity to our own. ("The World
> Well Lost", p. 656)

What we need to do is to consider why it is plausible to think

that ascribing personhood, ascribing a language, and ascribing beliefs and desires go hand in hand. So, if Davidson is right, ascribing person-hood and ascribing mostly the right beliefs and mostly the appropriate desires go hand in hand. This means that we shall never be able to have evidence that there exist persons who speak languages in principle untranslatable into English or hold beliefs all or most of which are in-compatible with our own. ("The World Well Lost", p. 656)

In "On the Very Idea of a Conceptual Scheme", Davidson attempts to show that the notion of any sort of alternative conceptual scheme is untenable. As parallels my earlier remarks on the role of intuitions in the argument for the RDT based on the untenability of certain Kantian notions Davidson says that alternative conceptual schemes require a common core (something about which they are alternatives):

Different points of view make sense, but only if there is a common coordinate system on which to plot them; yet the existence of a common system belies the claim of dramatic incomparability.[14]

Davidson's account of the relationship of languages and conceptual schemes (which incidentally gets us around the qualms earlier voiced concerning identifying languages and conceptual schemes) is that if conceptual schemes differ then languages differ.

But speakers of different languages may share a conceptual scheme provided there is a way of translating one language into the other. Studying the criteria of translation is therefore a way of focusing on criteria of identity for conceptual schemes.[15]

We can now see again why the principle of charity plays
so large a role in the argument for the RDT based on the
procedures of the radical translator (as set forth in
Chapter II of Word and Object). A conceptual scheme is,
on Davidson's account, to be identified with "sets of
intertranslatable languages".[16] As Rorty stresses in
"The World Well Lost", a necessary and sufficient condi-
tion given this, for saying that two persons have dif-
ferent conceptual schemes is failure of translatability.
We have already considered why we would never be willing
to say that anything was a language that could not be
translated into our language. However, what Davidson says
concerning the principle of charity is worth setting out
here in order to allow me to put my seeming denigration
of the principle of charity in a way that will make it
clear what I am saying about the principle of charity.

> Since charity is not an option, but a condition of
> having a workable theory, it is meaningless to
> suggest that we might fall into massive error by
> endorsing it. Until we have successfully estab-
> lished a systematic correlation of sentences held
> true with sentences held true, there are no
> mistakes to make. Charity is forced on us--whether
> we like it or not, if we want to understand others,
> we must count them right in most matters.[17]

The principle of charity (in the version that is operative
in the procedures of the radical translator and in
Davidson's argument for the RDT) requires only the
correlation of sentences held true with sentences held

true. It does not, as Grandy and Lewis do, suggest
that there be any greater sort of <u>similarity</u> between us
and the native. This version of the principle of charity
will, of course, eliminate translations that attribute
grossly false beliefs to the native, but it will not
eliminate anywhere enough translational schemes to get
determinate translation. (See my remarks earlier on
'gavagai', rabbits, rabbit stages, and manifestations
of the great rabbit spirit). Thus we can, if Davidson
is right, accept the principle of charity without
thereby getting determinate translation. Some further
insight into these matters can be gained by considering
some of what Davidson says in "Mental Events". ("Mental
Events" is a complicated paper with a good deal of
material that is related to matters considered in
Chapter III, however, there is no distortion in exegetical
hypotheses created by dealing only with the closing pages
of the paper, or so it seems to me.)

Davidson observes toward the end of "Mental Events"
that propositional attitudes can be ascribed to another
person only within a "viable theory of his beliefs, desires,
and decisions".[18] It is a commonplace (and a good one)
of post Wittgensteinian philosophy of mind that we can
understand a person's beliefs only as they are involved
in a generally cohering set of beliefs, desires, intentions,

hopes, etc. (Which is not to suggest that some philo-
sophers did not know about this a long time ago, of
course.) With this in mind, we can rather quickly come
to see why Davidson holds that if we cannot translate
an entity's noises in a way that makes him have mostly
true beliefs then this shows either that we have not yet
arrived at an adequate translation of his noises or that
the entity is not in fact a language user (a person) after
all.

> Crediting people with a large degree of con-
> sistency cannot be counted mere charity: it is
> unavoidable if we are to be in a position to
> accuse them meaningfully of error and some degree
> or irrationality. Global confusion, like univer-
> sal mistake, is unthinkable, not because imagina-
> tion boggles, but because too much confusion leaves
> nothing to be confused about and massive error
> erodes the background of true beliefs against which
> alone failure can be construed...To the extent that
> we fail to discover a coherent and plausible
> pattern in the attitudes and actions of others we
> simply forego the chance of treating them as
> persons.[19]

Thus, as we can do no better than to judge what is a
reasonable set of beliefs from the standpoint of our own
best ongoing theory (the echo of Chapter V here is inten-
tional), any set of beliefs a translational scheme attributes
to a person must not deviate too radically from our own.
Of course, the notion of what is too radical is vague
and admits of borderline cases but it does
suffice to eliminate some things as just too much. Pre-
sumably, attributing mostly false beliefs is too much for

us to do to a person. In his more recent paper "Belief and the Basis of Meaning" Davidson puts essentially the same point as follows:

> The point is rather that widespread agreement is the only possible background against which disputes and mistakes can be interpreted. Making sense of the utterances and behavior of others, even their most aberrant behavior, requires us to find a great deal of reason and truth in them. To see too much unreason on the part of others is simply to undermine our ability to understand what it is that they are unreasonable about.[20]

I wish now to very briefly indicate the import of all this for holistic verification.

The import of the RDT for the sort of holistic verificationism used in the argument for the ITT (and which is, as was shown in Chapter IV, a cornerstone of Quine's views) should be fairly obvious. One of the big objections traditionally raised against coherence theories of truth was that truth is one whereas there may well be numerous incompatible coherent systems. Against the holistic verificationist it might be urged that what there is a fact of the matter about is one whereas there may well be mutually incompatible sets of claims all of which are equally well motivated on the sorts of justificatory grounds the holistic verificationist accepts. (As is noted in Chapter VIII, the holistic verificationist can use pretty much whatever canons of justification anybody else can use and it is no more incumbent on him

per se to set out a theory of justification and confirma-
tion than anyone else.) Thus, the objection has it,
holistic verifiability cannot serve as a test for meta-
physical determinacy. The RDT then allows us to put off
such objections by saying that once we have adopted (there
need be no suggestion that this process of adoption
itself be conscious or even rational) a whole theory about
the world, we will not count anyone (including ourselves)
as uttering anything that would involve anything that would
count as a radical alternative to our ongoing theory.
Thus, the RDT allows the holistic verificationist to avoid
what looked like it might be a problem for him. As Rorty
says in summing up the import of the RDT for coherence
theorists:

> the trivial sense in which 'truth' is 'correspondence
> to reality' and 'depends upon a reality independent
> of our knowledge' is, of course, not enough for the
> realist. What he wants is precisely what the
> Davidson-Stroud argument prevents him from having--
> the notion of a world so independent of our knowledge
> 'that it might, for all we know, prove to contain
> none of the things we have always thought we were
> talking about'. ("The World Well Lost", p. 662-663)

A word of provision about the above is needed. It might
be remarked that we know that we are not wrong, even
though we might in principle be wrong, because we have
all sorts of great evidence for our views. The RDT does
not, at all, require any sort of skepticism about the
soundness of our ordinary views (Rorty, it seems to me,

should have made his points without casting the proponent

of alternative conceptual schemes in the guise of a

skeptic, but this is largely a point of rhetorical effective-

ness.) What it does require is that we rule out even the

possibility of the sort of massive error the RAVist says

is possible. The RDT does not depend on imputing to the

realist a belief that we cannot know what the world is

like. More needs to be said, of course, about holistic

verificationism and it is to this matter that the next

chapter is devoted.

Footnotes

[1]Rorty, R. "The World Well Lost", Journal of Philosophy LXIX (1972), 649-664. Additional references to "The World Well Lost" will be incorporated in the text with the initials "WWL".

[2]Stroud, Barry. "Conventionalism and the Indeterminacy of Translation", in Words and Objections, ed. by D. Davidson and J. Hintikka (Dordrecht: D. Reidel Publishing Co., 1969), pp. 82-96.

[3]Grandy, Richard, "Reference, Meaning, and Belief", Journal of Philosophy, LIXX (1973), 439-452, and Lewis, David, "Radical Interpretation", Synthese XXIII (1974), 331-344.

[4]Grandy, 440.

[5]Ibid., 441.

[6]Ibid., 441.

[7]Ibid., 441.

[8]Ibid., 443.

[9]Ibid., 443.

[10]Lewis, 336.

[11]Ibid., 336.

[12]Ibid., 332.

[13]Davidson, D., "On the Very Idea of a Conceptual Scheme", Proceedings of the American Philosophical Association, XLVII (1973-74), pp. 5-20.

[14]Ibid., 6.

[15]Ibid., 6.

[16]Ibid., 7.

[17]Ibid., 19.

[18]Davidson, Donald, "Mental Events", in L. Foster and J. M. Swanson (eds.), Experience and Theory (Amherst: University of Massachusetts Press, 1970), pp. 79-102.

[19]Ibid., 96.

[20]Davidson, Donald, "Belief and the Basis of Meaning", Synthese, XXIII (1974), 309-323. Further reflections on these matters can be found in Davidson's "Radical Interpretation", Dialectica, XXVII (1973), 313-328.

CHAPTER VIII

HOLISTIC VERIFICATIONISM

> "We can abandon realism without falling into subjective idealism".
> M. Dummett

As was argued in Chapter V, the argument for the ITT as a metaphysical thesis depends on some sort of verificationist principle. We need some sort of principle that allows us to infer from the fact that it is impossible to establish which of mutually incompatible translational hypotheses is correct (an epistemological thesis) to the claim that there is no fact of the matter involved or nothing for translational hypotheses to be right or wrong about (a metaphysical thesis). As we also saw, there appears to be some reason to think that Quine takes himself to <u>have</u> defended a version of verificationism that allows one to make such inferences. It is the case, as I think one must admit however large one's regard for Quine's work is, that he has not come anywhere near accomplishing this feat. I wish then in this chapter to provide such a defense (and in so doing, hopefully, provide some further clarification of what RAVism is and what

my criticisms of it are getting at).

I

The sort of matters involved in a consideration of
the plausibility of any sort of verificationism have been
obscured by a number of matters. Not the least of these
is Quine's own failure ever to say anything like "You
get the ITT from the unverifiability of analytic hypo-
theses in a manner that gives a unique result because a
verificationist principle allows us to make such
inferences". That Quine does hold such a principle can
be almost conclusively established by a consideration
of the texts but one does still think these matters could
have been understood much quicker by those interested if
Quine had been more explicit on the sort of principles
he used in arguing for the ITT. (One can also offer as
evidence for the fact that Quine does hold a verifica-
tionist principle the fact that if he does not then a
great deal of what he has made very much over doesn't
seem to be at all reasonable. The principle of charity,
in any of its several forms would almost compel us to
make such an attribution even in the face of the subject's
denial of the attribution.) This combined with the
rejection of the so-called "verificationist theory of
meaning" (those who can, I think, be fairly said to have
held such a view at one time or another include Schlick,

Carnap, Ayer, Neurath, Reichenbach, and Hempel[1]) in "Two Dogmas of Empiricism" led many, for few good reasons, to think that Quine himself repudiated (some would even be so bold as to say refuted) all versions of verificationism. This, to repeat, is a major exegetical blunder, perhaps large enough to indicate that one who would make it understand very little of Quine's basic principles. Further, one might think that if an argument works against one version of verificationism it will work against all versions. Hence any attempt to render plausible a version of verificationism will seem to many an occasion for nostalgic reassertions of criticisms brought against the verificationist of the 1930's. But, as was stressed in Chapter IV, back of all this resistance to any sort of verificationism lies a strong realist intuition that the world may well be such that we can never know anything (some things) about it plus a stubborn insistence that metaphysics and epistemology are two different things just as justification and truth are two different things. In Chapter VII "Realism and Alternative Conceptual Schemes" considerations were offered designed to show that the claim that we might be massively wrong about the world (as the RAVist holds) is a dubious one. There appear to be limits to what might be intelligibly called error.

In order to resolve the question as to whether RAVist or
holistic verificationism (HVism) (as the verificationism
attributed to Quine may be called) are tenable (after all,
it is at least possible, assuming we do not simply define
one as not the other, as we shouldn't do, that they might
both be untenable) it would be necessary to resolve a
very large number of philosophical issues. It is probably
the case that an exhaustive attempt to resolve the issue
would involve not one resolution of many "old" philosophi-
cal problems but would create some new ones along the
way. As Dummett has remarked

> the fundamental question of metaphysics, namely
> the resolution of the dispute between realism
> and idealism, comes to be seen as a dispute
> over the general form which a theory of meaning
> should take: a dispute between a theory in
> which the notions of truth and falsity play the
> central roles, as in Frege's theory, and one
> in which those roles are taken by the quite
> different notions of verification and falsifica-
> tion.[2]

Needless to say, I shall not herein claim that HVism (or
even less so, some sort of idealism) is absolutely correct.
What I shall be arguing is that HVism is at least as
tenable as RAVism and further that there are some good
reasons for prefering HVism to RAVism. I shall be
attempting to undermine what I take to be the simple
minded belief that there is no inferring from epistemological
considerations to metaphysical considerations. In fact,

with Quine (I think), there appears to me to be but a rough and ready distinction between epistemological and metaphysical questions which can bear little more weight than the analytic-synthetic or fact-convention distinction. Certainly the epistemology-metaphysics distinction will not bear the weight the RAVist puts on it. (See for example the closing paragraphs of "Carnap and Logical Truth".) This project has been made, fortunately, a good bit easier (at least in terms of providing an easy way into the issues by Dummett's consideration in Frege: Philosophy of Language of the sort of considerations that might be offered in criticism of realism and in support of HVism. (Dummett himself in his book withholds judgment on the matter and further appears to say that withholding a belief on this matter is preferable to either believing HVism is true or believing it is false.)

Further, it should be noted, my interest in HVism is primarily as a principle for telling us when something is indeterminate. Indeed, I think one can if one likes steadfastly refuse to allow any talk of meaning and maintain HVism as a "factuality principle". Or, I can claim that matters that fail to pass the HVist test can be talked of as meaningfully as this afternoon's weather. To take a purely hypothetical case, I might claim that most or all normative ethical questions are such that

there is nothing for them to be right or wrong about

while maintaining that the meaning of such talk lies in

its function in social transactions (analyzed in some

suitable way, of course). Hence it is not possible to

"refute" the HVist by appealing to some area of discourse

such as the ethical and saying "You can see for yourself

that people do not react to moral discourse as they do to

pure gibberish but the HVist says that such discourse is

meaningless, hence HVism is false". The HVist need not

accept the second premise in the above argument. (It is

worth pointing out that the HVist can, if he should want

to for some reason, maintain that he is in accord with

Wittgenstein's suggestion (Investigations #124) that

philosophy should leave everything as it is.[3] One should

note that it may well be the case, it is hard to say

definitely because of obscurities in the text, that if

the ITT is correct then the "actual use of language" and

such used by Wittgenstein's followers (if not

Wittgenstein himself) may be an untenable notion. This

sort of consideration is of importance in that many

persons might think that the HVist program does seem to

call for a "reform of ordinary language" and would, on

that ground, oppose it.) It may of course be the case

that holders of HVist principles might come to regard

areas of discourse which failed to pass their test as

foolish but that is no criticism of the HVist, as far as I can see. But, if we can defend HVist as a theory of meaning, so much the better. I would reject also the RAVist attempt to set himself up as the defender of common sense. (In other words, I do not think Wittgenstein's account of the realist-idealist dispute can provide an accurate schematic for what happens here: "For this is what disputes between Idealists, Solipsists, and Realists look like. The one party attacks the normal form of expression as if they were attacking a statement: the others defend it as if they were stating facts recognized by every reasonable human being".[4] My primary aim in this chapter will then be to make plausible the inference from the unverifiability of analytic hypotheses to the indeterminacy of translation construed as a metaphysical hypothesis. When does underdetermination give us indeterminacy? My strategy will be to attempt to show that unless we allow some sort of HVist principle to "bridge the gap" between underdetermination and indeterminacy in some cases we will get into some very bad quandaries. As would be expected, particularly by those influenced by the views of Quine, Goodman, and others on the complexity and indirectness of the evidence for theories, the RAVist-HVist dispute is not decidable by

any sort of "direct appeal" to observable evidence but by considerations based on the overall simplicity and workability of the theories. There are those who would claim that on matters as fundamental as whether or not one should adopt RAVism or HVism there are no independent grounds for choice and hence no nonquestion begging way to say which is correct, but I do not think this is correct and further I think that this concession is too much for the realist to make for it appears to make our most important principles of evidence "merely a matter of taste". (Probably the Carnap of "Empiricism, Semantics, and Ontology" would say that the choice between RAVism and HVism was but an arbitrary choice between linguistic frameworks. I include these "methodological remarks" here in order to show that I do not, I hope, attempt to argue the issue on the basis of principles that "stack the deck" against the RAVist. A couple more preliminary remarks are needed here. It may well be that someone might ask "Is the HVist an idealist?". This is a question I wish to steadfastly avoid answering for a couple of reasons.[5] Firstly, it seems to me to be counterproductive in most cases to explain developing theories as variations or rebirths of old philosophical views. Secondly, to say either yes or no would not really help anyone unless we

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specified what sort of position we meant by Idealism or what sort of Idealism HVism is or is not. In fact, as will become clear the HVist certainly need not hold that the objects of our theories have any but the most prosaic status (he can certainly not be "refuted" by kicking stones or anything like that). The HVist, counter to the RAVist, rather holds that you can infer from certain facts about the verifiability of hypotheses to claims about the factuality of hypotheses. (Of course, if the HVist maintains that no hypotheses about the physical structure of the world can pass his test, then he probably is going to be some sort of "revisionary metaphysician" who owes us an account of tables and chairs, etc.) It is also worth considering whether or not Quine would agree with the HVist defense. Well, from what he has written to date we cannot say for sure but I do claim that HVism is consistent with all Quine says (including what he says about being a realist) and is needed to defend Quine's views on translation (among other things). We are faced with a sort of dilemma here whose conclusion on either decision is that we should consider HVism. If HVism is implausible then we should consider it because we need to know how much of Quine we can keep without it. If HVism is plausible, then we need to consider it for the reason that we desire to understand the sort of basic principles underlying our

views about language and the world, not to say our

desire to more fully understand the implications of the

views of Quine.

II

I shall begin by considering Dummett's account of

the issues involved in the RAVist-HVist dispute. I shall

then, in part, attempt to develop HVist answers to the

objections Dummett proposes for the RAVist to raise for

the HVist. I will further attempt to add force to the

HVist criticisms Dummett suggests might be raised against

the RAVist. I shall then attempt to show that what the

HVist needs to make his account plausible can be done

whereas what the RAVist needs probably cannot be done.

To some small extent I shall be expanding the defense of

HVism I drew from Quine's writings in Chapter V.

Before considering the aforementioned material from

Dummett's <u>Frege: Philosophy of Language</u> I wish to set out

some distinctions the conflation of which can and has

led to confusion in our consideration of the ITT. We

need to set out certain distinctions between underdetermina-

tion and indeterminacy insofar as these are to be dif-

ferentiated from one another and insofar as these theses

are construed as either metaphysical or epistemological

theses.

A subject matter is underdetermined if the totality of possible observations is such that different theories concerning the matter in question can be compatible with that totality. A subject matter is epistemologically underdetermined if 'the possible observations' refers to the limits of human cognition and leaves open the possibility that if this capacity were greater (or just different for that matter) then it might be possible to confirm a unique best theory concerning the subject matter. A subject matter is metaphysically underdetermined if the limit in observation allows for all possible improvements in our observing equipment, i.e., to the point at which we could observe any distribution of fundamental particles or fields through space-time.

It is then possible (assuming here no verificationist principles) that something, say the translation of Fred's idiolect into Sam's, may be epistemologically underdetermined (E, U.) without being metaphysically underdetermined (M. U.).

If we allow that translation may be E. U. without being M. U. then we accord the fact of the matter concerning the translation the status of a "thing-in-itself" with all the de facto unknowability that suggests.

On the other hand, M. U. does give us E. U. straightaway. Now we come to indeterminacy. A subject matter, say

translation, is indeterminate, if within the best possible global physics (where this is understood as the best global background theory) that can be held (whether or not the theory is itself underdetermined in either fashion itself) there are incompatible purported accounts of that subject matter. As was argued in Chapter V, translation is indeterminate as well as underdetermined because, given all possible observations, it allows incompatible hypotheses to be developed all of which are compatible with the aforementioned physical theory and the observations. Epistemological indeterminacy (E. I.) arises of the possible true observation sentences accepted within the best global physics is a totality arrived at through some sort of limit in human cognitive powers. (What counts as a possible true observation sentence being determined by the extent of human cognitive power.) Metaphysical indeterminacy (M. I.) arises where, were the global physical theory to be determinate, we still would have metaphysical underdetermination (i.e., assume our ongoing physics to be in fact not underdetermined in the way Quine, of course, holds it is. If given this something is not such that there is a fact of the matter about it then it is M. I.) A subject matter is merely E. I. if were our ongoing theory to be determinate it would be impossible,

in the sense of beyond our power but possibly open to others, to make the observations necessary to establish what the fact of the matter is about the subject matter.

Metaphysical indeterminacy arises if, within the assumed determinate physics a given purported realm of facts is metaphysically underdetermined. Again, E. I. by itself leaves open the question of M. I. But, if we grant, for example, that translation is E. I., then we are not going to be justified in assuming that there is a fact of the matter in the case. At best we can withhold judgment and claim, as does the RAVist, that E. I. does not justify M. I. An extreme RAVist might well hold on a priori grounds that there was a fact of the matter about what one meant by one's words while conceding the matter was E. I. (In other words, Quine has shown that past, present, and future persons who appeal to meaning-preserving translation and such in defense of their views are mistaken if he has demonstrated merely that E. I. holds for translation. For all purposes of practice, if the ITT can be established as a claim of E. I. then the "death of meaning" as a philosophical "tool" has been effected. But, Quine claims that the ITT holds as a matter of M.I. as well as E.I.) If a subject matter is M.I. then it is straightaway shown to be E.I. Also if a subject matter is M.I., then it is also M.U. and E.U. E.I. gives us E.U.

but E.I. by itself need not give us M.U. for it might be
in principle impossible for reasons having to do with
human cognitive incompetencies or limits to ascertain if
x or not x is true even within the scope of our under-
determined theory. To summarize what has been stated so
far:

(1) M.I. gives us E.I., M.U., & E.U.

(2) E.I. gives us E.U.

(3) M.U. gives us E.U.

without some sort of verificationist principle this is all
that holds here. Now, what we need here is a principle
that will allow us to "bridge the gap" from E.I. to M.I.
We need a principle that allows us to add to the above
list

(4) E.I. gives us M.I.

if (4) holds then we also have

(5) E.I. gives us M.U. and E.U.

(Again, if translation is E.I. then the appeal to meaning
in philosophical theories is an appeal to a "thing-in-
itself" of some sort but if we can establish (4) then we
should dispense even with this thing-in-itself theory
of meaning. In fact, we might instead of saying that
the ITT depends on some sort of verificationism--say it
depends on "E.I. to M.I. bridge principles" (thus avoiding

the initial stigma we bring on ourselves through the use of the phrase "verificationism" but I shall go ahead and refer to the position developed here as a kind of verificationism.))

I shall say more later on the interconnections between realism and verificationism for one <u>can</u> be a realist and an HVist. (It may well be that Rorty in "The World Well Lost" can be construed, as a realist who is a HVist, perhaps Quine should be construed in this manner also.) I wish now to turn to a consideration of the material from Frege: <u>The Philosophy of Language</u> on the issues between verificationism and realism. A consideration of this will also, I hope, provide me with the occasion to state more clearly what sort of realism I am stigmatizing as RAVism. In the recent literature both Dummett and Kirk[6] have claimed that Quine's only argument for the ITT is the general claim that all hypotheses are M.U. This sort of criticism is a very serious exegetical blunder in that Quine has (see references in Chapter IV) offered considerations designed to show that we can infer in the case of translation from its M.U. and its role with respect to physics to the ITT as a case of M.I. Though Kirk and Dummett would be correct if they were to say that this argument is not developed as well as we would require, but to say this is far from saying that Quine has done

nothing to explain the matters that worry Kirk and
Dummett (and others of course) about the ITT.

III

Dummett's discussion of the matters here relevant
occurs in the context of a consideration of a theory of
meaning, HVism, that can provide a vigorous (Dummett with-
holds judgment on whether realism or HVism is to be
preferred) challenge to Frege's theory of meaning. On
Dummett's reading of Frege, Frege can be seen as a kind
of a RAVist, so hence it is important to Dummett's general
study of Frege's philosophy of language to consider alter-
natives to RAVism. (According to Dummett, the most plausible
alternative is HVism. Needless to say, I agree). Now, of
course, no one has made a really vigorous attempt to
develop HVism as a theory of meaning. This is partially
due to several reasons. Not the least of these is the
power of the sort of realist theory of meaning developed
by Frege and those following in his tradition. It is
also partially due, I think, to the stigma of the 30's
and further to the sort of RAVist intuitions talked about
in Chapter IV. Dummett's discussion of verificationism
can, then, best be used here as a way of getting an
account of the fundamental tenets of realism combined
with a correlation of these matters with our concerns over

verificationism and the ITT.

Dummett's rendering of the basic dictum of a version

of realism that is a version of RAVism is as follows:

> The fundamental tenet of realism is that any
> sentence on which a fully determinate specific
> sense has been conferred has a determinate truth-
> value independently of our actual capacity to
> decide what that truth value is. (Dummett, p.
> 466)

The RAVist then maintains that our incapacity to decide

which set of analytic hypotheses is true does not give

us the claim that there is no determinate truth value to

the claim that one set of analytic hypotheses does provide

a better translation than any others. There are certain

potential ambiguities in Dummett's Realism (Which I shall

call the version of realism considered in Dummett's book,

thus foregoing the simplification that would result from

calling it "DRism") which should be clarified for it

appears that the Dummett Realist is not quite as much a

realist as the RAVist sketched in Chapter IV. Our full-

blown RAVist maintains that a hypothesis can be such that

it is E.I. for every sort of being and still be M.D. The

Dummett Realist, however, does hold that a true statement

must be ascertainable as such by some sufficiently powerful

being.

> But, for our language in general, containing as it
> does many sentences whose truth-value we have no
> effective means of deciding, the possession of a

truth-value is, on a realist interpretation, divorced
from our actual means of recognizing truth-value;
although an ultimate connection still remains as
embodied in the principle that any true statement
must be capable of being recognized as such by
some suitably placed hypothetical being with suf-
ficiently extended powers. (Dummett, p. 467)

It is perhaps worth pausing here to consider why

Dummett adds on this stipulation. Dummett does not in

Frege: The Philosophy of Language offer an explanation as

to why he thinks it is necessary that any true statement

must be such that it is capable of being recognized as such

by a being with sufficiently extended powers. It is pos-

ible, however, to get some idea of why he claims this from

a consideration of his remarks in his paper "Truth".[7]

This matter can perhaps be best briefly considered by

setting out Dummett's conclusions relevant to these

matters and then briefly running through his reasons for

the conclusions.

> We thus arrive at the following position. We are
> entitled to say that a statement P must be either
> true or false, only when P is a statement of such
> a kind that we could in a finite time bring ourselves
> into a position in which we were justified either in
> asserting or in denying P; that is, when P is an
> effectively decidable statement. ("Truth", p. 66)

Dummett, as he makes quite clear, applies to ordinary

"statements what intuitionists say about mathematical

statements".

> Now what if someone insists that either the statement
> 'There is an odd perfect number' is true, or else
> every perfect number is even? He is justified if he

> knows of a procedure which will lead him in a finite
> time either to the determination of a particular
> odd perfect number or to a general proof that a number
> assumed to be perfect is even. But if he knows of
> no such procedure, then he is trying to attach to
> the statement 'Every perfect number is even' a
> meaning which lies beyond that provided by the
> training we are given in the use of universal
> statements; he wants to say, as B said of 'Jones
> was brave', that its truth may lie in a region
> directly accessible only to God, which human
> beings can never survey. ("Truth", pp. 66-67)

These passages are, I know, not without their obscurities,

but we can, I think, see what Dummett is getting at.

Imagine that there is a statement beyond our ability

to determine whether or not it is true or false. (This

request, is not, of course, a request that one imagine a

particular statement that is beyond our ability to deter-

mine if it is true or false.) Now, assume that the

assumption quoted on the previous page (in the first of the

two quotes from "Truth") is more or less correct as Dummett

does. What one would then say about the statement imagined

above is, if one is to maintain realism, that it is to

be understood in terms of an explanation of what it would

be like for one who was in a position to assert that it

was true. In other words, we would appeal to a being with

extended epistemic powers. Further Dummett assumes (see

"Truth", p. 65) that if a realist account of truth can

be given for a particular sort of statement it must be

the case that if one knew sufficiently many facts of the

kind we normally treat as justifying us in asserting

sentences of the kind in question we would be justified

in asserting the sentence. After all, if one is a realist

("Realism consists in the belief that for any statement

there must be something in virtue of which either it or its

negation is true", "Truth, pp. 63-64) one must claim that

if one knew sufficient amounts of facts normally taken as

justificatory with respect to the kind of statement at

hand one would be justified in asserting the statement.

(Dummett considers, in a highly instructive example,

whether or not a realist account of statements like 'Either

Jones is brave or Jones is not brave' can be given. The

example should also be considered by persons who might

attempt to claim that the above suggestion is a triviality.)

When a statement is such that we cannot be in a position

to know sufficiently copious number of facts of the sort

normally taken as justificatory we, rather than abandon,

realism, imagine a hypothetical being with extended

epistemic powers who could be in a position to be justified

in asserting the statement in question. More could, of

course, be said on these matters and it should be pointed

out that in "Truth" Dummett has considerably more sympathy

for a HVist sort of position than he does in his book on

Frege.

So the Dummett Realist is not a fullblown RAVist, he

is a RAVist insofar as he does not permit the inferences from E.I. to M.I. that the HVist wants and that is needed to establish the ITT as a case of M.I. and not just E.I. Further, the Dummett Realist position is as I see it, more defensible than the full blown RAVist position, hence if we can show the Dummett Realist's position to be implausible then it will be that much easier to show the RAvist position to be implausible. I do later, however, consider whether the RAVist need make the nod in the direction of E.I. to M.I. bridge principles that the Dummett Realist does and what happens to the RAVist position if the realist does refuse even this connection between justifiability and metaphysical determinacy.

A Dummett Realist is then open to attack from anyone who thinks that sense can be given only to sentences in terms of our own recognitional capaoity. Again, the central considerations turn, on our initial considerations, on matters arising from a reflection on language learning.

Consider what one learns when one learns to use the sentence "That's a rabbit". What we learn is what justifies us in uttering the set of sounds in question (see W__rd and Object, Chapter III,Section 18 for reflections on the matter of approximating phonetic norms and such). In learning to utter the sentence under appropriate conditions it comes to acquire a stimulus meaning for us.

This sort of case is, according to Quine, in the middle
of the spectrum of theoreticity, so perhaps it will be
helpful to consider cases from one and the other end
of the spectrum. (The notion of spectrum of theoreticity
at work here, as well as the assignment of cases to
places on the spectrum is drawn from Quine's "Grades of
Theoreticity",[8] in which a further account of the grades
of theoreticity can be found.)

> The notion of a molecule or positron is more
> theoretical than that of a golf ball or rabbit.
> By this I mean that it is more remote from the
> data. Then notion of a golf ball or rabbit is
> in turn more theoretical, in my view, than the
> notion of water or rubber. ("Grades of Theoreticity",
> p. 1)

(Quine's reasons for saying that the notion of a rabbit
is more theoretical than that of water has to do with
the distinction between mass terms and individuative
terms: "Rabbits come discontinuously, yes, but so does
water. Individuation is a big step, carrying us to
another grade of theoreticity". ("Grades of Theoreticity",
p. 8, see pp. 8-10 of same paper for details).

Let us then indulge in some armchair ontogenetic
reflections concerning water. (Rubber will have to be
left for another occasion.) What we need to do here is
not, of course, to recount the actual process whereby
anyone did acquire the use of certain notions, but rather
to attempt to explain what processes of language learning

and such would get us where we are. In other words,
we need to see if the HVist or RAVist can give a better
potential account of the ontogenesis of our use of a
large number of notions, ranging from molecules and numbers
to water and rabbits. .

Perhaps a bit more should be said about how this
sort of argument is to be construed here. It may be helpful
to consider another sort of ontogenetic argument in a
perhaps less abtruse area. Let us assume that we wish
to ascertain whether Hobbes or Marx have given a better
account of "human nature" and the development of social
institutions. What one well might do in such a matter
is to imagine a "state of nature" roughly neutral between
what Hobbes or Marx would accept, set out a description
of our current state of affairs both would agree on and
see who could give the simplest account of how we got from
the state of nature to our present state. If one of the
accounts were better than the other then, other things
being somewhat equal, then the theory that allowed the
simpler hypothetical story to be told would be the
better one. A more current example of such an argument
is found in Sellars' "Empiricism and the Philosophy of
Mind" in his use of a hypothetical account of the
development of reference to "inner episodes" in support
of his account of the thoughts and other "mental episodes".[9]

For that matter, ontogenetic arguments have a much more ancient origin than contract theories and such. Plato begins the VII book of the Republic with one, i.e., the "myth of the cave". However, the mere fact that a form of argument is very old does not necessarily mean that it is either a good one or a well understood one and it is for that reason that I have included this digression. (Also, I wish to try to make as explicit as I here can the modes of argument I use to attempt to render HVism plausible and to make it clear that I employ no modes of argument that "beg the question" against the RAVist). If the HVist can give a nicer ontogenetic account of a large number of the notions we employ then the RAVist can then that will be a very good reason for saying that HVism is a better theory than RAVism.

Assuming with Quine that water is a less theoretical notion than rabbit let us then consider what sort of ontogenetic account we can give of it. (Intuitively put, the broader community that can understand a notion the less theoretical it is. Rabbit requires the mastery of a complicated individuative scheme unlike water, where 'water' is construed as a mass term.) Now, sentences like 'This is water' will be strictly observational sentences. For 'This is a rabbit' we have the familiar ponderies about rabbits, stages, fusions, etc., which we

do not have for water. 'This is water' is as observational
as sentences get. (Observationality is also, of course,
something that comes in degrees, see Word and Object,
40-46). These purely observational sentences (of course
little if any real purity is to be found) are then at one
end of the spectrum of theoreticity, thus we should
consider it. (It may be useful here to set out one of
Quine's more "intuitive" definitions of observation
sentence

> Occasion sentences whose stimulus meanings vary
> none under the influence of collateral information
> may be called observation sentences, and their
> stimulus meanings may without fear of contradic-
> tion be said to do full justice to their meanings.
> (Word and Object, The notions of occasion sentence
> and stimulus meaning are explained herein in
> Chapter III.)

Could we learn observation sentences like 'This is
water' in the way roughly like Quine says we learn them
if we are PAVists? No, if a sentence is such that
collateral information is irrelevant to its stimulus
meaning and such that its stimulus meaning exhausts its
meaning (Quine refers to his use of meaning in (Word and
Object, pp. 42-43) the above quotation as wallowing in the
conceptual slough of meaning and collateral information
so we shall do likewise wallow for a moment) it will be
the case that when the relevant stimulus occurs the
sentence is verified or falsified as the case may be.
A sentence whose stimulus meaning never occurs will not be

an observation sentence. We cannot claim then that observa-
tion sentences might be such that there is no possible
observation in the totality of space-time that would
verify or falsify them. I.e., if a sentence is an observa-
tion sentence it is verifiable. An observation sentence
is, by definition, one that can be learned via stimula-
tion. If it is possible that the truth of the sentence
cannot be ascertained via stimulation then it is not
an observation sentence. Now, on the RAVist view, it may
be the case that any sentence is such that it cannot be
ascertained to hold via stimulation. By "ascertained"
it is here meant roughly that it can be known to be
correct. If this were the case there would be no known
observation sentences. Now, the RAVist might at this
time ask what the big deal about observation sentences is
anyway. The very large import of observation sentences
in our ontogenetic argument is that it is through observa-
tion sentences that we learn our first language:

> Observation sentences are crucial in two enter-
> prises: in the conveying of evidence and in the
> learning of a language. Such sentences are
> necessarily our entering wedge into our first
> language; for clearly we can begin only by connect-
> ing heard utterances with concurrent stimulation,
> and by being confirmed in our utterances by
> speakers who share the same concurrent stimulation.
> ("Grades of Theoreticity", p. 4)

The learning of observation sentences depends on shared
stimuli that learner and teacher can both in fact have.

Hence, as stated (given that vital to the RAVist position is the potential split between our well justified beliefs and the world) the RAVist does not, as does the HVist position, guarantee the existence of observation sentences. The reason it is here claimed that the RAVist cannot guarantee as securely as can the HVist the existence of observation sentences is simply that basic to his position is the split that potentially exists between our most well-warranted claims and the truth. Thus the HVist can guarantee observation sentences, whereas the RAVist cannot.

> Observation sentences at their strictest are
> sentences that we have learned to use, or could
> have learned to use, by direct conditioning to
> socially shared stimulation.... They are sentences
> that anyone, nearly enough, who understands the
> language can verify or falsify by observation on
> the spot. ("Grades of Theoreticity", p. 3)

(for remarks on "socially shared concurrent stimulation" see Word and Object, pp. 42-43 and "Epistemology Naturalized"). (It is worth pointing out here that we do not need for the purposes at hand the claim that there be any pure observation sentences. As Quine would not doubt agree, all sentences can be such that their stimulus meaning can vary under the influence of collateral information. This is quite in accord with Quine's familiar claim that all sentences (claims) are theoretical. This, I take it, is part of the reason why Quine claims that

theoreticity comes in degrees.) At the least theoretical end of the spectrum of theoreticity RAVism appears to have considerable difficulties. I wish now to turn to a consideration of the other end of the spectrum. However, before doing this, there are a couple of other things that should be said here.

It appears to me that one might say that HVism could be correct for sentences of one sort (or more than one sort), say observation sentences, and RAVism be correct for another sort of sentence, say those about subatomic particles or numbers. (One might here even claim HVism was correct in the "manifest image" and RAVism in the "scientific image", using Sellars' terminology.) Thus it may be that the defender of the thesis that correct translation is a M.D. matter (hereafter the MDTT) can concede the claims made earlier about observation sentences and hold, however, that RAVism holds for translational claims. The problem with this line of thought (other than the obvious complications of theory it would bring) for the RAVist is that it, in effect, undercuts the very strong intuitive appeal of the RAVist vis-a-vis the idea that all our beliefs about the world could be radically wrong, that the world could be radically other than we think it is (i.e., the Cartesian

Evil Deceiver, mentioned in explanation of the intuitive appeal of RAVism in Chapter IV could not deceive us about genuine observation sentences. Simultaneously, we should, in the interest of fair play, note that the HVist is ill-advised to handle his problems, for example the question as to whether or not the existence of numbers is a meta-physically determinate matter, by claiming that HVism holds in most sorts of discourse but RAVism holds for numbers. These suggestions about what the RAVist and HVist should do are based on my estimation of the prior probability of any sort of "mixed" theory being better than either a strictly RAVist or HVist theory. Doubtless a mixed theory would be considerably less aesthetically attractive than either RAVism or HVism. This is not to say that in the end the RAVist and the HVist may not both be forced to compromise but that stage of things will not be herein considered (except briefly in passing). Also, I cannot help but think that if the RAVist allows any bit of HVism to creep into his theory the whole edifice will eventually be taken over. I wish now to turn to a con-sideration of molecules, i.e., the other end of the spectrum of theoreticity. (It might be claimed that abstract objects like numbers were the occupants of the extreme theoretical end of the spectrum of theoreticity. Well, this is not important here and we could perform all

sorts of sophisticated observations of abstract entities

(proving a theorem in mathematics and logic and such)

before we were able to talk of molecules, so it is not so

clear what one should say here. If we attempt to explain

degree of theoreticity in terms of how wide the communities

who can handle a notion are we will find that more

persons can count well than can talk of molecules, or so

it seems to me.) I shall here attempt to show that the

HVist can give an ontogenetic account of such matters.

(I should note here that I did not give an account of

the HVist ontogenetic argument on water as I take it for

granted that one could be given.) However, before going

into this I wish to indicate briefly why I think the

HVist and the RAVist need to give or be able to give

accounts of language acquisition. Perhaps a bit more

should be said concerning why proponents of such meta-

theoretical principles as RAVism and HVism are here being

asked to give accounts of various aspects of human language

acquisition. The general strategy here is to take for

granted that we do acquire the ability to use sentences

of varying degrees of theoreticity (an assumption I

take to be quite uncontroversial) and on the basis of this

assumption attempt to ascertain whether or not the RAVist

or the HVist could (if he were to desire to) give a more

satisfactory account of these matters. (From this it does

not follow, of course, that I am suggesting that either

the RAVist or the HVist must give an account of

these matters, the question is rather if they were to

attempt it what would then occur.)

One might also make a couple of remarks about HVism,

RAVism, and the grades of theoreticity in order to show

why molecules and water are dwelt on. At the level of

observation sentences the HVist can obviously explain how

we acquire the ability to use observation sentences but

the trick is to show that RAVism will not and further to

see why this is important. At the other end of the

spectrum, the trick is to show that HVism will work. (I

take it that part of the intuitive appeal of the RAVist

position does indeed come from the fact that they can

appeal to theories that are now quite well entrenched

which at one time would have been considered but the

most fanciful sort of speculation. The RAVist suggestion

is, of course, that the same thing might happen again

and that this yet to be discovered and one day to be well

entrenched theory will go counter to our basic theories.)

The middle is more or less equally problematic (or un-

problematic) for both the HVist and the RAVist, thus I

stick to the extremes.

By returning to the import of the notion of a posit,

(discussed at some length in Chapter IV) it can be seen

that HVism can be used to give an account of the
ontogenesis of talk of molecules. (Needless to say,
like a very large proportion of philosophical use of talk
of molecules, what occurs here is based largely on what
various philosophers of science have said about such
matters rather than what workers in the field have said.)
The RAVist would have it that there is a fact of the
matter about molecules and also that there would be one
even if no one would ever be in a position to know that
they existed. Again, to abandon this is to abandon the
RAVist's split between the justifiable and the real. It
is a good part of the "intuitive" appeal of the RAVist
position that there is this very well founded theory of
molecules which seems at odds, at least prima facie, with
the manifest image. From this they argue that there might
be a never to be discoverable theory that would play an
analogous role for molecular theories. After all, we all
can agree that no one might have ever thought of molecular
theory even if we are rather suspicious if someone tells
us that we might never have discovered that other persons
have minds and that things fall when dropped.

A kind of "key" can be found in Quine's remarks on
what a realistic (paraphrase here with 'sensible') theory
of evidence should be like:

In either event, words mean only as their use
in sentences is conditioned to sensory stimuli, verbal
and otherwise. Any realistic theory of evidence
must be inseparable from the psychology of
stimulus and response, applied to sentences. (Word
and Object, p. 17)

Let us, with this in mind, consider our evidence for

molecules. What sort of ontogenetic account of molecules

could the HVist give? Quickly put, he could say that

positing of molecules was well motivated on HVist grounds

on the basis of the overall simplicity such posits introduced

into the holistic system:

It is the quest of system and simplicity that has
kept driving the scientist to posit further entities
as values of his variables. The classical example
is the kinetic theory of gases. Viewed in terms of
gross bodies, Boyle's law of gases was a quantita-
tive description of the behavior of pressurized
chambers. By positing molecules, the law could be
assimilated into a general theory of bodies in
motion. ("Grades of Theoreticity", pp. 16-17)

The link to observation sentences is, of course, rather

indirect. The posit itself can be more directly verified

in terms of its predictions as to what our future experi-

ences will be like. "Prediction is in effect the conjec-

tural anticipation of further sensory evidence for a

foregone conclusion". (Word and Object, p. 18) Thus, the

HVist ontogenetic account holds that talk of molecules

developed because the predictions of the theory would if

correct, greatly simplify the overall theory. If the

theory had no predictions to make, then the HVist would

not be able to explain the ontogenesis of talk of molecules

and such. But there is and he can. (To take one very
simple sort of case, the fact that certain sorts of
substances react in certain ways in the presence of other
sorts of substances would be the sort of thing in mind
here.) The strategy here has been, then, to take some-
thing we all agree (for the sake of argument at least)
there is a fact of the matter about and see if the HVist
can explain the ontogenesis of talk of such matters.

What we learn when we learn various sorts of
sentences is then a set of responses to stimuli. If a
theory of meaning is to explain what one learns when one
comes to have the capacity to use certain sentences,
then it has to take as its primary notions not those of
truth and falsity but notions related to the conditions
under which one is warranted in asserting it, conditions
of verification and falsification within the context of
our ongoing theory. Given that we learn to use sentences
which, assuming that they have truth conditions, are such
that their truth conditions may be permanently beyond
our reach it must be that what we learn when we learn
to use sentences is the conditions for their verification.
(This is, of course, compatible with either a holistic or
a nonholistic account of verification). We learn the
conditions which the practices found in our current theory
of the world tell us warrant the acceptance of the sentences

in question. (To reiterate, it is not at all obligatory
that the HVist per se tell us what these conditions are.)
For sentences such that our current practices do not
allow conditions that would establish their truth to be
ascertained to hold are such that the notions of truth
and falsity do not apply to them. For example, if within
our current ongoing theory there is no confirming analytic
hypothesis, then there is no fact of the matter about
translation. We can in such cases infer from E.I. to
M.I. I wish now to turn to a rather different sort of
ontogenetic argument for HVism, one based on Goodman's
so-called "new riddle of induction".

In making use of Goodman's new riddle of induction I
shall assume enough familiarity with Fact, Fiction, and
Forecast to make do with a brief summary of Goodman's
work.[10] I shall be arguing that HVism fits in nicely
with Goodman's views on induction, whereas RAVism does
not. I do not claim, however, that accepting Goodman's
views on induction without accepting HVism is inconsistent.
I shall also assume, without argument, that Goodman is
right both in his solution to the riddle and his claim
that the riddle is a serious problem, not simply a "mere
sophism" of some sort. I am well aware of the fact
that Goodman has a legion of critics but to the extent
that I am familiar with these writings I agree with

Goodman's general evaluation of their worth. I also think

that there are important things to be learned from

Goodman's remarks on realism (in section 8 "Realism"

of Languages of Art and "The Way the World Is"--and I

shall, briefly, go through some of these matters.)

As stated above, I shall take the so-called "new

riddle of induction" to be a matter of more or less

"common knowledge" and will assume, along with Goodman,

that no purely syntactical solution can be found to it.

A succinct and dramatic (though not flawless) exposition

of the essence of the new riddle of induction occurs in

Quine's "Natural Kinds".

> Goodman propounds his puzzle by requiring us to
> imagine that emeralds, having been identified by
> some criterion other than color, are now being
> examined one after another and all up to now are
> found to be green. Then he proposes to call
> anything grue that is not examined before tomorrow
> and is blue. Should we expect the first one
> examined tomorrow to be green, because all
> examined up to now were green? But all examined
> up to now were also grue; so why not expect the
> first one tomorrow to be grue and therefore
> blue?[12]

In passing, it is worth having a couple of petty

quibbles with the above. Firstly, it does not really

matter if we use green as our identifying criterion unless

we assume, as would be a mistake, that an identifying

property must be an essential property. (I take Kripke

and Donnellan to have shown that this is not so). Goodman's

puzzle remains a puzzle if green was used up until today

as the identifying criterion for emeralds. Secondly,

the reference to time is not needed (some have tried to

make something out of this) in the formulation of the

puzzle). Also, Quine's remark may give the unwary reader

the impression that Goodman advanced the new riddle of

induction in support of some quasi-Humean skepticical

attempt to undermine induction. This is not the case,

of course (cf. FFF, pp. 59-62).

Perhaps we can most quickly advance here by

setting out Goodman's (with J. Ullian) latest remarks

on how the puzzle arises:

> What is admissible--and needed--as information
> or assumption is just the following: (1) Some
> cases of each of the hypotheses have been
> examined and all are found to be positive (i.e., the
> hypotheses are supported and unviolated) (2) One and
> only one of the two consequent predicates applies
> to something to which both antecedent-predicates
> apply (i.e., the hypotheses conflict.[13]

Most briefly put, there are some cases where we do not

think that positive instances confirm a hypothesis

("all emeralds are grue") and somewhere we think it does

("all emeralds are green") and what we need is to explain

the rationale for this discrimination.

The task of interest here is going to be, once we

have set out Goodman's account of why some predicates

areprojectible and others are not, to show that HVism is

easily compatible with Goodman's views whereas RAVism is

not so nicely compatible. Why can we project 'green'
and not 'grue'? Well, Goodman tells us (in Chapter IV
of FFF and elsewhere) it is because we have used the
first of these in many past projections whereas we have
used grue in very few, if any, past projections. "Even
people who calmly accept the fact thatphysical solidity
rests upon particles in helter-skelter motion rebel at
the suggestion that scientific procedure rests upon
chance choices sanctified by habit".[14]

The more successful projections that have been
made using a predicate the better entrenched it is. A
hypothesis is, in Goodman's terminology, actually projected
"when it is adopted after some of its instances have
been examined and determined to be true, and before the
rest have been examined". (FFF, p. 87) Now, we are able
to state the difference between green and grue. The
difference lies in the fact that one of these predicates
has been projected many more times than the other. By
way of explanation of what it is to project a predicate
we have: "a predicate "Q" is said to be projected when a
hypothesis such as "All P's are Q's is projected". (FFF,
p. 94) We need now to set out the notion of
entrenchment.

> The predicate "green" we may say, is much better
> entrenched than the predicate "grue"....The
> entrenchment of a predicate results from the

> actual projection not merely of the predicate
> alone but also of all predicates coextensive with
> it. In a sense, not the word itself but the
> class it selects is what becomes entrenched, and
> to speak of the entrenchment of a predicate is to
> speak elliptically of the entrenchment of the
> extension of that predicate. On the other hand, the
> class becomes entrenched only through the projection
> of predicates selecting it; entrenchment derives
> from the use of language. (FFF, pp. 94-95)

More could be said here, needless to say, in exposition

of Goodman's new riddle of induction and his account of

the solution (which is offered by him in the spirit of

something to work on that looks promising) of the riddle.

The solution is to requote a passage, that "scientific

procedure rests upon chance choices sanctified by habit".

We can now turn to the ontogenetic argument based on

Goodman's work on induction. This matter can, I think,

be presented with some brevity. The question now is, who

can offer the best explanation of the process of entrench-

ment. Firstly, if a predicate or a hypothesis is to be

projected repeatedly it must be one we can ascertain has

been supported and has not been overridden. The conditions

for warranting the claim that "all emeralds are green",

if "green" is to become entrenched as it does, must be

ones that we could know hold. Thus predicates construed

RAVistically (ones that are such that it might be the case

that we could never know if they apply to anything) are

not predicates that are going to be sanctioned by habit

as there is no basis on which a habit could be built up.
However, as HVist predicates are explicated in terms of
their part in our habits, the HVist view fits in nicely
with Goodman's theories. These matters can, I think, be
made more perspicuous by a consideration of section 8 of
Languages of Art, "Realism" and Goodman's 1960 paper "The
Way the World Is".

Section 8 of Languages of Art is devoted to a discus-
sion of realism in representational art. The relevance
of this notion here is that RAVism is a defective theory
for reasons roughly analogous to those Goodman offers in
rejection of what he calls absolutistic theories of
representation (see Languages of Art, p. 36) (i.e., the
notion of what it is to be a realistic representation is
derivative from our practices, not vice-versa, likewise
the notion of reality that RAVism appeals to is one that
is dependent on the practices whereby we decide what is
real, as the HVist would have it.)

Goodman rejects several tests, which he says are
popular ones, designed to show that a particular picture
correctly (well) represents something or other; the
ability of a picture to make us confuse it with the
original, the amount of information a picture provides,
and the purported fact that for realistic representations
we need no key to tell us what is represented whereas for

representations that are not realistic we do need a key.
Goodman's reasons for rejecting the first two tests listed
above are not directly relevant here. (Goodman states
them with customary lucidity in Languages of Art, pp. 34-36).
Roughly put, according to Goodman's theory, a picture
is realistic if it is in accord with customary systems
for representing the object in question. For familiar
sorts of pictures, it is not there is no "key" at work
but that we have become so accustomed to using the key
that we forget it is there.

> Just here, I think, lies the touchstone of
> realism: not in quantity of information but
> in how easily it issues. And this depends upon
> how commonplace the labels and their uses have
> become. (Languages of Art, p. 36)

One cannot help but pause to note how similar Goodman's
views on induction are to his views on representational
realism in putting such heavy stress on the importance
and force of habit. The absolutist in representational
theory (Goodman gives us no names, for those who care
about such matters) tries to do without the notion of a
system of representation (or perhaps he might claim that
one representational system was just naturally better than
all others). It is the impossibility of this, as I
understand it, that the first seven sections of Languages
of Art (among other things, of course) was designed to
show.

Realism is relative, determined by the system of
representation standard for a given culture or
person at a given time....This relativity is ob-
scured by our tendency to omit specifying a frame
of reference when it is our own...."realism" thus
often comes to be used as the name of a particu-
lar system of representation....Most of the time,
of course, the traditional system is taken as
standard; and the literal or realistic or naturalis-
tic system of representation is simply the
customary one. (Languages of Art, pp. 37 and 38)

Before indicating the relevance of this to the RAVism/HVism

issue it is worth noting the similarity of the "man on the

streets" insistence that traditional European style

portraits "look more like people" than most other sorts

of portraits do the "stubborn feeling that a true bilingual

surely is in a position to make uniquely right correlations

of sentences generally between languages". (Word and

Object, p. 74) This objection (considered in some detail

in chapter III) overlooks the fact that the bilingual has

his own system of analytic hypotheses, and the naive art

theorist has his internalized traditional system of

representation. The RAVist, then, like the absolute

representationalist, in making his claim that the Real

might be beyond our knowledge deprives us of a scheme in

which to understand his claim that the Real and what we

think the Real is might be two different things. Without

a holistically verifiable system of claims we can no more

say something is real than we can say a picture is

realistic without making use of a system of representation.

It remains to consider the relevant material from Goodman's essay "The Way the World Is". Before turning to this, however, I wish to make a couple general remarks about my use of Goodman's work. Firstly, I am, obviously, drawing out Goodman's views only to support the case for HVism and against RAVism, not for the sake of studying Goodman's work for itself. Thus I feel no obligation to look at the numerous criticisms of Goodman that have appeared in the literature in the last 25 years. (Anyway, as I said, we have Goodman's word, backed up with argument, that the critics have made no dents in his system. However, just as I am in effect using Quine's work as a way of starting a consideration of the HVism/ RAVism issue [aside, of course, from the very large in- trinsic interest and importance that attach to the ITT] so, I think, it would be possible to approximate the same thing via a study of Goodman's theory of induction and his study of symbol systems in Languages of Art. I do not attribute HVism to Goodman, but as I shall shortly argue [based on "The Way the World Is"] he is plausibly viewed as anti-RAVist.)

In making use of material from "The Way the World Is", a paper which does not seem to me to have attracted the attention it deserves, I shall hold that RAVism does not fit in with the observations Goodman makes in the

aforementioned essay. If the RAVist were right, there would be a "way the world is", which, if we could find it out, would result in there being a most realistic way of depicting it. It might be remarked here that though the world itself could be RAVistically objective whereas representation may be relative to sets of conventions. This, if it is correct, still leaves us with something objective which is conventionally represented and thus leaves us with some representational system being as a matter of fact (as determined by the relevant conventions) the most realistic system. The RAVist also holds, that there is a way the world is even if we are not able to make the requisite depictions. Goodman's general thesis in "The Way the World Is" is that this sort of notion is untenable. In essence, he argues that the RAVist notion of the way the world is (here it does not matter whether we take representation as relative to sets of conventions or not) belongs in the same camp as the mystic and the Bergsonian (such being in his usage, by the way, a rather abusive juxtaposition). There is no one way the world is, the world is on Goodman's view any one of the various ways it is describable. Goodman runs through various traditional claims that a certain manner of representing the world is the way; the picture theory of language,

the appeal to the given, and the purported resemblance
of realistic pictures to what they depict.

RAVism, which explicitly holds that the world may
be undescribable (if a RAVist does not hold this then
he would be giving up the very essence of his view, i.e.,
the radical split between epistemological and meta-
physical matters) is not, thereby, a way of describing
the world. Thus, the world of the RAVist, if Goodman is
correct, is not only not the way the world is, but is
not even a way the world is.

A bit more can be said on this. RAVism is not a way
the world is, using Goodman's terminology, in that it
allows that the world might be indescribable. On
Goodman's view, there are as many ways the world is as
the world is describable. Thus a view which claims that
the world may be indescribable (as remarked elsewhere, it
is not to the point to remark that the RAVist can maintain
that the world is in fact describable) is untenable. The
HVist then argues that from unverifiability we can infer
metaphysical indeterminacy.

Let us then consider briefly the kind of view necessary
to deny this inference. We can claim, if we are the sort
of RAVist that the Dummett Realist is, that even though
translation is E.I. it still might be the case that if
our cognitive facilities were extended in certain ways we

could confirm one set of analytic hypotheses over all others. As Dummett says we understand these hypothetical faculties in terms of analogies with the faculties we do possess. (In other words, on the Dummett Realist account of this, the realist imagined himself to possess the ability to see submicroscopic particles and such by imagining his ability to see small objects such as fleas greatly extended). The problems come, however, when we try to spell out in some way what this hypothetical extension comes to. When, to take the case of translation, we try to be a realist about meanings while admitting that translation is E.I. we have considerable difficulty in spelling out which of our faculties would, if suitably expanded, allow us to determine that one set of analytic hypotheses was better than all others. Let us see then what hypothetical faculties the Dummett Realist (or anyone who does not want to completely sever justification and truth--I shall shortly consider why realists are better off as Dummett Realists than as RAVists) can appeal to claim that translation, assuming it to be conceded to be E.I. for the sake of the argument, is such that a suitably epistemically powerful being could confirm translational hypotheses. What is it that our expanded faculties could do? And further, does the RAVist need to make the sort of nod in the direction of HVism that the

Dummett Realist does?   (To pursue a bit the theological

ramifications of this, if one is a Dummett Realist, one

needs to know whether if God were the radical translator

he would be any better off than the person Quine describes

in Word and Object. If not, then on the grounds of the

Dummett Realist, translation is indeterminate. The

difference between the Dummett Realist and the HVist

can also be put in these terms, i.e., for the HVist the

appeal is to the actual epistemic powers of man in deciding

whether or not something is M.I. whereas for the Dummett

Realist it is to the powers of God. The RAVist, on the

other hand, draws no metaphysical consequences from the

failure of any powers, be they divine or human.) (Wittgen-

stein appears to be talking about the power and pervasive-

ness of the RAVist picture in the following remark:

> A picture is conjured up which seems to fix the sense unambiguously. The actual use, compared with that suggested by the picture, seems like something muddied. Here again we get the same thing as in set theory: the form of expression we use seems to have been designed for a god, who knows what we cannot know; he sees the whole of each of those infinite series and he sees into human consciousness. For us, of course, these forms of expressions are like pontificals which we may put on, but we cannot do much with, since we lack the effective power that would give these vestments meaning and purpose.
>
> In the actual use of expressions we make detours, we go by sideroads. We see the straight highway before us, but, of course we cannot use it, because it is permanently closed. (Philosophical Investigations, #426)

I wish then, to consider what extension of our faculties could remove the E.I. from translation, assuming it to be E.I. for us as we are now. Again, a couple of preliminary remarks may be useful here. If it can be shown that the Dummett Realist's extension notion does not make sense, then the Dummett Realist has to either give up the connection between justifiability and truth (something Dummett thinks would make it impossible for us to explain why we prefer true sentences to false ones if we do not want to make it "just a matter of taste") or adopt a version of HVism. (One might remark here that the Dummett Realist could in this situation suggest that we can know which translation manual is correct, but not on grounds of direct observation. This remark appears to suggest that we have so far assumed that the Dummett Realist was so limited, but we have not, i.e., at the extended level of sensory capability the Dummett Realist's individual can use whatever appeal to simplicity and such that is normally employable.)

It is pretty clear that the Dummett Realist will adopt a version of HVism. Further, if it can be shown that on whatever extension of our faculties there could be, we would not be able to remove the E.I. from translation, then on the grounds of the Dummett Realist, we will have shown translation to be M.I. I shall attempt to argue

for both of these claims. Given this, we can turn to the RAVist who, to reiterate, does not allow any sort of inference from E.I. to M.I. I shall try to show that the realist position if it is to be at all workable, collapses into a version of Dummett Realism and further than Dummett Realism collapses (collapsing need not necessarily have any normative import or carry a suggestion of some sort of fall) into HVism.

IV

In this section I wish then to consider the following questions in the following order:

1. Assuming the extension of the Dummet Realist makes sense, would any sort of extension give us E.D. translation?

2. Does the extension of the Dummet Realist make sense?

3. Assuming the extension of the Dummett Realist does not make sense, then does Dummett Realism collapse into HVism?

4. Is the position of the Dummett Realist more plausible or better than that of the RAVist? (Does RAVism collapse into Dummett Realism?)

The answers to these questions are, I shall attempt to argue, respectively no, no, yes, and yes. In the subsequent section I shall consider what the problems are

that the HVist faces and what sort of solutions to these at least prima facie difficulties may be.

Assuming we cannot be shown that one translational hypothesis is superior over all others, what extension of my faculties would allow me to confirm one particular translational hypothesis over others?

Let us then consider what sort of extension of the faculties of the Dummett Realist would give us determinate translation, assuming that with our current senses we are unable to obtain the requisite verification. Perhaps the simplest way to proceed here is to consider each of the five senses in turn and imagine them extended in power and consider if such extensions would give us E.D. translational hypotheses.

Consider first vision. Imagine ourselves endowed with the ability to see microscopic entities with the unaided eye. Imagine us possessed with the ability to see men walking on the moon, using merely the unaided eye. Would those extensions of our sensory powers give us access to evidentiary information that would confirm one translational hypothesis over all others? Quite obviously, this hypothetical extension of our ocular abilities can serve no purpose with respect to the activities of the radical translator. If translation is indeterminate in the face of the evidence available to us in our normal

state of visual acuity, then there is nothing to be

gained by this superior vision. The ITT does not arise

as a result of any physical limits which arise as a

result of our visual lack of ability. Let us then briefly

consider taste, hearing, smelling, and touching. Can we

or can our postulated super sensor smell, taste, touch,

or hear his way to determinate translation. Well, all of

these four hypothetical extensions of our senses would be

of use to the radical translator qua radical translator

that would an extension of the radical translator's ocular

powers, as mentioned above. So the answer to the first

of the questions proposed at the start of section IV of

this chapter is No.

Let us then consider the second of the questions set

out at the start of section IV of this chapter. Namely,

we need to consider what sort of extensions of the

sentences by our postulated Dummett Realist Verifier makes

sense. So far we have considered the extension of the

Dummett Realist only as an extension of our senses.

Perhaps this is improperly limiting. Maybe we should

allow the Dummett Realist to attribute wholly new sorts

of senses to his hypothetical verifier. One should note

that on the HVist principle the unverifiability of any

hypothesis is not the result of our physical limitations,

for if it were the case that greater tactile ability would

allow us to verify hypotheses, then the matter is not an unverifiability in principle and hence on HVist principles it does not follow that there is no fact of the matter in question.

What might these other senses be? One should note that the claim that one might be able, say, to "see colors" in some way is not a reference to a new sense. Rather it is a use of an old one in an unexpected way. It seems that what we would need here would be some reliable process whereby we gain information about events in other than usual methods. Imagine that one consistently is able to say who is knocking at one's front door. Further imagine that the person never expects any particular person and that we are correct in the reasonableness of this lack of expectation. How then do we explain this epistemic accomplishment? Well, the best explanation one could here offer would not be that the person gains this information through some sensory mode other than the normal but that in some yet unaccouted for way he garners the information in the normal modes. Reference to sensory powers that go beyond the normal will normally serve to render an explanation defective.

Thus it appears that the being the Dummett Realist appeals to if he is not simply the first entity we considered (the persons with the microscopic visions)

then he is someone whose existence cannot be sensibly

posited. So the answer to the second of the questions

is No.

We need then to consider the third of the questions

set out above. Does Dummett Realism collapse into HVism?

As was indicated, the Dummett Realist wishes to maintain

a connection between truth and verifiability. The con-

nection is that if statements are true it must be the

case that some entity must be able to verify the state-

ments in question. The difference between Dummett

Realism is here simply that the HVist substitutes a

reference to our ability to verify statements where the

Dummett Realist says something must be able to verify

the statements in question. Now, the limits on verifia-

bility that are involved in establishing a claim of

metaphysical determinacy cannot be overcome by conceiving

of an entity which can see through walls, etc. The

hypothetical Dummett Realist verifier turns out to be

humans, for any extension of epistemic powers that would

allow a claim to be verified merely serves to show that

the claim in question was not in principle unverifiable.

The limits on verification arise not from practical

matters of purported human sensory limitation but from a

potential exhaustion of all possible evidence which leaves

open at least two incompatible hypotheses equally in

accord with the aforementioned evidence. Perhaps it is
worth noting that we are not here limiting the Dummett
Realist to employing merely sensory evidence alone but
also simplicity, compatibility with strongly held physical
theories, etc. It should be noted that it is not here
being assumed that appeals to such sorts of things can
be accomplished by means of sense organs alone since
sense organs alone cannot, even when hypothetically
extended, verify analytic hypotheses translation is in-
determinate. The claim is rather that if the Dummett Real-
ist can with the use of his sensory apparatus plus appeal
to such things as simplicity and conservatism verify
analytic hypotheses then it is not in principle impossible
for us to do so. Thus Dummett Realism turns into HVism.
One might remark here that if HVism is understood in this
way then it is no longer the most controversial assumption
in Quine's argument for the ITT. What becomes more contro-
versial is Quine's claim that translation is E.I. This
may be so, however, my effort is to try to defend some of
the matters that need defending if the ITT is to be a
viable thesis. I make no claim, of course, to have
attempted to reply to all the objections one might raise
concerning the ITT, only some of them. (However, I do
consider some matters related to simplicity considerations
and analytic hypotheses in Chapter VII, these being the

sorts of things one would have to go into if one were to attempt to refute this objection.

V

It remains to consider what "problems" the HVist faces and to try to make out the claim that the HVist can plausibly handle these problems. In attempting to render a position plausible via the method of refuting objections one may come up against certain queries of a methodological nature that are worth mentioning here. Firstly, one is limited to the objections one either finds in the literature or to objections that one is able to think up. It thus may be the case that even if one is able to reply to a number of objections to a position it still may be the case that there exists an objection which is fatal to the position in question which one has not considered. One is normally without any sort of guarantee, a priori or otherwise, that one has considered the proper sort of objections. Further, some might even argue that merely to show that a theory can handle objections is not in itself an argument in its favor, or at least not an argument of very great power. Some might insist that there must be reasons of a more "direct" sort in favor of a theory before we can properly accept it. What one can say in the face of such objections

is that the more objections a theory is able to handle,
especially if one makes an effort to consider objections
of a diverse sort, the more likely it becomes that the
theory in question could handle the objections that we
have either not thought of or have not bothered to
consider. Given that worthwhile objections to HVism will
take the form of claims that the theory cannot account
for something or other replies to objections will
generally take the form of attempts to show that the
theory can account for the matters in question. Thus in
answering objections we do provide "positive" reasons in
favor of our theory. One should note, however, that by
these methods the best we can claim is that we arrive
at plausible results, results which are themselves poten-
tially corrigible. Those who think, for their own
reasons, that philosophical theses are not well warranted
unless they are shown to be necessary truths will regard
such a methodology as inadequate. However, it seems to
me that the very corrigibility of HVism is a mark of its
own "empirical character" and counts in its favor. Further,
as will be obvious, I think for reasons made familiar by
Quine and his followers, that no philosophical or
scientific theses can obtain the kind of status the
objector desires and hence I am not concerned with the
objector's objection. With these considerations in mind,

I wish to turn to a consideration of some objections to HVism.

One of the big objections to earlier verificationist principles (by which I mean those philosophers generally associated with the logical positivists) was the claim that verificationist principles were self-referentially inconsistent.[15] It was claimed that verificationist principles did not meet their own standards. We need then to consider whether according to HVist grounds there is a fact of the matter with respect to HVism itself. If it is the case that it is impossible to verify (in a suitably holistic way, of course) HVism, then there is no fact of the matter concerning HVism, assuming, of course, that we are not able to infirm HVism on HVistic grounds. The answer to this question is that HVism is justifiable on its own grounds in terms both of the simplicity it introduces into our account of language learning (see Chapter IV for details), problems with rival theories and the HVist ability to answer relevant objections. The above sort of reply to the "self-referential inconsistency" objection is a much more straightforward and less problematic one than any of those attempted by earlier verificationists. This is not to say, of course, that earlier verificationists could not have taken such a line of defense. As put forward

by the HVist, HVism is just another part of our total theory.

One of the primary problems of earlier verification- ists concerned the specification of verification condi- tions for particular sentences. It was in fact the case that by and large verificationists were unable to specify what would count as verifying sentences of almost any type. Can the holistic verificationist specify verifica- tion conditions for particular sentences and if not, is this a problem for the HVist? The general grounds for holistic verificationism involve the very well known con- siderations concerning the simplicity of theories, the correlation of sentences with the general task of predict- ing future experience in the light of past, conformability of observations, compatibility with currently held theories, etc. So the HVist can set out in general terms what it is for a sentence to be verified. (Is the problem of explaining what constitutes confirmation unique for the HVist? Well, pretty clearly it is not. It is a problem just as much for the realist (whether RAVist or not) as the HVist. In fact it may be that the HVist has less of a problem as he can appeal to Quine's considerations on evidence (advanced in Word and Object and elsewhere) plus the work of people like Lakatos and Feyerabend. However,

it does not seem to be the case that the RAVist, given his

claim that a theory is correct only if it describes the

way the world in fact is, can make use of such work.

However, there likewise seems no reason the HVist cannot

qua HVist say that he will wait until someone comes up

with an account of confirmation that does get adopted by

those qualified to make such evaluations and adopt it,

unless we have some a priori reason (as long as we grant

that no one has given such a persuasive definition of

'confirmation') to think that an adequate definition will

be unusable by the HVist. Thus, much as anyone, it would

be nice if the HVist can precisely specify what it is to

confirm something, the fact that he can give no more than

general reflections on the matter does not count especi-

ally against his theory.) So the HVist can appeal to the

very large body of work on confirmation theory. After

all there is in fact no reason that different HVists

could not adopt different accounts of confirmation and

justification so long as the theories adopted are

altered (if this should be needed) to fit in with the

general HVist position. The HVist can then set out in

general terms what it is for a sentence to count as

verified. Can the HVist specify with precision what

counts as verifying particular claims? To attempt this

in advance of the development of the branch of knowledge in question is a form of unwarranted a priorism. What counts as verifying a sentence in chemistry, for example, can be determined only via an acquaintance with the relevant material. Hence the attempt to specify what counts as verifying _particular_ sentences is itself the embarrassment, not the inability of the HVist (or anyone else) to do so in detail.

It might also be objected against the HVist (as it was against the earlier verificationists) that there are counterexamples to his theory. It seems to me, however, that the refutation of philosophical positions via the method of developing counterexamples is not as simple or as easy as many appear to think. If one is to be critically effective via the construction of counter-examples one must produce counterexamples which are "neutral" with respect to competing theories, else one merely begs the relevant questions in the claim that a particular example is a counterexample to a particular theory. For example, it is thought by many to be possible to refute utilitarianism (in any of its forms) by producing counterexamples in the form of acts which are correct according to utilitarian principles but are in fact not just. The obvious problem here is that if one

is a convinced and consistent utilitarian one will not

regard the actions, assuming they are indeed sanctioned

by the version of the principle of utility one uses, as

unjust. It may be the case that the so called "method

of counter-example" never shows anything but I shall

not endorse so strong a claim here, for it does seem to me

to be the case that some positions can be refuted by

counterexamples. (Though I have no account of when a

position is subject to counterexamples and when it is

not and I know of no account of this matter). What would

constitute a counterexample to HVism? Evidently it would

be a sentence which is such that it obviously has a

determinate truth value but is unverifiable even in

principle. Let us consider some cases. It appears to

be a determinate matter whether or not there lived a

caveman who was five feet tall in 1,000,000 B.C. on the

land that is now the location of the Ohio State University

Philosophy Department. How would we verify such a claim?

The proper tack to take here for the Hvist is to concede

for the sake of argument that the claim in question is

M.D. and attempt to show that it is, in principle at

least, potentially verifiable. Well, certainly it is by

all possible observations in space-time.

I wish now to consider a putative counterexample,

one proposed by Putnam.[16] It appears to be the case

that it might be that there is a golden mountain that

no one knows exists. The problem that this appears to

pose for the HVist is that this statement quite

obviously is one which may be such that there is a fact

of the matter concerning it but it could, according to

the RAVist, not be verified without being falsified. In

other words the RAVist claims that the only way we could

verify this claim would be to find a golden mountain

which no one knows about, but once we did this, it would

not be a golden mountain that no one knows exists. This

counterexample appears to me to turn upon a sophistic

misunderstanding of the sentence in question. Let us

consider another case. Someone in 1800 utters the

sentence "There is a 9th planet and no one knows it

exists." Such sentences have the prima facie appearance

of "self defeating sentences" of the "I do not exist"

kind but we shall here not worry over that for it

might be that, for example, I could be firmly convinced

of some existential claim, say that there is a 10th planet,

but know that I could not produce justification for the

claim and I could say (even though to do so would

probably not be very bright) "There is a 10th planet and

no one knows it" without in any superficially obvious way

be performing in a self-defeating manner. Or at least

so it seems to me and further to hold otherwise would

not be fair to the RAVist. Would the person who utters

the sentence (in 1800) "There is a 9th planet and no one

knows it exists" regard the state of affairs in astronomical

knowledge today as showing that he had spoken incorrectly?

No, he would say that his utterance contained (involved,

whatever) a temporal parameter of sorts and could be

properly paraphrased as "There is a 9th planet and no

one now knows it exists". Likewise the utterer of the

golden mountain sentence would not regard our discovery

as refuting his remark, only as showing that it is no

longer correct to say it.

If I were to say of Fred that he is an unmarried man

and were to not have any contact with Fred for ten years

and were then to become reacquainted with him and find him

to be now married I would regard as quite foolish the

person who claimed that this showed that my remark of

ten years earlier was false. Let us consider a type of

counterexample which looks like it is not subject to this

sort of treatment. Consider "There is a golden mountain

which no one will ever know exists". It appears that there

is a fact of the matter about this claim and it appears,

prima facie at least, to be one that is outside the

realm approved of by the HVist. There are a couple of

things that might be said about this example. Firstly, are we asked to assume that this is a mountain that is in principle unfindable? Such would be a rather strange mountain, so we shall assume that it is just an accident that no one has ever come upon this mountain. Now, let us imagine a comprehensive cataloguing of all the mountains in the world by a very large team of persons who seem to comb the entire world several times over looking for a golden mountain. No place large enough to hide a mountain is left unchecked, in fact everything is checked several times by different persons. Let us assume that no golden mountain is found. This does not show that it is absolutely certain that a golden mountain fitting the description given does not exist, but it does render it highly probable that there is no such mountain. Thus, if this procedure is carried through and yields this result, the sentence in question is incorrect. Let us suppose that as a result of this search they find one such mountain, and have incredibly good evidence that there is only one such mountain. Now, what one should do is to catalog all the "mountain knowledge" there was before the investigation began. If this mountain's existence was not among said knowledge, then the utterer of the claim about the golden mountain is vindicated.

One is right here to urge that simply finding a
golden mountain does not verify or falsify the claim, but
an exhaustive search of the world (along with the assumption
that mountains, even golden ones, are pretty big things
which are not such that it is in principle impossible to
see or touch them) can. Doubtless there are other sorts
of counterexamples worth considering, but it seems to be
the case that the HVist can handle such objections either
by offering a rough account of how they can be verified
(again, the HVist does not have to be able to say how one
would as a matter of practice search the world for golden
mountains in such a way that one could be sure that one
has not missed anything, that is up to technicians and
such) or by arguing that the sentence is not properly
understood if it is given a reading on which it is M.D. and
unverifiable. Again, our linguistic intuitions appear to
support such a claim irrespective of our views on verifica-
tionism. Further, the HVist might always say that the
claim that a sentence is a counterexample to his view
begs the question and hence refuse to consider it. But,
as mentioned earlier, this would tempt the RAVist to
claim that to say it is not a counterexample begs the
question also and thus the controversy degenerates to the
level of mutual question begging. Thus, both the HVist
and the RAVist should, I think, avoid such tactics for

both can equally well "rebut" objections in this way.
Perhaps, the RAVist might say that because his position is
the more commonly accepted one it is up to the HVist to
demonstrate his position whereas the RAVist does not have
to demonstrate that HVism is untenable. Thus, the RAVist
claims that it is ok for him to claim the HVist begs the
question and not vice-versa, as the burden of proof is
on the HVist. These tactics (which I do not attribute
to the bulk of RAVists, of course) are merely the worst
sort of dogmaticism and involve the assumption that
because a majority of philosophers who take a stand on
the issue are RAVists it thus is the case that the burden
of proof is on the HVist. The "burden of proof" is,
rather, on both parties equally.

It might also be objected against the HVist that the
HVist in some way confuses intelligibility with justi-
fiability or confuses truth with warranted assertability.
We need, then, to consider whether or not the HVist is
committed to saying that sentences that violate his criter-
ion are unintelligible, for it is presumably the case
that one who would raise such a charge would not do so
unless he thought the HVist were so committed. If this
is not so then this objection will not be an effective
one. Is saying that there is no fact of the matter
concerning translation, for example, tantamount to saying

it is unintelligible to speak of one expression as a translation of another. The obviousness of the propriety of a negative answer to this question (see Word and Object, pp. 73-80 for considerations related to this matter) is sufficient to indicate the untenability of the objection. Talk of matters concerning which there is no fact of the matter can be perfectly intelligible in that sentences in which we speak of such matters may violate no rules (regularities) of speech of a sort that would render them unintelligible. The objection here being considered seems to stem from a tendency to assimilate all discourse to the form of declarative statements and to assume that use of sentences of the form of declarative sentences is unintelligible unless we assume that there is a fact that they state. That this is an over-simple model for language has been made "common knowledge" by Wittgenstein, Austin, and their followers. It may be then, to pursue our example, that claims that a word in one language in the proper translation of a word in another language can be given some sort of "speech act" treatment which views their primary function to be other than "fact stating".

There is another objection, of a somewhat different sort, that I wish to consider here. One of the motivations of the earlier verificationists was to provide principles

that would show why certain forms of discourse (areas

of investigation) were illegitimate. For instance, some

earlier verificationists, thought that traditional meta-

physics was illegitimate (Nonsense, spurious, whatever)

and they wanted to formulate a principle that would

exclude metaphysical discourse (The Absolute is green)

and include scientific. It would thus be a forceful

criticism of one with such motivations to show that his

principle either failed to exclude talk of metaphysics

and such or did exclude the discourse of scientists.

Others, who did not dislike metaphysics, merely offered

verificationist principles to provide a criterion of

"demarcation" between science and metaphysics. Does

the HVist, then, wish either to provide a principle that

explains why certain forms of discourse are illegitimate

or to provide a "demarcation criterion" between metaphysics

and science? The answer to this query appears to me to be

No. Further, it seems to be the case that on the veri-

fication grounds of the HVist, there is no a priori reason

to think that numerous questions of a sort generally

considered metaphysical (I put none but the lightest

weight on any sort of distinction between scientific and

metaphysical questions) are such that they are unveri-

fiable. This attitude is then, it appears to me, another

manifestation of the reasonableness and the empirical character of the methods of the HVist. There are doubtless other objections that are worth considering that I have not considered because I have not thought of them, just as there are objections that I have thought of but have not considered because I regard them as not worth considering. I have tried to make plausible the claim that is reasonable to hope that the HVist can provide replies to objections of some force. As stated earlier, I regard this in itself as constituting a "positive" argument in favor of HVism.

VI

It remains to consider the relationship between realism ard HVism. Is the HVist committed to giving some sort of revisionary account of the status of the entities our theories commit us to. My answer to this question is no, but it is, of course, more important to attempt to set out the relevant issues and explain what would and what would not require a positive answer to this question. Assuming that there is no "first philosophy" (in the sense considered in Chapter V), and that we can do no better than to work in the context of uncontroversial scientific theories, then there is no a priori reason to think that there will not be all sorts of subject matters concerning which there will be unique well confirmed theories. To take a

prosaic sort of case, it is in all probability the case
that if I were to consider all sorts of hypotheses con-
cerning the type of machine which is producing the marks
on this paper the unique best hypothesis would be that it
is a typewriter. Thus given this, there is no reason
why the HVist cannot say along with the RAVist that
there is a fact of the matter concerning the machine that
is producing the marks on paper. Thus, for any subject
matter about which we can holistically verify hypotheses
we can claim that there is a fact of the matter. Now, if
one thinks that every truth is potentially verifiable
(or every fact can be discovered, whatever) then one can
be a HVist and hold that there is no hypothesis that is
not M.D. Thus it is <u>possible</u> that the RAVist and the
HVist can in fact both hold that there is a fact of the
matter about everything or, given that there may be ir-
redeemable obscurities in the claim that every fact may
be knowable, the HVist can at least hold that the RAVist
is right in holding that the facts about the world are
M.D. However, it seems to be the case that one would
not put forth a theory like HVism unless, as Quine does
concerning translation, one thought that there were areas
of discourse that failed the test of the HVist. Now,
and here is where it may be not unfair to call the HVist
a revisionary metaphysician, if the HVist holds that

very large areas of investigation are such that theories
in them are not verifiable then he does owe us an account
of the status of the objects of theories. What one might
call an extreme HVist (EHVist) would be one who maintains
that there is no hypothesis such that alternatives could
not be developed which are equally compatible with the
evidence and thus maintained that, in effect, everything
was M.I. It is the EHVist who will have the most diffi-
cult time explaining why some theories are more well-
warranted than others, assuming that M. I. holds universal
sway. It should be noted that the EHVist cannot opt
for some sort of "irrationalism" which holds that all
beliefs can be equally well supported (or are equally un-
supportable) so we need merely "pick what we like" for
in so doing he undercuts the case for HVism. The probable
best tactic for the EHVist is to make a great deal of
the importance of Goodman's notions on entrenchment and
other "conservative" epistemological considerations as
showing that it is better to use certain theories than
others while at the same time denying that this is evidence
for the truth of the preferable theories. Just as,
according to Quine (for references and details see Chapter
IV), we can have reasons for using one set of analytic
hypotheses over others which do not count as evidence

in favor of the preferable set of analytic hypotheses so

the EHVist could offer reasons for prefering one set of

beliefs to others which were not evidence conferring

reasons. (On my reading of Quine he is not an EHVist,

maintaining that certain matters can be holistically

verified, but I am reasonably sure that there are readings

of the relevant texts that would support the claim that

Quine is an EHVist, for example one might cite certain

of Quine's remarks on p. 23 of Word and Object in support

of this claim). I wish in closing this chapter to dis-

tinguish three distinct sorts of HVism.

The EHVist, as stated, maintains that no hypotheses

is verifiable. (Why one would think this is not too

important here, but, if one accepted the parallel between

translation and physics considered in Chapter V and took

this to show that both physics (and mathematics for that

matter) and translation were unverifiable, one might well

hold that this shows that hypotheses about the subject

matters of physics were indeterminate). Or rather that

no hypothesis but that of EHVism (the EHVist is in danger

of falling into self-referential inconsistency if he is

not careful). The RAVist could agree with the EHVist in

holding that no hypothesis is verifiable, the radical

difference comes in the metaphysical consequences they

take this to have. The moderate HVist, of whom I count

Quine an example, (MHVist) holds that some things are verifiable and some things are not. Thus, as is Quine, the MHVist can be a realist about atoms, numbers[17] and such and still hold that there is no fact of the matter about some things, say translation. Lastly, the realistic HVist maintains that there is a fact of the matter about everything (or at least everything that the RAVist thinks is M.D.) because he holds that we can, in principle, discover the correct account of any subject matter. One could, then, be a RHVist and maintain that translational hypotheses were M.D. for the RHVist would hold that such matters were in principle holistically verifiable. It should be pointed out, however, that the disagreement between the RHVist and the RAVist is considerably more than a "merely verbal" disagreement even though they do agree concerning the M.D. of the proper account of the world. The RAVist and the RHVist disagree in that the RHVist maintains that if it were the case that a hypothesis is unverifiable then it would not be M.D. (Unlike the EHV-ist and the MHVist, the RHVist thinks that the antecedent of the conditional is never true) whereas the RAVist holds that the antecedent of this conditional statement could be true and its consequent false.

In summary, then, in Chapter I a general statement of the plan of this work was given. In Chapter II I argued

that three recent accounts of truth in natural language
depend on the untenability of the ITT. In Chapters III,
IV, and V I attempted to rebut standard objections to the
ITT. In the course of doing this I offered certain exe-
getical hypotheses concerning Quine's views on evidence,
justification, theories, and truth. In Chapters VII and
VIII I attempted to render plausible the sort of veri-
ficationism I attribute to Quine in Chapter IV, holistic
verificationism. I do not, by any means, claim to have
provided anything like a full defense of holistic
verificationism. My primary purpose being to indicate
that there is available to us a coherent view that allows
us to infer from the underdetermination of analytic
hypotheses to the ITT which does not allow us to infer
from the underdetermination of physical theories to
the indeterminacy of physics.

Footnotes

[1] For that matter, there is reason to think that Kant was a verificationist. See Bennett, Jonathan, Kant's Dialectic (New York: Cambridge University Press), pp. 22-27.

[2] Dummett, Michael, Frege: The Philosophy of Language (New York: Harper and Row, Inc., 1973), p. 671. Further references to this work will be included in the text.

[3] I do not think that it matters if either RAVism or HVism is the view of common sense, but I include these remarks for those who think such things are of importance.

[4] Wittgenstein, Ludwig, The Philosophical Investigations (New York: MacMillan Co., 1964), #402. Further references to this work will be included in the text.

[5] As stated, I do not here wish to go into any sort of extended historical considerations, however, it perhaps is worth quoting a passage from Passmore, John, A Hundred Years of Philosophy (New York: Penguin Press, 1960), p. 50 which is descriptive of the sort of idealism to which HVism bears some resemblance:

> 'Idealists' we shall be discussing in this chapter
> have no particular interest in the theory of per-
> ception and would strongly object to being classed
> with Berkeley, let alone Huxley. The central core
> of their teaching is that to be real is to be a
> member of a 'rational system', a system so constructed
> that the real nature of its members is intelligible
> only insofar as the system as a whole is understood.

[6] Dummett, Michael, "The Significance of Quine's Indeterminacy Thesis", Synthese, 27 (1974), pp. 351-397 and Kirk, Robert, "Underdetermination of Theory and Indeterminacy of Translation", Analysis, 33 (1973), pp. 195-201.

[7] Dummett, Michael, "Truth", Proceedings of the Aristotelian Society, 59 (1958-59), pp. 141-162. Further references to this paper will be included in the text.

[8] Quine, W. V., "Grades of Theoreticity", in Experience and Theory, ed. by L. Foster and S. Swanson (Amherst: University of Massachusetts Press, 1970), pp. 1-18. Further references to this paper will be included in the text.

[9] Sellars, Wilfrid, "Empiricism and the Philosophy of Mind", in Science, Perception,, and Reality (New York: Humanities Press, 1963), especially pp. 183-196.

[10] Goodman, Nelson, Fact, Fiction, and Forecast (New York: Bobbs-Merrill Press, 1965). Further references to this work will be included in the text.

[11] Goodman, Nelson, Languages of Art (New York: Bobbs-Merrill Press, 1968). Further references to this work will be included in the text. Also Goodman, Nelson, "The Way the World Is", in Problems and Projects (New York: Bobbs-Merrill Press, 1970), pp. 24-32.

[12] Quine, W. V., "Natural Kinds", in Ontological Relativity and Other Essays (New York: Columbia University Press, 1969), pp. 114-138.

[13] Goodman, Nelson and Ullian, J., "Bad Company", Journal of Philosophy, 72 (1975), pp. 142-143.

[14] Goodman, Nelson, "Induction: Forward", in Problems and Projects, (New York: Bobbs-Merrill Co., 1970), p. 357.

[15] Fitch, F., "Self Reference in Philosophy", in Symbolic Logic (New York: MacMillan Co., 1952), pp. 201-213.

[16] Putnam, Hilary, "Logical Positivism and the Philosophy of Mind", in The Legacy of Logical Positivism, ed. by P. Acheinstein, (Baltimore: John Hopkins Press, 1969), pp. 105-117.

[17] It is not so clear that Quine can be so sanguine about numbers as he thinks he can, see for argument on this matter W. Lycan and G. Pappas, "Quine's Materialism", forthcoming.

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