DISPOSITIONS AND ANTI-REALISM

A.C.H. WRIGHT

Bedford College

University of London

Dissertation for M.Phil., May, 1971.

ProQuest Number: 10097320

ProQuest 10097320

ABSTRACT

This thesis aims at showing that the anti-realist conception of meaning and truth is fundamentally misconceived. There is a greater emphasis on the negative aspects of anti-realism than on positive arguments for rival accounts, and the main thrust of the negative argumentation is to draw unacceptable consequences of anti-realist theory. What unifies a rather hybrid thesis is a sustained attack on the anti-realist treatment of dispositions.

Many of the points made in the thesis cannot be claimed to be original; phenomenalism and behaviourism have already been heavily criticised for instance. What is to be hoped is that a wider scope is given to many of these points by showing how they form an attack on views whose raison d'etre is a particular analysis of truth and meaning. Argument rather than deep analysis is conspicuous: given the limitations of space, I hope this is justifiable in the interests of polemic.

Chapter 1, is partly taken up by a brief account of some of the ways in which the anti-realist theory of truth and meaning has played a significant part. The rest of the chapter introduces an anti-realist treatment of dispositions - that of Dummett's logical behaviourism - and obvious objections are raised.

Operationalism is argued to be a peculiarly lucid species of anti-realism, and for this reason its account of dispositional terms is studied in detail. Particular attention

is paid to Carnap's reduction sentences in order to show
the impossibility of specifying observational conditions
for predication of such terms. This leads on to a more
general weakness of anti-realism: its inability to give a
satisfactory account of the role of theory.

Chapter 4 is devoted solely to a discussion of the
Law of Excluded Middle, rejection of which is a startling
feature of the anti-realist thesis. It is contended there
that, on similar grounds, the anti-realist cannot accept
the Principle of Non-Contradiction.

We return to dispositions in Chapter 5, emphasising the
weaknesses of anti-realist accounts of dispositions and
attempting to show the requirement of a categorical basis
for a disposition. Here again, the emphasis is chiefly
on pointing to the defects of anti-realist accounts.

The last chapter aims to sketch what is fundamentally
at fault with anti-realism: its mischaracterisation of what
it is for words and sentences to have sense. The examples
of behaviourism and phenomenalism are employed here for
their help in clarifying the issues.

CHAPTER 1.

"... let us envisage a dispute over the logical validity
of the statement 'Either Jones was brave or he was not'.
Imagine Jones to be a man, now dead, who never encountered
danger in his life. B retorts that it could still be true
that Jones was brave, namely, if it is true that if Jones
HAD encountered danger, he would have acted bravely. A
agrees with this ... but argues it still might be the
case that we should still know nothing which would be a
ground for asserting either (i.e. either counterfactual,
which is considered by Dummett to be equivalent to the
disposition statement). If B ... wishes to maintain the
necessity of 'Either Jones was brave or he was not', he will
have to hold either that there must be some fact of the
sort to which we usually appeal in discussing counterfactuals
which, if we knew it, would decide us in favour either of
the one counterfactual ('if Jones had encountered danger,
he would have acted bravely') or of the other ('if Jones
had encountered danger, he would not have acted bravely');
or else there is some fact of extraordinary kind, perhaps
known only to God."

(Dummett - 'Truth', P.A.S., Vol. 59, 1958-9.)

The above quotation indicates the nature of, and gives
substance to, the controversy between realists and anti-
realists over the nature of truth and the meaning of statements.
In our particular example, the one we will be mainly concerned

with, A, who we can suppose represents Dummett's own position, takes up an anti-realist position of the reductive variety, viz. logical behaviourism. B assumes a realist pose. Identification of meaning with truth-conditions ensures that every meaningful statement has a truth-value, for if a statement is meaningful, it is furnished with truth-conditions. (I shall assume throughout that statements are bearers of truth-values, and, for convenience - Dummett does this - will always talk of statements in truth-bearing contexts. This dogmatism does not, I believe, affect the main issues discussed here. It is not meant to imply that statements are the sole candidates for the job of bearing truth-values; neither do I want to say that they are even successful candidates.) Provision of truth-conditions entails that it has been stipulated under what conditions a statement is to be considered true. If the truth-conditions are incoherent, the statement is not meaningful in the sense required (it might be emotively meaningful). Self-contradictory statements on this view are incoherent - no truth-conditions can be specified[1] - but this does not apply to complex statements, e.g. 'It is raining and it is not raining' where the contradiction lies in the utterance. (This has relevance to our later discussion on the question of logical laws, in particular, the Law of Non-Contradiction.) In our example, truth-conditions can be provided, says the realist, for the disposition statements, but not in the way Dummett envisages in the piece quoted.

Possession of truth-conditions does not entail that a truth-value can be attached to a statement; for the realist, statements can be true or can be false, statements ARE true or ARE false, independently of the possibility (let alone the actuality) of our discovering their truth-values. Even though there is no evidence for or against Jones' being brave, not only is it true that 'Either Jones was brave or he was not brave' but it is either true that Jones was brave or it is true that Jones was not brave: the Law of Bivalency holds as well as the Law of Excluded Middle. For the realist, truth is a logical notion, the latter being independent of the epistemological sphere.

The anti-realist, on the other hand, takes up a quasi-verificationist viewpoint on meanings: for him, the meaning of a statement is given in terms of the conditions which we recognise as establishing the truth-value of the statement. These truth-conditions might be equated with truth-grounds, if by the latter we mean those conditions which justify us in claiming that the statement in question is true and exclude those conditions which _might_ lead us to believe in the truth of the statement; i.e. for the anti-realist, the meaning of a statement is given in terms of a proper sub-class of the evidence for the truth of the statement; we might label this class as the correct assertability class. Thus, truth for the anti-realist is an epistemological notion, and, for the anti-realist, this can have repercussions in the logical sphere, for, to him, logic and epistemology are more interdependent than they are for the realist. A

statement cannot be true and cannot be false independently of the actuality or possibility of our coming to know of its truth-value. A statement is true if and only if it is correctly assertable (this is how we learn the use of the word 'true'). A statement is false if and only if its denial is correctly assertable. The conditions under which a statement or its denial is correctly assertable do not together exhaust all the possible recognisable phenomena.

DIGRESSION - (A version of anti-realism which made statements false under all those conditions where it was not true would undercut its own raison d'etre. For if we try to marry the notions of truth and falsity to the epistemic situations in which they are used, it is at least incumbent on us, and may be required by the thesis, to specify in positive terms assertability conditions for the falsity of the statement. It will not do merely to state that the statement is false if it is not true. Furthermore, the problem arises as to the truth-value of a statement when we have not recognised a truth-condition, and therefore cannot affirm the truth of the statement, but cannot necessarily assert the falsity of the statement because the conditions for falsity are given negatively. In such cases, how could we ever ascertain the falsity of a statement?)

Whereas for the realist, a statement is false under all those conditions when it is not true, for the anti-realist this is not so. Failure of truth-conditions to obtain does not entail that the statement is false; we may not be able

to say it is either, depending on what kinds of conditions
are considered as establishing the truth-values of statements
of the relevant class. In our particular example, there may
be no statement about Jones' behaviour which makes true (and
none that makes false) the statement that Jones was brave;
therefore, not only can we not say which truth-value such
a statement has, we cannot even insist that it has one.
For, just as it is claimed that statements whose grammatical
subject is a referring expression which refers to nothing have
no truth-value, so it is claimed by the anti-realist that
we cannot say _a priori_ that any statement has to have a truth-
value, for the conditions establishing the truth or falsity
of statements of the particular class may fail to be
recognised.[2] This is not to say that such statements are
not true or false, but simply that we cannot say they are.
We can assert the former only if we know that neither the
truth-conditions nor the falsity-conditions obtain. Likewise,
the typical anti-realist denies that we can assert the
universal validity of the Law of Excluded Middle, while
denying that we can deny it.[3] Some versions of anti-realism
might preserve the Law of Excluded Middle, e.g. those which
made truth- and falsity-conditions exhaustive of all
possible recognisible phenomena; Dummett suggests, in 'The
Reality of the Past', a way to preserve the Law in the
context of an anti-realist theory, but at the cost of
asserting the existence of two distinct notions of truth.
But it can be described as a salient feature of anti-realism

that it deny the logical necessity of the Law of Excluded Middle.

The quotation from Dummett gives some indication of the rationale of this denial. If the meanings of psychological predicates is given in terms of statements about behaviour — which account is what a reductive behaviourist will minimally accept — then it is clear why he cannot say a priori that we can in all cases affirm a psychological predicate or its contradictory of a person. For even if we rule out cases where the relevant behaviour was not observed, there will still be cases where no relevant behaviour took place. The ploy of insisting[4] that 'X was not brave' is true under two types of conditions — a) when X was cowardly, and b) when neither 'X was brave' nor 'X was cowardly' are true — in order to preserve the Law of Excluded Middle defeats the reductionist's own purpose. For what is the behaviour criterial of 'X was not brave'? Absence of behaviour criterial of 'X was brave' is unsatisfactory for reasons cited earlier. The realist's categorical bases for dispositions provide the rationale for his acceptance of the Law of Excluded Middle; either the person will possess the base or he will not. This anti-realist non-acceptance of the Law of Excluded Middle is odd and I will have more to say about it later.

*   *   *   *

But, perhaps it might be useful at this point to stand back and take stock of the general structure of the controversy

and the issues in dispute; also a brief outline of the
strategy of my argument may help to clarify things. It might
seem as though my way of setting up a discussion of truth and
meaning in terms of a two-sided dispute begs the question
as to there being only two possible positions on the issues
involved. In answer, I would say that firstly, the occasion
of this thesis _is_ the realist versus anti-realist dispute
and that therefore the objection, if it is one, misfires.
Secondly, it can be cogently argued that the two positions
are the only two which have any plausibility; what would
a third possibility be like?

Moreover, it is not incompatible with this assertion
that one need not take up a realist position in all areas;
anti-realism is, as far as I know, a highly plausible
viewpoint to take on the question of meaning and truth in
mathematical fields.

As to strategy, I shall be arguing positively for
realism in the field of psychological phenomena, much of
the argument for which will consist of argumentation against
anti-realism. If the anti-realist objects that this latter
is to beg one crucial question at issue, viz. whether the
negation of negation is the same as an affirmation, one
would agree but hold that nevertheless the exposing of the
anti-realist position as leading to undesired results,
e.g. solipsism, can by itself impugn the whole anti-realist
position, including that on the Law of Negation. Moreover,
I am concerned in this thesis with defending a realist

interpretation of psychological predicates; if that defence requires a general assumption of realism, I can only say that that perhaps shows that realism is more deeply ingrained in our ways of thinking than the anti-realist allows. But perhaps the situation is not so bleak - perhaps some of my arguments, e.g. those for an acceptance of the Law of Excluded Middle, have force as arguments for realism generally and are not open to the objection of petitio principii. Some of my specific arguments in favour of realism in psychology do assume realism; in particular, the assumption that if dispositions have categorical bases, then there are only two possibilities - either a person possesses or he lacks such a base. I hope that the absurdity of denying this assumption can be rightly construed as an argument for realism.

* * * * *

Further clarification of the issues central to the dispute may result from putting anti-realism into historical perspective by indicating its connexions with allied doctrines. The nature of the realist/anti-realist dispute is now much clearer, mainly due to the work of Michael Dummett, but the basic controversy is of fairly long standing in philosophy.

No doubt, Marxists would be more attracted to an anti-realist account than a realist one, if they ever give thought to bourgeois philosophy.

The attack on classical logic, explicit in the non-acceptance of the Law of Excluded Middle, was a fundamental

feature of the intuitionistic critique of 'Platonistic'
mathematics. For the Platonist, mathematical truths are
to be discovered, whereas for the Intuitionist, they are
created by mathematicians. Thus, for the former, mathematical
statements will be true or false independently of the
possibility of our knowing their truth or falsity, whereas
for the latter such statements will be true only if there
is a way of constructing a proof of them. For an
Intuitionist, knowing the meaning of a mathematical statement
consists in knowing under what circumstances that statement
is proved; for a Platonist, it consists in knowing under
what circumstances the statement is true. Intuitionists,
therefore, cannot accept the Law of Excluded Middle, for
there is no justification for holding that, for any statement
p, we can construct either a proof of p or a proof of '¬p'.
For 'p v ¬p' to be asserted, we must be able to either
assert 'p' or assert '¬p'; we can do this only if we have
shown or can show (in, I hope, an intuitively acceptable
sense of 'can') that either can be proved. In particular,
the onus is on the affirmative part of the Law of Excluded
Middle, for, although '¬p' can be proved if it can be
proved that 'p' leads to a contradiction, the other version
of reductio ad absurdum proof is not accepted in Intuitionist
Logic, viz., we cannot prove 'p' by proving '¬¬p'.

It is vital to note the level of generality of the anti-
realist thesis. It deals with a whole conception of what
truth is and how meaning is characterised. This is why it

is not _a priori_ absurd to claim that two _instances_ of the
thesis - phenomenalism and behaviourism - are dubiously
compatible.

Idealism provides another type of anti-realist
orientation. Berkeïian views get off the ground mainly
through emphasis on the role of the perceiver in the
perceptual process: this leads to the conclusion that
everything existent is mental, or, at the very least, that
nothing outside observation exists. The symmetry of this
with the anti-realist conception of meaning seems obvious.
Our grounds for asserting that a material object is present
may involve statements of observation. An idealist will
typically claim that material objects are nothing over and
above perceptual objects, and that therefore the meaning
of a material object statement can be given in terms of
observation statements relating to that object. The original
phenomenalist programme was a clear exposition of this
viewpoint. The conflation of truth-conditions with truth-
grounds or correct assertability conditions or sufficient
evidence on the part of the anti-realist argues for a
phenomenalistic interpretation of the existence of material
objects. The attempts of perceptual idealists to avoid
paradox by bringing in God as a permanent and omniscient
perceiver or by weakening their phenomenal claims by one
of two methods - a) postulating possible sense-date, b)
transforming phenomenalism into a purely linguistic device
do not affect the main point - that the meaning of material

object statements is essentially bound up with the
situations in which such statements are verified.[5] Both
behaviourism and phenomenalism look on, respectively,
mental states and physical states as logical constructions out
of statements of, respectively, physical behaviour and
phenomenal experience; in theory, therefore, such constructions
could be discarded.

Positivism, in its various forms, is anti-realist.
Many positivists were attracted towards behaviourism or
phenomenalism. Operationalism, with its insistence that a
concept has only as much meaning as is supplied by sentences
giving the test-conditions under which we can say the concept
is instantiated (on extreme operationalist interpretations,
e.g. that of Bridgman, different methods of verification
entail different concepts), ties the notions of truth and
correct assertability. In general, the verificationist
criterion of meaning allies the meaning of a statement (or
whatever) with the experiences undergone in verifying that
statement.

* * * * *

Before we begin the detailed discussion of the anti-
realist conception of dispositional statements, the central
part of this thesis, it would be appropriate, I think, to
discuss some objections to anti-realism. By this means we
will be able to clear away some of the more obvious points
to be made against the position so that the ensuing argument-
ation will not be vitiated by a doubt that the basic problems

of the position have been disregarded. The refusal to
accept the Law of Excluded Middle, perhaps the most significant
anomaly of the anti-realist protagonists, deserves separate
consideration and Chapter 4 is devoted to it.

Anti-realism seems more plausible, _prima facie_, in some
fields than in others. For a non-mathematician, the thought
that mathematical truths might be (merely) creations by
human beings does not sound unduly alarming. Mathematical
objects are such abstract things that the claimed necessity
of their having to be constructed does not do too grievous
violence to our unreflective conception of the world. So far
as intuitionist-type constructivism is concerned, there
are problems about intersubjectivity and consistency (for
the mathematics is 'prior' to the logic), but such problems
do not present such profound challenges to our conception
of the world as those arising from anti-realism applied in
other fields, e.g. to refuse to assert the Law of Excluded
Middle in the case of historical statements leads to very
odd conclusions; the statement expressed by the disjunction
'Either Richard III killed Edward V or he did not' is not
necessarily true (we can leave aside 'historical' statements
whose referring expression fails of reference). Dummett's
suggestion, in 'The Reality of the Past', of a distinction
between absolute truth and (ordinary) truth mitigates this
oddness very little. The refusal to say that one of the
above disjuncts is absolutely true, i.e. true in all _Possible_

past histories; 'possible' in the sense of cohering with all
other absolutely true historical statements, and the other
absolutely false, i.e. false in all possible past histories,
gives us a picture of indeterminateness in the past itself.
But surely it is absolutely true or absolutely false that
something happened, regardless of our knowledge or capacity
for knowledge? We are asked to view the past as existing only
in the present (cf. early verificationist statements on this).
I will not pursue this question in view of the fact that
Dummett himself does not see an anti-realist conception
of historical statements as a serious possibility.

A much stronger case can be entertained for the employment
of anti-realism in the field of philosophical behaviourism,
for it is by no means absurd to suggest that it is not always
true or false of someone that he possesses a particular
character trait. To that extent, it is not absurd to deny
the logical necessity of 'Either Jones was brave or he was
not'. For, unlike the situation with regard to past events
which might have been observed by a historian - he merely
happened to be living at the wrong time - it is not so
unreasonable to urge that, in the absence of behaviour,
nothing can be said about mental phenomena; neither can it
be urged that there IS anything to talk about. Behaviour
might be a manifestation of a disposition, but in the absence
of any relevant behaviour, there is nothing for behaviour
to be a manifestation of. This is not to reduce dispositions
to summaries of past behaviour.

Similarly, Intuitionist Logic, with its more rigorous demands on proving existence, might be appropriate for employment in mathematical work but is it justifiable to use it in other fields?

The absence of any inductive processes of reasoning in mathematics - mathematical induction is not a counter-example - is probably significant here: the Natural Deductive processes, which form the basis of Intuitionist Logic, are solely deductive in character. However, the demand on the part of anti-realism that a basis be provided for the truth of statements in other fields is a demand for deductive premisses from which to deduce conclusions - phenomenalism, for instance, aims to provide a basis for the truth of material object statements by means of showing how such statements can be deduced from statements about sensory experience. Perhaps we can detect here a suspicion of inductive processes; in particular, the anti-realist theory as to the requirements of language acquisition and knowledge of concepts, e.g. behaviourist insistence that learning psychological concepts demands that behavioural criteria be provided for their correct employment, seems to me a demand for epistemic bases from which to deduce correct assertions. To tolerate less for an anti-realist would be the first step on the road to scepticism.

Justification for its wider use must consist of something more than the mere fact that it is used, apparently successfully, in one field. This requirement is made the

more imperative in view of the fact that, in the Intuitionist
system, the logic is derived from the mathematics and not
vice-versa. It is the reasoning methods of the mathematicians
which justify the logic, not, as is more orthodox perhaps,
the logic which justifies the mathematical reasoning methods.
This requirement seems to demand that the areas in which
the Intuitionist Logic is to be employed be similar to that
of mathematics. The justification put forward does lie in
such a similarity, viz., the way in which sense is given to
words and statements via correct assertability conditions,
and not through truth-conditions. The sense of mathematical
statements, is given in terms of what would justify us in
asserting such statements, says the anti-realist: learning
requirements demand a similar theory of sense-possession
in other fields. Dummett says, in 'The Reality of the Past',

"... the process by which we came to grasp the sense
of statements of the disputed (i.e. between realists and
anti-realists) class, and the use which is subsequently
made of these statements, are such that we could not
derive from it any notion of what it would be for such
a statement to be true independently of the sort of
thing we have learned to recognise as establishing the
truth of such statements."

Just as in Intuitionist Mathematics, one can assert 'p v ¬p'
if and only if there is a known method for arriving at
the truth of one of the disjuncts, so in other fields we can
assert the Law of Excluded Middle only when we can assert a

disjunct. A statement is true if and only if we can correctly assert it; the meaning of that statement is given in terms of the conditions under which we have learnt to assert the statement (correctly).

I think this justification is mistaken because it mischaracterises what it is for words and statements to have sense. It is a wrong account of truth and meaning. I hope to justify this in the final chapter and will confine remarks on the subject here to stating the most obvious objection to anti-realism - viz. that it leads to solipsism. For, a) there are problems about specifying the reference of 'we' in 'The meaning of a statement of a given class is given in terms of the conditions which we recognise as establishing the truth or falsity of statements of that class'. And, b) the requirement that recognisable conditions provide the truth-conditions in the anti-realist account of meaning provides no guarantee that anybody would be speaking the same language and would understand words in the same way. And, most crucial of all, c) in the case of empirical statements, the anti-realist demands some observational basis as forming the foundation of our knowledge. This will usually involve reduction of non-observational statements to observational ones. Given that the anti-realist is miserly in his choice of basic data, why should he not be as miserly as possible? b) and c) dovetail: what guarantee is there that the observational data serving as evidence for the truth or falsity of statements is the same for any two people? To

claim that the same language is used is a _petitio principii_, for the meaning of statements explaining other statements' meanings is presumably given in terms of correct assertability conditions. This question will be dealt with in depth in the final chapter.

Lastly, and this leads directly into our discussion about Jones' bravery, there is the problem for philosophical behaviourists of specifying the conditions under which we can say that dispositional statements are true or false, particularly such statements referring to people. _Prima facie_, it would appear that the conditions necessitating the truth (and those necessitating the falsity) of such statements would be varied and copious. Furthermore, we shall see what sense anti-realism makes of theories, both within science and outside it, and of 'theoretical terms'. This is important both in relation to the widespread use of dispositional terms, and because it is surely a requirement of a theory of meaning that it make sense of theories.

Having thus, I hope, given an indication of the kinds of objections open to those who oppose anti-realism, some of which objections will be more fully developed and utilised in later chapters, let us now turn to the anti-realist treatment of dispositions, with particular reference to Dummett's account of bravery, part of which was quoted at the beginning of this chapter.

Footnotes to Chapter 1.

1. E. Erwin presents an attractive thesis on this.

    '... if "Virtue is square" is really empirical, then we
    should be able to describe what it would be like for this
    statement to be true. The inability to give such descriptions
    ... does not conclusively prove that such statements are
    not empirical, for we might be handicapped by ....
    Nevertheless, if no one is able to give such descriptions,
    then this is some ground ... for believing that the
    statements in question are a priori false and not merely
    empirically false.'

    (The Concept of Meaninglessness, p. 154.)

2. The controversy is over the meanings of classes of
    statements, not particular statements. Therefore, for an
    anti-realist, a statement is not necessarily meaningless
    merely because we have not attached a truth-value to it.

3. In Heyting's Intuitionist Calculus, an axiomatized anti-
    realist logic, '$\neg(p \lor \neg p)$' leads to contradiction, making
    '$\neg\neg(p \lor \neg p)$' consistent in the calculus. See Intuitionism by
    A. Heyting.

4. See J.L. Thompson, 'Third Possibilities', Mind, Vol
    LXXVIII, No. 310, 1969, p. 232.)

5. "What even on the linguistic phenomenalist view simply is
    there contains all the same fantastic coincidences of
    recreation, all the same causal incoherencies as before.

The point is analogous to a criticism of reductive views
of unobserved theoretical entities in science."
(J.L. Mackie, 'What's Really Wrong With Phenomenalism',
Proceedings of British Academy, Volume LV, p. 115.)
We discuss anti-realist treatment of theories in Chapter 3.

CHAPTER 2.   Dispositions.

The philosophical behaviourist, qua anti-realist, aims to reduce statements about 'non-observables' to statements about 'observables'. The general character of the two classes is reckoned to be non-problematical, though it is conceded that particular instances will present taxonomic problems. In general, it is not assumed by anti-realism that non-observables are non-existent: magnetic fields, for instance, are considered to exist even though, it is claimed, we can detect their existence only via indications of their 'effects'. The observational data that serve as criteria are to be considered as manifestations, not as complete characterizations, of the non-observable. However, even with these qualifications, the general claim is dubious, as I hope my discussion of operationalism later in this chapter will show.

Such a claim is not typically made by philosophical behaviourists, though some psychological behaviourists are methodological behaviourists, i.e. they regard the existence or non-existence of inner states as irrelevant for scientific purposes; such behaviourists might claim that all dispositions have categorical bases for all we know, thus preserving the Law of Excluded Middle, but because we have no means of ascertaining whether such a claim is true, we should disregard the claim as scientifically irrelevant. This qualification apart, behaviourists are hostile to any talk of mental states, processes, events, etc.. There are no mental entities, only special kinds of bodily processes. In the case of dispositions,

it is not the case that relevant behaviour is a manifestation
of an occult entity, called a disposition - such a view,
according to Dummett, would commit one to a 'blind' belief
in a spiritual or physiological mechanism - the dispositional
term refers to a type-of-behaviour producing tendency, which
is not claimed to have a categorical basis. In the case of
Jones, on this view neither the statement 'Jones was brave'
nor the statement 'Jones was not brave' can be claimed to
be true or false. (N.B. Dummett leaves it open as to what
kinds of criteria would justify assertions of Jones' bravery
or cowardice; we cannot conclude from his account that only
Jones' actions in dangerous situations are the only relevant
data here.) This is NOT to claim that the statements are
not true (or false), merely that we cannot SAY that they
are true, or that one of them HAS to be true (and the other one
false). The claim that one or the other is true is
justified only on the grounds that we know of evidence
enabling us to correctly assert one of the statements.

To facilitate our discussion of the anti-realist's
views on dispositions, and in particular, Dummett's position
on bravery, I will here provide a brief analysis of bravery.
First of all, let us distinguish the expressions, a 'brave
action', 'acting bravely', 'being brave' and 'bravery'. The
latter is clearly dispositional, and 'being brave' refers
to the possession by a person of that disposition. In brief,
we can say that bravery is a state of a person who has the
tendency to act bravely. This is not to beg the question as

to the nature of the state - whether it be a brain-state, etc. -
nor do I want to dogmatically claim here that the state is
any kind of categorical basis. My use of the word 'state' is
meant to be compatible with any account of dispositions,
including a behaviourist one. There is a difference between
saying that a person acted bravely and saying that he is brave.
The latter is a dispositional statement, the former is not.
A cowardly person can act courageously while remaining
cowardly; it is self-contradictory to say of a person both
that he is cowardly and that he is brave (with the usual
exception, viz., where the utterer is not sure which to say
because of the complexity of the evidence). A brave action
is one which a brave man would do in the same circumstances,
though it does not require that the actor act courageously;
a courageous action, we can say, is one which, considered
apart from the motivations, beliefs and attitudes of the
actor, was such that a courageous man, suitably motivated
and with suitable beliefs and attitudes, would perform,
ceteris paribus. The ceteris paribus clause is required
because even with suitable beliefs, etc., a brave man may
fail to act bravely because of illness, etc..

For acting bravely, one of the necessary conditions
seems to be the presence of danger. This way of expressing
the matter makes the notion of bravery an 'actualist'oone,
viz., by requiring that a person can act bravely only in
actually dangerous situations. A non-actualist analysis would
claim merely that the agent be in a 'phenomenally' dangerous

situation, i.e. a situation where the agent believes, rightly or wrongly, that danger is present. The other necessary condition, on both actualist and non-actualist analyses, involves the belief – on the actualist thesis, always a true one – that the situation is dangerous to the actor. On either analysis, fear is not a necessary condition: danger is normally feared, but the two concepts are not necessarily connected. There seems no reason why fear should be thought an ingredient in bravery other than its alleged connection with danger. The picture often sketched of a brave man is that of one who 'rises above his fear of harm to himself' to do the courageous thing. But harm can be defined independently of fear, whether that is analysed in terms of feelings or dispositions. Danger is not defined as what people fear (though perhaps people ought to fear danger). The corny cases of courage – heroic activity in battle, for example – lend plausibility to the inclusion of fear in the analysis, but what of a person who is courageous in human relationships? What is he necessarily frightened of? All that is required in this respect is that harm could ensue, or, alternatively, that consequent harm was believed fairly likely. Aristotle went astray on this point when he stated that a courageous man is he who endures and fears the right things, for the right motive, in the right manner, and at the right time.[1] We would probably have to rule out most cases of alleged bravery if fear were a necessary ingredient: the crux is the epistemic attitude of the actor.

Does he believe or know that the situation is dangerous? Again, Aristotle's requirement that the actor act for the right motive is too stringent a requirement. What follows then if someone acts selfishly in saving a child from cremation? What IS further required is that the benefit to be gained from putting oneself in danger be sufficient to outweigh an acceptable level of harm to oneself and others. Otherwise, one is being reckless.

Thus, on an actualist account, a brave man is one who, in dangerous situations, believing (truly) that the situations are dangerous, tends (on a behaviourist analysis of dispositions, this disposition gets cashed out in favour of occurrences) to act bravely (presumably, this latter could be cashed out non-circularly in terms of specifications of ways of acting bravely). On a non-actualist account, a brave man is one who, in putatively dangerous situations, has a tendency to act bravely. The anti-realist and, in particular, the philosophical behaviourist, will wish also to cash out the dispositional term 'tends', thus reducing an 'occult' term to a 'non-occult' one or ones. His way of achieving this is to give an 'if ... then' analysis of dispositions where both the protasis and apodosis refer to observable and/or oc-current phenomena. In the case of inanimate objects, the protasis would typically refer to some experimental situation in which the object is placed, the apodosis to the expected result of such an experiment, given that the object possesses the disposition. For animate objects, the protasis would

consist of a description of a situation, the apodosis a description of behaviour expected of the creature in that situation if he possesses the disposition. In the case of Jones, Dummett analyses 'Jones was brave' as 'If Jones had encountered danger, he would have acted bravely' and 'Jones was not brave' as 'If Jones had encountered danger, he would not have acted bravely'. Such an analysis is unsatisfactory on several counts.

Firstly, dispositional statements are about animate or inanimate objects whereas 'if...then' statements refer to objects _in situations_. Then how can they mean the same? The anti-realist's answer, of course, is that dispositional statements _are_ about objects in situations, for a) it is in such situations that we learn how to use dispositional statements. For the anti-realist, this entails that this constitutes their meaning. b) Dispositional statements are about _tendencies_ to do certain things in certain situations. Therefore, reference to such situations in the analysis is necessary. For the realist, only the first objection needs countering - the second one being one with which he is in complete accord. Even if dispositions are identified with physiological states of the organisms who possess them or with a particular micro-structure of the object, it is still a part of a correct analysis of them to give some indication of the ways in which they manifest themselves. For it is part of the meaning of 'disposition' that it is a tendency to do certain things in certain situations. If the anti-

realist counter-objects that the realist, with his identification
of meaning with truth-conditions, cannot admit this, on the
grounds that the meaning of a dispositional statement cannot
be given both in terms of the possession by something of a
physiological state or a particular micro-structure and in
terms of the tendency to do certain things in certain
situations, the realist can reply that possession of the
state or micro-structure and possession of the tendency to
act in certain ways is the same thing. In Chapter 5 I hope
to justify adequately the assertion here that dispositional
statements require categorical bases as part of their
meaning. For the realist, it is necessary that reference to
situations is a part of the specification of the meaning
of dispositional statements in the same way it is necessary
for the anti-realist. For the latter, it is not only necessary,
but sufficient. As for objection a), I shall discuss the
question of sense-possession in the last chapter. For now,
we can conclude that the objection, though over-stated
and thus misleading - for both sides accept situation-
relevance - points to a crucial difference in approach, viz.,
the different position each side accords the relevance of the
situation in an analysis.

The second unsatisfactory thing about Dummett's analysis
is that there is a difference between 'acting bravely' and
'being brave'. I have mentioned this in the brief analysis
of bravery, but, at risk of belabouring the matter, I shall
continue. It is surely self-contradictory to say of a person

both that he is brave and that he is not brave. But being
cowardly (or not being brave) is not incompatible with
acting bravely, on one or more occasions. For, whatever
analysis is given of being brave, whether it be an actualist
or a non-actualist one, a combination of these or neither,
it is surely possible for a person who has a tendency to act
in a cowardly fashion when faced with danger, nevertheless
to act bravely in one such situation or situations. The
requirements of acting bravely are specified in terms of
epistemic attitudes and perhaps objective circumstances at
the time of acting (Aristotle's and Brandt's requirements of
correct motivations seem superfluous[2]), whereas the conditions
specified for the application of a disposition-term to a
person require that he have a _tendency_ to have such attitudes
(again perhaps in objective circumstances). Such considerations
lead straightway to a rejection of Dummett's analysis of
bravery. For, just as a cowardly person can act bravely,
so a brave person can act in a cowardly way. One is tempted
to add a rider here to the effect that the occasions of a
brave person acting in cowardly ways should not be too
numerous in relation to the number of occasions of acting
bravely, but this would be incorrect. All that we can say about
such an odd situation is that we would be unlikely to SAY that
the man was brave: behavioural evidence concerning a person's
actions in dangerous situations is, after all, probably our
main guide at present as to a person's being brave or not.
While claiming this, I recognise the oddness of a logical

conclusion of my claim – that it is possible that brave
people should act in a cowardly fashion more often than not
and vice-versa. This state of affairs would throw our
assessments of people into confusion, but is unlikely to
be evidenced. Nevertheless it is possible. The remoteness
of its possibility lies in the fact that bravery is a
disposition to act bravely in dangerous situations, ceteris
paribus. The possibility of confusion is rooted in the
possibility of things consistently not being equal. The
brave man could always be 'thwarted' by other factors and
thus always act in a cowardly way. Dummett's analysis seems
to require that a brave man should, whenever tested, do the
brave thing. But acting bravely in all (putatively or objectively)
dangerous situations faced is not a necessary condition of
being brave.

It may be objected here that Dummett does not mean by
'acting 'bravely' what I mean by it; Dummett may want to leave
out intentional factors altogether by requiring only that a
man who acts bravely should do that thing or things which are
dangerous to the actor but which, if successful, would have
sufficiently beneficial results to justify the performance
of them. On my analysis of 'brave action' – an action such
that a brave man might do, but one that does not require the
actor to have any specific epistemic attitudes, etc. – Dummett's
analysis of 'Jones was brave' could re-phrased as 'If $J_o$nes
had faced danger, he would have done a brave thing or brave
things'. If such an interpretation is correct, then Dummett's

analysis is even more unsatisfactory, for it is not true
that a man who happens to do things which a brave man would
do is necessarily brave. He might not be aware of the danger;
the element of intentionality is necessary.

However, this counter-objection does suggest a difficulty
in my account of 'acting bravely'. If I can act bravely
without being brave, is it not possible that I could always
act bravely while still not being brave? An unlikely case,
perhaps, but a possible one. This is a problem for realism
because, on that position, granted that behaviour is the
only evidence we have of the presence of a disposition,
we could not tell the difference, in some such cases, between
a man who was brave and a man who was not (I say in 'some
such cases' because, although behaviour may be the only
evidence we have of the presence of a disposition, such
behaviour need not be of the type which could be construed
as a manifestation of such a disposition. The presence of
generosity, concernedness and general good-will might be
considered as evidence here. In other words, relevant
evidence is not exhausted by behaviour in dangerous situations.)
One way out of this difficulty would be to drop altogether
the requirement of intentionality. But such a step is
unacceptable, for reasons I hope have been made clear already.
Perhaps my account of 'acting bravely' is incorrect and a
non-intentional account more plausible. Such a suggestion
by making attributions of dispositions a matter of ascertaining
whether or not a person has a tendency to do brave things,

regardless of his attitudes, would dissolve the problem.
But a) it would render the realist's position less plausible
by making the discovery of categorical bases for dispositions
well-nigh impossible; for a multitude of such states
would presumably be required to serve as bases for all acts
of bravery. Indeed, there would be little point in
discovering such bases - their predictive value would be
practically nil. b) It would make irrelevant the presence
or absence of intentionality.

Therefore, I think the realist will have to live with
the difficulty - which is not a cconceptual one anyway[3] -
and trust that, with the advance of scientific knowledge, it
will arise less frequently. Symmetrically, it is also
possible for a brave man never to act bravely, even if
faced with (putatively) dangerous situations.

The third objection to be made against Dummett's analysis
is that not only is acting bravely in all dangerous situations
one is faced with not a necessary condition of being brave,
it is not a sufficient condition. We have already argued
for this in the previous objection. For there the realist
had to admit that it was possible for a person to act bravely
in all the dangerous situations he was faced with, yet not be
brave. The fundamental problem facing any 'if...then' analysis
of dispositional statements is that concerning the
specification of necessary and sufficient conditions for
the attribution of the term. It is fundamental because the
reductionist holds that if the reduction of tendencies to

occurrences and/or observables (or statements about tendencies
to statements about occurrences and/or observables) is not
effected or possible, we are left with the occult, and
therefore cannot be sure we are not talking nonsense. General
objections to reductionist attempts are familiar, e.g. a) the
distinction between sensation and perception has little use -
'neutral' observation is a myth. Conceptualisation enters the
process of perception at a much more fundamental level than
the empiricists claim. b) There is great difficulty in
deciding just when something is observable and when it is
not. c) The possibility of reducing non-observational
statements to observational ones seems remote: theoretical
terms seem incapable of such reduction. Such difficulties have
been recognised by empiricists and they have adjusted their
analyses accordingly. In the case of dispositions, reductionist
difficulties are particularly acute because of the 'openess'
of dispositional concepts,[4] i.e. no complete specification can
be given in terms of test-conditions and results (or, more
widely, observable conditions generally) of the correct
application of dispositional terms. The way is always left
open for further specifications to be furnished, as novel
situations, entailing the application of the dispositional
term, present themselves. Such a way of explaining the
openess of dispositional terms suggests a scientific imprint,
and it is indeed to positivist philosophers of science,
notably Carnap, that we must look to for attempts to solve
the problem.

Carnap, in 'Testability and Meaning', recognises the
problem of defining theoretical and dispositional terms (the
distinction usually drawn between these two lays emphasis on
the claim that a theoretical term refers to an occult entity,
whereas a dispositional term does not) by means of observation
sentences. Later in 1956, when positivist dislike of the
occult was somewhat muffled, he distinguished the two kinds
of terms thus: a term is 'theoretical' if the falsity of
the statement describing the expected result of a test - such
terms forming the observational basis of the correct application
of the term - to obtain does not entail that the theoretical
term is not applicable. A dispositional term is one
applicable where such entailment does hold. In 'Testability
and Meaning', however, he thought 'theoretical' terms were
dispositional. Ideally, on this view, a definition of a
dispositional term would consist of a specification of all
the recognisable conditions under which the term would be
applicable. Within the scientific sphere, such a definition
would consist of a description of test conditions, certain
results of which would constitute sufficient and necessary
conditions for the applicability of the term defined. However,
such a venture is doomed to failure, for there is the problem
of the truth-conditions of truth-functional conditionals.
The demands of positivism made it inevitable that reductions
should be made in an extensional language; thus, an
operationalist definition would take the form:

Def. $Qx \equiv (Cx \supset Ex)$ (Q = Dispositional property)

(C = Test Conditions)

(E = Expected Effect)

The truth-condtions for such conditionals entail that any object which is not subjected to conditions C, possesses the property P. Such a conclusion is clearly undesirable.

As a result of this, Carnap explored the possibilities of providing partial definitions by means of 'reduction' sentences. Such sentences were part of an extensional language and attempted to provide a partial definition of a term by means of describing test-conditions and effect; their form guaranteed non-vacuous applicability of the definienda. In short, they constituted positive tests of the presence of the property defined. Failure of test-conditions to obtain entailed truth of the whole conditional, but left undecided the question of the applicability or not of the term defined. We can see this by an example. If we are trying to partially define property Q, we try to provide a test for the presence of Q:

$P \supset (R \supset Q)$ (P = Test)

(R = Expected result).

Moreover, failure of the expected result when the test-conditions obtain does not entail that the object does not have the property[5] unless the tests for the presence and the absence of the property are, respectively, the affirmative and negative results of the same test. Suppose the test for the presence of the property (for convenience, we assume there is only one test) is

$P \supset (R \supset Q)$ (P, Q and R defined as above)

and the test for the absence of the property is

$P \supset (R \supset -Q)$

then the two sentences can be reformulated as a 'bilateral reduction' sentence:

$P \supset (Q \equiv R)$.

For any one concept, there will be as many reduction sentences as there are tests for the presence or absence of the property. Thus, we see that this characterisation of dispositional terms is a weaker form of operationalism than that which would assign a different concept for each test (Bridgman).

For convenience, let us assume that all such definitions are given in the form of bilateral reduction sentences. This will enable us to discuss the possibility of giving sufficient and necessary conditions, within the context of a test, for the applicability of the terms defined. It is my contention that the openess of dispositional concepts precludes the possibility of providing observational conditions, sufficient and necessary for the predication of disposition terms. In some cases, sufficient conditions can be provided, in others necessary conditions, in yet others neither. But in no case can both types of conditions be provided.

Let us take the contrary disposition terms 'visible' and 'invisible' (I acknowledge the absurdity of placing an invisible object in test conditions but beg indulgence: nothing hangs on it) and try to operationally define them by means of reduction sentences; A sufficient condition for correctly predicating 'visible' of an object is easily found –

that somebody see the object. So we have true reduction
sentence:

$$P \supset (R \supset Q)$$

(P = Test Conditions)

(R = Somebody sees the object)

(Q = Possession by object of proprty

'visible')

But what of a necessary condition? If an object is put under
test-conditions, say P, and nobody sees it, it does not
necessarily follows that the object is invisible. For there
could be many reasons why the object was not seen. Some
of these reasons can be specified in reduction sentences for
non-visible objects, or their negations made part of the
test-conditions P. But such a move will not exhaust all
possible such reasons, for there are probably an infinity
of possible reasons. The ploy of adding a ceteris paribus
is only a fake way of providing necessary conditions; for,
at its worst extreme, it makes trivial the assignment of
necessary conditions by ruling out all possible counter-
examples. Moreover, it is clearly an un-operationalistic
device, for it precisely does not spell out the operations
involved in testing the presence of a property. Therefore,
it cannot be used in reduction sentences. The term 'visible',
we have seen, can be given sufficient but not necessary
conditions.

Now, what of 'invisible'? It would seem that if 'visible'
can be given sufficient conditions, but not necessary conditions,
then 'invisible' could be assigned necessary, but not sufficient

conditions. This is, indeed, so. If an object is said to
be invisible, it follows that nobody should see it. However,
from the fact that nobody sees an object, it does not follow
that it is invisible. And what other possible candidates
for sufficient conditions are there?

In the cases of large numbers of dispositional terms,
applicable to inanimate objects (but not necessarily
exclusively so, e.g. visible men), either sufficient
observational conditions or necessary observational conditions
can be given for their application, but not both. For some
dispositional terms, however, neither type of condition can
be assigned, e.g. 'magnetizable.' The most plausible sufficient
condition for this term would be some description specifying
the movement of the object in question towards a magnet. But
such a movement might be due to the slope of the surface on
which the object is placed. Of course, such an eventuality
could be ruled out by the test-conditions, but a) it is not
clear that in every case, though every effort is made, such
a stipulation will be adhered to. This, perhaps, is not
important as it does not affect the philosophical question
of provision of conditions, but it may well impugn the
practical possibility of using the definitions. This, given
the fundamental purpose of providing operationalistc definitions
- the provision of working definitions of concepts which can
be used by scientists - may be justifiably thought to be a
cogent argument against the whole enterprise. b) The possibility
of other features of the situation causing the movement of
the object is high. Provision of necessary conditions is no

less problematic. 'If a magnet, of the relevant strength, is placed within a (specified) distance of the object, the object can be said to be magnetizable if it is attracted to the magnet'. Such a description seems to give the most plausible necessary condition of 'magnetizable'. But suppose in such a situation described, an object does not move. Are we to conclude that the object is not magnetizable? Surely not, for there may be other factors present which prevent movement, e.g. the presence of another magnet, the object's adhesion to the surface on which it stands, etc.. A multitude of such conditions could obtain. There is no reason to suppose that we could fully specify such conditions;[6] even if we could, objection a) above would probably apply. Moreover, even though we had specified all such conditions, how would we know we had included them all?

Thus, in the case of dispositional terms applicable to inanimate objects, we see that we cannot provide both sufficient and necessary observational conditions for their application. Such considerations seem even more cogent in the cases of dispositional terms applicable to animate objects, or, rather, those terms exclusively applicable to animate objects, e.g., brave. For those terms predicable of both kinds of objects, e.g. 'visible', the considerations adduced above seem to apply, just as forcefully in both cases. I have already pointed out the implausibility in Dummett's account of saying that a brave man is one who, when faced with danger, acts bravely. For acting bravely in all dangerous situations is not necessary for being brave. Therefore, to demand of a brave man that he

always come up to scratch, is incorrect. Again, as stated
earlier, neither is acting bravely in (putatively) dangerous
situations a sufficient condition of being brave. For, on
any account of 'acting bravely', it is possible for a man
to act bravely on numerous occasions without having a
tendency so to act. The counter-objection that no sufficient
condition is being given in the analysis of 'Jones was brave'
as 'If Jones had faced danger, he would have acted bravely',
only a necessary condition, misfires. Firstly, no distinction
would then be drawn between being brave and not being brave
but sometimes acting bravely. It is surely a requirement of
a behaviouristic analsyis that, if there be such a distinction,
it should be explained. If no such distinction is valid,
then a sufficient condition has been given. Given that
Dummett's analysis nowhere tries to make such a distinction,
we can only assume that for him no such distinction is to be
made. Secondly, we are given no further criteria for
recognising bravery. We can surely expect sufficient
conditions to be provided, given the raison d'etre of
behaviouristic analyses. Thirdly, it seems clear from
Dummett's account that 'Jones was brave' means 'If Jones had
faced danger, he would have acted bravely'. At the very
least, meaning relationships can be put in the form of logical
equivalences, which give sufficient and necessary conditions -
this gives extensional identity of meaning; this will be a
fortiori true of intensional identity of meaning.

It is clear, therefore, that the openness of dispositional terms sabotages any attempt to provide sufficient and necessary observational conditions for their predication. Reduction sentences are an attempt to provide, in the context of test conditions, such sufficient and necessary conditions. But we have seen that such sentences are of little use - in some cases they do provide sufficient conditions, in others necessary conditions, but in no case do they provide both, and, in many cases, neither kind can be specified. Moreover, it is doubtful that the attempt to give a partial definition of terms is compatible with the operationalist criterion of meaning. On that criterion, a term has as much cognitive meaning as has been assigned to it by means of sentences describing the operations involved in testing or for the presence or absence of the property denoted by the term. The use of reduction sentences implies that the term in question has more meaning than is supplied by such sentences. But this is inconsistent. Unless, therefore, it is made clear what other meaning is possessed by such a term, either by completing the list of reduction sentences specifying the meaning of the term, or by some other means, we shall have to conclude that the whole meaning is given by the reduction sentences at hand. This is why I claimed earlier that although failure of the expected result when the test-condition does not entail that the object does not have the property, it would entail it in the absence of other reduction sentences or other operationalistic methods of specifying meaning.

According to the operationalistic criterion of meaning, if
we cannot provide operationalist definitions (or perhaps non-
definitional specification of meaning) of a term, then it has
no meaning. In this case, a term has as much meaning as has
been assigned to it by operationalistic definitions.

Of course, if it is the intention of the formulator
of reduction sentences to _introduce_ a term into the language,
he would feel comfortable with such a conclusion. But if he
is aiming to define the present use of such a term, the
conclusion has to be refuted. He might reply that at the
present state of competence of operationalists, it is not
possible to give full definitions of terms, by giving
a full list of reduction sentences (let us assume that
provision of such sentences is the sole method of specifying
meaning known to the operationalists). Obviously, allowance
has to be made here for operationalists to be given time to
formulate their definitions. But it is false, as we have
seen, that, for all cases, sufficient conditions CAN be
given for the predication of the terms,.e.g., 'invisible',
'brave'. If this is so, then how can the operationalist
insist that the 'residue of meaning' is not cognitive
nonsense? The provision of reduction sentences, on the
operationalist criterion of meaning, is the only guarantee
that a term has cognitive meaning. Therefore, as long as
such definitions cannot be given, we can only conclude that
a particular term has only as much meaning (if any) as such
'partial' definitions give it (if any). For what reason has

an operationalist to conclude that a term has more meaning
than such sentences give it? If the operationalist assumes
(correctly) that terms have more meaning than can be
provided by specifications in the form of reduction sentences,
then either he will have to insist that satisfactory reduction
sentences could be provided, if we only knew how, or abandon
the operationalist criterion as the _sole_ criterion. To choose
the latter would make his work largely uninteresting – for
we can all accept some reduction sentence as explanatory in
some sense – but to choose the former is to beg the very
question at issue – whether or not operationalistic provision
of meaning is adequate. If, on the other hand, he insists
that terms do not have any more meaning than it provided
by actual reduction sentences, then he will have to reject
the whole notion of the 'openness' of dispositional terms.
And if his work, is descriptive of language use, and not
a reformatory project, we can claim justifiably that his
criterion is incorrect. I see no way out of these dilemmas
for the operationalist unless he claims he is attempting
a reform of the language in the interests of scientific
clarity, etc.. And that is an interesting and perhaps useful
project, but leaves the field of actual language meaning
to the anti-operationalist. This criticism turns out to
be a specific example of a more general criticism of anti-
realism – its inability to makes sense of theories – which
is developed in the next chapter.

Similarly, unless the behaviourist can provide sufficient and necessary observational conditions for the predication of a term, it cannot be said that behaviouristic specifications of meaning even approach a solution to the problem of behaviouristically defining psychological terms. As we have seen, such a problem seems incapable of solution in the case of dispositional terms. Dummett's attempt to provide behaviouristic definitions of such terms, therefore, is doomed to failure, even if we overlook more minor blemishes in his account. The openness of dispositional terms guarantees that no sufficient and necessary behavioural conditions can be discovered for the use of such terms. Fundamentallly, both Carnap's reduction sentences and Dummett's suggestion of counterfactuals fail because of this feature of dispositional terms. Any attempt by Dummett to salvage an 'if...then' analysis by reformulating the counterfactuals thus: 'Jones was brave' can be analysed as 'Jones has a tendency to act bravely in dangerous situations', which, in counterfactual form, would be 'If Jones faced danger, he would have had a tendency to act bravely' (if this makes sense) merely re-imports the 'occult'.

Indeed, although Dummett's formulation can be improved by taking note of the above objections — we could, for instance, introduce intentional factors into his analysis — any attempt at a complete characterization of dispositional terms by using 'if...then' sentences will fail. Reduction sentences and counterfactuals, expressible in 'if...then' terms,

have been seen to be inadequate; what of alternative 'if...
then' accounts? The only likely one is a formulation in terms
of causal conditionals (with or without the impication that
such conditionals are licensed by causal laws). This question
is to be clearly distinguished from that concerning the
causal or non-causal nature of dispositions. A solution
to the latter problem essentially provides an answer to the
question - Do dispositions have a categorical basis? If the
answer is Yes, then dispositions are said to cause their
behavioural manifestations. This question is pursued in a
later chapter.

A formulation of an 'if...then' analysis in terms of
causal conditionals says nothing of the ontological status of
dispositions, it merely correlates two (types of) events or
states, both of which are observables. It is compatible with
a behaviouristic analysis of disposition terms. For example,
where C = Test conditions, R = Response, 'If C, then with
causal necessity R' is an operationalistic analysis (IF we
assume that a non-extensional language is acceptable to
operationalists). Typically, the operationalistic analysis
will include reference to laws; a behaviouristic analysis need
not do so (we assume here that causality does not essentially
involve laws), e.g. to attribute a belief-disposition to
someone is to say that under certain conditions, specified
in terms of the environment of the agent, that person will
behave, verbally or otherwise, in certain ways. The
introduction of a law here would lead to the provision of

a _ceteris paribus_ clause - a move rendering practically
useless the introduction of the law. I think it true to say
that a law can always be formulated, given one case, provided
that enough conditions are specified, or, more usually, a
ceteris paribus clause inserted, as a 'cover-all' condition.
Thus, all counter-examples are excluded. Such vacuous laws
will typically apply, in actual fact, only to one case.
But such or other laws do not need to be presupposed in
assessing the truth of a dispositional statement.

It is my contention that if laws are held to be the
support of individual dispositional statements, a categorical
basis is needed to explain this fact. If laws are not obviously required,
as in the case of many dispositions attributable only to animate
objects, then the considerations adduced above on the inadequacy
of an 'if...then' analysis render necessary a different
analysis of the dispositional statements. It is my hope
that the account of dispositions given in Chapter 5 will
provide such an analysis.

In conclusion, let us summarise what has been established
so far. We have seen Dummett's account of bravery as a
particular case of anti-realism, and have put the realism/anti-
realism controversy, somewhat sketchily I fear, into
historical perspective. Some objections to anti-realism have
been raised: the misguided leap from belief in its plausibility
in mathematics, to a belief in its plausibility elsewhere;
that it leads to solipsism; its misapprehension for what is
it is for language to have sense; the difficulties for

operationalist and behaviourist, analyses of dispositions, such
a view of sense-fixing provides; most striking of all, its
non-acceptance of the Law of Excluded Middle. We turn to
this in Chapter 4.

Footnotes to Chapter 2.

1. See Aristotle, Nicomachean Ethics, 1115b6-1116a15.

2. R. Brandt, 'Traits of Character: A Conceptual Analysis',
American Philosophical Quarterly, Volume 7, No. 1, 1970.

3. I am not sure about this. Animate dispositions are
tendencies to do certain things in certain situations. If, say,
somebody does these things in these situations more often
than not and did not possess the disposition, and those who
possessed the disposition more often than not did not perform
the characteristic actions, would we have a coherent account
of what it is to attribute a disposition to somebody? Perhaps
it is a constraint on the realist position that most people
should act in characteristic ways most of the time, which is
why reference to situations is required even on a realist
analysis.

4. In this section, I am much indebted to a D.Phil. thesis
at Oxford - 'Dispositions' - by D.C. d'Alessio.

5. It does not entail this, but in the absence of other
reduction sentences or other operationalistic methods of
specifying meaning, it would, on operationalistic criteria of
meaning, mean this.

6. Shope has cogent arguments on this point concerning 'magnetic'.
R.K. Shope, 'Dispositional Treatment of Psychoanalytic Motivation
Terms', Journal of Philosophy, Vol. LXVII, No. 7, 1970.

CHAPTER 3.   Anti-realism on theories

Many objections to anti-realism became apparent in
Chapter 2, but perhaps the chief one - its inability to give
a satisfactory explanation of the role of theories - was only
suggested in criticism of operationalistic definitions of
dispositional terms. There we saw that not only did
operationalists have to abandon attempts to fully define
some kinds of terms, in this case dispositional ones, but
that even attempts to give sufficient and necessary
conditions in test-conditions (or, more generally, in
specified circumstances) were open to serious objection.
The openness of dispositional terms guarantees that no
solely observational account of their meaning can be given.
I shall argue in Chapter 5 that such openness demands a
categorical basis. But for the moment, let us inquire
again into operationalism with a view to exposing anti-
realist misconceptions about theories.[1] Some repetition
is inevitable, but I shall keep this to a minimum; and
what there is will be part of an approach with a different
purpose, viz., the study of operationalist conceptions of
theoretical and dispositional terms in general.

Carnap in 'Testability and Meaning', seems to divide all
scientific terms into two mutually exclusive and exhaustive
classes - observational and dispositional. Such a division
is striking if only for the non-appearance of the class of
theoretical terms in it, but these terms, Carnap insisted,

were dispositional. Positivist dislike of the occult
made suspicious the meaningfulness of any term purporting to
refer to an unobservable entity. Therefore, the widely held
view, that some theoretical terms referred to theoretical
entities, called 'theoretical' because they were unobservable
(in some sense of that term), was to be rejected. Including
them in the class of dispositional terms would, according to
the protagonists of this view, meet the problem of specifying
the meaning of theoretical terms (it was a common assumption
that they did have meaning) while preserving metaphysical
purity. Fundamentally, the general problem was to provide
analyses of scientific terms, analyses which would not go
beyond experience or the possibility of experience; the
methods of verification or confirmation of statements in
which theoretical terms appeared must be given in terms of
actual or possible experience. Therefore the meaning of
theoretical terms was to be given in terms of experiential
verification or confirmatory procedures; so any specification
of meaning which implied existence independently of such
procedures was ipso facto incorrect. 'No hypostasization
without verification' might be a sloganized summary of
this approach. To say that an electron is a 'thing' would
be to go against the slogan; the meaning of the term
'electron' is given in terms of the test conditions for
establishing the presence of an electron. The question of
the ontological status is at best irrelevant — those anti-
realists who hold an instrumentalist view of theories would

claim that the reality or number of electrons is not a question which has to be or even can be answered - and at worst, elicits a false answer - Yes, they are real. Anti-realists who think theoretical atatements are true or false will nevertheless wish not to overstep the metaphysically permitted limits of scientific endeavour, such limits being set by positivist philosophers of science, i.e. they will try to reduce talk of putative occult entities such as electrons to talk of the conditions in which scientists themselves talk of electrons, e.g. the presence of certain traces in cloud chambers. Such conditions are of the observational kind, and until if at all, scientists actually observe entities which they call 'electrons' it would be false to talk of electrons as entities.

I accept the charge that I am attacking a crude version of anti-realism: some anti-realists may not deny entity-status to electrons. All they claim is that statements about objects are translatable to or reducible to statements about verificatory procedures. The problem then centres around the relation between the meaning of a concept and statements about that concept. Can the whole meaning be given by means of statements about verificatory procedures?

This stated, it is nevertheless true that most of my arguments against crude anti-realism weigh against more sophisticated version, e.g. Arguments about solipsism, the 'anti-heuristic' tendency of anti-realism, objections to Craig's theorem, etc..

The use of dispositional terms does not seem to present such problems — they are different from observational terms in that their whole meaning is not given entirely by means of observational statements, but the true predication of such a term does not, according to an anti-realist, entail that there is any kind of 'thing' in the owner of the disposition which explains the behaviour characteristic of that disposition, i.e. dispositions need not have a categorical basis. Furthermore, to say,that they do is to go beyond the evidence we can have of the presence of dispositions. The meaning of a dispositional term is specified in terms of the tendency to act in certain ways (in the case of animate objects) or for certain things to happen (in the case of inanimate objects) in specified situations. Such actions and happenings are observable phenomena, and the tendency is not some occult factor which explains such behavioural (in the wide sense) manifestations. Thus, to say that theoretical terms are dispositional, is to claim that the meaning of such terms either is or must be made to become specified in terms of observable test conditions and observable test results. To take Carnap again, the introduction of reduction sentences as partial definitions entails that there is supposed to be more meaning to such terms than is given by such sentences. But, whatever residue of meaning is left over, it does not lie in the fact that dispositional (and therefore theoretical) terms refer to <u>entities</u>.

Criticism of this account of the meaning of theoretical

terms will raise the fundamental question of the role of
theory in scientific endeavour. In chapter 2 I criticised
the view that partial definition of dispositional terms was
acceptable on the operationalist criterion of meaning on
the grounds that on that criterion, there was no guarantee
that there was any more meaning to a term than had been
assigned to it by means of reduction sentences. The
operationalist's reply then was that operationalists were
not competent enough to give full explicit definitions of
dispositional terms; but to accept that as a satisfactory
reply is to assume that the terms we use could be given
more full operationalist definitions, if only we knew how.
But this is to assume just what the operationalist criterion
of meaning was supposed to be a test of, viz., the cognitive
meaning of dispositional (including theoretical) terms. This
assumption on the part of the operationalists not only points
to a discrepancy in their own account, but also to a view of
theory which is incompatible with their own approach. It
seems to me that operationalists want to have their cake and
eat it - they assume the validity of a conception of theories
which is at variance with their own account of the role of
such. It is indeed obvious that there is more meaning to
most dispositional terms than can be assigned to them
by a lengthy list of reduction sentences. The attempt to
provide both necessary and sufficient conditions, even in the
context of test conditions, seems doomed to failure.

It seems then that the operationalist holds two positions:

a) the only guarantee that a term is meaningful is that operationalist accounts of its use can be given. The corollary of this is that it can be strictly claimed only that a term has as much meaning and no more as operationalist definitions give it. b) Terms have more meaning than has actually been assigned to them by operationalist definitions. It does not follow from this, but it is a reasonable suggestion, that what the operationalist should see on his criterion as the role of theory, is different from the role that he has implicitly given to theory. And this is hardly surprising, for the operationalist cannot account for the role of theory; this will now be argued for.

A brief analysis of what theories in science are will be useful here. A theory is a set of statements which together explain something else (let us assume that this something else is put in propositional form): from the statements composing the theory we can deduce or induce some conclusion. Such conclusions will usually be in the form of experimental laws, for it is the generality which is important.[2] Let us further assume that such a conclusion is a statement about the observable world. The role of explanation, whatever else it is, is primarily the assembling of phenomena into some kind of order. What is to count as ordering involves subsumption under laws. Typically, such laws will be of great generality: it is hardly a theory that All oxygen expands when heated, though it is a law; what does count in this field as a theory is the kinetic

theory of gases. Essentially, theories <u>unify</u> the world
into a coherent whole (or unify a signigicant finite part
of the world). Again typically, a theory will involve the
postualtion of some unobservable entity or entities,
referred to by usually <u>ad hoc</u> introduction of theoretical
terms, e.g. Freud introduced the term 'super-ego' to refer to
what he considered an entity, which entity accounted for
certain aspects of the behaviour of human beings. (Though
as D. Lewis, points out,[3] it need not be the case that
theoretical terms name theoretical entities, i.e. entities
which are unobservable or are <u>ad hoc</u> inventions, e.g. 'H_2O'.
Theoretical terms are new or old terms used in a new way
which derive their meaning from the way in which they occur.
<u>Typically</u>, they will refer to unobserved entities.)

It is this latter aspect of theories - in what sense do
they account for their explananda? - which is in dispute. Do
theoretical terms refer to entities? Or are the reducible
to, and thus mere 'bundles' of, observational terms? Anti-
realists, who tend to emphasise description and correlation
rather than explanation, might dispute my interpretation
of the role of super-ego in Freud's theory and contend that
it is an intervening variable, i.e. something which
correlates two kinds of phenomena, and which is in principle
dispensable. It is used solely because it is a quick way
of correlating such phenomena - to work out the direct
correlations of the two kinds of phenomena would allegedly
be tiresome and time-consuming. 'Electrons' would be

considered not as a term referring to occult entities but
as a useful way of talking about various observable phenomena.
Such a position is immediately suspicious for one has to
look hard to find any case of the actual reduction of a
theoretical term to a bundle of observational ones, or the
dispensing of an intervening variable. Theoretical terms
seem to be necessary ingredients of scientific theories in
the form of irreducible expressions. Quite clearly, it was
not the intention of all scientists in presentation of their
theories to encourage the belief that their theories could
be reduced to correlations of observable phenomena. Freud
did think there were such entities as super-egos, etc..
To him, theories had the function of explaining observable
phenomena by means of a few postualted entities, entities
which might not be observable (in some sense), but which
might eventually be discovered. The possibility of later
discovery of unobserved entities raises problems for
the anti-realist. Craig, in 'Auxiliary Expressions'[4] a
paper which affords great comfort for the anti-realist, himself
says that "in the beginning it may have been more natural
to regard the term 'virus' as auxiliary. Nowadays, it may
be more natural to regard it as nonauxiliary". Auxiliary
expressions are those which are said not to refer to entities,
but are useful symbolic devices, e.g. 'electron', or
intervening variables, e.g. 'ego'. Nonauxiliary expressions
are those which do refer to entities. A simpler and more
reasonable way of accounting for the 'change' in the status

of 'virus' might be to point to the fact that viruses were observed first at some point in time and so their status as real entities, always postulated, was now confirmed.

However, such considerations may be said to miss the point, viz., that however scientists conceived of their theories, they should have held anti-realist tenets; i.e. s scientists insofar as they believed in the existence of unobserved (and given the technological state of science at the time unobservable) entities were methodologically unsound. Anti-realism may take this line, but it is at great cost to many of their brethren, for the latter wish to show that in fact theoretical terms are reducible to observational ones, that theories are in fact reducible in principle to 'explanations' including only observational statements (Craig's Theorem). Positivists wished to discredit transcendental metaphysics as meaningless nonsense but they did not wish to make empirical science a dubiously significant enterprise. Most positivists were scientifically oriented and wished to present the practices of scientists as some kind of model for other fields of knowledge. Thus it was imperative for them to cathect a vindication of the cognitive significance of scientific practices, terms, systems, etc..

The realist's insistence (let us assume it is one - the particular example is unimportant) that the discovery of viruses as entities did not change the meaning of any scientific statement in which the term 'virus' occurred is

is anathema to the anti-realist. The latter, with his
identification of meaning with any evidence , truth-
conditions which we dare capable of recognising, correct
assertion-conditions, etc., makes scientific meaning a
function of the actual knowledge of scientists. The meaning
of 'electron', e.g., is given in terms of evidence for the
presence of electrons, e.g. traces in a cloud-chamber;
mechanical models of atomic structures are crucially mis-
leading - the usual model is of different coloured balls
arranged in a geometric pattern, each ball representing
a different particle. We thus acquire the picture of an
atom as a configuration of small particles. The anti-
realist argues that this cannot be right, even with a literal
interpretation of the model, for a) we have no evidence that
electrons are things. v) we have evidence that electrons
could not be one kind of thing (and therefore not a thing
at all) - in some conditions electrons act like particles,
in others like waves. To return to our virus example, the
anti-realist will assert that 'virus' is an 'entity-word'
only when there is sufficient evidence available to support
that assertion, i.e. when viruses have been discovered.
Before that discovery, 'virus' did not refer to a thing, but
to various related phenomena it was convenient to correlate
by a useful verbal device. Such an account is unsatisfactory
on historical grounds. a)I have suggested earlier that
scientists typically think of theoretical terms as referring
to things. b) Why should there be a search for a thing called

'virus' if the theory in which the term 'virus' was used did not presuppose or even assert the entity-status of viruses. c) This point may not actually apply in this case, but is [unclear] generally relevant. Phenomena characterising the meaning of theoretical terms, according to the anti-realist, are often called 'effects'. This entails the existence of a cause or causes; phenomena are things; it is a reasonable assumption that causes are like their effects in categorial status; therefore, the cause or causes of such phenomena will be things also. d) Scientists want to find out _why_ things happen, not just _what_.

These considerations, and the earlier one that no example of a reduction of a theoretical term to observational ones has been provided – we saw what happened to Carnap's attempt, an attempt, indeed, which was castrated by the admission that a full reduction could not be made – lead to the conclusion that the anti-realist conception of theories is mistaken as a conception of the _actual_ role of theory in scientific endeavour. Any employment of anti-realist tenets to purge science of any concepts and statements not in accord with strict anti-realist cognitive requirements would drastically reduce the scope of scientifc practice. However, perhaps our conclusion is premature: even though no reduction of theoretical terms has been carried out, Craig's Theorem shows that, given certain allegedly undemanding conditions, such a reduction is always possible. Such a possibility is shown to be feasible by formal logic alone and

and may be incapable of implementation, given the competence
and time of scientists, but the theoretical possibility
is perhaps all that is required by reductionists. This
Theorem is an important asset for anti-realist conceptions
of theory so I will devote some space to it, starting with a
brief informal synopsis of the Theorem itself. I shall refer
to the article 'Replacement of Auxiliary Expressions'[5] and
not to his earlier one, 'On Axiomatizability within a
System'[6], as being much more explicit and full concerning
the implications of the Theorem for scientific theory.

The Theorem is intended to provide a means whereby one
class of expressions (called 'auxiliary') is replaced by
another class (called 'nonauxiliary').[7] I have earlier stated
what kinds of expressions are to count as auxiliary and what
as nonauxiliary. The first requirement is that these two
classes be defined effectively - what are contentious
cases in scientific practice should be decided one way or
the other. The second is that the theory which is to be
replaced should be systematized into an axiomatic system.
The restrictions on the system replacing this one are
relatively minor. What results is that all sentences in
the original system containing only nonauxiliary terms can
be deduced from the replacement system. In fact, any class of
terms can be eliminated from the axioms of the system provided
only that one wants to derive theories which do not contain
any of those terms from the axioms. In more specific terms,
and with strict relevance to the problem of reduction of non-

observational statements to observational ones, all the
observational deductions from the original axiomatised system
can be deduced from the replacement system. (I shall assume
from now on that this particular application of the Theorem
is our subject.) It is important to note that Craig's method
does not provide means for replacing theoretical terms as
such, but only insofar as they provide the means in the
original system of deriving observational deductions from
that system. As Craig says: 'The "theoretical superstructure"
of S (original system) has been discarded in S' (replacement
system) without changing what is reagrded to be the observable
content. Predicates regarded as theoretical are therefore in
a certain sense dispensable' (My emphasis). Of course, the
observational deductions are all that a typical anti-realist
is interested in, so he will not be over-bothered by the
proviso, but it is neverthelss significant.

Let us inquire into the two requirements of S mentioned
earlier. The first one we remember is that the two classes
– auxiliary and nonauxiliary – be defined effectively, i.e
it must be possible to decide in a finite time what class
any expression belongs to. Craig acknowledges that in
practice in some cases it is difficult to assign expressions
to a class, but he implies that some kind of artificial
assignment might be an answer. Objection to this 'rigidity'
(required by the Theorem) will be given in my comments on
the second requirement, so I will confine myself to the
remark that given the ambiguous status of many scientific

terms with regard to their status as (non)auxiliary
expressions, such 'arbitrary' assignments may fail to do
justice to the sublety of scientific endeavour and may,
indeed, ensure the uselessness of the Theorem for
reductionists. For, in precisely those cases about which
strict reductionists are adamant concerning the necessity
of reduction, Craig's methods may fail to satisfy them
because of the arbitrariness of assignment (this is only
a practical and not a formal objection, but it is one which
the anti-realist should answer.)

The second requirement - that S be systematized by
laying down axioms, rules of inference, etc. - is $\#$
open to more formal objection. For essentially it regards
scientific theories as static 'bodies' subject to change
but nevertheless at any one time embodying a clearly
accepted corpus of statements. But most scientific theories
are in a constant state of flux and most scientists if
asked to state a theory in full could get no further than
a statement of the more deep-rooted (and in the extreme cases
analytic) statements of a particular theory. Perhaps the use
of a single word 'theory' misleads here - one gets the
picture of a 'block' of statements called the 'Theory of X'.
We can perhaps demand of a scientist that he in a somewhat
artificial manner state a particular theory as he sees it at
a particular time, and show that a system S' containing no
theoretical terms is adequate for the deduction of all
observational sentences derivable from system S. If we swallow

objections cited before the misconstrual of theories as
static corpa of statments, this seems an adequate answer. But,
I suggest, only on a formal level. For it is surely never
going to be the case that the dividing line between
theoretical and observational terms is clear cut,[8] and
therefore there is not going to be a natural dichotomy between
those sentences of S which should and those which should
not be included in S'. Arbitrary division may tend only
to extract whatever use the Theorem had for a reductionist.
I suppose one could include only very clearcut cases of
observational axioms in S' and demand only that obvious
observational statements be deduced from them, but that
would be to denude the Theorem of any practical interest.
I suggest, then, that the possibility of systematizing S is
open to cogent objection.

Hempel, in 'The Theoretician's Dilemma'[9], puts it
concisely and effectively:

"The further requirement of an effective character-
ization of the postualtes and the rules of logic for T'
are so liberal that no doubt any scientific theory that has
yet been considered can be formalized in a manner that
satisfies them – as long as the connections between
theoretical and observational expressions can be assumed
to be expressible in the form of definite statements. The
only important case I am aware of in which this condition
would be violated is that of a theory for which no
definite rules of interpretation are specified – say, on
the ground that the criteria of application for theoretical

expressions always have to be left somewhat vague.

(<u>Aspects of Scientific Explanation</u>, p. 212, fn 56.)

(T' is an interpreted theory, i.e. the postulates of the
theory and the interpretation of those postulates in terms
of observational data). Such a requirement is suggested
in Craig's first condition on S - 'The class of applications
of a rule of inference of S is effectively defined'. Hempel
seems to think that such cases are rare, and furthermore that
most of these are within the scope of Craig's Theorem, if
the rules of interpretation themselves make provision
for vagueness of applicability-criteria of theoretical
terms. Probability statements furnish a connection between
Craig's Theorem and the 'vague theories', but it is not
clear that the problem is quite so venial as that. To
re-employ my stock example of the problem of the criteria
of application of dispositional terms (of which theoretical
terms formed a proper sub-class) which was never solved - the
openness of such terms demanded vagueness of application
criteria. Such vagueness cannot be incorporated into
application rules involving the statistical likelihood or
the logical probability of objects possessing or not
possessing a disposition given certain observable conditions,
for although it is true that certain kinds of behaviour
(construed widely to cover inanimate things) in certain
circumstances make it probable that the relevant object does
possess a particular disposition, in general such a probability
cannot be specified more precisely than that 'It is probable

given observable conditions ... that disposition D is
present'. For a) nobody as far as I know has ever worked out
such detailed probabilities and this _suggests_ at the very
least that it cannot be done. b) The openness of dispositional
terms makes such a calculation impossible to effect.

In certain cases, we _can_ specify probabilities, e.g.
the probability that, if an object dissolves in water, it is
water-soluble = 1. But this applies only in cases where
sufficient conditions can be provided for the application
of the dispositional term. Such conditions can be provided
for certain terms applicable to inanimate objects, but,
as suggested earlier, it seems impossible to supply sufficient
conditions in the case of terms predicable only of animate
objects.

If my choice of example seems unduly favourable to my
case, I would answer that even if it is, it is typical of a
very important class of cases. For, by Carnap's later (1956)
criterion of a theoretical term, _viz._, that only a _partial_
interpretation of it can be given in terms of observational
statements and this seems to carry with it the view that a
statement including a theoretical expression does _not_ entail
any observational statement, the dispositions peculiar to
animate objects are referred to by theoretical terms. (cf.
Carnap's definition of a pure disposition term - such
entailments do hold, so failure of the putative possessor of
a disposition to meet the required test entails that it does
not in fact possess the disposition. Dummett's analysis, of

course, suggests that such dispositions are 'pure'
dispositions.) In such cases, as I have stated earlier, not

not

only can we deduce the presence of the disposition from any
number of observational statements, but also, going the
other way, we cannot deduce any observational statement
from the truth of the dispositional statement, nor can we
even deduce the statistical likelihood of the truth of
observational statements (in this case, statements about
behaviour) from the truth of such a statement. A person
can be brave without acting bravely in the dangerous
situations he encounters.

To sum up, I hope to have shown that Craig's two
requirements, however innocuous looking, are really quite
strong conditions, and reductionists are liable to trip
up on one or both of them.

We can also see that the systematization involved in S' is
not so easy. In effect, to ensure that only observational state-
ments are deducible from S', we have to make every observational
statement in S an axiom of S' (I owe this point to Nagel). We
cannot be sure that such axioms will be finite in number (unless
we assume that the number of observational statements in S and
deducible from S is finite in number. I underline because
such statements may be deducible from S only via the use of
auxiliary terms - we therefore have to include them in the axioms
of S'). Therefore, we cannot be sure that we can arrive at S';
furthermore even if the number of axioms is finite, it might be
such a large number that S' would be useless for practical purposes -

science would not be able to use it as a means for
drawing conclusions. Therefore the use of Craig's Theorem
would be confined to showing that it was possible to replace
S by S', replace a system including theoretical expressions
by one not using such. Science would still need to use S
to further scientific knowledge. Ah, say the anti-realists,
but all we need to do is show how theoretical terms can be
eliminated, and this is what Craig's Theorem shows. Never
mind the practical impossibility of dispensing with
theoretical expressions.

I think this is an overstatement to say the least.
First of all, Craig himself acknowledges that his Theorem does
not provide a way of eliminating theoretical expressions as
such - "replacement of individual expressions does not
always seem possible and that replacement of a system as
a whole seems sometimes the best we can do".[10] The reason
for this is that it is well-nigh impossible to find exact
replacements for the auxiliary expressions -aall we can do
is take S as a whole and show that all observational
statements deducible from S can be deduced from S'. Secondly,
assuming that the problem of the number of axioms is solved,
i.e. assuming that they can be shown to be finite in
number, and assuming that the difficulties I raised in
relation to the applicability criteria of theoretical terms
have been solved satisfactorily, the Theorem shows that S
can be replaced by S' only insofar as S is seen as a static
corpus of knowledge. There are two parts to this objection,
the first I owe to Nagel.

a) S' is essentially only a replacement system; it presupposes the existence of another system embodying the corpus of knowledge acquired. The existence of S' is thus parasitic on the existence of that other system. We could not construct a completely new theoretical system of type S'. This suggests that in some sense theoretical terms are necessary for science to make any progress. b) As a kind of corollary to a) S' does not suggest new avenues of approach for science. In general, to have a list of observation statements with theorems drawn therefrom is not a heuristic device; S' is not a blueprint for further investigation. We now see the further implications of the fact that the Theorem provides no way of replacing individual auxiliary expressions, but merely enables us to derive the same observation statements from S' as from S. For, as Carnap acknowledges in his characterisation of the Correspondence-rules for theoretical statements as <u>partial</u> interpretative statements, the use of theoretical expressions suggests possible discovery avenues for scientists. The realists' use of unobservable or unobserved entities as explanatory of observable phenomena is calculated to further science by indicating what has to be 'discovered next'. The anti-realist rejection of explanation in favour of correlation of observables suggests no such avenues.

This objection also brings out further the importance of Craig's own admission that his Theorem does not aim at or succeed in clarifying the meaning of auxiliary expressions, but rather at providing a replacement procedure. To this the

anti-realist may reply that though meaning-clarification
was not the purpose of the formulation of the Theorem, it
is in fact one of its outcomes. Thus, the replacement of
theoretical statements by observational ones is a way of
showing under what observable conditions we can justifiably
assert a theoretical expression. But this is incorrect,
for as well as all the problems associated with specifying
the meaning of 'open' expressions in terms of observables,
(and I hope I have given cogent reasons for regarding theoret-
ical expressions as open in the sense that they suggest
avenues of scientific approach, and therefore their meaning
cannot be explicated in terms of existent observable
deductions, a fortiori for dispositional terms), there is
always a residue. We have the fact that it is not individual
expressions in S which are replaced in S', but the whole
system S. Even if the Theorem were not subject to objections
already cited, it would still be the case that it is the
system S as a whole which is replaced. Moreover, even if
individual expressions were replaced by other expressions,
it would not follow that a translation had been effected.
Expressions in S and S', given the truth of the protasis,
would have similar meanings in that deductions from them
were identical, but expressions in S would have 'more meaning'
in this sense, because of their openness.

It is my conclusion, therefore, that Craig's Theorem
provides no real aid and comfort for the anti-realist view
of theory within science. As a way of finishing off this
section, let us briefly state again the role of theory within

science. A theory is an explanatory device whose purpose is to explain, by means of postulated entities, observable phenomena; it does this by presenting postulates from which can be deduced experimental laws,[11] those laws whose confirmation lies in the results of experiments. By doing this job, it also explains the behaviour of individual things. Before viruses were discovered 'virus' was a term used in theories to refer to an entity or entities whose effects were the symptoms and criteria of the many diseases. Actually, this may be historically false, but it is a reasonable conjecture that the reason viruses were discovered was that people looked for them, and they did that because the theory or theories in which the term 'virus' was used presupposed that the term referred to an entity as a or the cause of the various symptoms and criteria of the disease. To suggest, as Craig does, that the term 'virus' changed its meaning sometime after the discovery of the referents of the term is surely to be guilty of an unreasonable clinging to a particular theory of meaning. In passing, it might be said that the anti-realists show a general refusal to countenance speculation. This is odd when applied to science, the paradigm discipline of those who hold anti-metaphysical views.

We have seen the role of theory in science, what of its role outside? I feel little need be said on this, for if the anti-realist identification of theoretical entities with bundles of observables is wrong within science, a fortiori

it will be false outside science. For scientists after all are empirically minded and they are professionally and perhaps temperamentally committed to anchoring all their work on observed accepted facts. Laymen have no such methodological predilection for the observable and are probably much less suspicious of the occult. Of course, such ways of thinking may need to be purged, but as a description it seems roughly correct.

Footnotes to Chapter 3.

1. My repeated choice of operationalism as a stock example
of anti-realism is justified, I think, on two grounds.
a) It is a relatively straightforward version of anti-realism
and thus manifests most clearly perhaps typical anti-realist
traits, while at the same time not being a straw man. b) In
operationalist literature, the problem of dispositional
terms has loomed large.

2. Actually, this is dubious. It may be argued that theories
explain individual events, which are deducible from them,
and show how experimental laws are vindicated. The latter
are not deducible from theoretical statements.

3. D. Lewis, 'How to Define Theoretical Terms', Journal of
Philosophy, Vol. LXVII, No. 13, 1970.

4. W. Craig, 'Replacement of Auxiliary Expressions',
Philosophical Review, Vol. LXXV, 1956.

5. See footnote 4.

6. W. Craig, 'On Axiomatizability within a System', Journal
of Symbolic Logic, Vol. XVIII, 1953.

7. It is not a translatability doctrine, as Feigl thinks -
The "Mental" and the "Physical": The Essay and a Postscript,
University of Minnesota Press, 1967. As Craig himself says:
    '... the empiricist aim is rather to clarify somehow the
    meaning of such (auxiliary) expressions ... the method

described in this paper _fails_ to provide any such

clarification." (My emphasis.)

('Replacement of Auxiliary Expressions', Philosophical

Review, Vol. LXXV, 1956, p. 52.)

8. One way of weakening the implications of Craig's Theorem
for anti-realism may be to point out the general lack of
a clear-cut distinction between theoretical and observational
terms and statements. It is almost considered platitudinous
nowadays to state 'All statements, including so-called "pure
observational" ones are theory-laden'. This, if true, may
push the anti-realist into a phenomenalist basis and so
into an ~~individual~~ unwelcome solipsism. But is the lack of a general
distinction of such dismal importance for anti-realism when
we are talking of observational/theoretical dichotomies _within_
a theory? I see no reason to conclude that it is.

See G. Maxwell, 'The Ontological Status of Theoretical
Entities', Minnesota Studies in the Philosophy of Science,
Volume III, edited by Feigl & Maxwell, University of Minnesota
Press, 1962. Also see P. Achinstein, 'The Problem of
Theoretical Terms', American Philosophical Quarterly, Vol. II,
No. 3, 1965.

9. C.G. Hempel, 'The Theoretician's Dilemma', amended version
published in C.G. Hempel, Aspects of Scientific Explanation,
The Free Press, 1965.

10. W. Craig, 'Replacement of Auxiliary Expressions',
Philosophical Review, Vol. LXXV, 1956, p. 50.

11. But see footnote2., this Chapter.

CHAPTER 4.

We now turn to perhaps the oddest consequence of the anti-realist thesis: its non-acceptance of the Law of Excluded Middle (LEM). Intuitionist Propositional Calculus does not include '(p)(p v ⌐p)' in its axioms, nor is it a theorem in the calculus. Fundamentally, this is because of the meaning of the negation sign '⌐' in Intuitionist Logic with the resultant rejection of the Law of Negation in Classical Logic: '--p ⊃ p'. If we can revert to Intuitionist Mathematics for clearer perspective on this, we see that to assert that a mathematical statement is true is to claim that we have, or could, knowing how to commence the process, construct in a finite number of steps a proof of such a statement. The successful employment of a reductio ad absurdum argument to show that '⌐p' leads to a contradiction (and is therefore false) does not entail that 'p' is true. For, on constructivist principles, to show that 'p' is true we have to have constructed a proof of it. To prove that '⌐⌐p' is true is not sufficient to show 'p',[1] though the reverse is true - if 'p' is true, then '⌐⌐p' is also true. Even if '⌐⌐p' is true, says the constructivist, 'p' may be undecidable, hence neither true nor false. Although Intuitionists accept the Principle of Non-Contradiction (PNC) as necessarily true (I shall argue later that at least one Intuitionist, Dummett, cannot consistently accept even this principle), their rejection of the Double Negation

Principle enables them to refuse the LEM as a logical law.
However, the denial of the LEM, would, if introduced into
Intuitionist Logic, lead to a contradiction. What is
acceptable in Intuitionist Logic, is the falsity of the falsity
of the LEM, i.e. '$\neg\neg(p \lor \neg p)$'. Intuitionists claim
that although we cannot say that every mathematical problem
is solvable (which the assertion of the LEM would amount to),
neither can we say that any problem is unsolvable.

Classical Logic clings fast to the LEM; it is derivable
via the Law of Double Negation from the PNC. The Principle
of Negation is justified on the grounds that the truth of
'p' entails the falsity of 'not-p' and vice-versa, where 'p'
entails p is true and 'not-p' entails p is false. 'p' and
'not-p' are contradictories, as are 'true' and 'false'. On
Intuitionist conceptions, 'true' and 'false' are only contraries.
Outside of mathematics, such a position makes more immediate
sense, for it establishes symmetries: both halves of the
Double Negation Principle, '$p \supset \neg\neg p$' and '$\neg\neg p \supset p$', are
accepted, as are both types of reductio ad absurdum arguments,
'$p \supset \land$', therefore '$\neg p$', and '$\neg p \supset \land$' therefore 'p'. No
special status is accorded to the affirmation of 'p' as
opposed to its negation.

Interpreted, the acceptance of the LEM seems to make
more sense of the world. It is highly plausible to claim that
either something is the case or that that thing is not the
case, although we might not know which, might never know
which and might not even know how to ascertain which. The

realist goes further and accepts the Principle of Bivalency
for all unambiguous statements, The anti-realist will
refuse to distinguish between the two principles in the case
of compound statements; for we can only assert or deny a
compound statement when we can assert or deny its constituent
statements - that is how we learn to use compound statements.
Thus, to assert (as true) the LEM is to assert one of its
constitutent statements (and deny the other), i.e. we can
only assert 'p v ¬p' when we can assert 'p' or assert '¬p'.
Thus, if in any case 'p v ¬p' is true, it is only contingently
true; to say that '(p)(p v ¬p)' is true would be to claim that
we can determine, in every case, whether a statement or its
negation is true. We see here the full force of the
epistemological grounding of Intuitionist Logic. Classical
Logic largely[2] follows Ambrose in 'On Entailment and
Logical Necessity',[3] in holding that 'We should not argue for
the truth of 'p v -p' (AW - let alone '(p)(p v -p)') by saying
that 'p' is true ... we do not establish the truth of 'p v -p'
by deducing it from a matter of fact'. Even if Intuitionist
Logic were to accept the LEM in its calculus, its truth would
lie in our knowledge that for any statement, we have a
procedure for determining its truth or its falsity, and
whether or not we have this knowledge is a matter of fact.
In any sense in which the axioms of Intuitionist Logic are
necessary, they seem to be grounded in contingent statements.
This is at least odd. The axioms of classical logic seem to
be grounded more in a sense of what order the world must have

in order for discourse about it to be intelligible.

Even if the above considerations about the oddness of the anti-realist conception of the LEM are not convincing, one further argument may serve to impugn the conception, viz., the argument concerning the inconsistency on the part of the anti-realist in accepting the PNC as true while rejecting the LEM. The anti-realist accepts the PNC, but, given the epistemological grounding of his logic, presumably must justify this acceptance in epistemological terms. In particular, his justification for asserting the PNC will be symmetrical, in relation to the kinds of justificatory grounds, with the justification for asserting the LEM (which justification fails in the latter case). Thus, just as it is claimed we can only assert 'p v ¬p' when we can assert 'p' or assert '¬p', so it must be required that we can only assert '¬(p & ¬p)' when we can assert 'p' or can assert '¬p'. Dummett, in 'Truth', states: "Thus we learn to assert 'p & q' when we can assert 'p' and can assert 'q'". Clearly, with 'p' and '¬p', we cannot assert both, but how can Dummett claim this? The justification for asserting any complex statement, according to him, lies in our justification for asserting or denying constituent parts, and justification for the latter lies in our ability to recognise truth-conditions. To assert the PNC requires that for any statement, either it or its denial is assertable.

Perhaps it would be clearer if instead of talking about asserting we took denying as the central activity here. After

all, the PNC is in the form of a denial of a conjunction.
We can deny 'p & q' when we can deny one or both of 'p' and
'q'; in the case of 'p & ¬p', therefore, we can presumably
only deny this when we can deny 'p' or deny '¬p' or both.
For the anti-realist to include the PNC in his list of
logical truths is for him to claim that, for every statement,
either it or its negation is deniable. Which is not much
different from saying, a propos the LEM, that for every
statement, either it or its negation is assertable. If the
anti-realist objects to the latter, why does he not object
to the former?

Yes, it might be said, this is fine, but look at the
absurdity ensuing on not asserting the PNC. The meaning of
the negation sign '¬' is surely sufficient for us to intuit
the truth of the PNC. (Minor point: simply not being able
to assert 'p & ¬p' does not entail that we can deny it.) That
may be so for realists, when they study the Intuitionist
Calculus, but the anti-realist does not simply intuit the
truth of statements of logic: he justifies them in terms
of justifying assertions or denials of their constituent
parts, and this justificatory process is rooted in epistemic
situations. In relation to truth-functional statements,
it is only the epistemic situations justifying assertions or
denials of simple statements that justify placing a truth-
value on the complex statements. Dummett says: "We no
longer explain the sense of a statement by stipulating its
truth-value in terms of the truth-values of its constituents,
but by stipulating when it may be asserted in terms of the

conditions under which its constituents may be asserted".[5]
For us to understand 'p v ¬p', we have to understand what it
is for us to assert 'p' and assert '¬p'. We learn the
meaning of 'v' by using statements containing it. With
the premise that we nearly always try to tell the _truth_
when making assertions, Dummett concludes that we cannot
acquire an idea of what it would be like for such statements
to be true independently of the kinds of situations in which
we use the statements. We learn the meaning of 'v', therefore,
by using it, and we can only learn to use it by being taught
when it would be correct to assert it.[6] Similarly, therefore,
for learning the meaning of '&'. If learning the meaning of
this is rooted in the situations justifying the employment of
the connective, what _a priori_ reason is there for stating that
every statement is either assertible or deniable? The PNC stands
or falls with the LEM, it seems, and the Law of Double Negation
is not required to show this.

Leaving this objection aside, why are the Intuitionists
so insistent on not asserting the LEM? Why is this perhaps
the most crucial difference between their Logic and Classical
Logic? The answer lies in their conception of logic and
mathematics, and the respective places accorded these two
in their conceptual scheme. For Intuitionists, Mathematics
is prior to logic and not **vice-versa**. Intuitionist
Logic is not the justification of the reasoning methods of the
intuitionists, but is itself justified by such methods.
Strictly, Intuitionist Logic is merely a formulation of the

underlying principles involved in Intuitionist mathematics. Without using any logic, Intuitionists succeed in achieving mathematical results by _performing_ certain operations.

The implications for Classical Logic are profound. As opposed to the idea of logic as a _post hoc_ summary of reasoning methods, Classical Logic has typically been thought of as embodying self-evident necessary truths and therefore as itself justifying all modes of reasoning. Secondly, Classical Logic is impugned even as an adequate _description_ of the reasoning methods of Intuitionists, for it embodies principles unacceptable to Intuitionists. Thirdly, Classical Logic is therefore decidedly different from Intuitionist Logic, and in at least one field, that of mathematics, is threatened by the latter's existence.

Footnotes to Chapter 4.

1. In general, to suppose that an 'indirect proof' leads
to 'p' is to presuppose that 'p' is to be discovered not
created.

2. Exceptions exist, e.g. C.I. Lewis claims that 'p ⊃ (p v -p)'.

3. A. Ambrose, 'On Entailment and Logical Necessity',
P.A.S., Vol. LVI, 1956.

4. M. Dummett, 'Truth', P.A.S., Vol. LIX, 1958-9.

5. See footnote 4., p. 161.

6. This makes Dummett's explanation of the conditions under
which we can assert 'p v ¬p' rather odd. Why should we want
to assert this when we can either assert 'p' or assert '¬p'?

CHAPTER 5.

We now turn to the task of providing a more correct
analysis of dispositional statements. I will present some
of this chapter dogmatically, but will try to justify these
portions in the last chapter. The merging of the two chapters,
would, I believe, only obscure fundamental points. In this
chapter, I hope to adequately thread together the perhaps
seemingly disparate points made earlier, and, in particular,
to show the importance of the discussion of the LEM, somewhat
disjointedly placed in the previous chapter. The importance
of the LEM for the realist/anti-realist dispute demanded a
whole chapter to itself, and the placing of the last
chapter was dictated largely by my feeling that, after
Chapter 3, some widening of the scope of the discussion was
needed.

The demand for a categorical basis for dispositional
statements and the acceptance as a law of logic of the LEM
are complementary. For to insist that each manifestation of
a disposition is a manifestation of a tendency whose basis
lies in the make-up of the object or creature possessing the
tendency, is to insist that every statement affirming or
denying the possession of that tendency by that object or
creature is either true or false, regardless of our knowledge
or capacity for knowledge. For either the object will
possess the tendency or it will not - either the categorical
basis will be present or it will not (I recognise this is an

assumption in favour of realism in the physical and physiological fields, but see Chapter 1 on this). Again, to insist on the truth or falsity of a dispositional statement, without requiring that we can find out or know which, is to insist that the object concerned possesses a tendency, or lacks that tendency, even if there is no evidence either way. We have seen how an anti-realist analysis of the meaning of dispositional statements leads to the refusal to accept the LEM as universally valid; Dummett is emphatic about the absurdity of insisting on the validity of this law:

"If B still wishes to maintain (after available evidence has been seen to be inconclusive either way - AW) the necessity of 'Either Jones was brave or he was not', he will have to hold either that there must be some fact of the sort to which we usually appeal in discussing counterfactuals, which, if we knew it, would decide us in favour of the one or the other; or else there is some fact of extraordinary kind, perhaps known only to God. In the latter case, he imagines a kind of spiritual mechanism - Jones' character - which determines how he acts in each situation that arises ... each chatacter trait must be there independently of whether it reveals itself ot us or not. Anyone of a sufficient degree of sophistication will reject B's belief in a spiritual mechanism; either he will be a materialist and substitute for it an equally blind belief in a

physiological mechanism, or he will accept A's conclusion
that 'Either Jones was brave or he was not' is not
logically necessary."[1]

For Dummett, the meaning of a dispositional statement being
given in terms of evidence, e.g. behaviour in certain
situations (and possibly psychological tests - Dummett
nowhere rules this out), no credence can be attached to
the claim that such evidence is only evidence of something
else whose nature we surmise. To possess a disposition
is, for Dummett, simply to have a tendency to do certain
things in certain circumstances - nothing can or should be
said about any 'categorical basis' for such a tendency, since
we have no way of verifying such a suggestion. IF a
correlation were to be established between the occurrence
of the evidence and certain neurophysiological states, say,
of the creature manifesting such occurrences, an anti-
realist might say that this is evidence that such dispositions
do have categorical bases, and this would be to change the
meaning of dispositional statements. For now, a way of
showing that a creature possessed a particular disposition
would be to show that it possessed this particular neuro-
physiological make-up (assuming a one-one correlation -
this, actually, would be hard to establish) in certain
situations. Such a make-up would have become part of the
recognisable truth-conditions of the dispositional statement.
But the anti-realist need not do this, and he certainly
need not IDENTIFY the disposition with the neurophysiological
state - he can claim merely that a correlation has been

established. However, even this has not yet (1971) been done - therefore, says the anti-realist, to insist on the correlation or the identification is to subscribe to a 'blind belief'.

Now, Dummett's talk of a spiritual mechanism - Jones' character - as the basis of behavioural manifestations is hardly likely to be accepted by anyone of a sufficient degree of sophistication. It is a red herring. Very few philosophers are dualistic to that extent; most accept character-traits as dispositions of one kind or another. 'Character' is merely a name for the set of character-traits a person possesses. i.e. the name of a set of dispositions (and nobody in this position would postulate a metacharacter as providing the spiritual basis for such dispositions). However, Dummett's straw man does have one useful function, viz., that of showing that it is not a necessary truth that the bases of dispositional statements must be material.(And here I must make an observation which should have come much earlier on/ - that not all dispositional statements require categorical bases: as Levi and Morgenbesser have pointed out: we cannot always associate a disposition with a basis"[2]). One example might be the tendency of things to fall downwards in the earth's atmosphere. These exceptions do not affect the main argument nevertheless; all dispositions peculiar to animate objects require categorical bases.[3]) The demand for a categorical basis can be seen as a denial that a hypothetical statement is adequate as an analysis. The meaning of dispositional statements does not of itself demand a

material basis, it merely demands some basis. The reasons
for demanding a material basis are, if these distinctions
are in any way clear-cut, partly philosophical, involving
all the usual philosophical objections to a dualism of
mind and body, and partly scientific, in the sense that
scientific discoveries lend credence to the view that the
mind is either correlatable with or identifiable with some
kind of matter. Scientists aim at explaining or
correlating phenomena in terms of matter, at least above
the micro-level. I think, therefore, that we can disregard
spiritual bases and confine ourselves to the materialist
version of the categorical demand.

We have seen how the 'if...then' analyses of dispositional
statements have failed to do justice to the meaning of
dispositional terms; the largest obstacle to such analyses
being the openness of such terms. Dummett's analysis
implies that dispositional terms, are, in Carnap's
terminology, 'pure' dispositional terms; we have seen that
if such terms are to be placed in the pure dispositional/
theoretical dichotomy, they should be assigned to the
theoretical class because such terms have more meaning than
can be assigned to them by observational statements. Thus we
see that Carnap himself demolishes the anti-realist notion
of dispositional terms. Recognisable truth-conditions do not
exhaust, even if they are a part of, the meanings of such
terms. However, I think a more general point can be made —
that even if it were possible to provide sufficient and
necessary conditions for the predication of dispositional

by means of 'if...then' analyses, then these hypotheticals themselves would demand a categorical basis.

Let us imagine that a series of reduction sentences have been specified which together provide sufficient and necessary conditions for the application of the term 'water-soluble'. Then, the fact that such sentences <u>did</u> specify necessary and sufficient conditions would itself cry out for explanation. Anti-realists will object that explanation is not a properly scientific activity, but I hope to have shown earlier that this is a misconception about science. Dispositional statements are about objects, whereas 'if...then' statements are about events. We have seen that such a dichotomy is a somewhat misleading way of characterising the realist/anti-realist difference of conception of dispositions, but nevertheless I think it has some purchase here. The fact that a particular object behaves in a certain way when in certain circumstances surely demands an explanation: an explanation which at the very least will contain some reference to the categorical properties of the object. There must be something about the object which makes it behave in one way rather than another (which is not to say that environmental conditions are not necessary).

Now the anti-realist can concede a categorical basis as a plausible (or not) empirical conjecture, but deny that it is part of the meaning of a dispositional term that such (or any other) basis has to be provided. It is the

philosophical requirement of a basis which is the crux
of the dispute (the LEM provides a strong philosophical
ground for this claim). I do not wish to claim that the
meaning of any dispositional statement can be given in terms
of the micro-structure, brain-state or whatever of the
object concerned (or any part of the meaning) - truth-
conditions for 'Jones was brave' would be that Jones had
a tendency to act bravely in dangerous situations. The
make-up of the tendency is a question for empirical science.
However, I do wish to claim that the meaning of dispositional
statements requires that there be some categorical basis which
is the cause of the behaviour symptomatic of the disposition.
'Jones is brave' does not mean 'Jones' brain is in a certain
state or states when certain stimuli are present and when he
possesses certain attitudes'. But the meaning of the
dispositional statement does seem to require that such
a statement as the latter is true (given the restrictions
on the kind of categorical basis alluded to earlier).

Let me rehearse again previous objections to a hypothetical
analysis of dispositional statements and provide some new ones.
a) Dispositional statements are about objects, 'if...then'
statements refer to events. To say 'Jones was brave' is
to say more than 'If Jones encountered danger, he would act
bravely'. Although reference to tendencies to do certain
things in certain circumstances is common to both accounts,
for the realist something is missing from the analysis, viz.,
some indication of why the object does act in the ways specified

in the circumstances specified. For the anti-realist, this
is not required and indeed cannot be part of the analysis -
for it is not part of the evidence[4]. He conceives of
dispositional statements as referring to events in the
history or possible history of an object, and not to the
possession by the object of a tendency to making such events
likely. b) The difficulty of specifying necessary and suffi-
cient conditions (and the impossibility of specifying both
for any one disposition) points to the difficulties
encountered by anti-realists over the role of theory and
the meaning of theoretical terms. The realist's view of
theoretical terms demands that he views them as referring
to entities which cause the behaviour, a description of
which the anti-realist regards as a specification of the
meaning of the terms. As shown in Chapter 3, this anti-
realist conception of theory is unsatisfactory. c) We can
attribute a dispositional property to someone or something
even when no manifestation of that property has occurred.
The evidence relevant for the ascription of a dispositional
property to an object is not confined to the behaviour
constituting/symptomatic of the presence of that disposition.
This an anti-realist can agree with, BUT he must accept the
consequences - that with his identification of meaning and
evidence (or rather, sufficient evidence), there is likely
to be an exceptionally long specification of the meaning of
the dispositional term in such cases. This is suspicious
in itself, and is made even more so when we consider that,

with the improvement in scientific knowledge, such specific-
ations will grow longer as the class of phenomena counting as
sufficient evidence for the presence of the property becomes
larger (and is it absurd to raise the objection here that
even if such a lengthening were legitimate, it would assume
that such specifications were internally consistent, i.e.
that the phenomena specified correlate with one another? Such
an assumption is needed to preserve the non-self-contradictory
nature of the meaning. Can we always assume such a
consistency?). Admittedly the cogency of this point depends
on the assumption that scientific knowledge willincrease
in the direction indicated, but past signs suggest the
reasonableness of this assumption. The scope of psychological
tests has increased, e.g., MMPI, and we can assume, I think,
that they will continue to do so. It is surely more in
accordance with what our conception of what it is to specify
meanings that we reject the anti-realist conception. It is
more in accordance with this conception to accept the realist
notion of meaning and regard the evidence merely as symptomatic
of the presence of the dispositional property.

All this may be conceded (and I shall give an account in
the last chapter of the inadequacies of the general theory
of meaning provided by anti-realism), yet doubts may remain.
In particular, are dispositions causes of their manifestations?
And is the situation over the realist/anti-realist dispute
symmetrical in the classes of dispositional properties - those
attributable to inanimate objects and those peculiar to animate
objects? We can take these questions together conveniently by

examining dispositional properties of inanimate objects.

Levi and Morgenbesser suggest that some dispositions
are associated with bases (we can disregard for our purposes
their assertion that not all dispositions are like this). The
reason for this as they see it is that dispositional predicates
serve as 'place-holders' for other predicates. They are
like ceteris paribus clauses in this latter respect but unlike
them in that, whereas ceteris paribus clauses do not in
any way limit the scope of the class of predicates to
replace them, dispositional properties do: the same kind
of specification of conditions must be filled in in all
cases where the same dispositional predicate is to be
replaced. The importance of this view for our purposes
is that it backs up my claim that the truth of a dispositional
statement cries out for explanation. In Ryleian language,
ceteris paribus clauses and dispositional statements are
'explanation-hungry'. This explanation must involve reference
to some feature or features of the possessor of the
dispositional property. To the extent that Levi and Morgen-
besser make this distinction between ceteris paribus clauses
and dispositional predicates as standing in for further
specifications of conditions in different ways, I would go
along with them; my somewhat dismissive and curt remarks
earlier about ceteris paribus clauses indicate my belief as
to the usefulness of such clauses. Within the field of
physics, Levi and Morgenbesser claim the bases of dispositions
are usually thought to be objects' micro-structures, and it

is easy to agree with them over this. If we want to
explain why sugar is water-soluble, we will investigate
the structure of sugar crystals. Why scientists make appeal
to this feature as having explanatory force rather than to
some other features of the situation or object is presumably
explained by reference to their preference for explaining
the world in micro- rather than macro-terms. To pick out some
feature of the object as the cause[5] of particular behaviour,
rather than to specify the totality of causal conditions,
is useful in pointing to a generalization which can be made
on the basis of that feature. It is that feature which, in
conditions C, makes the difference between how an object
behaves in C with and without that feature. It is perhaps
a philosophically uninteresting question how philosophical
and scientific concepts inter-relate; but what philosophers
would be foolish in doing would be to ignore the conceptual
apparatus employed by scientists. It is this that leads
me to the conclusion that the meaning of dispositional
statements (or most of them) requires that some such
statement as 'Jones' brain is in a certain state or states
when certain stimuli are present and when he possesses certain
attitudes' be true when 'Jones is brave' is true.

Have I shown that categorical bases are required by the
meanings of dispositional statements? Or have I merely shown,
in Stevenson's words[6] that the assertion that there is a
categorical basis underlying each dispositional property is
a "plausible empirical hypothesis". In a sense I accept the
objection, but feel it is not an important concession to

to make. I have argued that bases are required a) because
the LEM is true, b) because hypothetical analyses are
inadequate, and c) (really a part of b)) that dispositional
statements are about objects, not situations. And to
hypostasize possibilities, thereby avoiding the conclusion
that if dispositional statements are about objects, then
they are about the categorical properties of those objects
(and thereby asserting that such statements are about non-
categorical properties of the objects), is surely absurd.

Given that such dispositions require a categorical basis,
it does not follow that any particular kind of basis is
required in the sense that the existence of a material basis
is _entailed_ by the truth of such a requirement. As far as
logic is concerned, a non-material basis would be adequate.
What makes a material basis the appropriate one in the case
of animate objects are considerations of two kinds. 1. Objections
to a dualism of spirit and body. This by itself simply rules
out spiritual bases; it does not rule out other kinds of
non-material bases, if any such could exist. 2. The scientific
assumptions as to the kinds of things correlatable with
or identifiable with minds and mental phenomena. (I have
mentioned this point earlier in this chapter) In the case
of inanimate objects, what other categorical candidate presents
itself?

It is largely because of consideration c) that I conclude
that a categorical property (or properties) underlies a
disposition, and is indeed required by the meaning of a

statement asserting the presence of a disposition. And it
is largely in virtue of consideration 2. that it is a material
categorical basis, and not any other, which is the basis
required. This, I admit,is, if you like, an empirical
hypothesis, but it is one which, I hope, will be immediately
acceptable to those who accept the requirement of a basis.

But perhaps we have assumed too much here. It might
be objected that although we have shown that dispositions
require categorical bases in the case of dispositions
predicable of inanimate objects, we have failed to show
this in the case of dispositions peculiar to animate objects,
particularly human beings. Many may object that though it is
not implausible to claim that the basis for a dispositional
property of an inanimate object lies in its micro-structure,
to claim such or a similar basis for human dispositions ist
to be guilty of succumbing to a dogma. We can dismiss
objections based on assumptions that man is not part of
nature and that therefore no fully scientific account of
him can be given (in whatever form these objections are
expressed) as a petitio principii. What is worthy of
consideration, however, is the drawing of a distinction
between th two classes of dispositions on the grounds that
those attributable to inanimate objects more obviously
involve laws, whereas those peculiar to animate objects do
not. To make a ludicrously exaggerated comparison, yet one
which has cogency, to say that this piece of glass is brittle
is to commit oneself to holding that every piece of glass

like this in composition (which typically will boil down to
identity of micro-structure) is brittle, whereas to say this
man is brave is to make no such commitment. In the former case,
there is a causal law relating an object's micro-structure,
or other identifiable feature, with its brittleness: There
are no such laws in the latter case.

One incorrect way of interpreting this would be to
claim that this shows that whereas the meaning of dispositional
statements attributable to inanimate objects requires that
they have a categorical basis, this is not the case when
we consider dispositions peculiar to animates. The alleged
fact that we cannot generalise from one or more cases, it
might be argued, tends to show that dispositions are not
in any way associated with categorical properties. To claim
that any property P is responsible for fact F, is to claim
that in every like situation fact F would ensue property P's
obtaining, where P is a categorical property that 'P' causes
F. With dispositions, the problems of specifying like
situations are much less acute: to correlate or identify
brittleness with micro-structure means that identity of
categorical property, micro-structure, entails identity
of disposition. If, on the other hand, human dispositions
are not generalisable in this sense, how can we claim that
they have a categorical basis?

One answer to this is to point out that the objector is
assuming that causality necessarily involves causal laws;
It might be said that commitment to the truth of statement

'A causes B (in specified circumstances)' entails a
commitment to the truth of something like 'Events of type A
cause events of type B (in specified circumstances)'. (I use
the term "events" as shorthand for all kinds of causal
agents, e.g. events, states. I do not want to claim by
this formulation that only events can conceivably be causal
factors.) But, will be the objection, why should we make
such a further commitment? Why cannot causal statements be
true without a law covering the events (with prior
proviso on this word) being true? The answer to this is that
the assumption is correct -causality does necessarily
involve causal laws. For WHAT would we be claiming if we
said that A causes B but it is not always true that A causes
B (identical parameters of conditions assumed)? That B
followed A but B does not always follow A? Then what would
be the difference betwen saying A causes B and A precedes
B? Laws, I conclude, are required. I recognise that as
stated, my claim is false - restrictions have to be placed
on what kinds of descriptions "A" and "B" are, and this is
not easy to effect - but, with these restrictions, it can be
claimed that causal statements necessarily involve causal
laws.

But if this claim is justified, where does that leave
the assertion that dispositions peculiar to animate objects
require categorical bases? If the micro-structure of a piece
of glass is said to cause behaviour characteristic of
brittleness, and this feature is generalisable, then the fact

that we cannot generalise from men being brave to all men
being brave may be thought to kill the idea that human
dispositions have a categorical basis.[7] I think the realist's
answer is obvious - that, under certain descriptions, we <u>can</u>
generalise from one case of a human disposition to other
possible cases. Just as the other pieces of glass covered
by the brittleness generalisation may (this because it might be
argued that some kinds of glass, e.g. frosted glass, are
not brittle) have to satisfy certain criteria relating
to their micro-structure in order to qualify as instances
of the law, so <u>a fortiori</u> it will be a requirement that the
instances covered by a law relating to an animate disposition
satisfy certain criteria. The instances have to be alike <u>in</u>
<u>certain respects</u>: it is these respects that enable one to
generalise. And it is to the categorical properties of the
animaties that we look for provision of these respects. As
long as we provide certain descriptions - in practice, these
will be in the form of neurophysiological descriptions - we
make a law concerning brave men. The fact that the categorical
properties cause the behaviour characteristic of the
disposition in one case means that their presence in other
men will cause similar behaviour. (I should qualify this:
unless we have a one-one correlation between the physical
characteristics, we may not be able to generalise in the
simple way envisaged. If more than one physical state is
correlated with the disposition, the simultaneous presence
of factors peculiar to each state may well not result in the

behaviour characteristic of the disposition). What has
given credence to the alleged difference between animate
and inanimate dispositions is the fact that we know so much
more about dispositional states in the latter case than in
the former. What makes it so difficult to correlate in
the former is the fact that states of the human being are
complex and constantly changing, to an extent unlooked for
in inanimate objects. But such complexity cannot stand as
a theoretically well-grounded objection to universalize
in both cases. The arguments put forward to show that a
categorical basis is required for dispositions also show that
laws could be formed: categorical properties are the kinds
of things which behave uniformly. That can indeed be part
of the criteria for identifying such properties (and it is
of interest to note, here, that dispositions play a large
part in our deciding what 'thing-kind' a thing comes under).

What kinds of material bases are correlatable or
identifiable with dispositions is a matter for empirical
science. Whether brain-states or states of the Central
Nervous System, whether a one-one or a many-one correlation
is correct, these are up for investigation. What one might
say here, as a kind of armchair physiologist, is that a
one-one correlation, though most desirable from the point
of view of ascertaining the relationship between mental and
non-mental properties, is not the most plausible claim.
There are so many different ways in which one can be
courageous that there may well be no one feature common to

all cases of courage. This is another respect in which animate and inanimate dispositions are likely to differ. I do not think this difference has any significance for our purposes.

I therefore conclude that dispositions (or most of them) require categorical bases. Therefore it is now appropriate that we turn to the general character of anti-realist accounts of truth and meaning.

Footnotes to Chapter 5.

1. M. Dummett, 'Truth', P.A.S., Vol LIX, 1958-9.

2. I. Levi and S. Morgenbesser, 'Belief and Disposition', American Philosophical Quarterly, Vol. I, July, 1970.

3. G. Roberts has warned: 'Beware the contingency reminder'. Some dispositions may, as a matter of fact, be possessed only by animate objects yet come into the class of dispositions not requiring categorical bases. An answer to this would be on the lines of making the class of dispositional predicates peculiar to animate objects a matter of 'type-classification: what dispositional predicates when predicated of an expression referring to an animate object, contribute to the formation of a sensible statement.

4. This is dubious at least in the case of dispositions predicable of inanimate objects - identification of particular micro-structure may well be sufficient for scientists to predicate particular disposition(s) of the object - perhaps the anti-realist would claim that such evidence was not complete. We talk of complete and incomplete evidence in Chapter 6.

5. One point which might be made here. It is not peculiar to a realist view of dispositions that a causal explanation he gives of the behaviour characteristic of the disposition. As R. Rorty points out, in 'Incorrigibility as the Mark of the Mental', Journal of Philosophy, Volume LXVII, No. 12, 1970:

"...it is not clear that the distinction between
dispositions as states and dispositions as non-states
comes to more than the distinction between behavioral
law backed up by reference to new particulars, and those
not so backed up. But it is not clear why explanation
by reference to unbacked-up behavior laws should not
count as causal explanation."

True, but then it is not the dispositions which are causes
(of their behavioural manifestations), but stimuli.

6. L. Stevenson, 'Are Dispositions Causes?', Analysis, Vol XXIX,
no. 6, 1969.

7. A not too irrelevant remark here. W.D. Glasgow and
G.W. Pilkington, in 'Other Minds on Evidential Necessity', Mind,
Vol. LXXIX, No. 315. 1970, claim that "such human dispositions
(AW - the examples are honesty and integrity) are always of
a 'more or less' variety". This remark is mobilized by their
them in opposition to logical behaviourism - to which thesis
it does, indeed, seem fatal - but it is also a problem for
any account of dispositions. One has doubts about lumping
together dispositions like integrity, and those like courage
which Glasgow and Pilkington seem to do. It may be necessary
to classify various kinds of integrity: this disposition
may be a genus type whereas courage is a species (not, I
hasten to add, of the genus 'integrity'). But it can be said
that this feature of dispositions is a difficulty for any
account.

CHAPTER 6.

We have raised and discussed some of the problems of
adopting an anti-realist position on truth and meaning: the
implausible consequences of an epistemological account of
truth outside its birth-place - mathematics, the denial
of the logical necessity of the LEM, the problem of specifying
fully the experimental conditions under which we can say
a statement is verified (and the implausibility of an
operationalist analysis of dispositions), and the failure of
operationalist and verificationist attempts at providing a
satisfactory account of theories and theoretical terms,
attempts which were seen to be vitiated by a correct analysis
of scientific investigation itself. Considered as a reformatory
theory, as opposed to a descriptive one, anti-realism is not
refuted by these objections - though it might be said that the
impossibility of defining behaviourism behaviouristically
makes it at least very difficult to give any useful analysis
of mental phenomena in terms of behaviour: the distinction
between sincere and insincere actions for instance, if it is
not to be made arbitrary, may well prove impossible to
formulate - but one objection, mentioned earlier, may be
said to show the unacceptable nature of anti-realism, even
as a propsoal. That objection is that the adoption of anti-
realist tenets would lead to solipsism. Such a consequence
is a general feature of anti-realist accounts of truth and
meaning, and an attempt to prove this point can be seen,

therefore, as part of an attack on the general theory of anti-realism.

We claimed in Chapter 1 that anti-realism led to solipsisim in each of three respects: a) the problem of specifying the reference of 'we' in Dummett's formulation: 'The meaning of a statement of a given class is given in terms of the conditions which we recognise as establishing the truth or falsity of statements of that class'. b) The requirement that recognisable conditions provide the truth-conditions in the anti-realist account of meaning provides no guarantee that anybody would be speaking the same language and understanding words in the same way. c) In the case of empirical statements, the anti-realist demands some observational basis as forming the foundation of our knowledge. This will usually involve reduction of non-observational statements to observational ones. What level of observational statements should be chosen? Why should not the anti-realist choose the most miserly one - that describing "sense-data"? Let us deal with these points in turn.

a) Dummett makes no further comment on his assertion, presumably believing it to be unproblematic, but I think there are one or two difficulties about it. Who are 'we'? The whole human-race? A particular culture-group? Those who speak the same language? One or two minor problems may arise from choice of the first - different peoples may have different methods of verifying or confirming statements. A digression into what is to count as the same statement would be unnecessary

for our purposes. So we will assume that the 'we' are those
who speak the same language and have roughly similar cultures.
Then suppose that we, in 1970, have no way of verifying a partic-
ular class of statement, yet knew that our forefathers in
1870, had such a way (the technological knowledge might have
been lost in 1871). Would that make any statement we made
in that class meaningless? Such a possibility is perhaps
remote but is nevertheless feasible. Short of an account
of what is to count as the same class (perhaps defined in
terms of similar verifying conditions?)[1] it is difficult to
rig up a water-tight example. But technological disasters
are possible: in such circumstances, a realist would presumably
claim that the statement had the same meaning as before.
For instance, if we lost all means of observing, with the help
of apparatus, traces in cloud chambers, etc. an anti-realist
would presumably say that the term 'electron' had no meaning,
or rather that any statement in which the term 'electron' was
used (and not mentioned) would be meaningless. This assumes
that statements within the field of micro-physics form a class —
a not unreasonable assumption, I hope. Suppose 'we' went
blind and remained blind. An anti-realist would presumably
say that the meanings of material object statements (a class?)
had changed. Would the realist?

What has this to do with solipsism? In the absence of an
adequate statement of what is to count as 'we', there seems
no reason, on Dummett's account, why the fact that two people
make the same noises should mean the same things. For they

may have different views as to what is to count as verifying
or falsifying the statement they are each making. The reductio
of this is that nobody means the same by the same noises.
Now, this claim I realise is absurd - there are field where
groups of people all recognise identical truth-conditions,
e.g. in many scientific fields - but Dummett has given no
reason why we should regard this as being the case in non-
scientific areas of discourse. This claim is central to
the second way in which anti-realism leads to solipsism (and
this is a much more serious avenue than the first) so I
will take that point now.

   b) If meaning is given in terms of recognisable truth-
conditions, why should you and I, who speak the 'same'
language, i.e. utter the same noises, mean the sameby what
we say? I might verify my material object statements in
a way very different to yours. Suppose we compare methods
of verification and falsification. Surely that would establish
whether we meant the same or not. But even if we uttered
the same noises in answer to the question, the same point
would apply. Does identity of noise entail identity of
meaning. Do we recognise the same truth-conditions for THIS
statement, and so on. Granted that this is an implausible
picture, all that can be established is that you and I tend
to utter the same noises and do the same things as a result
of such utterances (and a strict behaviourists would be
satisfied with this). There is no guarantee that what we
regard as truth- or falsity-conditions are symmetrical.

Both of these objections are rather out-of-the-way
and they do not really convince. What I hope is a much
more cogent objection is my third route to solipsism.

If the meaning of empirical statements is given in
terms of the observational truth-conditions which we recognise
as verifying them, what is to prevent a slide into demanding
the most indubitable truth-grounds?[2] For instance, in the
case of material object statements the only sure way we have
of verifying these statements is by means of our perceptual
faculties. Therefore, specification of recognisable truth-
conditions will centrally include statements about our
perception. Such statements will be in the form of
statements about seeing, hearing, etc. the material objects.
But if that is what a material object statement _means_, we
are back with the phenomenalist insistence that what the
truth of a material object statement really does is to
license an infinity of statements about sense-data. And
this is is the short way to solipsism. T.L.S. Sprigge in
a review of William Todd's book _Analytical Solipsism_ denies
this consequence:

"Todd may say that phenomenalism is bound to be solipsistic
for it is precisely the doctrine that one's statements
about material things are reducible to statements about
one's own sensations .... There is no need for this,
however. One might claim that statements about the
material world are reducible to statements about possible
sensations, without restricting the range covered by

'sensation' to the speaker's sensations."

(<u>Inquiry</u>, Vol. 13, No. 4).[3]

One might, but at the severe cost of undercutting the
whole <u>raison d'etre</u> of the phehnomenalist programme.
For why should statements about other people's sensations
be immune from the same kind of phenomenalist analysis
given to other material object statements? Statements about
other people's sensations are also reducible. And they
can only be reducible to statements about MY sensations.
One interesting consequence here is analogous to what
Moore says about ethical disagreement.[4] If 'x is wrong'
means 'I disapprove of x', and 'x is right' means 'I approve
of x', then if Jones says that x is wrong and Smith that
x is right they are not contradicting each other. Of course,
I go further here and claim that even your claim to perceive
a material object not only does not contradict any of my
assertions about that object, but is reducible to a statement
about <u>me</u>.

But is this argument conclusive? It might be said
that we have shown that the anti-realist conception of
meaning entails solipsism only because, in one particular
area of its application, phenomenalism is a solipsistic
programme. Should we not provide more general arguments
for our claim? I think this objection is justified, though
I think that the above argument is in its way conclusive,
so we will inquire in some detail in to the general features
of anti-realist accounts of meaning.

The central motivating force of anti-realist accounts is the desire to avoid the sceptical consequences thought to ensue on other accounts of meaning and truth. It is supposed that only by building up an account from the situations in which we use the notions of truth and meaning can we provide an account which is based on hard fact and not open to all sorts of doubts, typical of philosophical sceptics. This is the justification for employing epistemic situations as the cradle for an acceptable account of truth and meaning. In some ways, it is more of an historical account of language-use and acquisition than rival accounts, relying as it does on the alleged way we come to grasp an understanding of these notion. This is why Dummett lays such emphasis on the fact that it is only through learning to correctly assert statements that we learn what 'true' means. This insistence on rooting the account in epistemic situations explains Russell's fear (in An Inquiry into Meaning and Truth[4]) that the acceptance of the Law of Excluded Middle would commit one to 'a realist metaphysic which may seem, in the spirit if not in the letter, incompatible with empiricism'. The claim that it is only through using the notions of truth and meaning that one can come to understand their meaning, and the further claim that it is therefore by means of their use that the meaning of 'truth' and 'meaning' must be explained, is complementary to the insistence on seeing epistemic situations as the key to a satisfactory account of these terms. This very

naturally goes hand-in-hand with an adherence to empiricism
so that the notions of 'truth' and 'provability' are
cemented together, so that 'meaning' is closely allied to
'truth-grounds', or 'sufficient evidence'. The positivist
employment of these beliefs is perhaps best summed up by
Malcolm in his article 'Behaviorism as a Philosophy'.[5]
Malcolm, in discussing Carnap's grounds for accepting
behaviourism as the only scientifically-based psychological
theory, says:

"Carnap's reply is that the person's statement does not
inform us of anything unless we understand (author's
emphasis) it, and we do not understand it unless we
know what observable phenomena would verify it."

The trouble is that if experience is made the sole
basis for provision of meaning – meaning being given in
terms of those (experiential) conditions which verify – we
are driven back to our own experience as the only acceptable
method of verification. There is more than a hint of this in
early verificationist (see Carnap) talk of 'methodological
solipsism'. The positivists saw that this was hardly
distinguishable from 'ordinary' solipsism and led to a
kind of idealism which was precisely the opposite destination
they wished to reach. The problem was that, to guarantee
the existence of an indubitable basis on which to build the
edifice of knowledge, and thus do justice both to
verificationist principles and to the accepted fact that
we do have knowledge, the positivists were open to least

objection, <u>on these scores</u>, if they chose personal experience as the basis. This meant, of course, that they were caught on the other horn of the dilemma - solipsism. We can put the problem this way: it is a consequence of the anti-realist theory of meaning that the truth of say a material object statement consists in its actually or possibly being verified by us. We verify by means of perceptual experiences. But the only perceptual experiences I can know of, in the sense of conclusively verify, are my own. For suppose I try to verify your perceptual experiences, or rather, the fact that you are having such experiences. The only way I have of doing this is to consult my <u>own</u> experiences (of your experiences). The statement 'You are having perceptual experiences of X' is presumably symmetrical with other material object statements in requiring conditions for verification. Such conditions can only consist in my own experiences. Therefore, what 'You are having perceptual experiences of X' comes down to is that I am having experiences of your .... We therefore arrive at solipsism. What necessitates this conclusion is the anti-realist insistence on experience as providing the <u>meaning</u> of material object statements, rather than merely claiming that experiences provide good grounds for believing in the truth of such statements. In general, rooting meaning and truth in epistemic situations has these disastrous results.

A related objection weighs against the demand for an

observational basis for our knowledge __and__ for the
specification of meaning. If such a basis is to do the
job of providing an indubitable foundation why should we
not make the basis as indubitable as possible (or, if
this is too strong, as incorrigible as possible)? Why
should we remain at the level of statement exemplified
by 'X sees Y' (therefore, Y exists)? After all, although
'X sees Y' entails that Y exists (in Professor D.R.P. Wiggins'
terminology, it is an expression of a type of propositional
attitude called 'epistemic'[6]), discerning the truth of
'X sees Y' is not without its difficulties. Empiricists
are particularly sensitive to the Argument from Illusion,
to take just one case. We therefore get pushed down the
road to sense-data statements. And if the meaning of
statements is given in terms of these kinds of statements
- for they are the truth-grounds for statements such as
'X sees Y' - solipsism is the only conclusion. For meanings
will be private to each person; by definition, people cannot
perceive the same sense-data.

Another possible objection, for which I am indebted
to G.W. Roberts, is that the data do not remain 'hard'. If
we require of an analysis of a material object statement that
it recognise the temporal continuity of such objects, there
simply will not be enough basis in observational data to
make up an acceptable equivalence relationship between the
analysans and the analysandum. Data become 'stuff'.

Of course, behaviourism is not open to this precise kind
of objection: phenomenalism and behaviourism are like two

sides of the coin. As Dummett says, in 'The Reality of the Past'[10], "it immediately occurs to us to wonder whether it is possible consistently to maintain an anti-realist position simultaneously in both regards". But one can view the two doctrines as being open to a similar objection in one respect, viz., that both demand necessary and sufficient conditions for the truth of, respectively, material object statements and statements about other minds. Unless behaviour is defined non-behaviouristically, thus making the whole doctrine trivial, this cannot be done; and the phenomenalist programme demands that an infinite number of phenomenal statements be true if the material object statement is true. How do we verify an infinite number of statements? What is at fault is the demand for sufficient and necessary observational conditions.

Here we see how the considerations brought forward in Chapter 2 in opposition to the operationalist and behaviourist attempts to provide observational conditions for the predication of disposition terms can be mobilised for use against the general anti-realist thesis. For what that thesis requires is that there be conditions such that our knowledge that they obtain is sufficient for us to know the truth of the statement in question, i.e. whose truth-grounds these conditions are. And such conditions must be, in a very real sense, knowable. In the case of reductive kinds of anti-realism, we must be able to infer from one or more true, i.e. known, statements of the 'basic' class the truth

of the 'constructed' class. But the examples of behaviourism
and phenomenalism are not favourable to the anti-realist here.
Behaviourism in any plausible form, does not define behaviour
behaviouristically: any attempt to give a behaviouristic
analysis of behaviour will meet with insuperable problems,.
e.g. the impossibility of distinguishing sincere and
insincere behaviour. And phenomenalism leads to solipsism,
as we have seen. A non-reductionist variety of anti-
realism is in no better health, for provision of sufficient
and necessary conditions here will also prove an impossible
task. Or rather, if such conditions are provided, they
will be such that their obtaining or not will not be
obvious. For instance, suppose we provide a sufficient
condition for 'Y exists' (where 'Y' is a four-dimensional
object) by stipulating that the truth of 'Y is touched by a
human being' entails the truth of 'Y exists', then we are
going to require a very stiff condition for the truth of 'Y'
'Y is touched by a human being'. Such stiffness is necessary
because the truth of the latter statement is supposed-to
entail the truth of the former: it is not just evidence for
the other statement. The problems of hallucinations,
illusions, etc. arise here to make it at least extremely
difficult to ever be sure one is in a position to assert such
a statement. The data have to be made 'hard'.

We can best bring out the importance of this, and its
force, by using an example. Putnam, in 'Psychological
Concepts, Explication, and Ordinary Language'[7], argues

against the claim that it is impossible for Jones to be
exhibiting all the symptoms of anger yet not be angry
really. (We assume that deception is somehow ruled out.)
What the discussion boils down to is this: either we regard
the 'symptoms' of anger, i.e. 'anger-behaviour', as criteria
for someone being angry or we regard such symptoms merely
as evidence of the person being angry. In the former case,
we regard the symptoms as what Putnam calls 'defining
characteristics', so that collectively they constitute
the meaning of the term 'anger'; in the latter case, we
sever the notions of evidence for anger from the meaning
of the term 'anger'. In the former case, the symptoms are
collectively sufficient for specification of the meaning of
'anger'; in the latter case, this is not so. Thus for the
behaviourist - someone taking the first option - the truth
of statements about all these conditions entails the truth
of the statement about anger. We can perhaps now more easily
see that we must be extremely cautious about what we allow
in to our epistemic basis, for it is not implausible to
claim that many kinds of behaviour are compatible with many
kinds of 'mental 'states'. In fact we cannot accomplish
this task. Not only dispositional statements, with the
inherent openness of the terms to contend with, but
statements about so-called mental states prove impossible to
reduce to behaviour statements (unless, trivially, it is
done circularly).

If we adopt the other anti-realist course - I am assuming

that the two cannot be adopted in toto together - and accept
phenomenalism, we face similar difficulties. Our basis is
going to have to be tight enough to accommodate satisfactorily
arguments from illusion, hallucinations, etc. To do this,
we have to make our basis as miserly in knowledge-claims as
possible, i.e. we are forced into solipsism. The insistence
in a basis means that the phenomena in the basis have
to be both readily recognisable and epistemologically sound
(this applies to all kinds of anti-realism, not just the
phenomenalist variety). This basis cannot be provided.

It might be mentioned here that Achinstein's argument
in 'The Problem of Theoretical Terms'[8], though not
conclusive in this respect, does provide ammunition for those
who claim that observational bases cannot be provided.

What is fundamentally at fault with the anti-realist
approach is its analysis of what it is for terms and
sentences to have sense. Epistemological principles, the
nature of which are determined largely by the fear of
scepticism, push the anti-realist into a doctrine of meaning
which allows him little room to manoeuvre with in the
parameters of his own chosen field of knowledge-acquisition.
We saw in Chapter 3 how the anti-realist doctrine of meaning
failed to do justice to scientific methods of investigation;
indeed, one could say that acceptance of anti-realist tenets
would be anti-heuristic for scientists. The demand for
justification of the intersubjectivity of language has
led the anti-realist into 'justifying' precisely the opposite.

The sense of a sentence is given in terms of the conditions which would make the sentence true. A sentence's being true or being false must make a difference to the world (unless we have a tautology); therefore what better way is there of making sense of what somebody says than by consideration of what difference the truth or the falsity of what he says would make? There is no more objection to this circle of truth and meaning than there is to the circle of 'analyticity', 'synonomy', self-contradictoriness', etc.. We go round in circles, but not necessarily viciously. The demand for one or the other to be basic seems to be a mistake peculiarly symmetrical with the demand for a basis for our knowledge. Even a philosopher such as Feigl has come to the conclusion that "the meaning of scientific statements consists indeed be their truth-conditions. But "truth-conditions" does not mean the same as "confirming evidence".'[9]

The peculiarity of anti-realism holds for truth as well as for meaning. An assertion is true, according to Dummett, if correct assertability conditions are present, which we are capable of recognising. Does this mean that the person who makes a particular statement has to be capable of recognising the said conditions? Or do such conditions merely have to be present in the world, available to all, but not necessarily in the possession of the person concerned? Dummett's "T anti-realist", in 'The Reality of the Past'[10], i.e. an anti-realist only with respect to the

past, can allow the latter possibility. Dummett says of
him: "he can allow the possibility that a statement may be
true even though we do not, and never will, know of any
evidence in its favour". He calls this sense of 'true'
'absolute truth' and claims we do not normally use the
word 'true' in this sense; rather we use the word in the
sense of claiming that a statement is justified on the
grounds of evidence. What one would want to say about
this is that Dummett has not drawn a distinction between
two senses of 'true', but has indicated the imperfection of
our knowledge. 'Ordinary truth' seems to refer to the
possibility of claiming that something is true, and its
possibly not being the case;- we are not infallible;
'absolute truth' to the fact that the Law of Excluded
Middle is a logical necessity, dividing the possibilities
into two, 'p' and 'not-p'. Dummett says, "it is only
in this sense of 'true' (i.e. absolute) that we are entitled
to assume that there is a determinate answer, even if known
only to God, to the question whether a given statement is or
is not true".

It is unlikely that we could claim many statements were
'absolutely true', in Dummett's sense, for we are hardly
ever in possession of 'sufficient' or 'complete' evidence.
Incomplete evidence will be our staple diet, unless the
anti-realist is liberal in laying down assertability conditions.
And that would be incompatible with his aim of providing a
solid basis for knowledge.

The anti-realist is pulled in two opposite directions by his presuppositions: he wants to provide a solid foundation for knowledge, which means the disguised attractions of solipsism are heightened, and he wants to provide clearly _recognisable_ assertability conditions – a desire which leads to the strong possibility of error and therefore ~~solipsism~~ _scepticism_.

To claim that we cannot assert a statement if we cannot assert (or deny) its constituents may be correct to the extent that we would not be justified in claiming knowledge, or even strong belief (if asserting X implies a claim to knowing or strongly believing X) – we would be misleading our hearers, for instance. But to refuse to assert the Law of Excluded Middle on these grounds is mistaken: the Law of Excluded Middle is not true in virtue of our ability either to assert 'p' or to assert 'not-p' in every case. It is true because there are only two possibilities in the world and either 'p' will, in some sense, 'correspond' to the actuality or 'not-p' will. Moreover, if the Law of Excluded Middle fails to meet the anti-realist test, then so does the Principle of Non-Contradiction. And this is surely a not inconclusive argument against anti-realism.

Footnotes to Chapter 6.

1. Of course, one can trivialise this by pointing to the
fact that a class can be constituted out of any members.
Obviously, Dummett did not mean that to be a possibility,
but he gives us no clue as to what the relevant classes
are - except citing the examples of statements about the
past and statements about other minds - nor what are the
qualifications for membership of a class.

2. One quotation from Dummett is interesting, if I have
interpreted it correctly

"for two people might agree in their dispositions to
recognise something as belonging to the totality (finite
totality of natural numbers), and still differ on the
criteria they accepted for asserting something to be
true of all the members of the totality."
('The Philosophical Significance of Gödel's Theorem',
Ratio, 1964.)

3. T.L.S. Sprigge, Review of Analytical Solipsism by W. Todd,
Inquiry, Vol. XIII, No. 4, 1970.

4. B. Russell, 'An Inquiry into Meaning and Truth, Allen
and Unwin, Ltd., 1940.

5. N. Malcolm, 'Behaviourism as a Philosophy of Psychology',
Behaviourism and Phenomenology, edited by T.W. Wann, University
of Chicago Press, 1964.

6. D.R.P. Wiggins, 'Freedom Knowledge, Belief and Causality', _Freedom and Necessity_, edited by G. Vesey, Macmillan, 1970.

7. H. Putnam, 'Psychological Concepts, Explication, and Ordinary Language', _Journal of Philosophy_, Vol. LIV, 1957.

8. P. Achinstein, 'The Problem of Theoretical Terms', _American Philosophical Quarterly_, Vol. II, No. 3, 1965.

9. H. Feigl, '_The "Mental" and the "Physical": The Essay and a Postscript_, University of Minnesota Press, 1967.

10. M. Dummett, 'The Reality of the Past', _P.A.S._, Vol. LXIX, 1968-9.

BIBLIOGRAPHY

ACHINSTEIN, P.   —   'The Problem of Theoretical Terms',
                          American Philosophical Quarterly,
                          Vol. II, No. 3, 1965.

AMBROSE, A.   —   'On Entailment and Logical Necessity',
                          P.A.S., Vol. LVI, 1956.

ARISTOTLE   —   Nicomachean Ethics, translated by
                          Martin Ostwald, The Library of Liberal
                          Arts: Bobbs-Merrill Co., In., 1962.

BRANDT, R.   —   'Traits of Character: A Conceptual
                          Analysis', American Philosophical
                          Quarterly, Vol. VII, No. 1, 1970.

CARNAP, R.   —   'Testability & Meaning', Philosophy of
                          Science, Vol. III, 1936, and Vol. IV, 1937.

                    —   'The Methodological Character of
                          Theoretical Concepts', Minnesota Studies
                          in the Philosophy of Science, Vol. I,
                          edited by Feigl and Scriven, 1956.

CRAIG, W.   —   'On Axiomatizability within a System',
                          Journal of Symbolic Logic, Vol. XVIII, 1953

| | — | 'Replacement of Auxiliary Expressions', <u>Philosophical Review</u>, Vol. LXXV, 1956. |

D'ALESSIO, B.C. — 'Dispositions', unpublished D.Phil. thesis deposited at the Bodleian Library, Oxford.

DUMMETT, M. — 'Truth', <u>P.A.S.</u>, Vol. LIX, 1958-9.

— 'The Philosophical Significance of Gödel's Theorem', <u>Ratio</u>, Vol. V, No, 2, 1963.

— 'The Reality of the Past', <u>P.A.S.</u>, Vol. LXIX, 1968-9.

ERWIN, E. — <u>The Concept of Meaninglessness</u>, John Hopkins, 1970.

FEIGL, H. — <u>The "Mental" and the "Physical": The Essay and a Postscript</u>, University of Minnesota Press, 1967 (amended version).

GLASGOW, W and PILKINGTON, G.W. — 'Other Minds on Evidential Necessity', <u>Mind</u>, Vol. LXXIX, No. 315, 1970.

HEMPEL, C.G. — 'The Theoretician's Dilemma', amended version published in C.G. Hempel, <u>Aspects of Scientific Explanation</u>,

The Free Press, 1965.

HEYTING, A.   —   'Intuitionism, North-Holland Publishing Company, 1956, revised edition, 1966.

LEVI, I and   —   'Belief and Disposition', American MORGENBESSER, S.   Philosophical Quarterly, Vol. IV, 1964.

LEWIS, D.   —   'How to Define Theoretical Terms', Journal of Philosophy, Vol. LXVII, No. 13, 1970.

MACKIE, J.L.   —   'What's Really Wrong With Phenomenalism?', Proceedings of the British Academy, Vol. LV, O.U.P.

MALCOLM, N.   —   'Behaviorism as a Philosophy of Psychology', Behaviorism and Phenomenology, edited by T.W. Wann, University of Chicago Press, 1964.

MAXWELL, G.   —   'The Ontological Status of Theoretical Entities', Minnesota Studies in the Philosophy of Science, Vol. III, edited by Feigl & Maxwell, University of Minnesota Press, 1962.

MOORE, G.E.   —   Ethics, O.U.P., 1912.

NAGEL, E.   — The Structure of Science, Harcourt,
                      Brace & World, Inc., 1961.

PUTNAM, H.   — 'Psychological Concepts, Explication,
                      and Ordinary Language', Journal of
                      Philosophy, Vol. LIV, 1957.

RORTY, R.   — 'Incorrigibility as the Mark of the
                      Mental'; Journal of Philosophy, Vol.
                      LXVII, No. 12, 1970.

RUSSELL, B.   — An Inquiry into Meaning and Truth,
                      Allen & Unwin, Ltd., 1940.

SHOPE, R.K.   — 'Dispositional Treatment of Psycho-
                      analytic Motivation Terms', Journal
                      of Philosophy, Vol. LXVII, No. 7, 1970.

SPRIGGE, T.L.S.   — Review of Analytical Solipsism by W. Todd,
                      Inquiry, Vol. XIII, No. 4, 1970.

STEVENSON. L.   — 'Are Dispositions Causes?', Analysis,
                      Vol. XXIX, No. 6, 1969.

THOMPSON, J.L.   — 'Third Possibilities', Mind, Vol.
                      LXXVIII, No. 310, 1969.

WIGGINS, D.R.P.   -   'Freedom, Knowledge, Belief and
Causality' in <u>Freedom & Necessity</u>,
edited by G. Vesey, Macmillan, 1970.

WIGGINS, D.R.P.   'Freedom, Knowledge, Belief and
Causality' in <u>Freedom & Necessity</u>,
edited by G. Vesey, Macmillan, 1970.