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SLOUCHING TOWARDS VIENNA:
MICHAEL DUMMETT
AND THE EPISTEMOLOGY OF LANGUAGE

DISSERTATION
Presented in Partial Fulfillment of the Requirements for the Degree
Doctor of Philosophy
in the Graduate School of the Ohio State University

By
Jon M. Cogburn, M.A.

* * * * *

The Ohio State University
1999

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Professor William Taschek

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N. Tennant
Adviser
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... Surely some revelation is at hand.
Surely the Second Coming is at hand.
The Second Coming! Hardly are those words out
When a vast image out of *Spiritus Mundi*
Troubles my sight... 

W.B. Yeats
ABSTRACT

Michael Dummett, Neil Tennant, and Crispin Wright all appeal to a priori restrictions on a philosophical explanation of linguistic competence (the "theory of meaning") to criticize classical logic and semantics. They also use these restrictions to defend verificationsim. In the negative part of my project I uncover interesting structural analogies between the Dummettian arguments for logical revision to show that they all require the truth of a stronger, and less plausible, form of verificationism than even the logical positivists were willing to countenance. This result, I argue, genuinely is a modus tollens from which we should conclude that the explanatory demands Dummett and others place on the theory of meaning are mistaken. I then draw from the linguistic, lexicographic, and psychological literature to undermine the Dummettian view that an explanation of competence should recursively correlate dispositions necessary and sufficient for grasp of meaning with the aspects of meanings generated by a compositional semantics. Thus is eliminated the Dummettian impetus to identify such dispositions with the ability to recognize verifications.

In the positive discussion I: (1) characterize and defend a conception of tacit knowledge which renders the postulation of the mental reality of classical semantics both explanatory and plausible, (2) show that many traditional philosophical questions concerning the epistemology of language can and should be recast as questions concerning how the theory of sentence meaning (compositional semantics) interacts with the theory of word meaning (lexical semantics), and (3) defend a roughly Davidsonian alternative to Dummett's theory of grasp of meaning.
Dedicated to Emily Beck
ACKNOWLEDGMENTS

I wish to thank my adviser, Neil Tennant. For many students, the most difficult thing about graduate school are the long periods of time between receiving positive feedback, encouragement, and suggestions for research avenues. Neil's students do not suffer this lack. Neil's commitment to philosophy and his students completely overshadows vanity; my philosophical disagreements with him have been nurtured. All of us who took courses with Neil during the composition of The Taming of the True count ourselves among the blessed. I have learned how to do philosophy from Neil.

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I thank William Taschek. At certain times I have needed someone to admonish me with Kripke's phrase, "there is no mathematical replacement for philosophy." If Neil introduced me to the joy of using and reflecting on formalisms, and Stewart the joys of an Aristotelian methodology in metaphysics and the philosophy of language, William constantly encouraged me to reflect on the limitations of formalisms and empirical theories and results. As with Stewart, the content of this monograph owes much to William. The bits of Quine and Davidson I love are Quine and Davidson as taught by William. The whole strategy of using the logical positivist's discussions
of verifiability to critically explore Dummettian anti-realism was suggested to me by William. Finally, philosophical conversations with William have been invaluable.

I thank Craige Roberts. Our discipline would be incomparably richer if every philosopher of language and mind took Craige's Montague Grammar and lexical semantics course sequences. Before taking classes from her I was ignorant and ignorant of my ignorance. My biggest regret upon graduating is that I cannot continue my linguistic studies here.

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Dummett’s challenge to classical truth conditional semantics rests upon the following three claims:

1. Knowing the meaning of a sentence requires knowing what it would be for that sentence to be true,

2. All that is involved in knowing the meaning of a sentence is knowing how to use that sentence correctly, and

3. Given 1. and 2., the assumption that all true sentences are verifiable allows us to provide the best explanation of knowledge of the meaning of a sentence.

Then, 1 Dummett goes on to argue that

4. The assumption that all true sentences are verifiable provides strong evidence for the incorrectness of classical truth conditional semantics, as well as evidence for the correctness of intuitionistic constructive semantics.

In this chapter I shall only be concerned with explicating the first three of these claims, and will not address the fourth until Chapter 3.

1 In some of his earlier work (e.g.: (Dummett (1959)) Dummett writes as if the notion of truth should be replaced with that of verifiability, but came to regard this as infelicitous. For example, On the way of putting it I adapted, one first proposes explaining meaning, not in terms of truth, but in terms of the condition for correct assertion, and then declares that, for statements whose meaning is so explained, the only admissible notion of truth will be one under which a statement is true when and only when we are able to arrive at a position in which we may correctly assert it. But, in that case, it would have been better first to state the restriction on the application of ‘true’, and then to have held that the meaning of a statement is given by the condition for it to be true in this, restricted, sense of ‘true’.

(Dummett, (1978, p. xxii))

Throughout, we shall follow Dummett’s later usage, presenting him as seeking to characterize truth a certain way, rather than replacing truth with something else.
For Dummett, the claim that knowledge of the truth conditions of a sentence is necessary for understanding that sentence is to be explicated with a “theory of reference” for the natural language in which the understood sentence occurs. Such a theory consists of a recursive syntax and compositional semantics; then, “what it would be for a sentence to be true” is taken to be the truth conditions assigned by the semantics to the sentence in question. The claim that correct use is necessary and sufficient for knowledge of meaning is to be explicated with a “theory of sense” added to the theory of reference. This theory will correlate with the truth conditions of the semantics a set of dispositions,\(^2\) possession of which are necessary and sufficient for understanding the natural language sentence correlated with that truth condition by the theory of reference.\(^3\) Finally, Dummett holds that these dispositions are a speaker’s ability to recognize verifications of the sentences.

\(^2\)One might rather say that the theory correlates truth conditions with descriptions of sets of dispositions, but if the theory is doing this, then it is also correlating truth conditions with sets of dispositions.

\(^3\)Strictly speaking, this is certainly false. Some of the most important semantic evidence concerns structural ambiguity of sentences such as “Frank signed the papers on the table.” Sentences such as this will have more than one truth condition assigned to them, and (typically) distinct syntactic structures as well. On the other hand, the same truth condition can be correlated with distinct sentences. So the ambiguity of single sentences and the equivalence of distinct sentences both work to make it false that possession of a set of dispositions which correlate with a truth condition could be sufficient for understanding of a sentence. Unfortunately, much of the philosophical writing on the theory of meaning (Dummett’s included) talks about understanding of first order logic, where the truth conditions of formulas of first order logic are taken to be truth conditions of sentences in question in a way that completely ignores the equivalence and ambiguity problems. I conjecture that this is because advocates of Davidsonian approaches to “the theory of meaning” largely assumed, not without reason at the time, that the architecture of a linguistic grammar would be that proposed in Generative Semantics, where the grammar started with a logical form, and then used transformations to derive a natural language sentence. As far as I can tell, a large portion of the philosophical community thought that such logical forms could be given in first order logic. This is a cozy view, since it allows philosophers of language to ruminate about grasp of first order logic, while linguists do the hard transformational work. However, it is a false view; nobody in the linguistics community, whatever other theoretical proclivities they might have (that is, even if they still believe in transformations) would dispute that old school Generative Semantics was falsified in large part because it attempted to transformationally derive sentences from logical forms. (See (Davidson & Harmon, 1975) for the canonical philosophical-theory-of-meaning-as-early-Generative-Semantics text, and (Huck & Goldsmith, 1995) as well as (Harris, 1993) for interesting accounts of the linguistic debates about deep structure and transformations in the 60’s and 70’s.)

In this chapter I will be fairly lazy about this issue, idealizing away from the problem where appropriate. However, in Chapters 4 and 5 I will return to it, showing that attending to it actually weakens the evidence for some of Dummett’s claims.
The purpose of this chapter is two-fold. First, I will present the evidence Dummett provides for equating knowledge of meaning both with knowledge of truth conditions and with the ability to use a sentence correctly. Then I will show how these considerations provide evidence for the truth of verificationism. This will involve examining Dummett's evidence for the claim that recognition of verifications is the kind of use necessary and sufficient for understanding.

§ 1.1 EXPLANATORY DESIDERATA AND TACIT KNOWLEDGE

Like others before him, Dummett is moved by our ability to understand a potential infinity of new utterances which we have never heard before. Following Davidson, Dummett holds that explaining this ability requires constructing a theory of meaning of which tacit knowledge can correctly be attributed to competent speakers. The Davidsonian argument can be represented in this manner:

1. We are able to grasp the meanings of a potential infinity of new utterances.
2. Our minds are finite.
3. Therefore, there must be some finitary process whereby, when hearing a new sentence, we determine what it means.
4. The existence of this process provides good evidence that the meaning of an arbitrary sentence is determined in a rule-governed way from a finite set of principles.
5. A competent speaker of the language can be said to have tacit knowledge of these principles and the way in which they determine what an arbitrary sentence means.

Thus, Dummett charges a theory of meaning with explicating the principles of which competent speakers can be said to have tacit knowledge. Such attribution is supposed to explain the ability to understand sentences.

4This argument did not commence with Davidson, though in the introduction to Seas of Language, Dummett credits Davidson with it. As far as I can tell, this argument was in the air in the early sixties as Chomsky's work in syntax first started to receive wide attention, and people began to think of using it to rigorously pursue semantics. Katz and Postal's influential 1964 An Integrated Theory of Linguistic Descriptions doesn't make any sense unless they and the researchers moved by it thought that von Humbolt's characterization of language as being that which is "infinitely generated by finite means" (the impetus for Chomsky's work in syntax) also applies to semantics.
To understand these desiderata we must first examine more closely the notion of tacit knowledge Dummett is working with. While Dummett’s actual views on tacit knowledge have changed quite a bit through the years\(^5\) it is possible to come up with a characterization of the concept which doesn’t run afoul of his later misgivings; albeit, this will require some work. However, we shall not be able to evaluate Dummett’s positive proposals without getting as clear as possible on an appropriate characterization of tacit knowledge.\(^6\)

A good place to begin this task is with the following brief passage from “Semantic Theory and Tacit Knowledge,” where Gareth Evans explicates the notion very clearly.

1. At the level of output, one who possesses the state of tacit knowledge that \(p\) is disposed to do and think some of the things which one who had the ordinary belief that \(p\) would be inclined to do and think (given the same desires).

2. At the level of input, one who possesses the state of tacit knowledge that \(p\) will very probably have acquired that state as a result of exposure to usage which supports or confirms (though far from conclusively) the proposition that \(p\), and hence in circumstances which might well induce in a rational person the ordinary belief that \(p\).  

\((\text{Evans, (1981, p. 336)})\)

For Evans, it is in this sense that it is correct to attribute to a competent speaker knowledge that a given claim (whether it be phonological, morphological, syntactic, semantic, or pragmatic) about her language is true, even though she may never have been explicitly taught the claim, and indeed may even be incapable of learning enough theory to explicitly understand it.

In syntax this is relatively unproblematic. For example, competent language users can distinguish between syntactically well-formed sentences, and groups of words that are not syntactically well formed. This complex ability can, to some extent, be explained by the postulation of tacit knowledge (in Evans’ sense) of a set

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\(^5\)See especially (Dummett, (1991, pp. 95-106)).

\(^6\)In the concluding chapter of this monograph, I shall be able to use the notion of tacit knowledge arrived at in this section to sketch a non-Dummettian picture of how a semantics can play an explanatory role \textit{vis a vis} linguistic abilities.
of recursive formation rules (axioms), such that a string of words is a sentence if and only if it is generated by the formation rules (and is thus a theorem of the syntax).\footnote{Nothing substantive which I say hinges upon accepting this characterization of syntactic theory, which doesn't best describe anybody's theory. I endeavor to make my substantive points invariant over distinct approaches to syntax, though, of course, this is not always possible.}

If the syntax is correct, then at the level of output a competent speaker will recognize the same sets of sentences as grammatical as are generated by the syntax. As well, Evans' characterization of tacit knowledge makes explicit how a competent speaker can be attributed tacit knowledge of a given syntax's postulation of, for example, the existence of specific word and phrasal categories, and the way in which these combine to make sentences. For example, if you ask a competent speaker of adequate cognitive capacity what is wrong with

*Happened is a pain in the neck.

she will at least tell you that "happened" is not the kind of word that can be followed by "is a pain in the neck." Likewise, one who knew a correct syntax for English, then she would make the same distributional judgment as the native speaker.

Thus, we can start to make good sense of how the attribution of tacit knowledge of a correct syntax to competent speakers is both explanatory and testable. Given Evans' characterization of tacit knowledge, we can represent it in this manner.

1. At the level of output, one who possesses the state of tacit knowledge that a correct syntax $S$ for a language $L$ is correct is disposed to make the same grammaticality and distributional judgments as one who has the ordinary belief that $S$ is a correct syntax for $L$ (given relevantly similar desires).

2. At the level of input, one who possesses tacit knowledge that $S$ is a correct syntax for $L$ will very probably have acquired that state as a result of exposure to grammaticality and distributional judgments which supports or confirms (though far from conclusively) the proposition that $S$ is a correct syntax for $L$, and hence in circumstances which might well induce in a rational person the ordinary belief that $S$ is a correct syntax for $L$.

Evans' characterization of tacit knowledge does provide plausible (if vague) necessary conditions for the correct attribution of tacit knowledge (at least when concerned
with syntax)\(^8\) of the truth of a proposition to a person. However, Evans' characterization certainly does not provide adequate sufficient conditions.

It is relatively easy to come up with situations where we would not say that a person \(A\) has tacit knowledge of a set of propositions, while both the input and output conditions of Evans' definition of tacit knowledge are satisfied. I will establish this by showing that Evans' characterization does not distinguish between a behavioristic (or, perhaps more appropriately, anti-cognitive) theory which solely predicts human behavior and a cognitive theory of which it is correct to attribute tacit knowledge to speakers. Then I will show how independent evidence for the falsity of behaviorism suggests a reasonable reformulation of Evans' characterization, one which can distinguish raw abilities (or know-how) from the kind of knowledge that is more appropriately attributed to users of a language.

Suppose that the person \(A\) has several irritating habits, including the inability to sit still, loud and incessant gum chewing, rude behavior to service personnel, loud playing of the stereo to the detriment of neighbors' well being. . . Now suppose that person \(B\) purchases a very well-received scholarly monograph titled \textit{Being Cretinous; An Operationalist Model}, which describes in great detail both the typical causes of \(A\)'s unsavory dispositions as well as the dispositions themselves. The monograph is not a how-to book, but rather a high-level psychological explanation of cretinous behavior. However, upon reading it, \(B\) gains great insight in how to make life less pleasant for those around him, and given that \(B\) shares this desire with \(A\), starts

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\(^8\) I need to make two caveats here. (1) It is probable that some strings of words which look pre-theoretically syntactically ill-formed cannot be blocked from generation by appeal to what we would intuitively regard as semantic or pragmatic information. Then perhaps the person who believed the syntax to be correct might also have to understand some of the language to make the correct grammaticality judgments. In any case, it is enough for what I'm saying that some fragment of syntax is autonomous enough, which is surely true. (2) On a certain interpretation of the input condition, the above claim is just false. From Chomsky's "poverty of stimulus" argument, discussed below, it follows that the usage children are exposed to is insufficient for determining a correct syntax. However, this just shows that the parenthetical claim in the input condition cannot be deleted. The behavior children are exposed to is such that they learn the particular language they learn, and thus does support the claim that a correct syntax for that language actually is correct.
to behave in relevantly similarly ways to \( A \). I want to claim that this situation, not entirely implausible, satisfies both conditions of Evans' characterization of tacit knowledge.

The relevant Evans style output and input conditions can be given in this manner.

1. At the level of output, one who possesses the state of tacit knowledge that \( \text{Being Cretinous; An Operationalist Model} \) is correct is disposed to do and think some of the things which one who had the ordinary belief that \( \text{Being Cretinous; An Operationalist Model} \) is correct would be inclined to do and think (given the same desires).

2. At the level of input, one who possesses the state of tacit knowledge that \( \text{Being Cretinous; An Operationalist Model} \) is correct will very probably have acquired that state as a result of exposure to usage which supports or confirms (though far from conclusively) the proposition that \( \text{Being Cretinous; An Operationalist Model} \) is correct, and hence in circumstances which might well induce in a rational person the ordinary belief that \( \text{Being Cretinous; An Operationalist Model} \) is correct.

Again, at the level of output, both \( A \) and \( B \) end up behaving the same way; given that they share a set of desires, the monograph ends up functioning as a how-to book for \( B \).

The clause for input is satisfied because, by assumption, the book in question contains some very good behavioristic explanations of the stimuli that bring about \( A \)'s behavior. These stimuli will consist of the characteristic behavior of others and the social consequences of this behavior. But the ability of the theory articulated in the monograph to predict conditions under which these stimuli occur as well as their characteristic consequences is part of the evidence that the monograph is correct.

Thus, if we take Evans' characterization of tacit knowledge to be correct, we are forced to say that \( A \) has tacit knowledge of the propositions expressed in \( \text{Being Cretinous; An Operationalist Model} \). However, it is very weird to say that \( A \) has tacit knowledge of psychological theory rich enough to adequately describe and predict \( A \)'s behavior.\(^9\)

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\(^9\)A simpler counterexample to Evans' characterization might involve a crane's ability to fly and the immense amount of physical theory and applied mathematics used in explaining this ability (I use cranes rather than birds generally because it is uncontroversial that teaching is involved
What is being pointed to is not merely an uninteresting instance of the standard philosophical problem of a definition's failure to be either necessary or sufficient.\textsuperscript{10} Rather, the insufficiency of Evans' account is interesting because it suggests that the notion of knowledge may in fact be irrelevant in the linguistic context we are concerned with. Perhaps we can eliminate the notion of knowledge altogether in an account of competence, and just satisfy ourselves with a behavioristic understanding of linguistic theory in the manner appropriate to Evans' input and output conditions. Some of Dummett's earlier writings in fact tend to suggest this is his view, as is admitted in this recent passage.

If linguistic competence could be straightforwardly classified as a practical ability, we could say, as I once did say, that in framing a meaning-theory we are giving a theoretical representation of a practical ability—the ability to speak the language. We are representing this complex ability as consisting in the knowledge of a theory, that is of an articulated structure of propositions. On this account, we are analyzing a complex of practical abilities by feigning to attribute to who has these abilities a knowledge of the theory. (Dummett, (1991, p. 105))

In this passage, Dummett goes on to admit that his earlier dogged insistence on seeing linguistic competence as only a matter of know-how yields an implausible form of behaviorism.

\textsuperscript{10}None of this dialectic commits me to the claim that the lexical norms involved in the words “knowledge” and “tacit” are such that by reflecting on their meanings we can correctly characterize “tacit knowledge” and then determine whether or not someone has tacit knowledge of a theory of meaning of their language. Rather, we are trying to characterize the notion of tacit knowledge such that it both serves the theoretical role Dummett and others want to assign it, and is also plausibly a species of knowledge. As with most attempts to find a satisfactory definition, given an antecedent purpose at hand, this process involves discovery as well as creation, along with the willingness to ignore “noise” (that is, in a philosophical context, attempt to pay no heed to hopelessly counterfactual counterexamples).
Rather than explicate Dummett's reasons for discomfort with his earlier views, I will here briefly examine four phenomena that might be taken to motivate cognitivism about linguistic competence. These are: (1) failure of empiricist accounts of acquisition, (2) the need to characterize the physical mechanisms of the brain in providing a characterization of psychological abilities, (3) the "privileged access" which competent (and reasonably intelligent) language users have in their ability to adjudicate the truth of claims about their language, and (4) the role of more common-place attributions of knowledge, in particular those involving normative assessment, in making competence attributions.

I have shown that Evans' characterization of tacit knowledge does not distinguish between a cognitive theory, which should be claimed to be known by the people it describes, and an anti-cognitive theory, which should not. Here I will first argue for a similar result concerning Chomsky's "poverty of stimulus" argument and neuroscientific computational aspects of thought. In fact, I argue that one can consistently deny all three planks of classical behaviorism while still refusing to characterize linguistic competence in epistemic terms. The three planks of classical behaviorism can be given in this manner: (1) linguistic abilities can be described in domain-general ways, in such a way that a description of the possession of these abilities is reducible to stimulus/response pairs, (2) linguistic ability is non-innate; acquisition of these abilities can be described as the outcome of general learning strategies involving rewards and punishments, and (3) realizability issues are wholly irrelevant, in that descriptions of a mechanism "computing" the stimulus/response pairs is irrelevant.

In what follows I claim that even if, moved by Chomsky's poverty of stimulus argument as well as the importance of realizability issues in neuroscience, one jetties-
sons classical behaviorism, one is not rationally constrained to deny that linguistic competence is just a practical ability. However, I will show that the third and fourth issues (the “privileged access” many speakers have to true claims about their language as well as the normative assessment of competence that the vast majority of speakers engage in) suggests a reasonable addition to Evans’ characterization. Thus, our inquiry in this section will suggest a reasonable reformulation of Evans’ characterization, a formulation which does not characterize raw abilities (or know-how), and which does characterize knowledge that is more appropriately attributed to users of language.

Given that the first concern, Chomsky’s poverty of stimulus argument, has been the most influential in combating behaviorism, it must be given some attention. While I will not argue that Chomsky was wrong in criticizing the view that a language can be characterized in terms of simple stimulus-response pairs, I will show that his argument does not entail a cognitive construal of competence, as he often seems to assume it does. The concern is very similar to the concern we have with Evans’ characterization. As I will show, the poverty-of-stimulus argument applies equally to the acquisition of a spider’s ability to weave a web. Again, we would not normally say that a spider has knowledge of a theory of web building. While (ironically) the third and fourth concerns I shall address might be taken to entail positions very close to those which Chomsky himself attempts to criticize, they are very helpful for defending a cognitivist construal of competence. As we shall see, they also suggest reasonable amendment of Evans’ definition.

Chomsky’s poverty of stimulus argument proceeds by pointing out that the stimulatory input available to a child while learning a language could only be sufficient to

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12Any time a view is attributed to Chomsky in what follows, insert a hedge of the form “it seems that.” Chomsky himself is a notoriously skillful hedger, and also notoriously skilled at answering criticisms of the form “Chomsky says A in all of these places, but A is inconsistent with B, which Chomsky says in all of these places,” with responses of the form “I never said A, I said A’ which is consistent with B.” For a discussion of a nearly limiting case of this kind of dialogue, see (Ney, J.A. (1993)).

13See especially (Chomsky, (1993, pp 18-27)).
predict the child's later linguistic dispositions if we assume that there exist domain-specific, innate, constraints on the learning process. This is probably most clear if we think of the child as a "little linguist," learning how to speak a language by observing adult behavior and making inductions about what is and isn't grammatical.

In classical behaviorism this inductive process was thought of as being on the one hand completely domain-general, in that the child was just thought of as perceiving whatever similarities she was exposed to and then generalizing from this exposure. On the other hand, this process was thought to be non-innate, in that systems of rewards and punishments were thought to be required for the requisite learning to take place. This view is false on both counts. First, learning mechanisms are innate, in that either no or very little operant conditioning is required for language acquisition. Second, learning mechanisms are highly domain-specific; inductions made by learners can only be regarded as being guided by true principles specific to that which they are learning. For example, a very young child will not apply count nouns to both inanimate and animate objects.

Now the brute fact that both semantic and syntactic aspects of language acquisition are in large part innate and domain-specific is not something that anyone could

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14 For one of the best general discussions of such constraints, see (Keil (1990)). It should be noted that endorsing Chomsky's poverty of stimulus argument does not entail an endorsement of whatever approach to syntax he is currently urging, for example, now one which attempts to model the syntax of a particular language as the result of constraining a universal syntax invariant over all languages. Then it is assumed the universal syntax is in some sense innately known by the child, and learning a language is learning which constraints to place on the grammar. For a critique of some of the guiding principles of Chomsky's Minimalism, as well as a criticism of the (according to the authors, pseudo-)scientific rationales Chomskyans give for these principles, see (Johnson and Lappin, 1995).

15 See (Skinner, 1957) and the third chapter of (Quine, 1960) for positions approximating this caricature.

16 See (Keil, 1994) and (Keil, 1990) for discussions the kind of domain-specific information children are best represented as utilizing. In (Keil, 1994) the author argues convincingly both that children are hardwired with a distinction between material objects, biological objects, artifacts, and people, and that these distinctions do not only play a strong role in acquisition of words for types of objects but also determine a set of a-priori beliefs children bring to the world. For example, children will accept that a coffee can is able to become a tree house, but reject that a tiger can become a cactus. Also, the domain-specific distinctions concerning types of causality have been shown to exist in children as young as four months.
responsibly question, and I don't intend to here. However, one should question the extent to which this requires what I have called a properly cognitivist construal of competence. Why can't one just say that the learning mechanisms are hard-wired, so that a child's behavioral dispositions are severely limited? Why must we say, with Chomsky, that the child has knowledge of a universal grammar, or has a priori knowledge of concepts which she attaches to words?

Chomsky often suggests a dichotomy between Skinnerian behaviorist accounts of acquisition (where the mechanisms are non-innate and domain-general) and cognitivist accounts (where the child is thought of as having a priori knowledge of linguistic theory). In a recent paper, after a discussion of the poverty-of-stimulus argument which contains the surprising suggestion that we possess innate grasp of the concept CARBURETOR, Chomsky writes,

However surprising the conclusion may be that nature has provided us with an innate stock of concepts, and that the child's task is to discover their labels, the empirical facts appear to leave open few other possibilities. Other possibilities (say, in terms of "generalized learning mechanisms") have not, to my knowledge, been coherently formulated. . . Since these facts are known essentially without evidence, it must be that the child approaches language with an intuitive understanding of concepts involving intending, causation, goal of action, event, and so on, and places the words that are heard in a nexus that is permitted by principles of universal grammar, which provide the framework for thought and language, and are common to human languages as conceptual systems that enter into various aspects of human life.

(Chomsky, 1996, p. 574)

The possibility which Chomsky doesn't explore involves the affirmation (with Chomsky the rationalist) that learning mechanisms are domain-specific and innate, coupled with the refusal (pace Chomsky) to take this as evidence of innate knowledge.

Even Frank Keil, one of the leading researchers in the current wave of anti-behaviorist developmental research, is very clear that options are not either old-
school behaviorism versus rationalism. In a recent article he writes,

I am reluctant to grant spiders intuitive theories of the mechanisms of physical lattices like webs, even though their behavior displays a precise honoring of such principles. Similarly, cockroaches and other cognitively "simple" creatures also seem to pick out objects and follow their trajectories and the like, yet one cringes at calling them object theorists... Thus, there are some unresolved questions concerning what constitutes evidence for theory versus less belief-laden systems of representation. (Keil, (1990, p. 152))

Thus, the proper anti-cognitivist response to Chomsky is to argue that what Chomsky calls innate knowledge of universal grammar and a-priori possession of concepts, are really just innate stimulus/response dispositions instinctually "hard wired" into people with a high degree of domain specificity. Then universal gram-

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17One interpretation of Keil's work is that it suggests that acquisition of word meaning goes through three stages. The first stage is largely non-criterial. The child forms an inductive class from perceiving one or more uses of the words; this process, as well as beliefs acceptable to the child involving the words in question, are guided by innate domain-specific dispositions. However, the inductive class often does not conform to the actual class of objects the word picks out. For example, for a few years, the word "uncle" will pick out men of a certain age who are on good terms with the family. Then there is a characteristic shift, where the child focuses on definitional criteria for using the word. At this stage the child will adamantly only call her parents' brothers "uncle." Finally, after this stage, much richer holistic and theoretical criteria are used to determine whether or not things are in the extension of the word (consider "atom" and "finite"). At this stage, the reference of words like "uncle" or "bachelor" once again admits instances which are determined not to be in the denotation by the earlier definitions.

So what is the concept "uncle"? Is it the word meaning appropriate to the usage at the first, second, or third stage of development? At the third stage, the indeterminacy between word meaning and associated theory is highly salient. (I take this indeterminacy to be the important upshot of the second chapter of (Quine, 1960).)

That Chomsky would endorse this view is quite irritating, given that he uses something very much like Quinean indeterminacy to argue against truth-conditional semantics (Chomsky, (1993, pp. 23-25)). Chomsky doesn't appreciate that use of denotational semantics to explain aspects of the syntax and semantics of mass terms like "water" entails nothing about whether or not this or that philosophical view of reference is correct. The inferential role of the word "water" isolated by a compositional semantics of mass terms is the same for the biochemist in her lab, talking about isolated, gaseous H₂O, and the fisherman pointing to the muck in Lake Erie. Quine's insights might cause a problem for Chomskyans committed to the determinacy of concepts and the tie of concepts to word meanings, but they have no immediate bearing on truth-conditional semantics. Missing this point requires a gross misunderstanding of contemporary semantic research.

18See (Pinker, 1994) for an argument that something like this is correct. Chomsky's philosophical work is full of animadversions against dispositionalist explanations of linguistic understanding. He complains that the notion of a disposition has not been shown to play a helpful role in syntactic theory proper. While he may be correct here, it is also true that the notion of the human brain has not been shown to play a helpful role in syntactic theory proper, and Chomsky contrasts the
mar itself, as well as any theory of concepts accompanying it, is treated as an abstract
description of mechanisms which effect a severe limitation on the kind of dispositions
human beings can bring to the acquisition process.

Therefore, Chomsky’s poverty of stimulus argument is no help in discerning a
notion of tacit knowledge according to which linguistic competence can correctly
be said to consist in such knowledge of a theory of meaning for the language. The
falsity of old-school behaviorism does not entail the truth of cognitivism. Perfectly
consistent with the falsity of old-school behaviorism is the view that language acqui-
sition is the result of highly domain-specific instincts, instincts of the sort that do
not require attribution of any kind of propositional knowledge, tacit or otherwise.

One might take the second primary challenge to behaviorism in the sciences of
the mind to be relevant in helping to discern a better characterization of tacit knowl-
edge. The somewhat cartoonish version of early behaviorism we are considering here
didn’t just suffer the inability to countenance innate and domain specific constraints;
it also maintained a dogged insistence that any mechanisms involved in producing
dispositions were strictly irrelevant to psychological characterizations. The behav-
iorist was merely to characterize stimulus-response pairs, and to treat mechanisms
causally efficacious for these dispositions as irrelevant. However, along with Chom-
sky’s rationalist rejection of behaviorism is what we can refer to as the computational
rejection of behaviorism.

The computationalist’s guiding metaphor is the digital computer. Thus, linguistic
behavior is characterized as being the ‘output’ of a program realized in the human
brain. While a computationalist could restate Skinner’s “black box” view by holding
that the proper study of linguistic competence does not involve studying the relevant
hardware (presumably the human brain), she might also hold that study of the
relevant hardware is a good guide of the computational architecture involved in
dispositional view with his view of the universal grammar as being realized in the “mind/brain.”
(If (Johnson & Lappin, 1995) is correct, then Chomsky’s specific mentalistic views of acquisition,
to the extent that they have motivated Minimalism, might actually have played a very damaging
role in linguistic theory).
linguistic competence. As I will show, evidence involving different sorts of aphasia does provide evidence for such a view.

Aphasics are people whose linguistic abilities are defective in various ways because of trauma to the brain. Moreover, these defects can be highly specific. Some of these are localizable in common places for most people. People with damage to Broca's area typically have severe inability to construct sentences. Damage to Wernicke's area does not cause problems with the construction of syntactically well-formed sentences. However, such aphasics typically compulsively speak word-salad and also have great difficulty naming objects. People with damage to the connection between Broca and Wernicke's areas typically cannot repeat sentences when asked to. People with the Broca and Wernicke's area intact, but severed from the rest of the cortex, can typically only repeat sentences they've heard, with no sign of comprehension. Damage to the area between Wernicke's area and the angular and supramarginal gyri often causes loss of the ability to speak or understand most nouns (exceptions involving gerunds, pronouns, and a few generic nouns). In addition to localization common to most speakers, there is much evidence that other grammatical functions are localized, but in areas of the brain very different for different speakers. A wide variety of extraordinarily specific malfunctioning has been observed, including: lack of ability to understand speech, coupled with retention of the ability to read, speak, and write, and lack of ability to speak or process very specific syntactic constructions or word groups (such as verbs, classes of nouns, function words, and so-called trace constructions such as "The man who the woman kissed hugged the child").

While such evidence does support the claim that human brains, in some sense, "compute" language in characteristic ways, I don't think that this form of anti-behaviorism is any more helpful for the cognitivist. Our questions involve the extent to which such data in particular, and computationalist metaphors in general, are

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19 This discussion is taken from (Pinker, (1994, pp. 297-331)).
helpful both in defending the claim that linguistic competence is a species of proposi-
tional knowledge and in successfully characterizing criteria for something counting
as tacit knowledge. The answer to both questions is that it is not very helpful.

As with the poverty-of-stimulus argument, there seems to be no reason to char-
acterize these phenomena as involving propositional knowledge rather than simply
practical abilities. Other human abilities, for example, those involving the sensory
modalities, also involve a significant amount of localization in the brain. Moreover,
a computationalist model of vision is at least as defensible as a computationalist
model of language abilities, and, again, it is very misleading to characterize people
as having tacit knowledge of a theory of, say, the physical and biological principles
involved in color discrimination and depth perception. Once again, the falsity of old
school behaviorism does not entail the truth of cognitivism.

The third kind of issue I shall call attention to does provide evidence for an
epistemic construal of linguistic competence. It concerns a sort of “privileged access”
many competent speakers have concerning true generalizations of their language. In
some of his earlier work, Dummett often states that the distinguishing feature of
tacit knowledge is the constraint that one can only properly be attributed tacit
knowledge of a proposition $P$ if one is able to recognize $P$ as correct when presented
with an explicit statement of $P$. For example,

...we have to take more seriously the ascription of knowledge to some-
one who possesses the practical ability in question: “knows how to do
it” is not here a mere idiomatic equivalent of “can do it”. Rather, we
may say of the agent that he knows that certain things are the case,
that he knows certain propositions about how the operation is to be
performed; but we need to qualify this by conceding that his knowledge
is not explicit knowledge, that is, knowledge which may be immediately
elicited on request. It is, rather, implicit knowledge: knowledge which
shows itself partly by manifestation of the practical ability, and partly
by a readiness to acknowledge as correct a formulation of that which is
known when it is presented. (Dummett, (1978b, p. 96))

This recognitional ability is best explained by positing a kind of knowledge.
In *For Truth in Semantics*, Anthony Appiah argues that most of our everyday beliefs have this property which Dummett calls attention to.

...someone can believe something—that there is, now, a war in Namibia—even when they are currently unaware that they believe it. Not only do I think this is obviously correct—consider a sleeping member of the South African Defense Force, or a waking British Prime Minister—but I think many of us have beliefs that are not only thus unconscious sometimes, but unconscious all our lives. If I had never thought about the matter, I would not know that I believe that the car I drove last week had a hand-brake. I used the brake in parking, and released it in starting; and that use was guided by my belief. But at the time, I did not need to bring this belief to consciousness; if I had not been looking for a philosophical example I might never have done so...

(Appiah, (1986, p. 7))

Given that most of our beliefs are not brought to consciousness, but are such that if queried we would confess to them, if principles true of our language had this property, we would have good evidence that we in some non-conscious manner believe the true theoretical claims about of our language.²⁰

However, some caveats are in order. It cannot be denied that many people who are certainly competent language users are probably incapable of learning enough linguistic theory to even understood many of the claims true of their language. Dummett did hold, in much of his work, the conjunction of the views that: (1) people are linguistically competent in virtue of tacit knowledge of a correct theory of meaning for their language, and (2) a necessary constraint on attribution of tacit knowledge of a proposition $P$ is the ability to recognize that $P$ is true when presented with an explicit statement of $P$. These two views together thus have the consequence that people who can't learn linguistics are not linguistically competent. If we mean to apply “linguistically competent” to everyday speakers (who possess the ability we

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²⁰One might object that the Appiah's considerations just get us to some sort of tacit belief in linguistic claims and not tacit knowledge of them.

However, since the position that one could have a false tacit belief in a principle of her language is of dubious coherence, Appiah's considerations do motivate the kind of cognitivism Dummett wishes to defend.
are trying to explain) then this is an absurd result. Therefore, this characterization of tacit knowledge is unacceptable as it stands.

In his later works, Dummett himself recognizes the problem with this view. For example:

The concept of implicit knowledge is of little assistance here. The term should properly be reserved for knowledge which its possessor is incapable, unaided, of formulating verbally, but of which he can recognize a formulation when presented with one.

Dummett then goes on to admit that competent speakers should not be attributed this kind of knowledge of linguistic theory. He writes,

...A piece of implicit knowledge may perhaps be attributed to someone who has only an implicit grasp of the concepts involved. If a speaker always uses the pairs 'I'/‘me’, 'he'/‘him’, 'she'/‘her’, 'who'/‘whom’ correctly, but, never having been taught the rudiments of formal grammar, has never heard the words ‘nominative’ and ‘accusative’, can he be said to have an implicit grasp of the concepts they express? A statement of the rule he tacitly follows will involve an explicit formulation of those concepts and will necessarily be somewhat lengthy. Still we may credit the speaker with an implicit knowledge of that rule, provided that, when he understands the statement of it, he acknowledges it as accurately describing his existing practice. The concept of implicit knowledge is not infinitely elastic, however: if we try to stretch it to cover our whole knowledge of our native tongue, it will snap. An explicit statement of the principles governing the use of the language will amount to a meaning-theory. It would be preposterous to suggest that all competent speakers would recognize such a theory as correct if it were presented to them. Most would not understand it: those who did would probably engage in disputes, far from easy to resolve, over whether it was correct. (Dummett, (1991, pp. 95–96))

Unfortunately, after stating this misgiving in The Logical Basis of Metaphysics, Dummett’s comments on what kind of knowledge constitutes linguistic competence are extraordinarily sketchy. However, the problem is that the purported definition of tacit knowledge does not specify necessary conditions (when we understand tacit knowledge of a meaning theory to be necessary and sufficient for linguistic competence).
On the other hand, people who are sufficiently cognitively situated are capable, when presented with claims true of their language, of recognizing that such claims are true. This fact both licenses the introspective method of much contemporary linguistics, and supports the claim that some form of knowledge is involved in linguistic competence. Of those who are not capable of recognizing true linguistic claims, it is still sensible to maintain that, were they better cognitively endowed, they would be able to recognize the generalizations.

As recognition is a success verb, we must be very careful. It should be clear that the process involved in recognition is defeasible. Thus, competent speakers might falsely take themselves to recognize a false claim as being true. Though Dummett does not discuss this issue we can cope with it by understanding our attribution of the recognitional capacity to involve holding our idealized speakers to be such that, were they presented with a false generalization and a true generalization attempting to account for some range of phenomena, they could distinguish the false from the true.

This recognitional capacity suggests amending our definition of tacit knowledge in this manner.

1. At the level of output, one who possesses the state of tacit knowledge that \( P \) is disposed to do and think some of the things which one who had the ordinary belief that \( P \) would be inclined to do and think (given the same desires).

2. At the level of input, one who possesses the state of tacit knowledge that \( P \) will very probably have acquired that state as a result of exposure to usage which supports or confirms (though far from conclusively) the proposition that \( P \), and hence in circumstances which might well induce in a rational person the ordinary belief that \( P \).

3. One who possesses the state of tacit knowledge that \( P \) is such that, with some finite (possibly null) extension of their cognitive capacities, she can recognize that \( P \) is true when presented with an explicit statement of \( P \).

Now I believe something like this is right, as far as it goes, and that it is suggested by Dummett's problem with his earlier view. However, this definition is not without
its own problems. Moreover, these problems are once again precariously similar to the problems that beset Evans' original characterization. Consider a crane. If its cognitive capacities were extended enough, it would be able to recognize that propositions of aerodynamics were true. So this characterization again seems to fail to provide sufficient conditions for possession of tacit knowledge.

Thus, from Dummett we have that if the third clause in the above definition is not idealized the definition does not provide necessary conditions. Our problem is that if the third clause admits too much idealization, then the definition is again not sufficient.

Clearly, one who wants to use the above characterization to claim that linguistic competence is a species of knowledge must say something informative and not obviously circular about which kinds of idealizations are appropriate. However, I do not wish to say anything too dogmatic or even specific about what ought to count as a proper idealization in linguistic theory. Instead, by calling attention to clearer cases where knowledge claims are involved in linguistic competence, I will simply suggest one manner in which the idealization adverted to in the third clause can be taken to be licit.

The fourth consideration I shall call attention to does support and help make sense of the view that all linguistic competence involves an epistemic component (the view that competence is held in virtue of tacit knowledge of a theory of meaning). This involves attending to clear cases where certain kinds of linguistic competence involve an epistemic component. For example, lexical competence concerning word meaning is so intertwined with theoretical knowledge of true sentences involving the words that one could reasonably argue that it is impossible to make any kind of principled distinction between lexical competence and encyclopedic knowledge.21

Another kind of example concerns the role of beliefs in actions. Intentional actions are paradigmatically explained by appeal to a set of desires and a set of beliefs held

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21 Again, I take this to be one of the important upshots of the second chapter of (Quine, 1960).
by the agent. Analyzing conversational contributions as intentional actions typically involves attributing to the actors several beliefs concerning both their interlocutor and their language.\footnote{The classic discussion can be found in (Grice, (1989)).}

The relevance of these sorts of epistemic components is that they are very close to standard everyday sorts of non-conscious knowledge or belief (e.g. Appiah's car example), and thus make it more plausible to attribute tacit knowledge of other, less easily retrievable, facts. Appiah gives the example of speaking more slowly to someone who has English as a second language. Most people would be able to recognize that they did this because they believed that people process information more slowly when it is not heard in their first language.

So there are clear cases where linguistic competence requires knowledge. Thus far we have only considered two species of such knowledge: (1) "world" knowledge necessary for lexical competence, and (2) knowledge relating information about an interlocutor with how to communicate information. However, these at best only license cognitivism in certain areas of lexical semantics and pragmatics. This still leaves untouched phonology, morphology, syntax, and compositional semantics. We must consider whether or not more ordinary attributions of knowledge are involved in competence in areas such as these. In particular, given our present focus, does anything like this hold for syntax and semantics?

Where $v$ ranges over English verbs, $vp$ ranges over verb phrases, $np$ ranges over English noun phrases, $(p)np_1$ ranges over the result of pluralizing the head noun in $np_1$, and $vpe$ ranges over extensional verb phrases, nearly any competent speaker of English will assert instances of the following schema

I know that "$v$" cannot come before "$vp$" in a sentence.

(E.g. "I know that "happened" cannot come before "went to the store before lunch
feeling very upset about the crisis in Kashmir " in a sentence.")

I know that if \( np_1 \) and \( np_2 \) name the same thing(s), then
\[
(p) \; np_1 \; up_e \; if \; and \; only \; if \; (p) \; np_2 \; (do) / \; does \; too.
\]

(E.g. “I know that if “rational animal” and “featherless biped” name the same things
then rational animals walk if and only if featherless bipeds do too.) Of course,
not all such everyday “meta-linguistic” behavior of competent speakers concerns
discussion of specific word and phrasal categories. Another important example of
this is ambiguity resolution, at which the vast majority of competent speakers are
skilled, and which also involves quoting whole sentences. Consider the following two
monologues, which are not at all implausible.

He said “Latoya and Michael washed ten cars.” I know that means
either that ten cars got washed or twenty cars got washed. Dang!
Which one is it?

He said “Frank signed the papers on the boat.” I know that means
either that the papers on the boat now are the ones Frank signed,
or that Frank was on the boat when he signed some papers. Dang!
Which one is it?

Furthermore, speakers don’t only make correct knowledge claims about language,
they also remonstrate with other speakers who doubt such claims.

But now we can ask, with what justification do speakers engage in such remon-
stration? What justifies each of the above knowledge claims, both transparently
(certainly at least in part) about language? A correct syntax and semantics for
English will predict all instances, respectively, of the above two embedded schemas,
as well as predict the distinct truth conditions corresponding to the ambiguous sen-
tences. Thus, one plausible thing to say is that speakers who make such knowledge
claims, and state instances of the embedded claims while remonstrating with others,
are justified in doing so by the fact that the correct syntax and semantics for English
predict they are correct in making the claims they make about their language. This
does provide some evidence for taking people to have tacit knowledge of syntax and
semantics.

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I'm not saying that whenever we don't have explicit knowledge of the justifiers of our claims we must then be attributed tacit knowledge of such justifiers. However, the justificatory role syntax and semantics plays with regard to competent speakers' knowledge claims about language does suggest a reasonable way to limit the counterfactual in the third clause of the above attempted definition of tacit knowledge. My suggestion is that what licenses the idealization of cognitive capacities in the above definition is that the proposition that would be known under the idealization must function as a justifier for normative assessments and correct knowledge claims that the tacit knower does, or is willing to, make. Therefore, we can say that A tacitly knows $P$ if and only if:

1. At the level of output, one who possesses the state of tacit knowledge that $P$ is disposed to do and think some of the things which one who had the ordinary belief that $P$ would be inclined to do and think (given the same desires).

2. At the level of input, one who possesses the state of tacit knowledge that $P$ will very probably have acquired that state as a result of exposure to usage which supports or confirms (though far from conclusively) the proposition that $P$, and hence in circumstances which might well induce in a rational person the ordinary belief that $P$.

3. One who possesses the state of tacit knowledge that $P$ is such that, with some finite (possibly null) extension of their cognitive capacities, she can recognize that $P$ is true when presented with an explicit statement of $P$.

4. The idealization of cognitive capacities in 3. must be licensed by the tacit knower's behavior, in that $P$ must function as a justifier for normative assessments and knowledge claims that the tacit knower does, or is prepared to, make.

I think this does limit the idealization in a reasonable way.

Moreover, I take it that this added component is what moves many linguists to characterize linguistic ability as a species of knowledge. A typical example is the introductory section of Peter Cullicover's new syntax textbook, which begins with
the poverty-of-stimulus concern.

It is hard to suppose that knowledge of a given language, such as English, is present at birth. Why wouldn't everyone grow up speaking the same language, then? On the other hand, no one has yet been able to demonstrate that general learning mechanisms that are not specific to language acquisition could acquire human languages in all their richness. (Cullicover, (1997, pp. 3-4))

However, he goes on to appeal to the fact that linguistic theory justifies many propositions about language held true by competent speakers.

One main reason for this is that there are many things that we know, as native speakers, for which there appears to be no evidence in the experiences that we have as language learners. For instance, we know that the question in (2b) is ungrammatical, while the question in (2a) is grammatical.

(2a) Who did you buy a picture of?
(2b) *Who did you buy Mary's picture of?

It does not appear that children are provided with specific information during the course of language learning that will indicate to them the relative grammaticality of such examples. In fact, it does not appear that children are provided with any systematic information about the ungrammaticality of particular examples. (Cullicover, (1997, p. 4))

Thus, nestled into a statement of the poverty-of-stimulus argument is the role of nontheoretical claims about language that competent speakers do know.

One could object to my characterization in one of two ways. The first kind of objection would be to argue that P's playing a justificatory role vis a vis true knowledge claims a person makes is not good evidence that that person tacitly knows P. One could even cite externalist theories of justification for other kinds of knowledge claims in epistemology as evidence that it is mistaken to assume that a person has tacit knowledge of the justifiers of their beliefs. While such an objection may be true, when considering the wide variety of true knowledge claims a person makes, I take it that the burden of proof is on the person making this objection to at least

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See chapter 3 of (Goldman, 1986) for a discussion of externalism in epistemology.
sketch a coherent and plausible externalism about true linguistic knowledge claims. Linguists' epistemological talk is somewhat at odds with such a position. For example, linguists describe a sentence as being grammatical only if an ideally competent speaker would judge it so. This is extraordinarily similar to the Aristotelian heuristic of an action being good only if an ideally intelligent agent would judge it so. Thus, the philosopher of language who wants to "save the phenomena" and make sense of epistemological talk of linguists, is likely to be drawn to virtue theory epistemology. However, this is a very weak argument against the externalist, unless one could independently argue that an externalist epistemology cannot in some manner play the same justificatory role which the linguist's virtue-theoretic talk does. I have no such argument.

On the other hand, if such an externalist account of tacit knowledge of grammatical theory could be motivated, it might make no difference for our purposes. Though it may be very important in epistemology proper to not say that the justifiers of \( P \) are in any straightforward way knowable by the person justified in believing \( P \), it is not at all clear that this is a problem in our context. That is, it might ultimately be the case that a plausible enough behavioristic account (motivated by a dogged externalistic insistence on reading something like constraints on reliable processes off the statements of linguistic theory) could be thought of as actually justifying the cognitive account presented above. The cognitivist could always respond that, in the context of the philosophy of linguistics, treating the constraints on externalists' reliable processes as tacitly known is more continuous with the practice of the relevant group of scientists, but this would be to view the externalists' theory as justifying the above definition. However, it must be admitted that we are at something of an impasse, prior to much more work at the intersection of epistemology and linguistics.

\(^{24}\)Not to be confused with Burgean or Putnamian externalism about concepts or certain expressions. See (Putnam, 1975), (Burge, 1982), (Burge, 1986), (Burge, 1986), and (Burge, 1989).

\(^{25}\)See (Zagzebski, 1998) for a recent development of virtue theory epistemology. In (Cogburn, 1998) I show how virtue theory can be utilized to defend the coherence of arguments for logical revision.
The continuity of our definition with characterizations by linguists at least renders our account more plausible prior to the development of an externalist alternative.

Another criticism involves arguing that the above definition is not necessary for tacit knowledge attributions (on the assumption that tacit knowledge is explaining competence), perhaps by pointing out that some people who are not linguistically deficient do not, and are perhaps unable to, either make knowledge claims about language or correct other's usage. In the first place, I think we would characterize people who are incapable of correcting other's usage or making true knowledge claims about their language to be linguistically deficient. In the second place, I'm not convinced that a characterization of tacit knowledge needs to prevent counterexamples such as this. It is enough if our characterization holds of the vast majority of people whom we call linguistically competent. Then, family resemblance considerations can pick up wherever the criteria fails. If my characterization does hold of the vast majority of people who are linguistically competent, and if a small minority are relevantly similar to those characterized but lack one of the criterial features in the definition, then the characterization is successful enough. Deviant cases, where it is clear that something relevant to the definiendum is deviant, do not normally count against the correctness of a definition.

The cases considered above were not borderline cases. With all of the cases I used in trying to show that given characterizations of tacit knowledge weren't sufficient, it was very clear that the creature in question did not have any kind of propositional knowledge, tacit or not, of propositions of the theory under consideration. Evans' characterization was shown to entail that people have tacit knowledge of psychological theory and, prima facie, that cranes have the tacit knowledge of aerodynamics.

We considered whether or not the fact that linguistic abilities were innate, domain-specific, and dependent upon physical realizability could be used in reformulating Evans' characterization. Given that instincts such as a spider's ability to weave webs are also innate, domain-specific, and dependent on physical realizability, it
was totally unclear how the falsity of behaviorism provided evidence for constructing linguistic abilities as a species of knowledge. Any change of Evans’ definition motivated by the anti-behaviorist facts would thus seem to also have clear counterexamples (such as the spider’s ability to weave webs) showing the new definitions not to be sufficient for tacit knowledge. The envisioned requirement that speakers have implicit knowledge (in Dummett’s sense) forced the definition to be non-necessary. Very few competent speakers have implicit knowledge of contemporary linguistics. Modalization of the notion (requiring speakers to be such that if their capacities were idealized, then they would be able to recognize the truth of the tacitly known claim) again rendered the definition non-sufficient. Again, if a spider were much smarter, then it would understand the theory of web building.

What is important is that none of these cases is borderline; each is a clear counterexample to a proposed definition. On the other hand, if there really could be a speaker who communicates and uses language fairly well, but is unable to engage in normative assessment of language use, this should, I think, be considered a borderline case of someone semi-competent.

We have gone slightly deeper into this notion than we shall need to in order to make sense of Dummett’s challenge, which involves specific characterizations of tacit knowledge of a meaning theory, rather than the generic notion of tacit knowledge itself. In fact, we will not need to reexamine our generic definition in detail until Chapter 5. I remarked earlier that in recent work Dummett’s comments about the epistemic component of linguistic ability have been extremely sketchy. As we shall see, this is in part because Dummett’s specific challenge and program don’t require a precise definition of the generic notion of tacit knowledge, but rather just a specific characterization of tacit knowledge of a meaning theory. Moreover, my main criticisms of Dummett do not rely on this account. It is not until our positive non-Dummettian discussion (in the concluding chapter) of how one can be said to have tacit knowledge of semantics that this definition looms large. Nonetheless, with
this rough characterization of tacit knowledge we can better understand Dummett's demand that tacit knowledge of a theory of meaning explains a speaker's competence with language.

Though our broader topic is what the theory of meaning must be like so as to do this explanatory work, the mere claim that tacit knowledge of the theory and a speaker's competence are equivalent is strong enough to get his main argument off the ground. Thus, where tacit knowledge of a theory is understood as based on tacit knowledge of some suitably large portion of the propositions composing the theory, we impose the

**Tacit Knowledge Requirement**
A speaker $S$ is competent in the language $L$ if and only if $S$ has tacit knowledge of a correct theory of meaning for $L$.

In fact, it is this desideratum that gives rise to one of Dummett's most substantive philosophical claims. In much of Dummett's work he is concerned with arguing that one cannot accept the above and still affirm the following proposition, which he attributed to Donald Davidson.\(^2\)

**Minimal Output Requirement**
A speaker $S$ of language $L$ can correctly be attributed tacit knowledge of the theory of meaning $M_L$ for $L$ if and only if it is not possible to have explicit knowledge of $M_L$ and fail to be a competent speaker of $L$.

Instead of this position,\(^2\) Dummett holds that the theory of meaning must actually correlate relevant sets of dispositions with the propositions of the syntax and semantics (Dummett calls the syntax and semantics "the theory of reference"). For Dummett, a speaker has the set of dispositions correlated with a proposition of the

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\(^2\)Dummett takes this attribution back in (Dummett, 1993, pp 23-33)) and the fourth chapter of (Dummett, 1991). The holistic, non-modest theory Dummett attributes to Davidson in these passages will be addressed in the concluding chapter of this monograph.

\(^2\)Dummett is best read as attacking this, though sometimes his position is unclear. In section 1.3.2, I show that Dummett later admits that endorsing this position led him into error. Moreover, Dummett does argue that the Minimal Output Requirement is, on its own, insufficient throughout his work.
theory of reference if and only if the speaker has tacit knowledge of that proposition. Dummett's arguments for such a claim are fairly subtle so I will present them in outline in this section and then go on to flesh them out in the next two sections.

Dummett argues that explicit knowledge of a theory of meaning (here identified with a syntax and semantics) can be had in two ways. In the first way the truth conditions generated by the semantics in question needn't be stated in a language understood by the person who explicitly knows the semantics. That is, if the relevant notion of explicit knowledge of the syntax and semantics allowed someone to know them in virtue of just having certain mathematical facility with them, the Minimal Output Requirement would not be jointly satisfied with the Tacit Knowledge Requirement. Given that recursiveness (or at least recursive enumerability) is a strong desideratum in syntax and semantics, it is clear that one could be adept at deriving sentences and corresponding truth conditions without understanding the language in question. But then the Minimal Output Requirement entails that a speaker of the language cannot be attributed tacit knowledge of the theory of meaning. So by the Tacit Knowledge Requirement, the theory of meaning is incorrect. This dialectic is explored in section 1.3.1.

On the other hand, if the attribution of explicit knowledge of the theory of meaning is understood to be restricted so that the syntax and semantics must be stated in a language understood by the explicit knower, then the Minimal Output Requirement would be satisfiable by a syntax and compositional semantics, but then (on the assumption that the syntax and semantics are the theory of meaning) the theory of meaning would at best model the process of coming to know a second language: This dialectic is explored in section 1.3.2.

Dummett suggests that the way out of this impasse is for the theory, in a principled way, to recursively correlate relevant sets of dispositions necessary and sufficient for grasp of meaning of a sentence with the meanings generated by the compositional
semantics.\textsuperscript{28} He calls the part of the theory of meaning that does this the theory of sense. Thus, Dummett urges the following output requirement.

\textbf{Manifestationist Output Requirement}

A person can correctly be attributed tacit knowledge of a theory of reference for \( L \) if and only if the person’s behavior conforms to the dispositions predicted by the theory of sense for \( L \).

Given that the theory of reference is compositional, that is, shows how the meaning of a complex expression is dependent upon its parts, Dummett’s great hope is that a theory of grasp of meaning will correlate with the axioms of a theory of reference a set of dispositions and then generate a recursion isomorphic to the recursion of the theory of reference, in such a way that for each sentence a set of dispositions necessary and sufficient for grasping the sentence in question is generated. However, understanding what he has in mind requires careful consideration of his arguments.

\section*{1.2 Compositionality and the Theory of Reference}

In the early 60’s many philosophers and linguists began to take seriously the idea that many kinds of linguistic knowledge can be represented in formal, generative theories. Since the broadest types of linguistic abilities correspond to the different kinds of speech acts such as asserting, requesting, commanding, etc., one might think that the proper place to begin such a theory is with a general account of such speech acts. For various reasons that properly extend the present discussion, however, Dummett holds that a general account of such speech acts will require an antecedent theory of the meaning of assertions in the indicative mood.\textsuperscript{29} Thus,

\textsuperscript{28}This, of course assumes that for each sentence of the language there does exist a relevant set of dispositions, possession of which is necessary and sufficient for understanding of that sentence. Dummett’s arguments for such a claim are treated in section 1.5 of this chapter. In Chapter 4 I argue that the claim is false.

\textsuperscript{29}Searle’s classic taxonomy of speech acts is consistent with Dummett’s viewpoint (see (Searle, 1979)). Dummett relegates an analysis of different types of speech acts to what he calls ‘the theory of force’, and sometimes writes as if the treatment of context sensitive phenomena such as deixis and conversational implicature will also be treated by this part of the theory of meaning, though his comments on this topic are very sketchy.
the Duimettian theory of reference, or syntax and compositional semantics, treats assertions in the indicative mood.

At the time Dummett wrote his two "What is a Theory of Meaning?" papers, he thought, along with most other philosophers influenced either by Davidson or Generative Semantics, that the theory of reference would consist in a Tarski-style truth definition for a formal language, and some manner to pair sentences of the formal language with sentences of the natural language in question. He writes,

> The core of the theory will be a theory of truth, that is, an inductive specification of the truth-conditions of sentences of the language. This core would be better called 'the theory of reference', since, while among its theorems are those stating the conditions under which a given sentence, or utterance of it by a given speaker at a given time, is true, the axioms which govern individual words, assign references of appropriate kinds to those words. (Dummett, (1976b, p. 40))

It would be a digression to inquire too much at this point about the actual mechanisms of a compositional semantics that could do the work Dummett wants it too, other than to say that the sort of categorial grammars with semantic interpretation rules homomorphic to the syntactic rules, typified by classical Montague Grammar, seem to do the job admirably.\(^{30}\)

Now this presents us with a difficult choice. In doing Dummett exegesis we can either simply interpret him so that his discussion applies to more recent approaches to constructing grammars, or we can be very careful to state his discussion in terms of the grammatical architecture which Davidson seemed to have in mind. Like nearly every philosopher who writes or wrote on the theory of meaning, Dummett assumes that the architecture of a grammar will be Davidsonian. That is, that the grammar will proceed by using logical forms as deep structures and contain something like transformations to derive natural language sentences, and that semantics proper consists of coming up with a Tarski style truth definition for the logical forms.

\(^{30}\) The following discussion, for better and worse, contains many simplifications. See (Dowty, Wall, & Peters, 1981) for a nice introduction to Montague Grammar, and (Carpenter, forthcoming), and (Morrill, 1994) for nice introductions to categorial grammar generally.
Though Dummett does not, so far as I know, explicitly defend the view that such logical forms will be formulas of first order logic, his discussions are wholly limited to discussion of the interpretation of formulas of first order logic. Today no linguist simultaneously accepts the three planks of Davidson’s program I have isolated. The closest realization of Davidson’s program was Generative Semantics; and the failure of Generative Semantics is one of the few things which linguists of different schools agree on.\(^{31}\)

I do not think that Dummett’s substantive claims are tied to his endorsement of a disconfirmed linguistic program. Rather than tediously showing that the specific claims about a Tarski-style truth definition for natural languages can be translated into claims about newer grammatical approaches, I will just present his claims as being about categorial grammars. It will be clear that the substantive philosophical points taken from Dummett hold equally of Generative Semantics logical forms and truth conditions of more current approaches.\(^{32}\)

In these types of grammars, each word in the fragment of language under consideration is associated with an expression of a logical language such as the typed lambda calculus. Then for each rule putting words together to form more complex expressions, there is a rule putting together the corresponding logical expressions. This rule-to-rule property, given as the Homomorphism Constraint below, is why categorial grammars have the best claim to showing how properties of the meaning of a sentence depend upon the meaning of the parts of that sentence and the way they are put together. For each sentence of the fragment of natural language under

\(^{31}\)It is something of a scandal that no one has published a study concerning the relationship between the various considerations that disconfirmed early Generative Semantics and the substantive philosophical claims made by philosophers taking part in the theory of meaning debates, or any other area of philosophy where the concepts of a sentence’s “logical form” or “deep structure” are utilized. In Chapter 4 of this monograph I argue that Dummettian “molecularism” would only be plausible if something like the Generative Semantics architecture were plausible.

\(^{32}\)I use of the expression “logical form” in the broader sense philosophers are prone to, a sense compatible with the die hard categorial grammarian’s insistence that the logical language is ultimately dispensable. I do not mean to be talking about the logical forms of either Government and Binding theory or Minimalism.
consideration the rules generate a corresponding sentence of the logical language, which then has (in the vast majority of cases) a truth conditional model theoretic interpretation similar to the sort to which Dummett adverts in the above quote (the main difference being that truth-in-a-model is defined rather than truth). The model theory then defines a notion of logical consequence in such a way that some aspects of the resulting logical expressions' inferential role (and hence some aspects of the inferential role of the corresponding natural language expression) are clearly defined.

Informatively characterizing the types of inferences that linguists take to be appropriate to being thus captured by a compositional, as opposed to lexical, semantics would be a boon to the philosophy of language. However, it won't be attempted here. It is enough for our purposes to say that the inferences generally are ones that can be said to have a wide syntactic role. That is, they crop up in predictable and systematic ways for large fragments of the language in question. Traditional logical words such as 'and', 'or', 'not', 'every', 'some', 'can', and 'ought' are words that permit characteristic inferences, depending upon where they occur in the sentences. Also, phenomena such as morphological pluralization and anaphora also affect the meaning of sentences in characteristic ways. This is the kind of thing I have in mind when I talk of wide syntactic role.

Following Tarski, it is standard to so define entailment that a sentence α is entailed by a set of sentences Γ if and only if every model for which all of the sentences in Γ are true is a model for which α is true. Thus, a categorial theory of reference will generate for every sentence P in the fragment of the language in question: (1) every disambiguated syntactic structure for P (and hence P by a Montague style "ambiguating relation"), (2) a sentence α in an interpreted formal language for each of the disambiguated structures, and (3) via the semantics for the formal language, the conditions under which α is true in a model.\[33\]

\[33\] I am simplifying. Many very interesting compositional approaches to semantics in the Montogovian tradition depart from this in neat and well motivated ways, perhaps the most interesting being approaches growing out of Heim, Kamp, and Reyle's Discourse Representation Theory (Heim, 1982), (Kamp & Reyle, 1993). For a recent overview see (Chierchia, 1995).
It is typical and desirable in this approach to syntax and semantics for the rules which generate the logical formulae (and hence the interpretations) to correlate strongly with the syntactic rules which generate the (disambiguated) sentences of the natural language. The strongest such correlation is a homomorphism requirement between the syntactic and semantic algebras. In practice this almost always involves a homomorphism between the syntactic algebra and the logical forms which are then interpreted by a logical semantics. Thus, where \(*\) is the function under which the set of syntactic structures is closed, \(+\) is the function under which the set of expressions of the logical language is closed, and \(t\) is the translation function from syntactic components to expressions of the logical language, we require the

\[
\text{Homomorphism Constraint} \quad t(*(x^1 \ldots x^n)) = +(t(x^1) \ldots t(x^n)).
\]

In addition, where \(L_M\) is equal to the smallest set containing the morphology \(M\) and closed under \(+\), where \(L_{M'}\) is equal to the smallest set containing the morphology \(M'\) and closed under \(+\), \((x^1 \ldots x^n) \in L_M\) and \((x^1 \ldots x^n) \in L_{M'}\), \(i\) is an interpretation of \(L_M\), and \(i'\) is an interpretation of \(L_{M'}\) we also require

\[
\text{Logician's Compositionality} \quad \text{If } i(x_1) = i'(x_1), \ldots, i(x_n) = i'(x_n), \text{ then } i(+((x^1 \ldots x^n)) = i'(+((x^1 \ldots x^n))).
\]

Note that the Homomorphism Constraint and Logician's Compositionality place no restrictions on what the syntax, logical language, or interpretation functions are like, other than that the compositional aspects of the meaning a sentence be a function of the compositional aspects of the meaning of the words in that sentence and the way they are put together.\(^{34}\)

\(^{34}\)I realize that purists will rightly balk at the way I have put this. In linguistic discussions of compositionality an important issue concerns whether or not the logical language is dispensable, so that an interpretation function can be assigned directly to the syntactic algebra, as it could be in classic Montague Grammar. For example, in Dowty, Wall, and Peters' classic introductory text we have

Translating English into Intensional Logic was therefore not essential to interpreting the English phrases we generated; it was simply a convenient intermediate step in
To illustrate how such an architecture works, we shall consider a syntactic derivation of a sentence and a parallel derivation of the "logical form" of that sentence. In doing so, we shall be able to show how both are licensed by a parallel set of rules in a manner satisfying the homomorphism requirement. First, we shall consider two syntax rules (taken from Dowty, Wall, & Peters, 1981). Where \( P_x \) denotes the set of phrases of category \( x \), and \( B_x \) denotes a word of the category \( x \) (e.g. \( B_{IV} \) denotes the set of intransitive verbs, \( B_{TV} \) the set of transitive verbs, \( B_{IV/I} \) the set of sentence complement verbs such as "believe" and "assert," and \( B_{IV/I} \) the

assigning them meanings. This step could have been eliminated had we chosen to describe the interpretation of English directly, rather than indirectly by stipulating that it be the one induced under our translation into Intensional Logic by the interpretation given to that language. This point is important, because anyone who does not appreciate it may misunderstand the role of Intensional Logic in applications of Montague's descriptive framework to natural languages.

Some writers have suggested that Intensional Logic plays a role in 'Montague grammar' like that of Logical Form in Generative Semantics or in Chomsky's current conception of grammar [1977]. This is definitely not the case. The idea is fundamentally mistaken in that translation into Intensional Logic is, as we have just seen, not in any way essential to semantic interpretation in Montague's framework—that step of PTQ is entirely eliminable without altering anything crucial about the interpretation assigned each phrase. It is further mistaken in that the particular translations assigned play no role in determining well-formedness of the syntactic structures with which they are associated, as Logical Forms may do in Chomsky [1980]. And it is also mistaken in its implication that the entailments licensed by a sentence of English are formalized by the translation of that sentence into Intensional Logic and the inference rules of that logic. The source of this error, which is less explicit in some authors than the two just commented on, is the mistaken belief that a complete and sound proof theory exists for Intensional Logic.

(Dowty, Wall, & Peters, 1981, pp. 263-264)

I have presented things the way I have because: (1) a discussion of Montagovian Universal Grammar where the homomorphism is required to be between the syntactic and semantic algebras would take us too far afield, (2) in actually doing semantics the logical language is not really dispensable, and the Homomorphism Constraint (between the syntactic algebra and operations of the formal language), or some suitably weakened version of it, is absolutely necessary for the semantics of a given fragment to be tractable and testable, (3) philosophers of language who treat natural language as if it was first order logic think of Logician's Compositionality when they talk about compositionality, and thus adding the Homomorphism Constraint in this manner rather than discussing (Montagovian) Universal Grammar makes our discussion more tractable to the greatest number, (4) in my example of an extensional fragment of English, I show how the logical language is sometimes ultimately dispensable. (5) Whenever both the Homomorphism Constraint and Logician's Compositionality are satisfied, it should be trivial to dispense with the logical forms in any case, and translate syntactic structures directly into interpretations. For two classic discussions of compositionality in this framework see (Dowty, 1979), and (Partee, 1984).

35
set of infinitive-complement verbs such as "try" and "wish") we have the following two rules.

**Determiner Noun Rule**

S2 If $\delta \in P_{T/CN}$ and $\xi \in P_{CN}$, then $F_2(\delta, \xi) \in P_T$, where $F_2(\delta, \xi) = \delta' \xi$, and $\delta'$ is $\delta$ except in the case where $\delta$ is $a$ and the first word in $\xi$ begins with a vowel; here $\delta'$ is an.

**Subject Predicate Rule**

S4 If $\alpha \in P_T$ and $\delta \in P_{IV}$, then $F_4(\alpha, \delta) = \alpha \delta'$ and $\delta'$ is the result of replacing the first verb (member of $B_{IV}$, $B_{TV}$, $B_{IV/T}$, or $B_{TV/IV}$) in $\delta$ by its third person singular present form.

Given a lexicon, rules such as these will allow derivations of sentences such as the following.

**Syntax Derivation**

1. every $\in P_{T/CN}$  
2. man $\in P_{CN}$  
3. every man $\in P_T$  
4. walk $\in P_{IV}$  
5. every man walks $\in P_T$

Now consider the following translation rules (extensional versions of the same rules in (Dowty, Wall, & Peters, 1981))

**Determiner Noun Translation Rule**

T2 If $\delta \in P_{T/CN}$ and $\xi \in P_{CN}$, and $\delta, \xi$ translates into $\delta', \sigma'$ respectively, then $F_2(\delta, \xi)$ translates into $\delta'(\sigma')$.

**Subject Predicate Translation Rule**

T4 If $\alpha \in P_T$ and $\delta \in P_{IV}$, and $\alpha, \delta$ translate into $\alpha', \delta'$ respectively, then $F_4(\alpha, \delta)$ translates into $\alpha(\delta')$.

Then, given a manual for translating basic expressions, we get the following deriva-
tion, completely parallel to the syntactic derivation.

<table>
<thead>
<tr>
<th>Translation</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>1'. (\lambda P[\lambda Q \forall x[P(x) \rightarrow Q(x)]])</td>
<td>1 translation manual</td>
</tr>
<tr>
<td>2'. man'</td>
<td>2 translation manual</td>
</tr>
<tr>
<td>3'. (\lambda P[\lambda Q \forall x<a href="man'">P(x) \rightarrow Q(x)</a>)</td>
<td>1,2,1',2' T2</td>
</tr>
<tr>
<td>4'. walk'</td>
<td>4 translation manual</td>
</tr>
<tr>
<td>5'. (\lambda P[\lambda Q \forall x<a href="man'">P(x) \rightarrow Q(x)</a>(walk'))</td>
<td>3,4,3',4' T4</td>
</tr>
</tbody>
</table>

Clearly these rules satisfy the Homomorphism Constraint.

Typically, when interpreting such sentences, simplification via lambda conversion and meaning postulates is performed before using the semantics to state the truth conditions. For our extensional fragment we would continue the above derivation in this manner.

<table>
<thead>
<tr>
<th>Translation</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>6'. ([\lambda Q \forall x<a href="walk'">man'(x) \rightarrow \neg Q(x)</a>)</td>
<td>5' lambda conversion</td>
</tr>
<tr>
<td>7'. (\forall x[man'(x) \rightarrow walk'(x)])</td>
<td>6' lambda conversion</td>
</tr>
</tbody>
</table>

By standard semantics for first order logic, where \(v\) is any function from the morphology \(M\) of the language \(L_M\) (which includes \(\forall x[man'(x) \rightarrow walk'(x)]\)) to sets of n-tuples whose members are from the domain \(D\), \(v^{d/u}\) denotes the function just like \(v\) except that \(d\) is assigned to \(u\) by \(v^{d/u}\), we get the following interpretation of our sentence

<table>
<thead>
<tr>
<th>Interpretation</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>(v(\forall x[man'(x) \rightarrow walk'(x)]) = T \iff )</td>
<td></td>
</tr>
<tr>
<td>(\forall d \in D v^{d/u}[man'(u) \rightarrow talk(u)] = T \iff )</td>
<td></td>
</tr>
<tr>
<td>(\forall d \in D (v^{d/u}[man'(u)] = F \text{ or } v^{d/u}[talk(u)] = T) \iff )</td>
<td></td>
</tr>
<tr>
<td>(\forall d \in D (d \notin v[man'(u) \text{ or } d \in v[talk'])).</td>
<td></td>
</tr>
</tbody>
</table>

In this manner we see how any grammar obeying the Homomorphism Constraint and Logician’s Compositionality will be such that the aspect of the meanings captured by the semantics of our logic really is a function of the meanings of the words of the English language sentence and the way those words are put together.

Moreover, in this case it is easy to illustrate the superfluousness of the logical forms in question. Where \(D_0\) denotes the set the set 0, 1 ("0" standing for "false" and "1" standing for "true"), \(D_{<e,t>}\) denotes the set of functions from the domain \(D\)
into $D_t$, "$D^{<e,t>,t}$" denotes the set of functions from $D^{<e,t>}$ into the set $D_t$, and $F$ is a function that takes (among other things) common nouns such as "man" and intransitive verbs such as "walk" into $D^{<e,t>}$, consider the following derivation.

**Translation'**

1". That function $h$ from $D^{<e,t>}$ into $D^{<e,t>,t}$ such that for all objects $k$ in $D^{<e,t>}$, $h(k)$ is equal to that function $h'$ from $D^{<e,t>}$ into $D_t$ such that for all objects $k'$ in $D^{<e,t>}$, $h'(k')$ is equal to 1 iff $\forall e \in D_e(k(e) = 0$ or $k'(e) = 1)$.

2". $F(man')$

3". $h(F(man'))$

4". $F(walk')$

5". $(h(F(man')) F(walk'))$

Of course we can perform the steps analogous to lambda simplification to simplify this interpretation.

6". That function $h'$ from $D^{<e,t>}$ into $D_t$ such that for all objects $k'$ in $D^{<e,t>}$, $h'(k')$ is equal to 1 iff $\forall e \in D_e(F(man')(e) = 0$ or $F(walk')(e) = 1)$.

7". 1 iff $\forall e \in D_e(F(man')(e) = 0$ or $F(walk')(e) = 1)$.

This conclusion is just a notational variant of prior interpretation of "$(\forall x [man'(x) \rightarrow walk'(x)])$". Thus, we can see how easy it would be to state translation rules from the syntactic algebra straight into the semantics, as long as the Homomorphism Constraint and Logician's Compositionality are true of the syntax and semantics in question. Of course, it wouldn't be easy to use such a theory. The ugliness of the above derivation, where the derivations only involve an extensional first order lambda calculus, show this to be the case.

In any case, we can say that the Homomorphism Constraint and Logician's Compositionality together embody a Fregean compositionality requirement. This is important, because satisfaction of such a requirement is essential to Dummett's demands for a theory of meaning. This is because Dummett's initial puzzle about how speakers with limited computational machinery are able to recognize a potential infinity of new sentences is taken to be solvable in virtue of the fact that important aspects of the meanings of the complex new sentences (with the exception of some
finite number of idiomatic expressions) are determined by the meanings of their parts and the way they are put together.

Given that Frege was perhaps the first to adequately emphasize this principle we can represent it as the following.

**Frege's Thesis**

Important aspects of the meaning of a sentence are a function of the meaning of the sentence's parts and the way those parts are put together.

Quite independently of our concern about theories of meaning, it is clear that something in the neighborhood of Frege's Thesis is correct.35

However, in the context of Dummett's proposals for a theory of meaning, Frege's thesis strongly motivates something like the categorial grammatical approach sketched above. For Frege's thesis in this meaning-theoretic context becomes

**Compositionality Requirement**

A theory of reference should specify a recursive function \( F \), such that the compositional aspects of the meaning of a sentence is the value of \( F \) when applied to the sentence's parts and the way the sentence is put together.

The Homomorphism Constraint and Logician's Compositionality, are nothing more than a mathematically informed way of stating the Compositionality Requirement. This is as much as I shall say about the mechanisms of a theory of reference. For now it is important to understand why Dummett holds that a syntax and compositional semantics cannot, in themselves, constitute a theory of meaning.

§ 1.3 THE PROBLEM

The main argument of Dummett's "What is a Theory of Meaning I" proceeds by reducing the following conjunction, which Dummett initially attributed to David-

\[^{35}\text{See (Partee, 1979), (Partee, 1980), (Partee, 1982) and (Taschek, 1998) for discussions of just how difficult it is to maintain a strict compositionality thesis for sentences involving propositional attitudes.}\]
son,\textsuperscript{36} to absurdity.

**Modest View**

(A) A speaker $S$ is competent in the language $L$ if, and only if, $S$ has tacit knowledge of a correct theory of meaning for $L$ (we call this theory $M_L$)

(B) $M_L$ is identical to a correct syntax and compositional semantics for $L$, and

(C) A speaker $S'$ of $L$ has tacit knowledge of the theory of meaning $M_L$ for $L$ if, and only if, it is not possible to have explicit knowledge of $M_L$ and fail to be a competent speaker of $L$.

The target of Dummett's attack in both "What is a Theory of Meaning" articles is just the demand that the theory of meaning be "implicitly known" in the sense explicated above. Exegetical difficulties may seem to abound, as Dummett also gave up the equation of tacit and implicit knowledge in his late works. However, as we shall see, the problem is not so horrible. The Dummettian Manifestationist View, presented below as his alternative to the Modest View, an alternative he has held throughout his career, involves neither (B) or (C).

Another strangeness arrives from the fact that clause (B) really should read "$M_L$ is identical to a Tarski-style truth definition for $L$" given that this is the view Davidson held.\textsuperscript{37} Again, I'm trying to show that Dummett's challenge is not an artifact of the Generative Semantics architecture of a linguistic theory.

\textsuperscript{36}In Chapter V I argue that Davidson's actual philosophical views are much more plausible than this.

\textsuperscript{37}I'm not convinced that there ever was a determinate content to the concept of a "Tarski-style truth definition for $L$" where $L$ refers to a natural, as opposed to formal, language. In the essays contained in (Davidson, 1984), we are told that the theory should entail all instances of "$P$ is true if and only if $Q$" where "$Q$" is a translation of "$P$" into the metalanguage. Unfortunately, Davidson leaves undetermined the mechanisms for deriving instances of these schemas as well as what the object language and meta-languages are. That is, it is never clear whether $P$ and $Q$ are supposed to range over: (1) sentences of a natural language such as English (i.e. "Jones walks in Barcelona"), (2) sentences of a semi-formal language such as mathematical English (i.e. "There exists an $x$ such that $x$ is a walking and $x$ is by Jones and $x$ is in Barcelona"), or (3) sentences of some formal language (i.e. "$\exists(x)(Wx \land (Jx \land Bx))\) An interesting project would involve determining how many of the possible six permutations (of substituends for $P$ and $Q$ in the T-sentences) can be found in the literature. The only proposals I've found specific enough to have clear empirical implications for linguistic theory are in the essays by Davidson and Harman in (Davidson & Harmon, 1975), where $P$ ranges over sentences of natural language, and $Q$ ranges over sentences of a formal language. The derivations of the T-sentences were identified with the Generative Semantics transformations from a deep structure logical form to natural language sentences.
Though constructing an adequate compositional semantics for a natural language is extraordinarily difficult, for now we shall follow Dummett and assume that there exists a correct theory of reference for some natural language \( L \), one which satisfies the Fregean compositionality requirement. Call this theory \( M_L \). In the main body of “What is a Theory of Meaning? I,” Dummett reduces the modest view to absurdity by arguing for the following.

**Dilemma**

Either \( M_L \) does not function as a translation manual from \( L \) into the language of a person who explicitly knows \( M_L \) or it does. (1) If it does not, one could have explicit knowledge of \( M_L \) and not be able to understand the sentences of \( L \), but then clause (C) of the Modest View would be false. (2) If we restrict the modest view such that \( M_L \) must function as a translation manual, then the theory of meaning no longer explains linguistic competence.

In this section I will provide Dummett’s arguments for this dilemma, and in the next I will show how these arguments motivate the demand for a theory of sense which correlates sets of dispositions with meanings generated by the theory of reference.

§ 1.3.1 The First Horn

Dummett’s argument for the first horn of his dilemma can best be understood by comparing the Modest View for \( M_L \) with a modest claim for syntax alone, where it is much more reasonable. As stated above, a formal syntax can be thought of as correctly modeling what people know when they know the syntax of a language, since the theory can be thought of as predicting grammaticality and structural judgments of competent speakers of the language. In this sense a formal syntax is not just an abstract mathematical toy of some sort, but can also rather easily be thought of as a theory of grasp of syntactic structure and well-formedness. Interestingly, when Dummett discusses syntactic competence he does not mention the kinds of distribu-
tional judgments mentioned above. For example,

Here, an implicit grasp of certain general principles, naturally represented by axioms of the theory, has issued in a capacity to recognize, for each sentence in a large, perhaps infinite, range, whether or not it is well formed, a capacity naturally represented as the tacit derivation of certain theorems of the theory. (Dummett, (1976b, p. 37))

For now, it will be sufficient to go along with Dummett's simplification. Thus, for Dummett, a speaker grasps syntactic structure and well-formedness, to the extent that her grammaticality judgments are consistent with those predicted by a correct syntax for her language, in that every theorem of the syntax in question is recognizable by the speaker as a sentence of the language.

Dummett questions whether there is any such straightforward connection between a correct theory like $M_L$ and a speaker's ability to understand sentences of the language. He argues that an answer appropriate to syntax is not available for the whole theory of meaning (on the assumption that the theory of meaning just is a compositional syntax and semantics).

This point can be motivated by considering an extensional fragment of English. Consider the sentences, "Every man walks or talks," "John is a man," and "John walks or talks." Now suppose a German speaker, Karl, has the ability to derive these sentences in a syntax along with the corresponding Lambda expressions

\[
\lambda P[\lambda Q \forall x[P\{x\} \rightarrow Q\{x\}]] \,(^{\text{man'}}) \,(^{\lambda x[\text{walk'}(x) \lor \text{talk'}(x)]})
\lambda P[P\{j\}] \,(^{\lambda x[\text{walk'}(x) \lor \text{talk'}(x)]})
\lambda P[P\{j\}] \,(^{\lambda x[\text{walk'}(x) \lor \text{talk'}(x)]})
\]

and the first order results of lambda converting

\[
\forall y[\text{man'}(y) \rightarrow [\text{walk'}(y) \lor \text{talk'}(y)]]
\text{man'}(j)
\text{walk'}(j) \lor \text{talk'}(j)
\]

Moreover, suppose that Karl knows an algorithm which takes the first order formulas as inputs and gives as outputs the conditions (stated in mathematical English) under

\footnote{For the derivations see (Dowty, Wall, & Peters (1981, p. 201 & p. 229)).}
which the first order formulas are true in a model.

\[ u(\forall y [\text{man}(y) \rightarrow [\text{walk}(y) \lor \text{talk}(y)]] = T \iff \]
\[ \forall d \in D(u^{d/u}([\text{man}(u) \rightarrow [\text{walk}(u) \lor \text{talk}(u)]] = T) \iff \]
\[ \forall d \in D([u^{d/u}([\text{man}(u)] = T] or \]
\[ ([u^{d/u}(\text{walk}(y) \lor \text{talk}(y)]) = T]) \iff \]
\[ \forall d \in D([u^{d/u}([\text{man}(u)] = T] or [u^{d/u}(\text{talk}(y)]) = T]) \iff \]
\[ u(\text{walk}(j) = T \iff \]
\[ u(j) \in u(\text{walk'}) \]
\[ u(\text{walk}(j) \lor \text{talk}(j)) = T \iff \]
\[ u(j) \in u(\text{walk'}) or u(j) \in u(\text{talk'}) \]

Finally, suppose Karl is very adept at doing deductions in first order logic, so that he can determine that the third formula above follows logically from the first two.

Now suppose that Karl can do this with the entire theory \( M_L \). Given that most syntaxes and translations are recursive, and that entailment relations at least have interesting recursively enumerable subsets,\(^39\) it is clear that somebody like Karl could do this with an explicit syntax and semantics for a fragment of natural language without understanding the language. At the very least, Karl may not understand the non-logical words such as "walk," "man," or "talk."

If we are assuming that it is possible for \( M_L \), a syntax and semantics for all of English, to exist, then it should follow from this assumption that it is possible for Karl to have this mathematical facility with \( M_L \), again, without understanding of English. This is a very straightforward violation of the Modest View as defined above. Karl has explicit knowledge of \( M_L \), yet he is unable to understand the language which \( M_L \) is a semantics for.

As stated earlier, Dummett’s initial argument in “What is a Theory of Meaning? (I)” argued against the Modest View, when the theory of reference was thought to

\(^39\) See (Link, (1998, p. 161)) and (Londe, (1989, 1997)) for whether or not a complete (and hence one with a recursively enumerable set of logical truths) formalism for the logic of plurals is sufficient for a semantics of plurals.
be a Davidsonian grammar. But it is clear that the point he is making survives transplantation from its Davidsonian pot. For example, where an M-Sentence is one of the "‘P’ is true if and only if Q" sentences entailed by a Davidsonian Grammar, Dummett writes,

It is not sufficient, for someone to know what the sentence "The Earth moves" means, for him to know the M-sentence relating to it to be true; he must know the proposition expressed by that M-sentence. And the natural way to characterize what, in addition, someone who knows the truth of the M-sentence has to know in order to know the proposition it expresses is: the meanings of the component words. If, now, we explain an understanding of the component words as consisting in a knowledge of the axioms of the theory of truth which govern those words, the same question arises: is it sufficient for him to know those axioms to be true, or must he know the propositions which they express? The objection to requiring only that he know the axioms to be true is parallel to that we allowed in the case of the M-sentence: anyone who knows the use of "denotes", and who knows that "the Earth" is a singular term of English, must know that the sentence "‘The Earth’ denotes the Earth" is true, even if he does not know what specifically, the phrase "the Earth" means or what it denotes. (Dummett, (1975, pp. 11-12))

Clearly the same holds if the two sides of the M-Sentence are, a sentence and the conditions under which it is true in a model.

Dummett considers not just the situation where a speaker knows the M-sentence for a given natural language sentence (in our case this would be knowing the sentence-interpretation pair), but also how they were derived.

Now Davidson himself has fully recognized the obligation upon a theory of meaning to yield a theory of understanding: he has been quite explicit about what, on his view, an understanding of a sentence consists in, namely in a knowledge both of the theory of truth for the language which satisfies the constraints imposed upon such a theory for it to be acceptable. The analogue, for the understanding of a word, would presumably be a knowledge of the axiom governing it and also of the fact that that sentence was an axiom of a theory of truth satisfying our constraints.

(Dummett, (1975, p. 14))

He shows that his earlier criticism applies to the requirement that it not be possible
for a speaker to know the whole theory and not know the language.

It appears to me that only a very little consideration is needed to recog-
nize that this appeal to background information cannot supply what we
need. If someone does not know what “the Earth” means, he will learn
something from being told that the sentence “ ‘The Earth’ denotes the
Earth” is true, provided that he understand the verb “denotes”: he will
learn, namely, that “the Earth” is a singular term and is not empty.
But if, now, he asks to be told the specific meaning of the term, he will
not be helped in the least by being told that the sentence in question is
an axiom in a theory of truth for English satisfying certain particular
constraints.  
(Dummett, (1975, p. 14))

Again, this criticism still holds of the derivational machinery we have been consider-
ing. The argument is perfectly general. Given that any formal syntax and semantics
are algebraic objects, one can have knowledge of them, qua algebraic objects, without
understanding the relevant language.

§ 1.3.2 Fullbloodedness?

In Dummett’s earlier writing he sometimes suggests that clauses (A) (that a
speaker $S$ is competent in the language $L$ if, and only if, $S$ has tacit knowledge of a
correct theory of meaning for $L$), and (C) (that a speaker $S$ of $L$ has tacit knowledge
of the theory of meaning $M_L$ for $L$ if, and only if, it is not possible to have explicit
knowledge of $M_L$ and fail to be a competent speaker of $L$) of the Modest View could
be satisfied without the theory of meaning being, in effect, a translation manual.
Early Dummettian “fullbloodedness” can best be characterized in this manner.

**Early Fullblooded View**

(A) A speaker $S$ is competent in the language $L$ if, and only if, $S$ has tacit
knowledge of a correct theory of meaning for $L$ (we call this theory $M_L$),

(B) $M_L$ is identical to a correct syntax and compositional semantics for $L$ plus
a theory of sense, and

(C) A speaker $S$ of $L$ has tacit knowledge of $M_L$ if, and only if, it is not possible
to have explicit knowledge of $M_L$ and fail to be a competent speaker of $L$.

In “What is a Theory of Meaning? (I)” Dummett contrasts a modest meaning the-
ory with a full-blooded one in this manner: a modest theory of meaning merely “give[s] the interpretation of the language to someone who already has the concepts required,” while a full-blooded theory “should serve to explain new concepts to someone who does not already have them.” (Dummett (1976a, p.5)) This suggests that somehow the addition of the “theory of sense” in clause (B) of the above definition renders the theory of meaning capable of teaching a language to someone. In some manner a non-language user should be able to get explicit knowledge of the theory of meaning and then gain an understanding of the language. But this is clearly absurd.

In later work Dummett recognizes the wrongness of his early requirement. In *The Logical Basis of Metaphysics* he writes

> Such a demand would obviously be exorbitant: the demand which proponents of a modest meaning-theory resist should be stated in a more conciliatory form. A modest meaning-theory assumes not merely that those to whom it is addressed have the concepts expressible in the object-language but that they require no explanation of what it is to grasp those concepts. A more robust conception of what is to be expected of a meaning theory is that it should, in all cases, make explicit in what a grasp of these concepts consists—the grasp which a speaker of the language must have of the concepts expressed by the words belonging to it.

(Dummett, (1991, p. 108))

Thus, by Dummett’s own admission, the initial modesty-versus-fullbloodedness dichotomy proved to be the wrong one. Given this, we will need to examine to what extent Dummett can be interpreted as urging something which both does not reduce to fullbloodedness and is a genuine alternative to the Modest View.

§ 1.3.3 The Second Horn

Dummett suggests that the only way the Modest View could be satisfied is if we understand the explicit knowledge referred to in clause (C) of the definition in a question-begging way. For example, if we stipulated that the theory should generate satisfaction clauses for the formal language sentences in German as well as translate the English ‘non-logical’ words into German, then Karl, being a German speaker,
could not have explicit knowledge of the theory without being able to understand English.

With such a stipulation, the theory of meaning itself would function as a translation manual. But what would be wrong with such a requirement? Dummett is not entirely helpful on this score. He writes,

\[
\text{It now appears clearly that we must ascribe to anyone able to use the theory of truth in order to obtain an interpretation of the object-language that he have a prior understanding of the meta-language... a theory of meaning of this kind merely exhibits what it is to arrive at an interpretation of one language via an understanding of another, which is just what a translation manual does.} \quad (\text{Dummett, (1976a, p. 15)})
\]

Unfortunately, this isn't so helpful because it uses the notion of explaining "what it is to have mastery of a language," which is the topic of discussion. We want to know why the restriction on the notion of explicit knowledge in clause (C) of the above statement of the Modest View renders the theory insufficient to explain what it is to have mastery of a language. However, if we explicitly give the individually necessary and jointly sufficient planks that characterize the envisioned position, we shall be able to discern what is wrong with a translational theory. I will call this position the "modest view prime."

\textbf{Modest View'}

(A) A speaker \(S\) is competent in the language \(L\) if, and only if, \(S\) has tacit knowledge of a correct theory of meaning for \(L\) (we call this theory \(M_L\)),

(B) \(M_L\) is identical to a correct syntax and compositional semantics for \(L\), and a translation manual from \(L\) to some language \(L'\) (that is, the theory contains an algorithm for translating the basic expressions and statements of truth conditions of \(L\) into \(L'\)), and

(C) A speaker \(S\) of \(L\) has tacit knowledge of \(M_L\) if, and only if, it is not possible for a speaker of \(L'\) (which \(L\) is required to be translated into by clause (B)) to have explicit knowledge of \(M_L\) and fail to be a competent speaker of \(L\).

One way to understand Dummett is to take him as arguing that, once the translation manual is forced to be added to the theory of meaning, the syntax and semantics
are superfluous. Assume that a meaning theorist argued that the correct syntax and semantics for a language $L$ consisted in the empty set. For this theorist the Modest View reduces to

\begin{quote}
**Modest View'**
\end{quote}

(A) A speaker $S$ is competent in the language $L$ if, and only if, $S$ has tacit knowledge of a correct theory of meaning for $L$ (we call this theory $M_L$),

(B) $M_L$ is identical to a translation manual from $L$ to some other language $L'$, and

(C) A speaker $S$ of $L$ has tacit knowledge of $M_L$ if, and only if, it is not possible for a speaker of $L'$ (which $L$ is required to be translated into by clause (B)) to have explicit knowledge of $M_L$ and fail to be a competent speaker of $L$.

Thus, the Modest View' does render the syntax and semantics superfluous. Clause (C) of the Modest View' allows a translation manual to be a perfectly fine theory of meaning.

However, we must still discern why a translation manual does not make good theory of meaning. When comparing his own "interpretive" approach with Quine's translational approach to issues concerning meaning, Donald Davidson writes,

When interpretation is our aim, a method of translation deals with a wrong topic, a relation between two languages, where what is wanted is an interpretation of one (in another, of course, but that goes without saying since any theory is in some language). We cannot without confusion count the language used in stating the theory as part of the subject matter of the theory, unless we explicitly make it so. In the general case, a theory of translation involves three languages: the object language, the subject language, and the metalanguage (the languages from and into which translation proceeds, and the language of the theory, which says what expressions of the subject language translate which expressions of the object language). And in this general case, we can know which sentences of the subject language translate which sentences of the object language without knowing what any of the sentences of either language mean (in any sense, anyway, that would let someone who understood the theory interpret sentences of the object language).

(Davidson, (1984, p. 129))

This is unhelpful because it merely differentiates between translational theories
where the object language is stipulated to be part of the (explicit knower’s) metalanguage, and translational theories where it is not. It still does not tell us what is wrong with a translational theory where it is stipulated that the object language is part of the (explicit knower’s) metalanguage.

I think we can best appreciate Davidson’s point if we consider the case where the semantics for the logical language is stated in the same natural language which the theory of meaning treats (the so-called “homophonic meaning theory”). In this case, the Modest View’ could consist in merely the identity relation, given here.

**Modest View’**

(A) A speaker $S$ is competent in the language $L$ if, and only if, $S$ has tacit knowledge of a correct theory of meaning for $L$,

(B) a theory of meaning for $L$ is identical to a translation manual from $L$ to $L$, and

(C) A speaker $S$ of $L$ has tacit knowledge of the theory of meaning $M_L$ for $L$ if, and only if, it is not possible for a speaker of $L$ (which $L$ is translated into by clause (B)) to have explicit knowledge of $M_L$ and fail to be a competent speaker of $L$.

But this is clearly trivial; a perfectly sufficient theory of meaning satisfying this criterion would just be the identity function restricted to sentences of the language the speaker in question speaks. The Modest View’’ states that a person has knowledge of their own language if, and only if, they have knowledge of their own language. No positive argument need be given to show that such a theory is unexplanatory.

All we have at this point is a negative argument to the conclusion that the Modest View cannot be the correct account of linguistic competence. To recap, Dummett first argues that one could have explicit knowledge of a formal syntax and compositional semantics of a language without knowing that language. Though his original arguments were given in terms of a Davidsonian $T$-theory, I have shown that they apply to more contemporary linguistic approaches as well. Dummett then realizes that the reason one can have explicit knowledge of the syntax and semantics without knowing the language is because the syntax and semantics needn’t
translate expressions of the language they treat into expressions of the language spoken by the person who explicitly knows the syntax and semantics. In Dummett's second argument he countenances the possibility of maintaining the Modest View by explicitly specifying that the syntax and semantics include a translation manual into the language of the person who explicitly knows the syntax and semantics. However, once this specification is made, the Modest View allows the trivial identity function to count as an explanation of linguistic competence.

§ 1.4 DUMMETT’S SOLUTION

One might plausibly view Dummett’s explanatory desiderata for a theory of meaning as overreaching. Dummett’s problem is perhaps more easily grasped if we compare it with a problem concerning jazz interpretation as stated by Duke Ellington. When Ellington was asked what jazz is, he famously replied, “If you have to ask, you’ll never know.” With language, if you can ask, there’s a sense in which you already know. Given that all of our non-problematic, paradigm instances of knowing presuppose linguistic mastery by the knower, it is very difficult to characterize knowledge of a language. Moreover, if the only options for such a characterization were either pursuit of early Dummettian “full-bloodedness” or abandoning the idea of a theory of meaning as a theory of understanding (this was the way Dummett presented his problem), we would have a fairly good a-priori argument against the possibility of such a theory of understanding.

I think the only way to not conclude this from Dummett’s considerations is to read him as balking at the Minimal Output Requirement. Again, this requirement was given as clause (C) in all of the above positions (the Modest View, the Modest
Minimal Output Requirement
A speaker $S$ of $L$ has tacit knowledge of the theory of meaning $M_L$ for $L$ if, and only if, it is not possible to have explicit knowledge of $M_L$ and fail to be a competent speaker of $L$.

As I will show, reading Dummett as rejecting this requirement is consonant with the thrust of Dummett's positive proposals.

For Dummett, the theory of reference is a necessary part of a theory of meaning because satisfaction of the compositionality requirement of a theory of reference suggests how a theory of grasp of meaning should proceed. Given that the theory of reference will show how the meaning of a complex sentence is a function of the parts of the sentence and the way they are put together, this suggests that the account of grasp of meaning may likewise be compositional. Dummett writes,

> Surrounding the theory of reference will be a shell, forming the theory of sense: this will lay down in what a speaker's knowledge of any part of the theory of reference is to be taken to consist, by correlating specific practical abilities of the speaker to certain propositions of the theory.

(Dummett, (1976b, p. 40))

That is, if one can isolate some salient properties of a speaker ensuring that she grasps the recursive clauses of the syntax and model theoretic semantics (which interprets the logical formulae paired with natural language sentences), as well as salient properties of a speaker ensuring that she can be credited with grasping the 'non-logical' translates into the formal language, and if grasp of meaning is taken to be compositional in the requisite sense, then the theory of grasp of meaning could be compositional in the same manner as the theory of reference. Dummett hopes that the derivations which pair natural language sentences with logical sentences, and the derivations of truth-conditions of the logical sentences, could be used in a derivation of conditions necessary and sufficient for grasp of the meaning of sentences.

Dummett's envisioned use of the theory of reference is very similar to a thesis that the linguist David Dowty puts forward in Word Meaning and Montague Grammar.
Dowty’s “thesis of parallel structure of reference and understanding” is given in this manner:

If certain ways of deriving the meaning of English sentences compositionally from the meanings of their parts can be shown to be necessary in a theory of truth and reference, then it may be concluded that the same compositional analysis is necessary in a theory of language understanding. (Dowty, (1979, p. 383))

In the same manner as Dowty suggests, for Dummett it is because of some sort of projected isomorphism between the syntax and semantics and the theory of grasp of meaning that it is correct that an understander can be said to have tacit knowledge of the syntax and semantics of a natural language.

For Dummett, the theory of sense will recursively correlate the interpretations of the logical formulae of the theory of reference with practical abilities, so that a speaker has the appropriate kind of tacit knowledge of the logical formula (realistically, paired with a disambiguated syntactic structure) if, and only if, she possesses the practical abilities which correspond to that logical formula (and, realistically, the disambiguated syntactic structure as well).

Thus, Dummett is best presented as urging the replacement of the Modest View with the following.

 Manifestationist View
(A) A speaker $S$ is competent in the language $L$ if, and only if, $S$ has tacit knowledge of a correct theory of reference for $L$ (we call this theory $R_L$),

(B) The theory of meaning for $L$ (which we will call $M_L$) is identical to $R_L$ plus a theory of sense (which we will call $S_L$), which correlates sets of dispositions with truth conditions generated by $R_L$, and

(C) A person can correctly be attributed tacit knowledge of $R_L$ if, and only if, she possesses the dispositions correlated with the truth conditions of $R_L$ by $S_L$.

It is not clear how one could motivate the view that a theory of meaning should recursively generate such sets of dispositions unless one had an argument that for each sentence in a natural language there does exist a set of dispositions posses-
sion of which are necessary and sufficient for understanding of it. Such arguments as Dummett provides for this view will be presented in the next section, but not evaluated until Chapter IV.

Also, nothing about the Manifestationist View, as stated, precludes the use of intentional or mentalistic vocabulary in the statements of the relevant dispositions. The extent to which correct attribution of mental states to others presupposes linguistic abilities (and thus might, in some sense, render the theory of sense unexplanatory) is very close to the issue of discerning to what extent one can have thought without language. I will not have very much to say about this issue.

At this point we are starting to get a clearer idea of Dummett's understanding of the first premiss in the argument which opens this monograph. For Dummett, to say that knowledge of meaning is knowledge of truth conditions is to say that a person has tacit knowledge of a correct theory of reference in the manner given by the manifestationist view. The truth conditions thus known are the interpretations of the logical formulae in the theory of reference which forms the central part of the theory of meaning in question.40

§ 1.5 MEANING, USE, AND VERIFICATION

Dummett's projected recursive pairing of interpretations of sentences with sets of dispositions is of course, only plausible to the extent that: (a) there exist sets of dispositions that fulfill the role, (b) such dispositions can be informatively given in a theory. Here I will merely present the arguments Dummett does give to support the claim that there are such dispositions, and then I will show why Dummett thinks verificationism provides the best means of delineating such sets of dispositions. Not until the next chapter will I evaluate the claim and criticize the claim that

40 Thus, for Dummett two different accounts of the truth conditions of sentences can be compared and evaluated depending upon how well a theory of sense can be provided for the compositional semantics which includes them. This is the key to understanding Dummett's criticism of classical logic, which will be considered in Chapters III.
verificationism is motivated by these considerations, and not until Chapter IV will I criticize Dummett’s understanding of the claim that meaning is use.

§ 1.5.1 Meaning and Use

In “The Philosophical Basis of Intuitionistic Logic” Dummett gives somewhat more straightforward arguments for the conclusion that “The meaning of a mathematical statement determines and is exhaustively determined by its use.” (Dummett, (1973, p. 216)). Unfortunately the arguments he gives are pretty sketchy. For example, he writes,

An individual cannot communicate what he cannot be observed to communicate: if one individual associated with a mathematical symbol or formula some mental content, where the association did not lie in the use he made of the symbol or formula, then he could not convey that content by means of the symbol or formula, for his audience would be unaware of the association and would have no means of becoming aware of it. (Dummett, (1973, p. 216))

and then concludes from the fact that, since meaning is communicable, “if two individuals agree completely about the use made of a statement, then they agree about its meaning.” (Dummett, (1973, p. 216))

We can represent this consideration by first supposing for reductio that two speakers agree on all possible uses of a sentence but disagree about its meaning. Since they agree in all possible uses of the sentence they could never communicate their disagreement about its meaning. But it is plausible to maintain that all meaningful disagreements can be communicated. Therefore, it is not the case that it is possible for two speakers to agree in all possible uses of a sentence and disagree about the
meaning of that sentence. This directly entails the following claim.  

If two speakers agree in all possible uses of a sentence, then they agree about the meaning of the sentence.

The problem with this as the bare explication of the “meaning is use” claim is that it is trivially true, unless the notion of a “possible use” is limited in some way. What Dummett intends, and what he doesn’t really provide an argument for, is this stronger claim.

The Entailment of Identity of Speaker’s Meaning from Identity of Use

If two speakers agree in all possible correct uses of a sentence, then they agree about the meaning of the sentence.

Moreover, “correct use” needs to be explicated in great detail before the this claim can be evaluated.

Dummett gives a distinct argument for the claim that meaning is use. First he states that when we attribute understanding of a sentence to someone, we are attributing a form of implicit knowledge. Then he writes of implicit knowledge that it,  

... cannot, however, meaningfully be ascribed to someone unless it is possible to say in what the manifestation of that knowledge consists: there must be some observable difference between the behaviour or capacities of someone who is said to have that knowledge and someone who is said to lack it. Hence it follows, once more, that a grasp of the meaning of a mathematical statement must, in general, consist of a capacity to use that statement in a certain way, or to respond in a certain way to its use by others. (Dummett, (1973, p. 217))

Again, the passage consists more in bald assertion than in argument, though there is a non-question-begging way to view the matter.

41 For Dummett it doesn’t really “directly follow” as a strictly classical (non-intuitionistically valid) inference is involved. It should be briefly noted that the classical inference required to get the above principle, given the principle’s ultimate role in an argument (discussed in Chapter III) to the conclusion that classical logic is not unrestrictedly valid, does not necessarily yield a *tu quoque* argument against Dummett. For example, one might hold that two speakers either agree about the meaning of a given sentence or don’t agree about the meaning of that sentence. An intuitionist would not consider this a logical truth, but could argue that it is true in virtue of non-logical considerations.
In the previous passage, Dummett is arguing towards the conclusion that:

**The Equivalence of Correct Use and Grasp of Meaning**

One can correctly use a sentence if, and only if, one correctly grasps its meaning.

Perhaps Dummett’s claim about this is that a person who identifies grasp of a sentence’s meaning with the ability to use it correctly can provide the best account of what it is to grasp the meaning of a sentence. Put this way, the claim is offered more in the spirit of a scientific hypothesis to be tested by its theoretical productivity than as the conclusion of a philosophical argument.

Dummett gives a third brief direct argument in “The Philosophical Basis…” He claims that supposing that “meaning transcends use” is to countenance the possibility of a person behaving in every manner like somebody who understands the language of a mathematical theory, but still not understanding the theory. Dummett then tries to establish that such a conception of meaning is mistaken. He writes,

> But to suppose this is to make meaning ineffable, that is, in principle incommunicable. If this is possible, then no one individual ever has a guarantee that he is understood by any other individual; for all he knows, or can ever know, everyone else may attach to his words or to the symbols which he employs a meaning quite different from that which he attaches to them. A notion of meaning so private to the individual is one that has become completely irrelevant to mathematics as it is actually practised, namely as a body of theory on which many individuals are corporately engaged, an enquiry within which each can communicate his results to others. (Dummett, 1973, p. 218)

Suppose we agree with Dummett and hold that there couldn’t be a creature which acted in every respect as though it understood a language, yet didn’t. This would entail that ability to correctly do with language what we do is sufficient for grasping the language. However, Dummett’s argument against such a possibility above is pretty transparently question-begging. It requires the assumption that every aspect of meaning is communicable. But this is just another way to say that (a kind of) correct use is necessary and sufficient for grasp of meaning.
Again, I think the best way to understand Dummett on these points is in the same manner I have been explicating his central challenge to truth conditional semantics. We want to provide an explanation of grasp of language, in particular the ability to understand new sentences. If we assume that meaning is use in the way given above, we will have an easier time of this. In any case, I don't think at this point that anyone would gripe at the above two theses. Where one would properly disagree with Dummett is in his account of what "correct use" comes to.

I have somewhat clarified the substance of Dummett's explications of the claim that meaning is use without attempting to evaluate them or further explicate the central terms in them. The plausibility of the claims certainly depends upon one's account of what "correct use" and "grasp of meaning" and "communicable" amount to. As noted, I will not attempt such evaluation until Chapter IV. Rather I will here show how Dummett tries to use verificationism both to provide a substantive account of what "correct use" is in the manifestation constraints as well as specify the set of dispositions which are relevant to understanding a sentence (as are called for by the Manifestationist View).

§ 1.5.2 Verificationism

For contemporary verificationists, the Dummettian manifestation constraints are explicated by understanding the notion of "correct use" in the constraints as the ability to recognize verifications of sentences. Thus, the two constraints become:

_the Publicity Constraint_

If two speakers agree, for all possible constructions \( c \), whether \( c \) verifies \( P \), and whether \( c \) falsifiess \( P \), then they agree about the meaning of \( P \).

_the Recognition Thesis_

\( X \) understands \( P \) if, and only if, were \( X \) presented with a construction \( c \), then \( X \) could recognize whether \( c \) verifies \( P \), and whether \( c \) falsifiess \( P \).

One might think that the Dummettian theory of sense can simply be equated with
the Recognition Thesis. This view can be given as follows.

Wrong Manifestationist View

(A) A speaker \( S \) is competent in the language \( L \) if, and only if, \( S \) has tacit knowledge of a correct theory of reference for \( L \) (we call this theory \( R_L \)),

(B) The theory of meaning for \( L \) (which we will call \( M_L \)) is identical to \( R_L \) plus a theory of sense, which is equal to the claim that, for all sentences \( P \) in \( L \), \( S \) understands \( P \) if, and only if, were \( S \) presented with a construction \( c \), then \( S \) could recognize whether \( c \) verifies \( P \), and whether \( c \) falsifiess \( P \), and

(C) A person \( S \) can correctly be attributed tacit knowledge of \( R_L \) if, and only if, for all sentences \( P \) in \( L \), were \( S \) presented with a construction \( c \) then \( S \) could recognize whether \( c \) verifies \( P \), and whether \( c \) falsifiess \( P \).

Here the theory of sense isn't really engaging the logical semantics at all. The "theory" is just a thesis pairing a competent speaker's dispositions with sentences in the language. This view is mistaken for two important reasons.

First, it's possible that the interpretations given the logical formulae by standard model-theoretic semantics force truth to be verification transcendent. If this were the case, and truth conditional semantics were correct, then the above position would not work. By assumption, the truth-conditions of the theory of reference show how the meaning of a sentence is dependent upon its parts and the way they are put together. Now assume that our choice of semantics forces the truth conditions of some sentences to be such that no person could ever recognize whether or not they held. Also assume that our semantics forces, for example, Goldbach's conjecture that every even natural number is the sum of two primes to be determinately true or false, yet verification (and falsification) transcendent in this manner. Then every purported verifier \( V \) for Goldbach's conjecture, and every purported verifier \( V' \) for the negation of Goldbach's conjecture, fails to verify or falsify Goldbach's conjecture. But then the Recognition Thesis would not distinguish between those who do understand Goldbach's conjecture and those who do not understand it and deny that anything is a verifier of it or its negation.

\[42\text{Dummett's arguments to this conclusion will be evaluated in Chapter 3.}
\[43\text{Of course, this seems absurd. Intuitively the verificationist should require the latter to say}

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Given that the Recognition Thesis is supposed to isolate conditions for correctly using sentences, such that the manifestation constraints hold, every undecidable sentence would have the exact same conditions for correctly using it as every other undecidable sentence. It would be enough to just say of everything that it is not a verification of the sentence in question. If this is a reasonable argument, and if the Recognition Thesis is correct, it follows that the semantics itself must be restrained so that the truth conditions do not permit sentences to be in-principle undecidable. It is entirely unclear how to effect such a restriction, without stating how the verification conditions of sentences are compositionally built up isomorphic to the way in which the truth conditions are built up. Thus, the Wrong Manifestationist View, arrived at by just taking the Recognition Thesis to be the theory of sense, does not work.

Second, we will certainly never verify or falsify most well formed sentences. But we can meaningfully ask the verificationist what is being attributed to someone who's credited with the ability to recognize such a verification. Again, consider Goldbach's conjecture that every even number is the sum of two primes. It is entirely possible that we never will actually prove the thing, yet we can still determine pretty well whether or not people understand it. Moreover, it is not very mysterious how we do this. It is much more plausible than not to maintain that we defeasibly determine of people that they understand Goldbach's conjecture (or really any sentence, for that matter) in part by determining that they know a lot of true sentences involving the subsentential units in Goldbach's conjecture and by gathering evidence that they grasp the compositional principles by which Goldbach's conjecture is built up.

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44 This argument will be treated in more detail, and criticized, in the following chapter.

45 Some intuitionists I have talked to balk when I talk about the possible unprovability of Goldbach's conjecture. I need to make clear that we are not considering any kind of strong "in-principle" unprovability here, but rather the kind of possible unprovability that follows from some contingent fact or sets of facts that prohibit us from finding a proof, this could be human computational limitations, or perhaps something more societal like nuclear annihilation, which is certainly possible for reasons that have nothing to do with Goldbach's conjecture or intuitionism versus classicism.
Given Dummett's desiderata for a theory of sense, the Dummettian needs to say what dispositions a person must have in order for them to be credited with mastery of the compositional principles necessary for understanding sentences. Again, it is entirely unclear how the verificationist can accomplish this, without stating how the verification conditions of sentences are compositionally built up in a manner isomorphic to that in which the truth conditions are built up.

Many contemporary verificationists avail themselves of Heyting, or proof theoretic, semantics for the logical operators, in part, to be able to provide answers to the above problems. Such an approach recursively stipulates verification conditions for sentences of the formal language in question. For 1st order logic the stipulations can be given in this manner.

**Heyting Semantics Definition of Truth**

Φ is true if and only if there exists a verification k of Φ,

where one inductively defines what it is for k to be a verification of Φ as follows:

1. If Φ is a conjunction, Φ = (Φ₁ ∧ Φ₂), then k verifies Φ if and only if one can extract from k constructions k₁ and k₂ that verify Φ₁ and Φ₂ respectively.

2. If Φ is a disjunction, Φ = (Φ₁ ∨ Φ₂), then k verifies Φ precisely when one can extract from k information about which of the terms Φᵢ of the disjunction is true and a construction kᵢ verifying that term Φᵢ.

3. If Φ is an implication, Φ = (Φ₁ → Φ₂), then k verifies Φ precisely when k yields a general method that, from every construction l₁ verifying Φ₁, enables us to find a construction l₂ verifying Φ₂.

4. If Φ is a negation, Φ = (¬Φ₁), then k verifies Φ precisely when k verifies (Φ₁ → ⊥), where the constant ⊥ has no construction verifying it.

5. If Φ is an existential, Φ = (∃x Φ₁(x)), then k verifies Φ precisely when k determines, for which object a Φ₁(a) holds and yields a construction k₁ verifying Φ₁(a).

6. If Φ is a universal, Φ = (∀x Φ₁(x)), then k verifies Φ precisely when k yields a general method that, for every object a, enables us to find a verification kₐ of the proposition Φ₁(a).

Thus, for any formula of 1st order logic the above clauses associate informal ver-
ifications conditions with the formula. For example, for a formula of the form $\forall x \exists y \Phi(x, y)$ we have

$$\forall x \exists y \Phi(x, y)$$ is true if and only if there exists a verification $k$ such that:

1. By 5, $k$ yields a general method that, for every object $a$ enables us to find a verification $k_a$ of the proposition $\exists y \Phi(a, y)$.

2. By 6, $k$ yields a general method that, for every object $a$ enables us to find a verification $k_a$ such that $k_a$ determines for which object $b$ $\Phi(a, b)$ holds.

Of course the above definition is incomplete pending clarification of the notions such as “one can extract information,” and “we give a general method.” There is little agreement among contemporary verificationists about how this is to be done. For example, Neil Tennant and Dag Prawitz think that the introduction and elimination rules of natural deduction formulations of proof systems for the operators in question can be thought of as a very good way to make the above precise. Michael Dummett disagrees and urges the use of Beth Frames to do this. In mathematics proper there are many different proposals for ways to firm up the constructive notions in the above definitions. In any case, for our purposes the above clauses do recursively correlate informal verification conditions with sentences of 1st order logic, and in so doing show how the verification conditions of a complex sentence depend upon the parts of the sentence and the way they are put together.

In this manner, Heyting semantics lends itself to a solution to the two problems mentioned above. First, given that the semantics is supposed to be adequate to meaningful sentences, it carries the presupposition that a sentence is true if and only if there exists a construction verifying it, and that a sentence is not true if and only if there exists a construction refuting it (or verifying its negation). This requirement (called by anti-realists the Knowability Requirement) clearly solves the first problem that besets the Wrong Manifestationist View, since the problem was

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46 These clauses have been taken, with slight modification, from (Dragalin, 1980, pp. 2–3)).
47 See (Prawitz, 1965), (Prawitz, 1974), (Tennant, 1997).
48 See (Dummett, 1991), and (Beth, 1962).
49 See (Dragalin, 1980).
50 In Chapter 2 I show why this is the case more rigorously.
caused by simultaneously supposing that competence is to be explained by the ability to differentiate correct verifications from incorrect ones and that some sentences are true but not verifiable.

Second, the clauses provide canonical, if informal, criteria for the verification of sentences. They show how a person’s grasp of the meaning of a sentence (via the counterfactual which opens this section) is determined compositionally by grasp of its parts and the way they are put together. Thus, they lead to an explanation of what it is to know what the verification conditions of a sentence are, even if the sentence has not been verified. For example, taking the Recognition Thesis and the Heyting semantics clauses above we can directly generate,

\[ X \text{ understands } \forall x \exists y \Phi(x, y) \text{ if, and only if, were } X \text{ presented with a purported verification } k \text{ of } P, \text{ then } X \text{ could recognize whether or not } k \text{ yields a general method that, for every object } a, \text{ enables us to find a verification } k_a \text{ such that } k_a \text{ determines for which object } b \Phi(a, b) \text{ holds.} \]

In this manner, Heyting semantics and the Recognition Thesis together allow a set of dispositions to be specified which satisfy Dummett’s criteria. It should be noted briefly that none of this makes it the case that one who understands a sentence has to be able to discover a verification of that sentence, but rather that she merely be able to recognize it if presented with one.\(^{51}\) The strong verificationism and commitment to the view that verification conditions grasped are essentially compositional allow the Dummettian to make sense of what is constitutive of understanding currently unverified sentences.

Therefore, Dummett’s view is best represented as equating the theory of sense with the claim that a understanding of a sentence is equivalent to the ability to recognize verifications and falsifications, coupled with the demand that one’s semantics compositionally characterize verification conditions. As we have shown, that the semantics are constructive guarantees that the meanings of sentences are such that

\(^{51}\)See (Tennant, 1981), (Tennant, 1984), and (Tennant, 1985) for the canonical discussion of this point.
speakers can be charged with the capacity to recognize verifications and falsifications of those sentences.

Presented in a manner structurally analogous to the other Views we have been considering, we say that Dummett is committed to the following,

**Verificationist Manifestationist View**

(A) A speaker \( S \) is competent in the language \( L \) if, and only if, \( S \) has tacit knowledge of a correct constructive theory of reference for \( L \) (we call this theory \( R_L \)), which compositionally determines verification conditions of the sentences of \( L \),

(B) The theory of meaning for \( L \) (which we will call \( M_L \)) is identical to \( R_L \) plus a theory of sense, which is equal to the claim that, for all sentences \( P \) in \( L \), \( S \) understands \( P \) if, and only if, were \( S \) presented with a construction \( c \) then \( S \) could recognize whether \( c \) verifies \( P \) and whether \( c \) falsifies \( P \), and

(C) A person \( S \) can correctly be attributed tacit knowledge of \( R_L \) if, and only if, for all sentences \( P \) in \( L \), were \( S \) presented with a construction \( c \), then \( S \) could recognize whether \( c \) verifies \( P \) and whether \( c \) falsifies \( P \).

One might wonder, why, after all the hoopla the theory of sense just ends up consisting in the Recognition Thesis. One might wonder why we call it a theory at all. Dummett is quite explicit in maintaining that this is, in fact, one of the primary theoretical advantages of adopting a verificationist semantics. He writes,

> The theory of sense specifies what is involved in attributing to a speaker a knowledge of the theory of reference... When, however, the central notion [of the theory of reference] is an effective one—one the conditions for the application of which a speaker can recognize as obtaining whenever they obtain, like the notions of verification and of falsification—then there appears to be no need for a theory of sense to round out the theory of reference; we could say that, in the theory of meaning of such a type, the theories of reference and of sense merge. In a verificationist or falsificationist theory of meaning, the theory of reference specifies the application to each sentence of the central notion of the theory in such a way that a speaker will directly manifest his knowledge of the condition for its application by his actual use of the language.

(Dummett, (1976b, pp. 84-85))

Thus, use of constructive semantics and commitment to the Recognition Thesis does
the work that the theory of sense was supposed to. Therefore, Dummett's challenge to the non-constructivist is to produce a theory of sense isomorphic to her non-constructive theory of reference such that the manifestation constraints hold.

§ 1.6 CONCLUSION

I begin my conclusion with a caveat, first given by Dummett. Of constructive theories of meaning, Dummett writes

...no serious attempt has ever been made to work out such a theory, even as applied to a formalized version of natural language (i.e. quantificational language for everyday use), we encounter difficulties of detail as soon as we start to think of how such a construction might proceed. I do not in the least rule out the possibility that a thorough investigation would reveal these difficulties to be, after all, difficulties of principle, blocking the construction of any such theory of meaning.

(Dummett, (1976b, p. 35))

In spite of a reservation I have with the way this is stated, I take it to still be true, and (given my reservation) I think it is far more important than Dummett allows.

First the reservation: "a formalized version of natural language (i.e. quantificational language for everyday use)" doesn't mean anything, or doesn't mean anything from a linguistic perspective. Moreover, careful study of Dummett's oeuvre forces one to conclude that he takes rumination about the grasp of simple formal languages like first order logic to be sufficient for the relevant philosophical concerns about meaning. Therefore, though Dummett clearly takes his concerns to have bearing

52First order logic is certainly not expressively rich enough for natural language semantics. Let me list some of the ways: (1.) lambdas are required for compositionality (see (Dowty, Wall, & Peters, 1979)), (2.) the material conditional in first order logic is so syntactically perverse that it is safe to conclude that it does not occur in natural language (rather conditional claims are generalized quantifiers over possible worlds) (see, for example, (Kratzer, 1991)), (3.) first order logic cannot account for anaphora or discourse properties or indefinite descriptions (see, for a recent overview of these debates, (Chierchia, 1995)), (4.) first order logic cannot account for pluralization and mass terms, (see, for example, (Link, 1998)), (5.) first order logic cannot account for generalized conjunction (see, for example, (Gazdar, 1980), (Keenan & Falz, 1985), and (Partee & Rooth, 1983)), (6.) first order logic cannot account for the full range of determiners, (see, for example, (Barwise & Cooper, 1981)), (7.) first order logic cannot account for modals such as 'can' and 'ought' (see, for example, (Kratzer, 1977)), (8.) first order logic cannot account for so-called propositional attitude verbs.
on grasp of natural languages, there’s a sense in which he doesn’t really take the idea of a theory of meaning seriously. For example,

It is not that the construction of a theory of meaning, in this sense, for any one language is viewed as a practical project; but it is thought that when once we can enunciate the general principles in accordance with which such a construction could be carried out, we shall have arrived at a solution of the problems concerning meaning by which most philosophers are perplexed. (Dummett, (1975, p. 1))

Or, more recently, he writes

I am in agreement with Davidson that the correct methodology for the theory of meaning is to enquire into the general principles upon which a meaning theory is to be constructed. (Dummett, (1991, p. 22))

I find this *a priori* approach to philosophical semantics to be irresponsible, in part because it leads us to misjudge the success conditions of our endeavors. Linguistic semantics is an empirical enterprise, and one cannot claim that an extant empirical theory is disconfirmed without offering a successor theory that better explains the phenomena within the explanatory scope of the initial theory. Enquiry into general principles doesn’t cut it.

Thus, while constructivisms have a very rich history in mathematics proper, until someone has actually provided a constructive semantics for a suitably rich fragment of natural language, or at least explained clearly what such a semantics would look like, Dummettian anti-realism as a viable position concerning the syntax and semantics of natural languages remains a promissory note. One’s Grandmother might say, “If we’re so smart, why aren’t we rich.”

In this chapter I have explicated three things: (1) Dummett’s explanatory desiderata for a philosophical theory of meaning, (2) Dummett’s understanding of a Wittgensteinian “meaning is use” claim, and (3) how verificationism allows one to both iden-

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It’s one thing to argue philosophically that higher order logics should be considered math instead of logic (though see (Shapiro, 1991)), but sentences such as “I believe that natural language is both extensional and first order” (in so far as they meaning anything at all) are dangerously close to saying “I believe that heat consists of phlogiston leaving an object.”

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tify grasp of meaning with correct use as well as satisfy Dummett's desiderata for the theory of meaning. In the next chapter I will begin the process of adjudicating these positions, by arguing that explaining understanding of sentences via evidential prowess does not entail a verificationist construal of truth, and in fact undermines such a construal. Thus, in Chapter 2 I shall argue that if verificationism (about grasp of meaning) is true, then truth isn't verifiability. Then, after Chapter 3's critical detour through arguments against classical logic, I shall critique Dummett's claim that meaning is use, and his broader explanatory demands upon a theory of meaning. These critiques will lead to a defense of the alternative heuristic sketched in Chapter 5.

Thus, we have so far merely explicated three of the claims with which we opened this chapter,

1. Knowing the meaning of a sentence requires knowing what it would be for that sentence to be true,
2. All that is involved in knowing the meaning of a sentence is knowing how to use that sentence correctly, and
3. Given 1. and 2., the assumption that all true sentences are verifiable allows us to provide the best explanation of knowledge of the meaning of a sentence.

An evaluation of these three claims, as well as the fourth,

4. The assumption that all true sentences are verifiable provides strong evidence for the incorrectness of classical truth conditional semantics, as well as evidence for the correctness of intuitionistic constructive semantics, will not be complete until the end of our endeavor.
CHAPTER 2

VERIFICATIONISM

In the last chapter we sketched Dummett’s positive proposals concerning the theory of meaning. Here we begin the process of evaluating these proposals. For Dummett, understanding “meaning is use” in terms of people’s ability to recognize verifications and falsifications of claims is supposed to provide strong evidence for the correctness of a constructive account of truth, and hence force commitment to the claim that verifiability is both necessary and sufficient for truth (the position I refer to as the Knowability Requirement). Pace Dummett, I will argue that reasonable verificationist construals of the Dummettian manifestation requirements, in fact, do not provide evidence for the correctness of a constructive account of truth.

Using Heyting Semantics terminology, the verificationist versions of the manifestationist constraints can be given in this manner.

**Publicity Requirement**
If two speakers agree, for all possible constructions $c$, whether $c$ verifies $P$ and whether $c$ verifies $\neg P$, then they agree about the meaning of $P$.

**Recognition Thesis**
$X$ understands $P$ if, and only if, were $X$ presented with a construction $c$, then $X$ could recognize whether $c$ verifies $P$ and whether $c$ verifies $\neg P$.

Our primary concern in this chapter is to consider to what extent these two theses
provide evidence for the Knowability Requirement, stated again here.

**Knowability Requirement**

$P$ is true if, and only if, there exists a construction $c$ verifying $P$.

I shall argue that, in so far as the manifestation constraints are interpreted in a plausible manner, they do not provide evidence for the Knowability Requirement. But then one possible argument for the replacement of standard truth conditional semantics with something more exotic will have been blocked.

First, in section 2.1, I attend to A.J. Ayer's discussion of verifiability in Chapter 1 of *Language, Truth, and Logic* to show that the Recognition Thesis and Publicity Requirement, as given, need to be slightly amended so that they do not preclude a speaker from understanding sentences which are unverifiable due to technological or epistemic limitations which are arguably irrelevant to linguistic understanding. In section 2.2 I argue that since the (amended) manifestation constraints are supposed to provide evidence for the Knowability Requirement, the notion of "a construction $c$" verifying a sentence must be, in a way to be made precise, at least as strong in the Recognition Thesis as it is in the Knowability Requirement.

Then, in section 2.3, by recapitulating more of Ayer's discussion I will show that the ineliminable defeasibility of warrants for empirical claims forces the notion of verification in the Recognition Thesis to be very weak. But then it is easy to show, given the restriction arrived at in section 2.2, that, when applied to nonmathematical sentences with paradigmatically defeasible warrants, the Knowability Requirement is false. I will do this by deriving a contradiction of the following sort from it. Where "$\exists(P)$" means "there exists a sentence $P$ such that..." , "$\forall(P)$" means "for all sentences $P$ it is the case that...", and "$\exists c$" means "there exists a construction

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1I think the silent majority would be willing to entertain the idea that verifiability was sufficient, but not necessary, for truth in mathematics, and necessary, but not sufficient, for truth in empirical discourses. This chapter will only go so far as to show that the silent majority has a plausible view about empirical discourses.
ce verifying $P$,” the structure of our argument shall be the following.

\[
\begin{align*}
TP & \rightarrow VP \\
\text{Ayer's considerations} & \quad \text{Short proof} \\
\exists P(VP \land V\neg P) & \quad \forall P\neg (VP \land V\neg P)
\end{align*}
\]

Since the Knowability Requirement states that the existence of a verifying construction is necessary and sufficient for the truth of a claim, Ayer's considerations will show that when the verifiers are considered as defeasible warrants that are plausibly recognizable as bits of evidence humans can assess (as they need to be if the Recognition Thesis is to be true of nonmathematical sentences), verifiability cannot be both necessary and sufficient for truth.

Then, in section 2.4, I discuss orthodox Heyting Semantics provability, Tennant's notion of constructive falsifiability, and then Wright's superassertibility. For each position I am able to pose a dilemma by considering two different versions of the Recognition Thesis, each employing the respective epistemic construal of truth (or falsity in Tennant's case). For each construal I shall argue that the first version of the Recognition Thesis is false, and that the second version provides no evidence for the correctness of the Knowability Requirement. In this chapter I will not attempt to show that the Knowability Requirement is false, but rather that it needs independent motivation above and beyond acceptance of the Recognition Thesis.

In the next chapter I will attend to the revisionary arguments from Wright, Dummett, and Tennant which are properly understood as attempting to provide such independent motivation. Wright, Dummett, and Tennant's arguments all attempt to show that any semantics sound with respect to classical logic cannot be consistently utilized for natural language semantics if the Recognition Thesis and Verification Constraint are true. However, they claim, commitment to a constructive semantics which equates truth with some kind of knowability (such as provability) does not run afoul of the Recognition Thesis. Before attending to such arguments in the
next chapter, we must explicate and render reasonable the Recognition Thesis and Verification Constraints themselves.

§ 2.1 A Necessary Idealization

We begin with one of the distinctions which Ayer made in *Language, Truth, and Logic*, the distinction between verifiability in practice and verifiability in principle. Verifiability in practice can defined in this manner.

A construction $c$ verifies in-practice $P$ if, and only if, $c$ is a warrant for $P$, and $c$ can be recognized by minimally competent people with little or no idealization of their cognitive capacities or technologies.

For example, I can currently verify the sentence "No dog is on this table" right now. While it must be noted that the distinctions between both minimally competent people and incompetent people, as well as between minimally competent people without idealized capacities and technologies and minimally competent people with idealized capacities and technologies is surely vague, nothing I shall say hinges on their being precise.

It is not difficult to show that adopting verifiability in practice in the Recognition Thesis leads to absurdity. Since the second conjunct in the definition of verifiability in practice concerns how much idealization is required for people to be charged with recognizing the warrant, the appropriate form of Recognition Thesis might be taken to be,

Recognition Thesis'

$X$ understands $P$ if, and only if, were $X$ presented with a construction $c$ (where $c$ can be recognized by minimally competent people with little or no idealization of their cognitive capacities or technologies), then $X$ could recognize whether $c$ is a warrant for $P$ and whether $c$ is a warrant for $\neg P$. The problem with this is that it is trivially satisfied for sentences which are such that the warrants for and against them cannot be recognized by minimally competent people with little or no idealization of their cognitive capacities or technologies. For
any such sentence, there is no possible world containing a warrant for or against it which can be easily recognized. Therefore, at best the speaker is able to recognize failed warrants for the claim. But then the thesis wouldn’t distinguish between a speaker’s grasp of any two sentences with distinct meanings which are both in practice undecidable.²

This result is unacceptable. For if two sentences are different in meaning, and a speaker understands both sentences, then the speaker should know that they are different in meaning. However a speaker can satisfy the above criteria for understanding two sentences which are distinct in meaning, and still remain unaware that the two sentences are distinct in meaning. Thus, we can conclude that criteria for grasp of meaning ought to individuate meanings as well. This point will be considered in slightly more detail in the following section. For now it is sufficient to realize that the above version of the Recognition Thesis violates it.

A more direct way to criticize the “in practice” definition of verification is to attend to the unpalatable consequences of adopting such verifiability as a necessary condition upon truth. Since there are many sentences such that neither they, nor their negations, are verifiable in practice, imposing verifiability in practice as a necessary condition upon truth would render our language inconsistent, simply because our current abilities and investigative techniques are limited.³

As Ayer realized, the verificationist has to modalize her conception of verifiability. If verifiability is to serve as a necessary condition on truth then verifiability must be

²Neil Tennant (Tennant, (p.c., 1997)) has suggested that this argument makes the implausible assumption that all failed and easily surveyable warrants won’t reveal a difference through the kinds of defeats discovered in them. This strikes me as more plausible than not. However, as mentioned in a previous footnote, such an admission immediately undermines the anti-realist’s claim that a verificationist explanation of competence entails that all truths are verifiable.

³This argument does not presuppose excluded middle. Let “VP” be equal to “P is verifiable in practice.” Take sentence P such that neither P nor ¬P is verifiable in practice. Then if we have ∀Q(TQ → VQ), we can substitute both P and ¬P for Q, and by two applications of Modus Ponens get TP ∧ ¬TP.
in-principle, which we can define in this manner.

A construction $c$ verifies in-principle $P$ if, and only if, $c$ is a warrant for $P$ and $c$ can be recognized by minimally competent people with arbitrarily large but finite idealization of their cognitive capacities and technologies.

For example, there are infinitely many simple number-theoretic claims involving numbers too large to allow of human or machine-aided computation, which are not verifiable in practice, but are verifiable in principle. Paradigmatic empirical sentences which may be verifiable in principle, but not in practice, involve conditions in inaccessible or simply far away regions of space-time. Thus, verifiability in practice is clearly a contender at best for being a sufficient condition for truth.

As well, it lends itself to an acceptable formulation of the Recognition Thesis. However, the appropriate manner in which to reformulate the claim is not immediately apparent. We might first try to reformulate it in this manner.

**Recognition Thesis$^\prime$**

$X$ understands $P$ if, and only if, were $X$ presented with a construction $c$ (where $c$ can be recognized by minimally competent people with arbitrarily large but finite idealization of her cognitive capacities and technologies), then $X$ could recognize whether $c$ is a warrant for $P$ and $c$ is a warrant for $\neg P$.

This formulation would still fail, not because of vacuous satisfaction of the counterfactual conditional, but rather because the counterfactual conditional would be false in many cases where it is clear that the person $X$ in question does understand the claim $P$. If $X$'s abilities aren't idealized, then even if $X$ is presented with constructions which warrant claims about, say, planets in other galaxies or intractably large sums, $X$ would still not be able to recognize such constructions as warrants, and hence wouldn't understand the claim.

Clearly what is required is an idealization of the speaker in the antecedent of the counterfactual conditional. This is in fact how anti-realists reformulate the Recog-
nition Thesis when considering this problem.

**Recognition Thesis**

X understands P if, and only if, were X's cognitive capacities and technologies finitely extended (in an appropriate manner), and X were then presented with a construction c, then X could recognize whether c is a warrant for P and whether c is a warrant for ¬P.

One might argue that this Recognition Thesis is not relevant to a speaker's understanding of sentences because of the idealization in the antecedent of the counterfactual conditional. More simply, what does a possible worlds counterpart of a speaker which can recognize huge sums as being correctly added have to do with that speaker's understanding of "plus"?

I take this to be a challenge to the anti-realist, but one which can be met satisfactorily for many areas of discourse. The idealization strikes me as fine as long as it is appropriately related to a speaker's actual inferential and evidentiary dispositions as she manifests them around her. In his recent book, *The Taming of the True*, Neil Tennant suggests how such an idealization can be appropriately limited by the actual behavior of speakers. He gives the constraint in this manner.

\[
(R_F) \text{ for a speaker } S \text{ to be credited with a grasp of the meaning of a sentence } \Phi \text{ we should have good grounds for believing that, if presented with some finite piece of discourse } \Psi, S \text{ would be able to deliver a correct verdict on any aspect of } \Psi \text{ that is relevant to arriving at a correct judgement of the form '}\Psi \text{ is a proof of } \Phi \text{ ' or of the form '}\Psi \text{ is a disproof of } \Phi \text{ ' or of the form '}\Psi \text{ is neither a proof nor a disproof of } \Phi \text{ '; that is, for any such aspect, } \alpha S \text{ would, after some time, be able to judge whether } \alpha \text{ was as it ought to be, in order for } \Psi \text{ to have the status in question.} \]

\[(\text{Tennant, (1997, p. 154))}\]

It is to be hoped that by adopting something like Tennant's \((R_F)\) constraint, the idealization in our final form of the Recognition Thesis can be adequately restricted.

Without such a constraint it would be the case that we would be forced by the Recognition Thesis" to say that the spider understands propositions of real analysis. This is because a spider's cognitive capacities could be extended enough
for it to be a very good proof checker for proofs in real analysis. Tennant’s condition, used to constrain the appropriate idealizations, would both prevent the spider from being attributed grasp of real analysis, and sanction the use of the idealizations for competent users of the language.

If someone has the ability at present to check every step of a proof correctly (even if the proof itself has too many steps for them to be able to check every step in their lifetime), then it is the case that were their cognitive capacities extended they could gestalt the whole proof as correct or incorrect. More important though, the lack in capacities that prevents them from checking the whole proof is not relevant to their understanding of the sentence in question.

Moreover, arguably, the extension in question is not question-begging, in that the Recognition Thesis' is not equivalent to saying that a person understands a sentence if and only if, were they a whole bunch smarter, they would then understand the sentence. This is as much discussion of this idealization as I will engage in. Given that this chapter is wholly critical of anti-realism, my argument is more charitable if I allow that the idealization is reasonable.

Though Ayer himself distinguished between verifiability in practice and verifiability in principle primarily in terms of characterizing the notion of a verification, I have shown that the problem which motivated characterizing verifiability in terms of verifiability in principle can be addressed by simply changing the Recognition Thesis. Given this, and given that the rest of our discussion in this chapter concerns the appropriate type of verifiability occurring in the Recognition Thesis, I will renotate the Recognition Thesis' by substituting “c verifies P” in for “c is a warrant for P” in the definition. Then we shall be able to return to Ayer’s other discussion, one which (given the Dummettian context we are in) is more appropriately about the kinds of verifiers in the Recognition Thesis, rather than about the Recognition Thesis itself.

Thus, in what follows, when I refer to the “Recognition Thesis” (without any primes after it), unless otherwise noted, the reference should be understood to be to
the reformulated version here.

**Recognition Thesis**

\( X \) understands \( P \) if, and only if, were \( X \)'s cognitive capacities and technologies finitely extended (in an appropriate manner), and \( X \) were then presented with a construction \( c \), then \( X \) could recognize whether \( c \) verifies \( P \) and \( c \) verifies \( \neg P \).

From the above argumentation it is clear that the other manifestation constraint similarly requires idealization of the speaker’s capacities and technologies in their statement. The amended versions can be given in this manner.

**Publicity Requirement**

If two speakers with cognitive capacities and technologies finitely extended (in an appropriate manner) agree, for all possible constructions \( c \), whether \( c \) verifies \( P \) and whether \( c \) verifies \( \neg P \), then they agree about the meaning of \( P \).

Now we are in a position to begin considering the extent to which the manifestation constraints can be claimed to provide evidence for the claim that the existence of a verification is both necessary and sufficient for truth.

§ 2.2 Verification in the Manifestation Constraints and Knowability Requirement

For semantic anti-realists the satisfiability of the manifestation constraints by a speaker and a sentence is supposed to explain the speaker's understanding of the sentence in terms of how the speaker uses the sentence. The dispositions adverted to in the Recognition Thesis are to be the kinds of practical capacities which Dummett argues must be correlated with the truth conditions generated by a compositional semantics (if we are to be able to attribute tacit knowledge to a speaker of the compositional semantics in question).

Moreover, the Recognition Thesis, along with the other constraints, is supposed to provide evidence for the Knowability Requirement. In many writings of Dummett, Tennant, and Wright, issues concerning language acquisition, normativity, and com-
munication are raised to provide evidence for the Recognition Thesis. However, it is very difficult to discern a direct argument from the Recognition Thesis to a Knowability Requirement.

Here I will just focus on one half of the biconditional composing the Knowability Requirement, where the existence of a verification is taken to be necessary for the truth of a claim. We can give this principle in this manner.

Verification Constraint
If \( P \) is true, then \( P \) is verifiable.

Thus, in this section I will solely be concerned with the question of what the proper notion of “verifiable” occurring in the Verification Constraint is, given that the Verification Constraint is supposed to follow from the Recognition Thesis. Again, my conclusion will be that in-so-far as the Verification Constraint is plausible, the Knowability Requirement is not. The Verifiers mentioned in the statement of the Verification Constraint are not sufficient for truth.

Given that the notion of “verifiability” is so ambiguous, we do well to stipulate some notational conventions at this point, and stick to them throughout our discussion. “\( P \) is verifiable” means that it is possible that \( P \) be verified. What is the strength of this possibility though? In a sense it is clearly possible to verify that cows fly, because we know what it would be to verify it. However in another sense it is not possible to verify that cows fly, because we’re not going to run into any flying cows. This is the first ambiguity we shall have to separate. The two modals can be elegantly distinguished by sorting the domain of the quantifiers which range

4See (Dummett, 1973), (Tennant, 1987, part I), and (Wright, 1993, pp. 1-43)) for arguments by anti-realists. My justification for not going into these arguments here (though see Chapter 4 for a counterargument) is that the assumption of their correctness renders my criticism of anti-realism more compelling.
over constructions.

\(\exists^0 \! c(\! VcP)\) = there exists a construction \(c\) which verifies \(P\) in some possible world.

\(\Diamond \! V \! P = P\) is verifiable in the sense explicated by \(\exists^0 \! c(\! VcP)\).

\(\exists \! c(\! VcP)\) = there exists a construction \(c\) in the actual world which verifies \(P\).

\(\! V \! P = P\) is verifiable in the sense explicated by \(\exists \! c(\! VcP)\).

It should be noted briefly that in both the Recognition Thesis and the Publicity Requirement as given, the constructions in question are possible constructions. In the Recognition Thesis, they are mentioned in the consequent of a counterfactual conditional; and in the Publicity Requirement they are explicitly stated to be possible constructions. Now with our notational conventions, we are able to attend to the business at hand.

To the extent that an argument for the Recognition Thesis entailing the Verification Constraint can be extracted from the relevant writings, it involves reflection on what goes wrong when we assume a sentence (say \(P\)) is both understood by someone (say Frank) and neither verifiable nor falsifiable. Then we can state this semi-formally using the notational conventions from above.

1. \(\neg \exists^0 c(\! VcP)\) \(\land\) \(\neg \exists^0 c(\! Vc\neg P)\).

2. Frank understands \(P\).

3. Were Frank's cognitive capacities and technologies finitely extended (in an appropriate manner), and were Frank then presented with a construction \(c\), then Frank could recognize whether \(c\) verifies \(P\) and whether \(c\) verifies \(\neg P\). (from 2. and the Recognition Thesis).

4. In every possible world where Frank's cognitive capacities and technologies are finitely extended (in an appropriate manner) and Frank is presented with a construction \(c\), Frank could recognizes that \(c\) doesn't verify \(P\) and \(c\) doesn't verify \(\neg P\). (from 1. and 3.)

Then the worry concerns how to individuate the meanings of such sentences.\(^5\) If sentences are individuated by the set of possible constructions which succeed and

\(^5\)Much of the discussion about individuating meanings here owes its genesis to personal communications from Bob Hale, Neil Tennant, and Crispin Wright.
fail to verify them, then any two undecidable sentence would have the same meaning. The anti-realist unsurprisingly uses verification conditions to individuate the meanings of sentences. This position can be given in this manner

**Anti-Realist Identity Criterion for Meanings**

Two sentences $P$ and $Q$ have the same meaning if, and only if, for all possible constructions $c$, $VcP$ if, and only if, $VcQ$, and $Vc\neg P$ if, and only if, $Vc\neg Q$.

This on its own entails immediately that any two undecidable sentences have the same meaning. But then, the anti-realist can either say that there are no undecidable sentences or that every undecidable sentence has the same meaning. Clearly, the former option is more palatable.

Of course we don’t yet have an argument for the above identity criterion. However, by considering Frank’s predicament concerning $P$, we can use the Publicity Requirement to motivate such an argument. The basic form of this argument is a *reductio*. We will show that if there can exist undecidable sentences with distinct meanings, then it would be possible for two speakers to disagree about the meaning of a sentence and still agree on the set of constructions which verify or falsify the sentence, contrary to what the Publicity Requirement asserts.

Again assume (for *reductio*) that there exists a sentence $P$ which, given the meaning Frank attaches to it, is neither verifiable nor falsifiable. Again, it follows from the Recognition Thesis that Frank could recognize of any construction $c$ that $c$ neither verified nor falsified $P$. Also assume that another speaker, Bill, attaches a distinct meaning to $P$; but given Bill’s meaning, the sentence is still neither verifiable nor falsifiable. But this straightforwardly contradicts the Publicity Requirement. Bill and Frank agree for all constructions $c$ that $c$ neither verifies, nor falsifies, $P$. Thus, it seems the anti-realist, committed to the Publicity Requirement and the Recognition Thesis, needs to individuate sentence meanings in terms of possible

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[^8]: See especially any of Dummett’s discussion of Fregean senses (e.g. (Dummett, (1976a. (pp 23-33)), and (Dummett, 1975)).
verification of those sentences. But this is just Identity Criterion given above. If
the Identity Criterion is correct, then we do have good evidence that no sentence
can be such that there does not exist a possible verification or falsification of it. For
if such undecidable sentences existed, by the Identity Criterion they would all have
the same meaning, which is bizarre. Thus, the anti-realist concludes that no such
undecidable sentences exist.

Another similar way to argue that the Recognition Thesis provides evidence for
the Verification Constraint, one perhaps more of a piece with the writings cited
above, involves the notion of a canonical manner in which grasp of meaning is mani-
ifested. That is, we assume that the manner in which grasp of meaning is manifested
in the Recognition Thesis is the correct way in which issues concerning the individ-
uation of the meanings of claims are adjudicated.

On the assumption that the Recognition Thesis is correct, we do have some evi-
dence that the verifying and falsifying constructions mentioned in it do provide such
a canonical manner to individuate the meanings of sentences. As I have argued, if
two sentences are different in meaning, and a speaker understands both sentences,
then the speaker should know that they are different in meaning. However, if the
anti-realists' identity criterion for meanings is false, then the Recognition Thesis
entails that a speaker can understand two sentences distinct in meaning and still
remain unaware that the two sentences are distinct in meaning. Thus, on the as-
sumption that grasp of meaning of two sentences different in meaning is sufficient
for recognizing that the two sentence are different in meaning, we can conclude that
criteria for grasp of meaning ought to individuate meanings as well. Put in terms of
Dummettian Manifestationism, this requirement becomes,

**Canonicity Requirement**
The dispositions to correctly use a sentence mentioned in the Publicity
Requirement and Recognition Thesis must individuate the meanings
of sentences.

Then, once one decides to attempt to provide a Dummettian theory of meaning in
terms of verification conditions, the requirement becomes the following, which is es-
entially the same as the Anti-Realist Identity Criterion for meanings, given above.

**Verificationist Canonicity Requirement**
The constructions quantified over in the Publicity Requirement and Recognition Thesis must be such that two sentences $A$ and $B$ have the same meaning if, and only if, for all constructions $c$ (from the same domain of constructions quantified over in the Publicity Requirement and Recognition Thesis), $c$ verifies $A$ if, and only if, $c$ verifies $B$, and $c$ verifies $\neg A$ if, and only if, $c$ verifies $\neg B$.

This requirement won’t be extensively discussed and criticized until the closing sec-
tion of Chapter III and Chapter IV, where I both show that it forms a proper locus of disagreement between the Dummettian anti-realist and Dummettian anti-anti-
realist, and go on to defend the anti-anti-realist as principled in denying it. For now, we shall assume that it is true.

Now that we have presented the Verificationist Canonicity Requirement we can show how the Recognition Thesis provides evidence for a Verification Constraint. To do this we again begin with the assumption (for reductio) that some sentences with intuitively distinct meanings are neither verifiable, nor falsifiable. From this it follows that speakers who understood the sentences could never, via the manner suggested by the Recognition Thesis, manifest that they understood the different sentences to be different in meaning.\(^7\) Then if the Recognition Thesis provides a canonical manner in which grasp of meaning is manifested, the only way they could manifest their knowledge that the sentences were different in meaning would be for them to recognize of distinct verifiers or falsifiers that they were verifiers or falsifiers of the sentences in question. But then, since the sentences in question are neither verifiable nor falsifiable, there would be no way for them to be able to discern whether or not they attached the same or different meaning to the problematic sentences.

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\(^7\)Most will think these arguments transparently fallacious, as the speakers could determine that they didn’t agree in the meaning of the sentences, presumably by realizing the sentences had different inferential roles. Dummett tries to block such a move by criticizing it as unacceptably holistic. In Chapters 4 and 5 I defend the holist.
Thus, if the canonical test of grasp of meaning is given by the Recognition Thesis, all sentences must be such that they cannot be neither verifiable nor falsifiable.

Given our notational conventions, we can formalize this claim about the non-existence of an undecidable sentence as $\forall P \neg (\neg \exists^0 c(VcP) \land \neg \exists^0 c(Vc\neg P))$ or, more perspicuously, $\forall P \neg (\neg \Box V P \land \neg \Box V \neg P)$.

This claim follows logically from verifiability being a necessary condition upon truth. However, we are trying to discern an argument to the conclusion that verifiability is a necessary condition for truth, not discern independent arguments for something that follows from taking verifiability to be a necessary condition on truth. The classically equivalent claim, $\forall P (\Box V P \lor \Box V \neg P)$, does classically entail that verifiability is a necessary condition for truth, on the assumption that verifiability is sufficient. The proof is straightforward. Assume that $P$ is both true and not verifiable ($TP \land \neg \Box V P$). Then, since any claim or its negation is verifiable, we know that $\neg P$ is verifiable ($\Box V \neg P$). But then, since verifiability is sufficient for truth, it follows that $\neg P$ is true, which contradicts our initial assumption. Thus, no claim is such that it is true and not verifiable. This is classically equivalent to saying that if a claim is true, then it is verifiable.

Thus, if the notion of verifiability in the Recognition Thesis is such that the existence of a verifier is sufficient for the truth of a claim, and if the above reasoning is sound, it is the case that the Recognition Thesis entails the Verification Constraint. However, if no such assumption is made about the verifiers mentioned in the Recognition Thesis being sufficient for truth, we do not yet have an argument for the Verification Constraint. From the Recognition Thesis and the above train

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8I could ignore any issue concerning the rampant use of strictly classical (non-intuitionistically valid) reasoning in my explication of what is treated by its authors as a lemma in a proof to the conclusion that strictly classical reasoning is unsatisfactory. As far as I am aware, nobody has produced an intuitionistically valid argument for the Verification Constraint. The previously cited sources all start with the assumption that a sentence is true yet unverifiable, derive a contradiction, and then conclude that if a sentence is true, then it is verifiable. This reasoning is, of course, intuitionistically unsound, as it derives $(P \rightarrow Q)$ from $\neg (P \land \neg Q)$. More importantly though, I show that this argument isn't convincing even if one allows the classical moves. Moreover, the inductive argument I do discern is intuitionistically valid.
of thought, we have that either a claim or its negation is verifiable, but this does nothing to prohibit the existence of a true claim that is falsifiable, and not verifiable, without the assumption of the sufficiency of such verifiers for truth.

Since the conclusion of section 2.3 will be that the kind of verifiability mentioned in the Recognition Thesis is not sufficient for truth, it would behoove us to attempt to find an argument from the Recognition Thesis to the Verification Constraint which does not rest on the sufficiency claim. I will not here incur the burden of defending the claim that all extant arguments from a Recognition Thesis to a Verifiability Constraint presuppose the sufficiency of verifiability for truth, so I will make the weaker claim that I have yet to find, or discover, a direct argument that doesn't.\(^9\)

It should be clear that this consideration (as well as the consideration concerning how the kind of verification in the Verification Constraint should be the same kind as occurs in the Recognition Thesis) is still valid, even if we resolutely present both the Recognition Thesis and the Verification Constraint as entailing the intuitionistically weaker claim that every sentence is not such that it is both not verifiable and not falsifiable.

However, the somewhat complicated dialectic presented in Chapter I does provide evidence for the necessity claim. As established in Chapter I, Dummett was one of the first to argue that there are difficulties in subscribing to the Chomskyan assumption that understanding of a language is in virtue of tacit knowledge of a grammar for that language, where the semantic component of this grammar is understood to be model-theoretic. For Dummett, the Verification Constraint was a way to attempt to constrain a semantics as being one in which the Chomskyan desideratum can be fulfilled. Given this dialectic, the entailment of \(\forall P - (\sim \diamond V P \land \sim \diamond V \neg P)\) by the Verification Constraint can be thought of as providing evidence for the claim that verifiability is a necessary condition on truth.

\(^9\)Neil Tennant (p.c.) has told me that his impression is that the notion of verifiability proposed in the Recognition Thesis has always been assumed to be sufficient for truth by proponents and opponents of Dummettian anti-realism.
If the notion of truth determined by our semantics is such that verifiability is plausibly a necessary condition for truth, then we do have assurance that the semantics is appropriately psycholinguistically constrained, such that it will not violate the Recognition Thesis. While, deductively, this is equivalent to the fallacy of affirming the consequent (if \( RT \) then \( X \), if \( VC \) then \( X \), \( RT \); therefore \( VC \)), construed inductively this kind of evidence for the truth of theoretical claims is not unreasonable, and quite common in linguistics; so to the extent that Dummett’s broader dialectic is reasonable, we do have some evidence for verifiability being a necessary condition upon truth. Moreover, the kind of evidence in question is of a piece with Dummett’s broader claim that a verificationist semantics can be used in a theory of meaning which is both cognitive and semantic (the main claim considered in this and the prior chapter), while classical semantics cannot (the main claim considered in the following chapter). Thus, with the notational conventions as before, Dummett’s reasoning can be given in this manner.

1. Considerations about normativity, communication, and acquisition provide evidence for the Recognition Thesis.

2. The Recognition Thesis entails \( \forall P \neg (\neg \diamond V P \land \neg \diamond V \neg P) \).

3. The Verification Constraint entails \( \forall P \neg (\neg \diamond V P \land \neg \diamond V \neg P) \).

4. Therefore, we have some good theoretical reasons for so characterizing truth that the verification constraint is correct.

In this sense we say that the Recognition Thesis provides evidence for the Verification Constraint.

From this we can conclude that the notion of “verifiability” cannot be stronger in the Verification Constraint than it is in the Recognition Thesis, if the Recognition Thesis is taken to provide evidence for the Verification Constraint in the manner sketched here. Being clear about the necessity of this restriction requires disambiguating the \( V \)'s as they occur in both the Recognition Thesis and the Verification
Constraint. Thus we have.

**Recognition Thesis**

$X$ understands $P$ if, and only if, were $X$'s cognitive capacities and technologies finitely extended (in an appropriate manner) and $X$ were then presented with a construction $c$, then $X$ could recognize whether $\Diamond V_{RT}cP$ and whether $\Diamond V_{RT}\neg P$.

**Verification Constraint**

If $P$ is true, then $\exists^\diamond c(V_{VC}cP)$.

With the disambiguation, the above argument becomes the following.

1. Considerations about normativity, communication, and acquisition provide evidence for the Recognition Thesis.
2. The Recognition Thesis entails $\forall P(\neg \Diamond V_{RT}P \land \neg \Diamond V_{RT}\neg P)$.
3. The Verification Constraint entails $\forall P(\neg \Diamond V_{VC}P \land \neg \Diamond V_{VC}\neg P)$.
4. Therefore, we have some good theoretical reasons for so characterizing truth that the Verification Constraint is correct.

Now we can clearly state and defend the restriction that the notion of “verification” in the Verification Constraint (call it $V_{VC}$) cannot be stronger than its occurrence in the Recognition Thesis (call it $V_{RT}$). Since the argument as presented essentially presents inductive evidence for the Verification Constraint we can give our restriction in this manner.

**Inductive Restriction**

The Recognition Thesis entails $\forall P\neg(\neg \Diamond V_{VC}P \land \neg \Diamond V_{VC}\neg P)$.

To see why this is the case, consider an even more schematic version of the above inductive argument.

1. We have good evidence for $RT$.
2. $RT$ entails $V$.
3. $VC$ entails $V'$.
4. We have some evidence for $VC$, because adopting it will constrain our semantics so as not to violate $RT$.

From this it is clear that $VC$ could be taken to place an appropriate constraint on the semantics only if $RT$ entailed $V'$. This is because, if a semantics allowed $\neg V'$ to
be a coherent possibility, and \( RT \) entailed \( V' \), by one application of *Modus Tollens* we would have that \( \neg RT \) were a coherent possibility. Thus, the assurance that \( VC \) entails \( V' \) is only evidence for \( VC \) (given \( RT \)) if \( RT \) entails \( V' \). But, given that \( V' \) stands for \( \forall P \neg (\neg \Box V_{VC} P \land \neg \Box V_{VC} \neg P) \), the claim that \( RT \) entails \( V' \) is equivalent to the Inductive Restriction.

Since there are no arguments I am aware of for the claim that the Recognition Thesis provides evidence for the Verification Constraint other than those considered here (either the deductive arguments which assume that the verifiability in question is sufficient for truth, or the inductive argument which can be extracted from the broader Dummettian dialectic), I will assume that the Inductive Restriction is motivated in the same way as the claim that the Recognition Thesis entails \( \forall P \neg (\neg \Box V P \land \neg \Box \neg V \neg P) \). But then, since the arguments are the same, it is safe to assume that the two kinds of verification in both the Recognition Thesis and the Verification Constraint are equivalent.

This fairly obvious claim is extraordinarily important for the dialectic of this chapter. From Ayer's considerations, presented in the next section, we will have that the kind of verifiability appropriate to the Recognition Thesis is such that, if it is necessary for truth, then it is not sufficient for truth. But then we shall be able to show that the kind of verification in the Verification Constraint is not sufficient for truth. But then, the Knowability Requirement, which states that verifiability is both necessary and sufficient for truth, is false (as long as "verifiability" is the same in the Recognition Thesis and the Knowability Requirement). On the other hand, if the kind of verifiability in the Knowability Requirement is stronger than that in the Recognition Thesis, no evidence has been given by the Dummettian for the truth of the Knowability Requirement.

Prior to beginning our discussion of defeasibility, I must consider one dialectical move that the opponent of the anti-realist will certainly make at this point. Given that the Verification Constraint motivated by the Recognition Thesis only demands
that there exist a construction in some possible world which verifies the claim, it
might already be obvious that no evidence has been provided for the claim that
verifiability is sufficient for truth. Taking the verifiability mentioned above to be
sufficient for truth as well would yield the following Knowability Requirement.

Knowability Requirement

\[ P \text{ is true if, and only if, it is possible that there exists a construction } c \text{ which verifies } P \text{ (or more formally, } TP \leftrightarrow \Diamond V_{RT} P, \text{ or equivalently } TP \leftrightarrow \exists^0 c(V_{RT} c P). \]

But then, the anti-anti-realist will argue, it is clear that the possible existence of a
verifier is not sufficient for truth. The uncharitable view would then be that anti-
realists systematically equivocate between the two types of verifiability, going from
verifiability as the possible existence of a construction to verifiability as the actual
existence of a construction which may not yet be discovered.

I think the passage from one type of verifiability to the other could be motivated
by an epistemic understanding of the modal “is possible.” If “It is possible that P”
were understood as something like, “If enough investigation were performed P would
be discovered to be true” then no equivocation has been committed, but in fact, given
the notational conventions followed thus far, we would have both that \( (VP \leftrightarrow \Diamond V P) \),
and \( (\exists c(V_{RT} c P) \leftrightarrow \exists^0 c(V_{RT} c P)) \). Then the Knowability Requirement above would
be equivalent to

Knowability Requirement

\[ P \text{ is true if, and only if, there exists a possibly undiscovered con-
struction } c \text{ which verifies } P. \text{ (or more formally, } TP \leftrightarrow V_{RT} P, \text{ or }
equivalently } TP \leftrightarrow \exists c(V_{RT} c P). \]

Unfortunately, as far as I’m aware, nobody has argued that the modals should be
understood in this manner. I conjecture that this is because most of the discussion
in the literature is about mathematics, where it is not implausible to think that if a
claim is possibly true, then it is true, and if a claim is true then it is necessarily true.
If one accepts this near truism about mathematical claims then the modals in the
above definition do naturally collapse. We can make no sense of a mathematical claim being provable in another possible world and not provable in this one. However, it *prima-facie* very plausible to hold that "Cows fly" is verifiable in another possible world and not verifiable in this one.

I want to bracket this issue for now. In this chapter I will assume that the account of modals necessary to make it initially plausible that the Recognition Thesis provides evidence for the Knowability Requirement is acceptable. This is because the defeasibility considerations I raise in the next section invalidate the claim that the Recognition Thesis provides evidence for the Knowability Requirement, even with the epistemic account of possibility considered here. However, in Chapter 3, when considering Dummett and Tennant's arguments for the revision of logic, where we again find use of modals which the realist would balk at, I shall return to this issue.

§ 2.3 Defeasibility

Here I will show that the notion of a construction $V$ verifying a sentence as that notion occurs in the Recognition Thesis cannot be sufficient for truth. In Ayer's discussion he argues that his distinction between strong and weak verifiability is necessary for verifiability to be even minimally plausible as a requirement upon

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10 I say "near truism" because there are several interesting cases where the truth value of mathematical claims have either flip flopped in the history of mathematics, or still provoke disagreement among mathematicians (e.g. many sentences about infinite collections or sums), and I don't want to say of people who disagree about a mathematical claim either that one of them is uttering a claim that is necessarily false, or that they mean different things by their claims. Consider a simple example. Does mod 5 addition "mean something different" than standard addition, or is it simply that different structures are being described with the same "meaning" for addition? If different structures are being described then it looks like the truth value of "5 + 5 = 0" is contingent on the structure being talked about. Nonetheless, in some appropriately fuzzy way, it is certainly to claim that certain discourse contexts license the claim that mathematical possibility entails mathematical necessity.
Ayer’s strong verifiability can be defined in this manner.

A construction $c$ verifies$_{\text{strong}} P$ if, and only if, $c$ is a warrant for $P$ that makes the probability of $P$ equal to one.

Ayer held that very few propositions are strongly verifiable. While strong verifiability may or may not be defensible as a characterization of sentences in certain areas of mathematics, depending upon one’s background assumptions about these areas, it surely is not defensible for empirical claims with defeasible warrants.

What motivates Ayer’s rejection of strong verifiability as a necessary condition upon the truth of a claim is a property which nearly all garden-variety warrants have. This is the property of being defeasible. While it may or may not be incoherent to maintain that we could be mistaken about most of our beliefs all at once, it is true of most of our beliefs taken one at a time that we could be mistaken about them. This is because, for most claims, warrants for their truth can be overturned by future information. Thus, for most of the truths we believe, we can’t be absolutely certain that they are true. A Quinean fallibilist holds this to be the case with all of our claims, though one might not want to maintain this with respect to mathematical or logical truths. While defending such fallibilism with respect to mathematical truths requires a lengthy excursus into the philosophy of mathematics, I am just considering empirical claims, where fallibilism should be our default position.

More important for our purposes is how unacceptable the Recognition Thesis is when this form of verifiability is taken as an explication of the notion of verifiability.

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11 Anthony Appiah, in an otherwise excellent discussion of how defeasibility causes problems for anti-realisists (though one properly orthogonal to our current discussion), is terribly wrong when he writes, "When Dummett treated verification as absolute, he was harking back to earlier, less careful, statements of verificationism for example, Ayer (1936)." While it is true that Ayer defended a hopeless phenomenological reductionism (a position considered gauche by all of us now) and tended to use the verification criteria to besmirch the cognitive credentials of sentences he didn’t like (a position considered gauche by some of us now), it is also true that Ayer did defend a much more benign form of verifiability than the sort Appiah, correctly, accuses Dummett and Wright of maintaining. Also, Chapter 5 of Language, Truth, and Logic has a very nice defense of anti-positivist positions currently associated with Quine and Sellars. The tragic beauty of Ayer’s book comes from the fact that the materials in Chapters 1 and 5 undermine most of the rest of the book. It is not unreasonable to understand Quine and Sellars as trying to show us some of why this is the case.
occurring in it. The notion of strong verifiability can be embedded in the Recognition Thesis in two ways, both of which can be shown to be unacceptable. The first version is appropriately called a \textit{de dicto} version of the thesis, as the status of the verifier (as being strong) is embedded in the clause which attributes the ability of the speaker to recognize that the verifier does provide a warrant for the claim.

\textbf{Recognition Thesis (strong, \textit{de dicto})}

\begin{itemize}
  \item $X$ understands $P$ if, and only if, were $X$'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$, then $X$ could recognize whether $V_{\text{strong}}cP$ and whether $V_{\text{strong}}c\neg P$.
\end{itemize}

This version clearly does not work. All the speaker would have to know is that the claim in question is empirical and she would then know (assuming she had some epistemological savvy) that it can be defeated by future information.\footnote{I don't think that so-called "perceptual beliefs" have indefeasible warrants. Assume that Karl is given a drug which makes him think that things that look green to him are red, and vice versa. So he says "This looks red to me now," and later realizes he was mistaken about how it looked to him.} Then she would know without even having to inspect the proffered verifier that it does not make the probability of $P$ equal to one. In this case, the counterfactual in the above definition would be trivially satisfied, as long as the speaker knew the claim in question was empirical. But then though she doesn't understand the claim in question, the above definition entails that she does, as the consequent of the counterfactual conditional is made true for the wrong reasons.

Perhaps the problem is that the status of the verifier was illicitly placed in the propositional attitude construction beginning with "recognize whether." If this were the problem, then the solution is to provide the following sort of \textit{de re} construction

\begin{itemize}
  \item $X$ recognizes whether $P$ if, and only if, were $X$ then presented with a construction $c$, then $X$ could recognize whether $V_{\text{strong}}cP$ and whether $V_{\text{strong}}c\neg P$.
\end{itemize}
of the Recognition Thesis.

**Recognition Thesis (strong, de re)**

$X$ understands $P$ if, and only if, were $X$’s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$ (such that $\forall Q (c \text{ verifies } Q \rightarrow V_{\text{strong}}cQ)$, i.e. for all $Q$, if $c$ veriﬁes $Q$, then $c$ makes the probability of $Q$ equal to one), then $X$ could recognize whether $c$ veriﬁes $P$ and whether $c$ veriﬁes $\neg P$.

Apart from the question of what $X$ would actually be recognizing in the attitude ascription, this thesis suffers from being vacuously fulﬁlable. That is, given the defeasibility of warrants for empirical claims, for no empirical claim $P$ and warrant $V_{RT}$ that is a warrant for $P$, is it the case that “if $V_{RT}$ veriﬁes $P$, then it makes the probability of $P$ equal to one.” Thus, for any real warrant for $P$ that $X$ is presented with, the counterfactual conditional will be satisﬁed, independently of whether or not $X$ recognizes that the warrant in question is a warrant for the claim.

Ayer was motivated to characterize veriﬁability in a weaker manner than that considered above because of the problem with requiring strong veriﬁability to be a necessary condition upon truth. In our discussion, we have just focused on formulating a plausible notion of the Recognition Thesis, as a reason for not characterizing the notion of a veriﬁcation as it occurs in the thesis as strong veriﬁability. Ayer characterized weak veriﬁcation in this manner.

A construction $c \text{ verifies}_{\text{weak}} P$ if, and only if, $c$ is a warrant for $P$ that increases the probability of $P$, other things being equal.

Like the appeal to modality in the distinction between veriﬁability in practice and veriﬁability in principle, the notion of being “probable, other things being equal” is difﬁcult to make precise. None-the-less, something in the neighborhood of weak veriﬁability is, at least, *prima facie* plausible as a necessary condition upon the truth of a claim.

Again, Ayer’s motivation for taking veriﬁability to be weak rather than strong, stemmed from his commitment to veriﬁability as a necessary condition upon the
truth of any meaningful claim, and not wanting to be forced by this (and the concerns regarding defeasibility) to say that the meaningful part of our language is inconsistent. However, our motivation concerns the prospects of so characterizing the notion of verification that the Recognition Thesis is a plausible thing to hold. As with our attempted versions of the Recognition Thesis using the notion of strong verifiability, we can give both _de dicto_ and _de re_ versions where the notion of a verification is characterized as weak.

**Recognition Thesis (weak, _de dicto_)**

$X$ understands $P$ if, and only if, were $X$'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$, then $X$ could recognize whether $V_{weak}cP$ and whether $V_{weak}c\neg P$.

**Recognition Thesis (weak, _de re_)**

$X$ understands $P$ if, and only if, were $X$'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$ (such that $\forall Q(c$ verifies $Q \rightarrow V_{weak}cQ)$, i.e. for all $Q$, if $c$ verifies $Q$, then $c$ increases the probability of $Q$, other things being equal), then $X$ could recognize whether $c$ verifies $P$ and whether $c$ verifies $\neg P$.

I don't think that there is a serious difference between these two. However, the _de dicto_ version might be taken to be slightly less plausible, as it might be interpreted in a manner which forces the understander to actually have substantive beliefs about objective probabilities. Thus, I think the weak _de re_ version of the thesis is the most plausible version, if the notion of "verify" in the opaque context is understood as a variable for the kind of verifiability delineated outside of it.

Now we can ask why this should pose a problem for the anti-realist. At this point it seems we have just gestured at a manner in which the anti-realist's Recognition Thesis can be made more plausible. However, if we return to the discussion in section 2.2 the problem surfaces. In 2.2 I defended the claim that the kind of verifiability occurring in the Recognition Thesis must be the same kind of verifiability occurring in the Verification Constraint, if the Recognition Thesis truly provides evidence for
the Verification Constraint. Now we can consider the extent to which this restricts the notion of a construction c verifying a claim P as it occurs in the Verification Constraint. With the restriction, the Verification Constraint becomes either,

If P is true, then it is possible that there exists a construction c such that c is a warrant for P that increases the probability of P, other things being equal ($TP \rightarrow \exists^0 cV_{weak}cP$),

or, if the modal is collapsed in the way sketched in the previous section.

If P is true, then there exists a construction c such that c is a warrant for P that increases the probability of P, other things being equal ($TP \rightarrow \exists cV_{weak}cP$).

Now assume that we give the anti-realist the collapse of the modal. Then, for the Recognition Thesis to provide evidence for the Knowability Requirement the question concerns whether or not ($\exists cV_{weak}cP$) can reasonably be considered a sufficient condition upon truth. I think it is clear that it cannot.

It should be very clear that, with the definition of weak verifiability as given, some claims will be such that both they and their negations are verifiable (in the sense of verifiable given by $\exists cV_{weak}cP$ or, equivalently, $V_{weak}P$). For example, consider the historical claim that Caesar prosecuted Gaius Rabirius before Cicero had Cataline's co-conspirators killed. Cicero writes that he did and this does provide some evidence that Caesar did so; however, Cicero's self aggrandizing tendencies along with rationality considerations concerning why Caesar might have done such a thing provide evidence for the claim that Caesar's behavior took place after Cicero's action. Here we have positive evidence both for and against the claim in question. Both considerations qualify as weak verifiers in the sense we have delineated. But then it is easy to show that such verifiers cannot be sufficient for truth. Moreover, we can do this without the full assumption that the kind of verifiers in the Recognition Thesis are the same as the kind of verifiers occurring in the Verification Constraint. Again, where "$V_{RT}P$" means that P is verifiable in the sense of verifiability occurring in the Recognition Thesis, and "$V_{VC}P$" means that P is verifiable in the sense of
verifiability occurring in the Verification Constraint, we can use the following weaker claim

\[ \text{Inductive Constraint'} \]
\[ \forall P(V_{RT}P \rightarrow V_{VC}P). \]

Given that there do exist sentences which are both verifiable and falsifiable in Ayer's weak sense (since \( V_{weak}P \) is equivalent to \( V_{RT}P \) we write \( \exists P(V_{RT}P \land V_{RT}\neg P) \)), the following argument shows that the verifiers in the Verification Constraint cannot be sufficient for truth.

1. \( \forall P(V_{RT}P \rightarrow V_{VC}P) \) \hspace{1cm} Weaker Inductive Constraint
2. \( \exists P(V_{RT}P \land V_{RT}\neg P) \) \hspace{1cm} From defeasibility considerations
3. \( | \forall P(V_{VC}P \rightarrow TP) \) \hspace{1cm} Assumption for \textit{Reductio ad Absurdum}
4. \( || (V_{RT}P \land V_{RT}\neg P) \) \hspace{1cm} Assumption for Existential Elimination
5. \( || V_{VC}P \land V_{VC}\neg P \) \hspace{1cm} 1,4 Universal Elimination, \textit{Modus Ponens}
6. \( || TP \land T\neg P \) \hspace{1cm} 3,5 Universal Elimination, \textit{Modus Ponens}
7. \( | TP \land T\neg P \) \hspace{1cm} 4-6 Existential Elimination
8. \( \neg \forall P(V_{VC}P \rightarrow TP) \) \hspace{1cm} 3-7 by \textit{Reductio ad Absurdum}

Therefore I conclude that, in-so-far as the anti-realist has offered evidence for the Verification Constraint, the notion of verifiability occurring in the Verification Constraint is not sufficient for truth. But then, since the Knowability Requirement claims that verifiability is both necessary and sufficient for truth, no evidence has been given by the anti-realist for the Knowability Requirement.\(^{13}\)

\section*{§ 2.4 The Dilemma for Dummettian Anti-Realists}

Another way to present the criticism of the prior section is as a dilemma. Either the notion of a construction verifying a sentence in the Knowability Requirement is the same notion as that of a construction verifying a sentence in the Recognition

\(^{13}\) Some might have the intuition that, in the kind of case considered, the probabilities would properly cancel each other out. On this reading it is not possible to have good evidence for and to have good evidence against a claim. This intuition only makes my case stronger then, because then I have arguably succeeded in showing that weak verifiability isn't even necessary for truth. In either case, I have definitely shown that weak verifiability isn't sufficient for truth, as it is incontestable that an increase in the probability of a claim, where probabilities are modeled as they are in standard probability theory, clearly isn't sufficient for truth.
Thesis, or it is not. If it is the same, then the Knowability Requirement is false. If it is not the same, then no evidence has been presented for such a Knowability Requirement. However, one could reasonably wonder if this result isn’t an artifact of the positivistic waters we’ve been swimming in. Can the anti-realist eat her verificationist cake and still have it? That is, might there be another notion of verifiability distinct from those considered by Ayer: one that is both plausibly a necessary and sufficient condition for truth (as codified by the Knowability Requirement), and one concerning which competent speakers can reasonably be charged with a recognitional capacity (as is codified in the Recognition Thesis)?

It is tempting to think that my discussion is an artifact of positivistic currents, as Ayer himself intended to use the notion of verifiability in a very different way than Dummettian anti-realists intend to use it. Ayer was seeking to characterize cognitive significance in terms of verifiability, in such a way that all and only meaningful sentences would verifiable. Thus, Ayer wanted all true sentences, as well as all false contingent sentences, to be verifiable. For anti-realists however, the existence of a constructive verifier is generally assumed to be sufficient for truth, provided that verification conditions can be recursively generated by anti-realistically acceptable logical semantics.

Thus, the Dummettian anti-realist is likely to suspect that my discussion of Ayer’s principle of verifiability is beside the point, since Dummettian verificationists and positivistic verificationists do very different things with the notion of verifiability. However, this suspicion seems to me to be unfounded. Both positivistic and Dummettian verificationists agree that verifiability is necessary for truth, and Ayer’s arguments for his characterization of verifiability, presented above, in fact merely

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14 Neil Tennant (p.c.) made this criticism when reading an earlier draft of this chapter. In a sense, he was right, because I haven’t yet taken into account the revisionary arguments of anti-realists, distinct from the considerations we are mulling over here, for the conclusion that classical model-theoretic truth should be replaced by something more constructive. Such arguments are part and parcel of the anti-realist’s arsenal; thus, it is incumbent upon me to disarm such arguments. This is achieved in the following chapter.
rested upon verifiability’s necessity for truth combined with his demand that verifiers be something which human beings can responsibly be charged with being able to recognize. But this latter charge is simply what the Recognition Thesis commands. Thus, I did not equivocate between the positivistic notion of verifiability and the Dummettian one. Rather, I presented arguments, deriving from Ayer, which ultimately show that if one is going to maintain that a notion of verifiability is necessary for truth (a position defended by anti-realists as well as positivists), and characterize the verifiers in question as constructions which human beings can be charged with recognizing, then one cannot hold any stronger notion of verifiability than the positivists did.

However, the suspicion that my argument has succeeded in virtue of false positivistic dichotomies (e.g. strong versus weak, in principle versus in practice) shall be laid to rest. In this section I shall consider two additional constructive notions of truth, one the standard informal Heyting Semantics clauses for provability considered in the last chapter, and the other Crispin Wright’s notion of superassertibility, as well as one of constructive falsifiability, due to Neil Tennant, and in each case establish a similar dilemma to that presented in this section.15 For each constructive notion I will show that if the Recognition Thesis were correct when interpreted in a strong manner (in a way to be made precise), we would have evidence for a constructive notion of truth being correct. But, as in the prior section, it will be easy to show that the strong versions of the Recognition Thesis are incorrect, and as before, that while the weaker versions might be correct, they provide no evidence for a constructive notion of truth being correct. Since constructive notions of truth are essentially characterizations of truth for which the Knowability Requirement is guaranteed, the reasoning of this section is very similar to the last.

Before starting this discussion I need to point out that the dialectical waters we are swimming in this chapter are not the same dialectical waters that Wright

15See (Wright, 1992), (Wright, 1983), and (Tennant, 1998).
and Tennant swim in when they propose their respective truth predicates. In this dissertation I am narrowly concerned with the question of whether or not it is correct to attribute tacit knowledge of classical truth-conditional semantics to competent speakers of a language. In this chapter I am narrowly concerned with whether or not there is a valid inductive argument from the Recognition Thesis to adoption of a constructive truth predicate.\(^{16}\)

In *Truth and Objectivity*, Wright proposes superassertibility as the correct explication of a least constrained "minimal" truth predicate, and then tries to make sense of metaphysical debates in terms of the extent to which this truth predicate should be further characterized. In *The Taming of the True* Tennant's presentation of constructive falsifiability as a falsity predicate for empirical discourses answers to many dialectical pressures, including but not exhausted by: a general program to understand logical concepts proof-theoretically, the attempt to explain how the same logic can be used in both mathematics and empirical discourses, the reformulation of a positivist criterion of cognitive significance not subject to traditional collapse proofs, a solution to the paradoxes of truth and relevance, and a host of other interesting and difficult systematic philosophical problems. It would be absurd to automatically assume that the quite narrow context we are working in, as well as the arguments I shall present, in any way delegitimate the important uses which Wright and Tennant make of their respective constructivized predicates.\(^{17}\) Here I will merely show that adoption of the respective predicates in the Recognition Thesis does not render valid the argument from a Recognition Thesis to the correctness of a constructive notion of truth. Thus, we shall be closer to our stated purpose of

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\(^{16}\)In the next chapter we shall concern ourselves with whether or not there is a valid argument from the Verification Constraint to the conclusion that the law of bivalence is invalid. At that point we shall again engage Wright and Tennant's arguments.

\(^{17}\)If Wright is correct in arguing that we should be pluralists about truth (or rather if Wright is correct in arguing that truth can profitably be characterized differently for different subject matters) then it is entirely possible that the dialectic I am engaged in has very little purchase with the substantive points Tennant and Wright make. Truth in a model may be a good model of truth for the purposes of doing semantics of natural language, but truth in different parts of language might healthily be characterized very differently. I am not here pursuing the truth about truth.
showing that the anti-realist arguments for the revision of logic (considered in the next chapter) bear a heavy dialectical burden.

§ 2.4.1 Heyting Semantics?

In the last chapter I showed how Heyting Semantics for first order logic could be thought of as providing an informal Dummettian theory of reference for first order logic. There I showed how Heyting Semantics compositionally assigns verification conditions to the logical words in a formal language under consideration. However, in the discussion of the previous section no mention was made of these clauses at all. Here I will dispel any lurking suspicion that lack of such mention in some manner rendered the above discussion more plausible. To do this we need not rewrite all of the standard clauses for first order logic (given in the last chapter), as, for our purposes, we need only consider a few of the standard ones, those concerning conjunction, negation, and the conditional.\(^\dagger\) Thus, we can say that a formula $\Phi$ (in a simple propositional language with the connectives $\land$, $\lor$, and $\neg$) is true if and only if there exists a verification of $\Phi$ such that:

1. If $\Phi$ is a conjunction, $\Phi = (\Phi_1 \land \Phi_2)$, then $k$ verifies $\Phi$ if and only if one can extract from $k$ constructions $k_1$ and $k_2$ that verify $\Phi_1$ and $\Phi_2$ respectively.

2. If $\Phi$ is a negation, $\Phi = (\neg \Phi_1)$, then $k$ verifies $\Phi$ precisely when $k$ verifies $(\Phi_1 \rightarrow \bot)$, where the constant $\bot$ has no construction verifying it.

3. If $\Phi$ is an implication, $\Phi = (\Phi_1 \rightarrow \Phi_2)$, then $k$ verifies $\Phi$ precisely when $k$ yields a general method that, from every construction $l_1$ verifying $\Phi_1$, enables us to find a construction $l_2$ verifying $\Phi_2$.

The above clauses are essentially informal. For our purposes, however, they are sufficient.

By the considerations of the preceding section we have that where $V$ is the weak verifiability predicate occurring in the Recognition Thesis, there will be sentences $P$ such that $(VP \land V\neg P)$. However, on the assumption that the $V$ predicate is short-

\(^\dagger\)The following are taken, again, with slight modification, from (Dragalin, (1980, pp. 2–3)).
hand for the existence of verification, the above informal clauses for a conjunction and negation render a sentence such as \((VP \land V\neg P)\) intolerable.

To show this we first assume (for \textit{reductio}) that for some sentence \(P\) it is the case that \((VP \land V\neg P)\). Call the verifer for \(P\), \(k_1\) and the verifier for \(\neg P\), \(k_2\). Then, by the above clause for negation, \(k_2\) verifies \((P \rightarrow \bot)\), where \(\bot\) has no construction verifying it. But then, since, by the clause for \(\rightarrow\), \(k_2\) yields a general method that, for every construction \(l_1\) verifying \(P\), enables us to find a construction \(l_2\) verifying \(\bot\), there is no construction verifying \(P\). But this contradicts the initial claim that \(k_1\) verifies \(P\). Therefore, where \(V\) is equal to Heyting semantic verifiability we have \(\forall P(\neg (VP \land V\neg P))\). Thus I have shown that Heyting semantics is committed to a claim about constructive verifiability which is false for any notion of verifiability which could be plausibly be taken to be the notion of verifiability occurring in the Recognition Thesis.

In motivating this result I have supposed that the Heyting Semantics notion of verifiability would find its way into the Recognition Thesis in something like the following manner, where "\(V_{\text{Heyting}}P\)" means that there exists a construction \(c\) verifying \(P\) in the manner given by the standard Heyting provability clauses.

\textbf{Recognition Thesis (Heyting, strong, de dicto)}

\(X\) understands \(P\) if, and only if, were \(X\)'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were \(X\) then presented with a construction \(c\), then \(X\) could recognize whether \(V_{\text{Heyting}}cP\) and whether \(V_{\text{Heyting}}c\neg P\).

\textbf{Recognition Thesis (Heyting strong, de re)}

\(X\) understands \(P\) if, and only if, were \(X\)'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were \(X\) then presented with a construction \(c\) (such that \(\forall Q(c\text{ verifies }Q \rightarrow V_{\text{Heyting}}cQ)\), i.e. for all \(Q\), if \(c\) verifies \(Q\), then \(c\) verifies \(Q\) in the manner given by the Heyting provability clauses), then \(X\) could recognize whether \(c\) verifies \(P\) and whether \(c\) verifies \(\neg P\).

That is, I have assumed that the verifications mentioned in the Recognition Thesis would be exactly the same verification conditions described by Heyting Semantics.
But then, given the indefeasibility of the Heyting-style verifiers, exactly the same arguments given in the last section against strong verifiability occurring in the Recognition Thesis apply against this view. We cannot responsibly equate understanding of a sentence with the ability to recognize such indefeasible verifiers and falsifiers of it.

However, I did not consider the possibility of embedding the Heyting Semantics notion of provability within the earlier statement of a weak Recognition Thesis. Since the weak Recognition Thesis charges people with recognizing warrants that increase the probability of a claim's truth, what is to prevent us from restating the weak thesis so that it charges people with recognizing warrants that increase the probability of a claim's Heyting Semantics provability? Thus, the constructivist could, and I think should, opt to defend something like one of the following, much more plausible, weaker versions of the Recognition Thesis.

Recognition Thesis (Heyting, weak, de dicto)

$X$ understands $P$ if, and only if, were $X$'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$, then $X$ could recognize whether $c$ is a warrant for $P$ that increases the probability that $V_{Heyting}kP$, other things being equal, and whether $c$ is a warrant for $\neg P$ that increases the probability that $V_{Heyting}k\neg P$, other things being equal.

Recognition Thesis (Heyting, weak, de re)

$X$ understands $P$ if, and only if, were $X$'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$ (such that $\forall Q (c$ verifies $Q \rightarrow$, then $c$ makes it more probable that $\exists k(V_{Heyting}kQ)$, i.e. for all $Q$, if $c$ verifies $Q$, then $c$ increases the probability that $Q$ is Heyting provable, other things being equal), then $X$ could recognize whether $c$ verifies $P$ and whether $c$ verifies $\neg P$.

In this case I think it is clear that the de re version is to be preferred, as the de dicto version forces the speaker to have knowledge of Heyting Semantics recursive definitions. If the constructivist defends one of the weaker versions of the Recognition Thesis, then the problem I have drawn attention to might disappear.
The verifiers mentioned in the above versions of the Recognition Thesis (possible warrants which increase the probability of the existence of a verifying construction) are not sufficient for truth. But then any verificationist constraint which was to follow from this Recognition Thesis would only state that if a claim is true then it is possible that there exists a warrant which increases the probability of the existence of a verifying construction. It is not at all clear why these kinds of verifiers should bother Ayer or me, as a possible warrant which increases the probability of the existence of a verifying construction is both not sufficient for truth, and such that many claims and their negations will have verifiers of this kind. Moreover, as the following schema shows, we no longer have an argument against Heyting Semantics provability. The constructivist can consistently hold that the Heyting Semantics notion of verifiability is both necessary and sufficient for the truth of a claim, and that our new kinds of verifiers (possible warrants that increase the probability of the existence of a Heyting Semantics proof) are also necessary for the truth of a claim.

If this were the case, then, where \( V_{weakHeytingP} \) is a notationally convenient way to represent \( \exists^0c(V_{weakHeyting}(\exists k(V_{HeytingP}))) \) the dilemma originally presented would look like this.

\[
\begin{array}{ccc}
TP \rightarrow V_{weakHeytingP} & V_{HeytingP} \rightarrow TP \\
| & | \\
Ayer's considerations & Short proof \\
| & |
\end{array}
\]

\[\exists P(V_{weakHeytingP} \land V_{weakHeyting\neg P}) \quad \forall P(\neg (V_{HeytingP} \land V_{Heyting\neg P})\]

Then, the two subconclusions of the reductio argument clearly do not contradict one another. However, resorting to the weak versions of the Recognition Thesis spectacularly invalidates any argument for the claim that Heyting provability is a proper characterization of the truth predicate. Any argument of the kinds considered above, from a Recognition Thesis to a Verification Constraint, would only entail of true claims the existence of possible worlds with the appropriate warrants (ones which increase the probability of the existence of a constructive verifier). But this
could be the case even if truth were not equivalent to the existence of a constructive verifier. The appropriate kind of verification constraint would look like this

\textbf{Verification Constraint (Heyting)}

If \( P \) is true, then there exists a construction \( k \) which increases the probability that [there exists a construction \( l \) verifying \( P \) in the manner given by Heyting Semantics], other things being equal.

But this could clearly be the case without truth being equal to the existence of a constructive verification. Therefore, the weak Heyting-style Recognition Thesis does nothing to prohibit a claim being true without a constructive verifier for it existing.

At this point the anti-realist might retort that the Verification Constraint given above does indeed provide evidence for a constructive account of truth being correct. For if we assume the notion of a construction verifying \( P \) above is doing proxy work for truth, then certainly a great deal of theoretical economy will have been achieved. Put in terms of a positive challenge, the anti-realist might claim that if truth is not equal to the existence of a constructive verification, and the Verification Constraint as just given were correct, then one would have to explain how a construction for \( P \) could increase the probability that there existed a verifying construction for \( P \) without increasing the probability of \( P \)'s truth, or vice versa.

But such a complaint would be ill-founded, given the nature of the anti-realist's dialectic. The Recognition Thesis is supposed to be motivated by considerations about language, or rather by considerations about what is involved in providing a principled and explanatory theory of linguistic understanding. Then it (the Recognition Thesis) is supposed to provide evidence for the correctness of a constructive account of truth. I have shown that if the anti-realist is clever enough about how she embeds a constructive truth predicate into the Recognition Thesis, she does not get an absurd conclusion. But this provides no positive evidence for the claim that a constructive notion of truth is correct. The anti-realist dialectic, assuming it is correct, gets us the following two theses, which clearly provide no evidence for intu-
tionistic semantics.

Recognition Thesis (weak, de re)

$X$ understands $P$ if, and only if, were $X$'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$ (such that $\forall Q(c$ verifies $Q \rightarrow V_{\text{weak}}cQ)$, i.e. for all $Q$, if $c$ verifies $Q$, then $c$ increases the probability that $Q$ is true, other things being equal), then $X$ could recognize whether $c$ verifies $P$ and whether $c$ verifies $\neg P$.

Verification Constraint (with collapsed modal)

If $P$ is true, then there exists a construction $k$ which increases the probability that $P$ is true, other things being equal.

I have shown that the constructivist can, given both the de re context and the fact that the verifier mentioned only increases the probability of the claim, substitute her constructive truth predicate in for "is true" in the above clauses.

Recognition Thesis (Heyting, weak, de re)

$X$ understands $P$ if, and only if, were $X$'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$ (such that $\forall Q(c$ verifies $Q \rightarrow c$ makes it more probable than $\exists k(V_{\text{Heyting}}kQ)$), i.e. for all $Q$, if $c$ verifies $Q$, then $c$ increases the probability that $Q$ is Heyting provable, other things being equal), then $X$ could recognize whether $c$ verifies $P$, and whether $c$ verifies $\neg P$.

Moreover, after such substitution, assuming the arguments from a Recognition Thesis to a Verification Constraint are correct, I have shown that, at best, the following is entailed by the new Recognition Thesis.

Verification Constraint (Heyting)

If $P$ is true, then there exists a construction $k$ which increases the probability that [there exists a construction $l$ verifying $P$ in the manner given by Heyting Semantics], other things being equal.

Well, this is all very nice, but even if the claim about theoretical economy was correct, we still don't have an argument to the conclusion that the version of the Recognition Thesis with a constructive notion of truth smuggled into it is correct. I'm not claiming that anti-realists don't provide such arguments; I am just claiming
that there is no direct argument from considerations about linguistic understanding.
In the next chapter we will consider and criticize indirect arguments from Wright, Dummett, and Tennant, which should be thought of as providing evidence for something like the above by trying to show that classical, model-theoretic semantics runs afoul of the Verification Constraint. However, there is no direct argument motivating the "smuggling in" strategy countenanced here.

Moreover, given that the vast majority of linguistic semantics is model theoretic, to the extent that we have evidence for a Recognition Thesis being correct, we have good evidence that a Recognition Thesis where truth is understood in a manner consistent with the use of classical model-theoretic semantics by linguists is correct. The anti-realist still needs to present an argument for the conclusion that her versions of the Recognition Thesis and Verification Constraint, with constructive notions of truth smuggled in, are to be preferred over the following explicitly classical versions.

**Recognition Thesis (classical, weak, de re)**

$X$ understands $P$ if, and only if, were $X$'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$ (such that $\forall Q(c \text{ verifies } Q \rightarrow V_{\text{weak}}cQ)$, i.e. for all $Q$, if $c$ verifies $Q$, then $c$ increases the probability that $[Q$ is true, where truth conditions are characterized as they are in standard classical model theoretic semantics], other things being equal), then $X$ could recognize whether $c$ verifies $P$ and whether $c$ verifies $\neg P$.

**Verification Constraint (classical, with collapsed modal)**

If $P$ is true, then there exists a construction $k$ which increases the probability that $[P$ is true, where truth conditions are characterized as they are in standard classical model theoretic semantics], other things being equal.

In the next chapter we will examine such arguments as anti-realists provide for this.

Here it should be noted that the classicist can strengthen her hand by once again calling to task the anti-realist's collapse of the modal. Given that we could only find plausible reasons to collapse the modal in mathematical discourses, the classicist
should refuse to assert the Verification Constraint with the collapsed modals and rather assert.

Verification Constraint (classical)
If $P$ is true, then it is possible that there exists a construction $k$ which increases the probability that $[P$ is true, where truth conditions are characterized as they are in standard model-theoretic semantics], other things being equal.

But then, the analog claim gotten by smuggling in Heyting provability is

Verification Constraint (Heyting)
If $P$ is true, then it is possible that there exists a construction $k$ which increases the probability that $[there exists a construction $l$ verifying $P$ in the manner given by Heyting Semantics], other things being equal.

Here, even if smuggling in Heyting provability were licit, it is very clear this Verification Constraint could hold without truth being equal to Heyting provability. It does not prohibit claims being true in our world, yet so unverifiable that there only exists a non-actual, possible world (even very far away in the multiverse of possible worlds) where a construction exists which increases the probability that they are provable.

As far as I know, the only evidence that has been given in the literature for the claim that the Recognition Thesis (Heyting, weak, de re) is to be preferred to the Recognition Thesis (classical, weak, de re) is the claim that classical semantics entails undecidable sentences which violate a formulation of the Verification Constraint stated in a manner invariant over which form of truth occurs in it. Then, since semantics with constructive notions of truth do not, we are well advised to use such semantics. These arguments will be considered and criticized in the following chapter. However, at this point it should be clear that ability to defend a Recognition Thesis by substituting in a constructive notion of truth in the manner above does not provide evidence that the constructive notion of truth is correct.

Thus, if the strong version of the Recognition Thesis were correct, we would have evidence for a constructive notion of truth being correct. But the strong version
of the Recognition Thesis is incorrect. The weak version might be correct, but it provides no evidence for a constructive notion of truth being correct.

§ 2.4.2 Constructive Falsificationism?

One way to construe the problem I have been calling attention to is to note two important points: (1) empirical warrant is defeasible while truth is not, and (2) any sentential property which is preserved by logical inferences must, like truth, be non-defeasible. Thus, if our linguistic competence is to be explained by our evidential prowess at determining when warrants for claims are good warrants for them, then the existence of the relevant warrants cannot be what is preserved by logical inferences. If the existence of the relevant kinds of warrants were what gets preserved by deduction we would never get the claim that there exists a warrant for some sentence $P$ and there exists a warrant for $\neg P$.

The generality of this argument should make us very skeptical of any kind of anti-realist semantics which identifies the property preserved by logical inference with some evidential virtue attaching to a sentence in question. However, thus far I merely used the sort of orthodox informal Heyting Semantics characterization of the logical connectives which intuitionist mathematicians and logicians use.

In his recent book, *The Taming of the True*, Tennant faces some of the problems I have been raising for verificationists. He writes,

> As soon as we move away from non-theory-laden observation sentences (and their truth-functional compounds) it is no longer appropriate to think of our observational warrants for assertion as non-defeasible. For every statement that ventures beyond the present observational evidence is vulnerable... Every such statement (and especially ones involving generality) is hostage to future evidence that is not yet available. (Tennant, (1997, p. 421))

Tennant admits that these considerations undermine the identification of truth with provability that characterizes anti-realism about mathematics, and goes on to suggest that these problems strongly motivate embracing a form of falsificationism about
the content of claims with defeasible warrants.

In general, the anti-realist content of any empirical assertion $P$ of which by virtue of its general or hypothetical nature, we can say *a-priori* that it cannot admit of proof, is: Nature will not refute $P$.

(Tennant, (1997, p. 432))

We can utilize the above arguments to show that falsifiability, as I will show by Tennant's own admission vulnerable to the kinds of defeasibility considerations Ayer considered, cannot be motivated by the Recognition Thesis to replace falsity. The same arguments I have presented above show that if falsifiability can both occur in the Recognition Thesis and be a necessary condition for the falsity of a claim, then it cannot be a sufficient condition for it.\(^{19}\)

To show this we first assume that falsifiability is a sufficient condition for falsity. Then, in exactly the same manner as above, we can show that there is no claim $P$ such that $P$ and its negation are both falsifiable. For *reductio* assume that $P$ and its negation are both falsifiable. Then, since $P$ is falsifiable, and falsifiability is sufficient for falsity, $P$ is false. Since the negation of $P$ is falsifiable, likewise the negation of $P$ is false. Contradiction. Therefore, if falsifiability is sufficient for falsity, it follows that there is no claim $P$ such that $P$ and its negation are both falsifiable.

Once again the defeasible nature of the kinds of falsifiers we can reasonably be charged with tracking as competent language users implies that we will often get falsifiers for a claim and its negation. We can discern this by again examining the two relevant types of recognition theses, where Ayer's characterizations of verifiability and falsifiability are explicitly stated in the theses. So where $\text{"F}_\text{Strong}cP$ means

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\(^{19}\)In some places in (Tennant, 1997) Tennant seems to be arguing that the relevant notion that need be recognized by a competent user is falsification *modulo* other assumptions. However, the point of our current discussion is to show that "going Popperian" about grasp of meaning does not help provide an argument for a version of the Verification Constraint (where the constructions are falsifiers necessary and sufficient for the falsity of a claim). Falsification *modulo* other assumptions is clearly not necessary for falsity (the other assumptions must be true). Therefore, it ought provide no solace for the anti-realist, at least in so far as it might be thought to save the anti-realist from our present dialectic.
that $c$ makes the probability of $P$ equal to zero we would get.

**Recognition Thesis (Falsificationist, strong, *de dicto*)**

$X$ understands $P$ if, and only if, were $X$’s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$, then $X$ could recognize whether $F_{\text{Strong}}cP$ and whether $F_{\text{Strong}}c\neg P$.

**Recognition Thesis (Falsificationist strong, *de re*)**

$X$ understands $P$ if, and only if, were $X$’s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$ (such that $\forall Q(c \text{ falsifies } Q \rightarrow F_{\text{Strong}}cQ)$, i.e. for all $Q$, if $c$ falsifies $Q$, then $c$ makes the probability of $P$ equal to zero), then $X$ could recognize whether $c$ falsifies $P$ and whether $c$ falsifies $\neg P$.

Again, by considerations that Tennant accepts above, these formulations of the Recognition Thesis are incorrect, given that they charge the speaker with recognizing that constructions exist which really shows $P$ to be indefeasibly false.

Sometimes Tennant seems sympathetic to the strong versions, in virtue of his support of hypothetico-deductivism, which seems to make the strong versions more palatable. This is because, by hypothetico-deductivism a scientific theory (along with descriptions of the system being measured and, realistically, a whole lot of difficult approximation theory to make the mathematics feasible), entails observation sentences. Because of this one might think that disproving a scientific theory is exactly analogous to proving a mathematical claim, and Tennant seems to endorse this at times.

The appropriate filling out of the slogan ‘Once empirically refuted, always empirically refuted’ is then: if evidence $E$ refutes theory $T$, the future evidence $(E + F)$—which of course we assume to be consistent—also refutes theory $T$. Similarly, in the logico-mathematical case: if axioms $X$ enable us to prove theorem $A$, then future axioms $X + Y$—which of course we assume to be consistent—enable us to prove theorem $A$.

(Tennant, (1997, p. 416))

Even if this is correct it does not help in recasting the Recognition Thesis. If we to-
tally idealize away from the typical theory-ladenness of the description of the system being measured and the sometimes troubling waves of hand used in approximation theory, it is still the case that what is being refuted is the theory itself, that is, a set of sentences which (together with the approximations and description of the system) contradict the observation sentence. How can we possibly go from this to a characterization of grasp of a single sentence via our ability to recognize an indefeasible falsification of it? The problem for the Dummettian is that the auxiliary theory involved in falsifying a theoretical claim may have nothing to do with the meaning of that claim. Why then should people be forced to understand this auxiliary theory to understand the theoretical claim in question? Or perhaps we should inquire about how much auxiliary theory they should be required to grasp.

Perhaps if the sentence occurs in a theory which is taken to be disconfirmed this is good evidence that the sentence is false. But then, as before, we would have to retreat to a weaker version of the Recognition Thesis. The weaker versions can be

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Tennant is much more holism-friendly in many places of his *oeuvre* than Dummett. For a very nice discussion of Dummett's somewhat disappointing characterizations and attacks on holism see the sections on holism in (Tennant, 1987); for a wonderfully holistic response to the "Kripkenstein paradox" see (Tennant, 1998, pp 91–142).
given in this manner.

**Recognition Thesis (Falsificationist, weak, *de dicto*)**

$X$ understands $P$ if, and only if, were $X$'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$, then $X$ could recognize whether $c$ is a warrant for $P$ that increases the probability that $F_{\text{Strong}}cP$, other things being equal, and whether $c$ is a warrant for $\neg P$ that increases the probability that $F_{\text{Strong}}c\neg P$, other things being equal.

**Recognition Thesis (Falsificationist, weak, *de re*)**

$X$ understands $P$ if, and only if, were $X$'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$ (such that $\forall Q(c \text{ falsifies } Q \rightarrow c \text{ makes it more probable that } 3k(F_{\text{Strong}}kQ)$, i.e. for all $Q$, if $c$ falsifies $Q$, then $c$ increases the probability that the probability of $Q$ is equal to zero), then $X$ could recognize whether $c$ falsifies $P$ and whether $c$ falsifies $\neg P$.

Also again, by considerations exactly analogous to those of the previous section, while these may be true, they provide no evidence for the claim that if a sentence is false then it is constructively falsifiable. The kind of verification constraints which would follow from the weak thesis above would merely state,

**Verification Constraint (Falsifiability with collapsed modals)**

If $P$ is false, then there exists a construction $k$ which increases the probability that [there exists a construction $l$ which makes the probability of $P$ equal to zero]. other things being equal.

**Verification Constraint (Falsifiability)**

If $P$ is false, then it is possible that there exists a construction $k$ which increases the probability that [there exists a construction $l$ which makes the probability of $P$ equal to zero], other things being equal.

As with the constraint for Heyting provability, this constraint is clearly also consistent with there being false claims which do not have constructive falsifiers.

In some ways this should not be surprising. A claim is false if and only if it is not true. One would expect many of the problems in characterizing truth a certain way to infect attempts to characterize falsity in the same way. In fact, in *Language, Truth, and Logic*, Ayer explicitly considered whether or not falsificationism could
allow one to avoid weak verificationism, and concluded that the falsificationist would need a notion of weak falsificationism in any case. Ayer wrote,

Nor can we accept the suggestion that a sentence should be allowed to be factually significant if, and only if, it expresses something which is definitely confutable by experience. Those who adopt this course assume that, although no finite series of observations is ever sufficient to establish the truth of a hypothesis beyond all possibility of doubt, there are crucial cases in which a single observation, or series of observations, can definitely confute it. But, as we shall show later on, this assumption is false. A hypothesis cannot be conclusively confuted any more than it can be conclusively verified. For when we take the occurrence of certain observations as proof that a given hypothesis is false, we presuppose the existence of certain conditions. And though, in any given case, it may be extremely improbable that this assumption is false, it is not logically impossible. We shall see that there need be no self-contradiction in holding that some of the relevant circumstances are other than we have taken them to be, and consequently that the hypothesis has not really broken down. And if it is not the case that any hypothesis can be definitely confuted, we cannot hold that the genuineness of a proposition depends on the possibility of its definite confutation. Accordingly, we fall back on the weaker sense of verification...

(Ayer, (1936, p. 38))

Ayer realized that the move to falsifiability would not make it any more plausible to hold that one could take falsifiability to be sufficient for falsity, again, not as long as the falsifiers were to be anything humanly recognizable.21

Tennant is completely aware of what Ayer speaks in the above passage,22 and he doesn't argue that people should be able to tell with certainty that something has been falsified. As far as I can tell, Tennant's position is more that people should be able to tell with certainty that they are committed to a contradiction,

21 Again, claims about relative falsificationism might be absolutely refutable. That is, claims such as "If the sentences in $\Delta$ are true, then $\Gamma$ must be false" might be absolutely refutable. But it is so clear that such claims are not sufficient for the falsity of $\Gamma$ (i.e. in cases where $\Delta$ contains false sentences) that they do not help in motivating falsificationism (the claim that a sentence is false if and only if it is falsifiable).

22 Ironically, what Ayer speaks of is usually referred to by American and British philosophers as the "Quine-Duhem hypothesis." This is ironic because it is not at all clear that Duhem believed it, and it is very clear that the Logical Positivists did.
and then rationally be able to readjust their beliefs in light of this. However, this is again a property that holds of sets of sentences, and it is not clear how it could be incorporated into a Recognition Thesis, such that an appropriate Verification Constraint were entailed (a Verification Constraint where the falsifiers occurring in them were plausibly sufficient for falsity). Why should we go from such a view of competence to the belief that a sentence is false if and only if it is falsifiable?

One might try to say something like, "$\Phi$ is false if and only if for some true evidence $E$, $\Phi$ is falsifiable modulo $E$." But this is just absolute falsifiability, the notion which competent speakers cannot be charged with a recognitional capacity concerning—because of the defeasible nature of evidence.

As with Heyting provability, I have shown that one can smuggle in a notion of falsifiability into the Recognition Thesis and Verification Constraint in a manner that is not wholly implausible. However, again, the Recognition Thesis no longer provides evidence for replacing classical falsity with a more constructive account of falsifiability. As with the previous account, the only proffered dialectical move to offer such evidence requires direct arguments to the conclusion that model-theoretic semantics violates the Verification Constraint.

§ 2.4.3 Superassertibility?

One possible response to our initial dilemma involves time-indexing warrants for and against the truth of sentences. Joe Salerno (p.c.) thinks this is the way the anti-realist should respond to problems about defeasibility.
at time $i$, our initial dilemma would look like this.

\[
\begin{align*}
TP &\rightarrow V_2P & \quad V_iP \rightarrow TP \\
Ayer's considerations & | \quad \text{Proof} \\
\exists P(V_mP \land V_n\neg P) & \quad \forall P(\neg(V_iP \lor V_i\neg P))
\end{align*}
\]

Then, if one can strengthen the notion of verifiability just enough from prohibiting a claim from being both verifiable and falsifiable at the same time, one would never get a contradiction.

However, this strategy would still force the requisite notion of "verifiable" to be one insufficient for truth, because the existence of a defeasible warrant at a time is not sufficient for a claim's truth. If it were sufficient for a claim's truth, then it couldn't be overturned by a future warrant for the claim's negation. None-the-less, this does suggest another strategy of combining the time-indexing with an account of warrant, on which possession of the relevant kind of warrant at a time is plausibly sufficient for a claim's truth.

Crispin Wright's strategy, when he proposes characterizing truth as superassertibility, essentially does attempt to characterize warrants as follows.\(^\text{24}\) In *Truth and Objectivity* Wright defines superassertibility as follows:

A statement is superassertible then, if and only if it is, or can be, warranted and some warrant for it would survive arbitrarily close scrutiny of its pedigree and arbitrarily extensive increments to or other forms of improvement of our information.

\[
\text{(Wright, (1992, p.48))}
\]

In interpreting the above we must note that Wright clearly intends superassertibility to be what is preserved by at least intuitionistically valid deductions; so he is committed to the claim that, for all $P$, $\neg(SA P \land SA\neg P)$. Thus Wright's use of the modal "can" in the definition must be understood in a sense which does not allow a claim and its negation both to be superassertible. Thus Wright can't mean "can be warranted" in the sense that we might say that the sentence "Cows fly" can be

\(^{24}\text{See (Wright, 1983) and (Wright, 1992).}\)
warranted but won’t. For Wright, it can never be the case that a false sentence can be warranted in the manner he considers.

I take it that the distinction between a claim being warranted and being such that it can be warranted for Wright simply demarcates whether or not anybody is aware of the existence of the relevant warrant. Given this, Wright’s definition is equivalent to the simpler

A statement is superassertible if, and only if, there exists a warrant for it which would survive arbitrarily close scrutiny of its pedigree and arbitrarily extensive increments to or other forms of improvement of our information.

Even more simply, where a superwarrant is defined as a warrant “such that it would survive arbitrarily close scrutiny of its pedigree and arbitrarily extensive increments to or other forms of improvement of our information” we have:

\[ P \text{ is superassertible if, and only if, there exists a superwarrant for } P. \]

Here I will again show that the kind of evidential prowess required by the Recognition Thesis does not provide any reason for us to accept superassertibility as a truth predicate.

Given our considerations from A.J. Ayer, the warrants or verifiers for a sentence which must be evaluated by competent speakers are evidence which make the sentence in question more probable, other things being equal. Even more clearly than in the cases of Heyting provability and Tennant’s constructive falsifiability, we can see that superassertibility will not work as a representation of the kind of evidence speakers must be attuned to. This would be to render the Recognition Thesis in the following manner, where “\( V_{\text{Super}c}P \)” means that there exists a construction \( c \) such
that \( c \) is a superwarrant for \( P \).

### Recognition Thesis (Superassertability, strong, \textit{de dicto})

\( X \) understands \( P \) if, and only if, were \( X \)’s cognitive capacities and technologies finitely extended (in an appropriate manner), and were \( X \) then presented with a construction \( c \), then \( X \) could recognize whether \( V_{Super}cP \) and whether \( V_{Super}c\neg P \).

### Recognition Thesis (Superassertibility strong, \textit{de re})

\( X \) understands \( P \) if, and only if, were \( X \)’s cognitive capacities and technologies finitely extended (in an appropriate manner) and were \( X \) then presented with a construction \( c \) (such that \( \forall Q(c \text{ is a verifies } Q \rightarrow V_{Super}cQ) \), i.e. for all \( Q \), if \( c \) is a verifies \( Q \) then \( c \) is a superwarrant for \( P \)), then \( X \) could recognize whether \( c \) verifies \( P \) and whether \( c \) verifies \( \neg P \).

The \textit{de dicto} version is clearly absurd. The amount of extension of capacities and technologies a speaker would have to undergo to ever determine that a claim was superassertible would be sufficient to render such a speaker god-like.

The \textit{de re} version might be more plausible, as the speaker isn’t being charged with recognizing whether or not \( c \) is a superwarrant for \( P \), but rather is being charged with recognizing that some property necessarily coextensive with the existence of a superwarrant holds of \( c \). Though this might be more plausible, it is hard to see how it is. Any property necessarily coextensive with superassertibility is going to be just as indefeasible as superassertibility, and Ayer’s considerations showed us that we can’t charge speakers with recognizing that such properties held of sentences.

I don’t think Wright intended to uphold the Recognition Thesis as given above. In his discussion of superassertibility in \textit{Truth and Objectivity} he is concerned with questions such as whether or not it is plausible to maintain that a warrant for \( P \)’s superassertibility is the same as a warrant for \( P \)’s truth. Thus, since the claim that \( P \) is more probable in the statement of the Recognition Thesis is equivalent to the claim that it is more probable that \( P \) is true, I assume that Wright would prefer one
of the following.

Recognition Thesis (Superassertibility, weak, de dicto)

X understands P if, and only if, were X's cognitive capacities and technologies finitely extended (in an appropriate manner), and were X then presented with a construction k, then X could recognize whether k is a warrant for P that increases the probability that $V_{Super}cP$, other things being equal and whether k is a warrant for $\neg P$ that increases the probability that $V_{Super}c\neg P$, other things being equal.

Recognition Thesis (Superassertibility, weak, de re)

X understands P if, and only if, were X's cognitive capacities and technologies finitely extended (in an appropriate manner), and were X then presented with a construction c (such that $\forall Q(c$ verifies $Q \rightarrow c$ makes it more probable that $\exists k(V_{Super}kQ)$, i.e. for all Q, if c verifies Q, then c increases the probability that there exists a superwarrant for Q), then X could recognize whether c verifies P and whether c verifies $\neg P$.

In this case the de dicto version again is less plausible than the de re version, if anything because it is perverse to charge ordinary speakers with having a grasp of the concept of superassertibility. In any case, as with the other two constructive predicates, resorting to either of these forms of the Recognition Thesis would invalidate the dilemma initially presented in this chapter. However, also as with the case of Heyting verifiability and constructive falsifiability, the recognition thesis as given does nothing to provide evidence for the claim that truth is equal to superassertibility. As before, the above could easily hold while there were true claims which were not superassertible. Once again, the two forms of Verification Constraints which
might follow from these theses are.

Verification Constraint (Superassertibility with collapsed modal)
If \( P \) is true, then there exists a construction \( k \) which increases the probability that [there exists a superwarrant \( c \) for \( P \) in the manner defined by Wright], other things being equal.

Verification Constraint (Superassertibility)
If \( P \) is true, then it is possible that there exists a construction \( k \) which increases the probability that [there exists a construction \( c \) for \( P \) in the manner defined by Wright], other things being equal.

Again, both of these could be true, even if superassertibility wasn’t sufficient for truth. Moreover, all we have done is replace “is true” with “is superassertible” in the Recognition Thesis in such a way that its occurrence is fairly innocuous.

That’s not how it’s supposed to go. The anti-realist dialectic is supposed to be that the Recognition Thesis provides evidence for the Knowability Requirement. However, we have shown that the Recognition Thesis only provides evidence for a Verification Constraint, in whose antecedent (the statement that a possible world exists with a warrant that makes it more probable that the claim is true, other things being equal) the notion of verifiability cannot be sufficient for truth. But again, replacing truth with superassertibility still doesn’t make it the case that verifiability characterized as [the existence of a possible world containing a warrant that makes it more probable that the claim in question is superassertible, other things equal] is sufficient for truth.

As with the others, Wright needs to provide us with evidence that a weak Recognition Thesis with superassertibility (understood to be necessary and sufficient for truth) occurring in it is more plausible than one with classical truth in it. Again, the importance of a valid argument that classical truth is unacceptable is vitally important if replacing standard truth with a constructive truth predicate is to be motivated at all.
§ 2.5 Conclusion

In this chapter we have succeeded in narrowing down Dummett's challenge. First, I argued the Recognition Thesis only entails either

Verification Constraint
If $P$ is true, then it is possible that there exists a construction $c$ such that $c$ is a warrant for $P$ that increases the probability that $P$ is true, other things being equal ($TP \rightarrow \exists^0 c(V_{weak}cP)$).

or, if the modal is collapsed,

Verification Constraint (with collapsed modal)
If $P$ is true, then there exists a construction $c$ such that $c$ is a warrant for $P$ that increases the probability that $P$ is true, other things being equal ($TP \rightarrow \exists c(V_{weak}cP)$).

Then we showed that the Recognition Thesis itself does not constrain truth to be construed constructively in any manner whatsoever. What is needed is an independent argument that something goes wrong if true, as it occurs in either the Verification Constraint or the Recognition Thesis, is construed in a non-constructive manner. However, without a very good argument to this effect, we have positive reason to consider the occurrence of true in a manner consistent with the manner in which most semanticists construe it, given that the dialectical context we are in is a debate about how to characterize truth in semantics. Therefore, as verificationists who hold that verifiability is a necessary condition on the truth of a claim, we have very good reason to prefer a Recognition Thesis and Verification Constraint where "a warrant for $P$ that increases the probability that $P$ is true" is understood as "a warrant for $P$ that increases the probability that $P$ is true (in some manner consistent with model theoretic semantics)" over a Recognition Thesis and Verification Constraint where it (replacing "true" with "false" in the case of falsifiability) is understood as "a warrant for $P$ that increases the probability that $P$ is Heyting provable/falsifiable/superassertible." In the next chapter we shall examine such reasons proffered by Wright, Dummett, and Tennant.
CHAPTER 3

REVISION

In the last chapter I argued that when Dummett's dialectic is restricted to empirical sentences with defeasible warrants, the following constraints are the strongest possible Verification Constraints which have been motivated.

Verification Constraint
If $P$ is true, then it is possible that there exists a construction $c$ such that $c$ is a warrant for $P$ that increases the probability that $P$ is true, other things being equal ($TP \rightarrow \exists c \text{Weak } cP$),

or, if the modal is collapsed,

Verification Constraint (with collapsed modal)
If $P$ is true, then there exists a construction $c$ such that $c$ is a warrant for $P$ that increases the probability that $P$ is true, other things being equal ($TP \rightarrow \exists c \text{Weak } cP$).

I have also shown that the anti-realist, who wants to replace classical truth with some suitably constructivized notion, must offer arguments that "truth" as it occurs in the Verification Constraint cannot be understood in a manner consistent with linguistic use of classical semantics. This is in fact what arguments for the revision of logic attempt to do. However, as we shall see, every extant argument for logical revision requires stronger versions of the Verification Constraint than are either motivated by the Recognition Thesis or independently reasonable.

Contemporary arguments in favor of intuitionistic revision fall into two classes, both of which are addressed with varying levels of clarity in Dummett's work. The first class, which we can call "single sentence arguments," attempt to show that
adherence to either classical model theory or use of classical reasoning commits one to the existence of a meaningful and understandable sentence which is neither verifiable, nor falsifiable. The second class, which we can call “discourse arguments,” attempt to show that use of either classical model theory or principles leads to a false characterization of either a given set of sentences or our grasp of that set of sentences.\(^1\)

My strategy for all of these arguments is to show that once one accepts a reasonable form of verificationism, the revisionary conclusions do not follow. As we shall see, once the revisionary arguments are clearly and charitably formulated the point is fairly obvious. Recent work by Wright and Tennant help considerably towards achieving the goal of clear, charitable formulation of the arguments in a few ways: (1) the standard of clarity in their own revisionary arguments is very high, and (2) Wright’s earlier and Tennant’s newer criticisms of Dummett’s arguments motivate a more charitable, albeit more complicated, reading of Dummett’s arguments than is extant in the literature.

In the following, order of presentation is solely determined by considerations of readability. Thus, I start with Wright’s revisionary arguments, because their clarity and simplicity allows the strategy of refuting them by appeal to a reasonable form of verificationism to also be very clear. Then, when I go on to address Dummett’s arguments, where competent explication is itself no small task, the similar form of refutation is somewhat clearer. Tennant’s argument, while more clearly presented,

\(^1\)One might focus on a third class which can be called “proof-theoretic arguments”; these arguments attempt to show that if one characterizes meaning and grasp of meaning of sentences in terms of canonical proofs of those sentences, then one should eschew classical logic. See (Tennant, (1997, p. 305-354)) I don’t focus on these for three reasons: (1) they require independent argument to the conclusion that meaning should be characterized proof-theoretically, and the arguments being refuted in this chapter are the arguments for this claim, (2) they may actually be non-revisionary, as they only establish that classical principles are not principles of logic, and I am not concerned with whether or not classical principles are logical, but rather, whether or not use of classical semantics in natural language semantics has been refuted by Dummettian considerations, and (3) the arguments in (Tennant, 1986) can be viewed as a proof theoretic justification of model theory (if proof theory is epistemically safe and model theory is proof theory in disguise, then model theory is epistemically safe).
is itself somewhat more complicated than Dummett's, as it utilizes limitation results from recursion theory. However, we shall see that some of the distinctions used in charitably interpreting Dummett help in explicating Tennant's arguments. Given its difference from Dummett and Wright's arguments, that Tennant's argument also fails (by his own admission) because it requires too strong a notion of verifiability is of independent interest.\(^2\)

Though the point of this chapter is to show that an adequate case hasn't been made for logical revision, I none-the-less take these arguments to be important. By clearly stating them we can better understand the sorts of commitments certain philosophical positions carry with them. This last point is important, for it is all too easy, given the reflective equilibrium nature of Dummett's main meaning-theoretic arguments, for the classicist to dig in her heels and hold that nothing will disabuse her of classical logic, without further investigating the dialectical commitments which the Dummettian arguments show are incurred by one who holds all instances of classical inferences to be valid. In a related context David Lewis writes,

I'm moved to laughter at the thought of how presumptuous it would be to reject mathematics for philosophical reasons. How would you like the job of telling the mathematicians that they must change their ways, and abjure countless errors, now that philosophy has discovered that there are no classes? Can you tell them, with a straight face, to follow philosophical argument wherever it may lead? If they challenge your credentials, will you boast of philosophy's other great discoveries: that motion is impossible, that a Being than which no greater can be conceived cannot be conceived not to exist, that it is unthinkable that anything exists outside the mind, that time is unreal, that no theory has ever been made at all probable by evidence (but on the other hand that an empirically ideal theory cannot possibly be false), that it is a wide-open scientific question whether anyone has ever believed anything, and so on, and on, \textit{ad nauseam}?

\cite{Lewis, 1991, p. 59}

While this may or may not express the proper relation of philosophy to the sciences, \(^2\)As we shall see, Tennant's argument only fails when understood as resting on premisses both the anti-realist and realist accept. As a "partisan explication of the anti-realist's position" (Tennant, p.c.) it succeeds.
on its own it is not very illuminating. Even if the conclusions of Dummett's meaning-
theoretic arguments are ruled false \textit{a priori}, we do well to examine them to find out
what, if any, further commitments they burden the classicist with.\footnote{Also, I take it that part of the desiderata of constructive approaches to mathematics involves, in some inchoate sense, saving as much of the phenomena of classical mathematics as possible. This is constructivism as research program, not censure. Moreover, independently of these arguments, constructive approaches to mathematics and logic are legitimate mathematics and logic in their own right. See (Shapiro, 1985).} That is, if there is a valid argument that concludes that we need to revise our logic, then the committed classicist will need at least be confronted with one or more interesting \textit{modus tollenses}.

As I will argue, the appropriate \textit{modus tollens} for all of the arguments concludes
with the incorrectness of certain strong forms of verificationism. That Chapter
II provides independent reasons for rejecting these forms of verificationism greatly
enhances the classicist's case.

§ 3.1 \textsc{Wright's Discourse Arguments}

In his recent monograph, \textit{Truth and Objectivity}, Crispin Wright shows that un-
restrictedly accepting both classical inference, as well as verifiability as a necessary
condition for the truth of any claim, commits one to \textit{prima facie} objectionable
claims. As a result it seems as if the verificationist cannot accept all instances of
valid classical reasoning. However, as I will show, an avenue unexplored by Wright
is also open to the verificationist. If she accepts the form of verificationism argued
to be the most reasonable in Chapter II, then Wright's unsettling conclusions are no
longer objectionable.

§ 3.1.1 Wright's First Argument

Wright's first argument begins by noting that any necessary condition $V$ upon
the truth of a sentence $P$ (i.e. where $\forall P (TP \rightarrow VP)$ holds), entails a sentence of
the form $\neg VP \rightarrow V \neg P$. Though Wright's specific discussion concerns the sentential
predicate "...is warrantedly assertible," I will use the predicate "...V" throughout, as the classicist's strategy I will suggest involves interpreting "...V" not as warranted assertibility, but rather in a manner such that: (1) one moved by Dummett's meaning-theoretic arguments might feel that "...V" constrains the truth predicate as a reasonable and appropriate verifiability predicate, as characterized in Chapter II, and (2) the conclusions that Wright shows follow from placing "...V" as a necessary condition upon the truth of a sentence, and accepting classical logic, are not so untoward.

Wright's first argument relies upon the following Lemma, which can easily be formalized in a Fitch style proof system.

Rules of inference:
- quotation: P; therefore TP
- Vn rule: TP; therefore VP

Claim: \( \neg VP \rightarrow V\neg P \)

Proof:
1. \( \neg VP \) Assumption for \( \rightarrow \) introduction
2. \( \neg TP \) Vn rule and modus tollens
3. \( P \) Assumption for \( \neg \) introduction
4. \( TP \) 3 by quotation
5. \( TP \land \neg TP \) 2,4 \( \land \) introduction
6. \( \neg P \) 3-5 \( \neg \) introduction
7. \( T\neg P \) 6 by quotation
8. \( V\neg P \) Vn rule
9. \( \neg VP \rightarrow V\neg P \) 1-6 \( \rightarrow \) introduction

Thus, Wright has shown that any theorist who takes the verifiability of a sentence \( P \) to be a necessary condition on the truth of \( P \) is committed to the conditional, "If it is not the case that \( P \) is verifiable, then it is the case that not-\( P \) is verifiable."

On the face of it, this is an unacceptable result. In an earlier criticism of deflationary views of truth, where Wright argues that deflationists are committed to the inference "\( P \) is true; therefore \( P \) is warrantedly assertible," Wright enjoins us to consider neutral states of information. Where, "\( WA\ P \)" means "\( P \) is warrantedly
assertible," he writes

[the conditional \( \neg WA P \rightarrow WA \neg P \)] must fail for any discourse whose
ingredient statements are such that a state of information may be neutral—may justify neither their assertion nor their denial. For with
respect to such a state of information, and such a statement P, it will be
correct to report that it is not the case that P is warrantedly assertible
but incorrect to report that the negation of P is warrantedly assertible.

(Wright, 1992 (p. 20))

In the context of our discussion about verificationism, the above result shows that
the mere existence of any sentence of which it is correct to say that \( \neg V P \land \neg V \neg P \)
is absurd, as this contradicts the principle that \( \neg V P \) implies \( V \neg P \). That is, anyone
who takes some form of verifiability to be a necessary condition upon truth cannot
hold that it is ever correct to say of a sentence that both it and its negation are not
verifiable, on pain of contradiction.

Later in the book, Wright suggests that an intuitionist, whose account of the
meaning of the logical operators differs from the classicist, can accept the conclusion
of the above argument, but only because her interpretation of it differs significantly
from the classicist’s understanding. Wright states,

Now for the mathematical Intuitionists, the negation of a mathematical
statement is taken as proved just in case we have a proof that any (con­
structive) proof of the original statement would lead to contradiction.
The natural interpretation of that understanding is that a proof of the
negation of a statement is any proof of the impossibility of constructive
proof of the original; and the natural generalization of that proposal
to statements which do not admit of proof, but merely of defeasible
evidence, is that evidence for the negation of a statement is, again,
essentially evidence that no evidence is available for the statement.

(Wright, 1992 (p. 43))

This is not the appropriate place to assess the ability of a Heyting semantic, or
proof-theoretic, understanding of the logical operators to motivate the validity of
the principle that if a sentence is not verifiable, then its negation is. Rather, I

\footnote{See (Taschek & Shapiro, (1996)), and (Tennant, (1995)) for good explanations of how Heyting
semantics negotiates the conditional. The further question of whether or not a deflationist can
consistently avail herself of the intuitionistic interpretation of the logical operators is orthogonal to

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want to make explicit the revisionary nature of this argument. If an intuitionistic understanding of the logical operators can make reasonable the problematic claim, and a classicist's understanding cannot, then a verificationist, committed to the problematic claim, should understand her logical operators intuitionistically.

In section 3.13, I will argue that the nonrevisionary verificationist's proper response is to understand the $V$ predicate in such a way that $\neg V P \rightarrow V \neg P$ is not unreasonable, while retaining a classical understanding of the logical operators.

§ 3.1.2 Wright's Second Argument

The following argument shows that use of the law of excluded middle forces the verificationist to accept, for any sentence, that either it or its negation is verifiable.

Rules of inference:
- quotation: $P$; therefore $TP$
- Vn rule: $TP$; therefore $VP$
- Claim: $VP \lor V\neg P$

Proof:
1. $P \lor \neg P$ By the law of the excluded middle
2. $|P$ Assumption for $\lor$ elimination
3. $|TP$ 2 quotation
4. $|VP$ 3 Vn rule
5. $|VP \lor V\neg P$ 4 $\lor$ introduction
6. $|\neg P$ Assumption for $\lor$ elimination
7. $|T\neg P$ 6 quotation
8. $|V\neg P$ 7 Vn rule
9. $|VP \lor V\neg P$ 8 $\lor$ introduction
10. $VP \lor V\neg P$ 1, 2-5, 6-9 $\lor$ elimination

Thus, any sentential predicate $V$ which holds of a sentence whenever it's true, will classically yield $VP$ or $V\neg P$. Thus, Wright thinks even the extremely weak verifi-

the present discussion.
cationist principle,

\[(\text{EC})\]
If \(P\) is true, then evidence is available that it is so,

forces some form of intuitionistic revision upon us. He holds that since, by the above proof scheme, \((\text{EC})\) entails

\[(\text{C})\]
Either evidence is available for \(P\) or evidence is available for its negation,

even an extremely weak form of verificationism needs to reject the instance of excluded middle on line one of the preceding proof scheme. Unfortunately, Wright says very little about what he finds offensive about \((\text{C})\). The quote in full reads,

> If we may not assume that evidence either is or is not available for an arbitrary statement, then the convertibility of lack of evidence for a particular statement into evidence for its negation demanded by the Negation Equivalence when truth is evidentially constrained, need not impose \((\text{C})\)—and so need not be in contradiction with the \(a\ priori\) unwarrantability of the claim that the scales of in principle available evidence must tilt, sooner or later, one way or the other, between each statement and its negation. (Wright, 1992 (p. 42))

In the context of our discussion, Wright’s proof shows that the nonrevisionary verificationist has the task of showing that the principle that any sentence or its negation is verifiable is reasonable.\(^5\) At this point it should also be noted that it is clear from Wright’s quote that he assumes that the verifiers in question must be sufficient for

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\(^5\)In the above quote, Wright advert to a version of the proof that performs excluded middle on the claim that evidence is available for a sentence \(P\), though nothing hangs on this. Wright’s proof uses the instance of the preceding conclusion that if no evidence is available for \(P\) then evidence is available for the negation of \(P\), though it needn’t have. There’s a sense in which this principle is irrelevant to the considerations of the first proof as well. Quotation and conditional proof get us from the schematic if “\(Q\) is true, then \(Q\) is verifiable” to “if \(Q\), then \(Q\) is verifiable.” Then, if a neutral state of information is one in which, for some claims \(P\), \(P\) is not verifiable and \(\neg P\) is not verifiable, then by two applications of \textit{modus tollens} on the schematic “if \(Q\), then \(Q\) is verifiable” we get \((\neg P \land \neg \neg P)\). Nonetheless, the use of the result that if \(P\) is not verifiable then \(\neg P\) is, in the first argument, is heuristically valuable, as motivating this claim is a good way for any verificationist to illustrate how she can preclude neutral states of information in the problematic sense.
truth; otherwise it could be the case of every sentence that it was verifiable or falsifiable, without it being the case that “the scales of in principle available evidence must tilt, sooner or later, one way or the other, between each statement and its negation.”

§ 3.1.3 Reasonable Verificationism and Wright’s Proofs

Here I will show that Wright’s results motivate logical revision only if the notion of verifiability taken to be necessary for truth is stronger than what is either warranted by Dummett’s arguments, or independently reasonable. From our discussion in Chapter II we have the following characterizations of verifiability.

Verification Constraint
If $P$ is true, then it is possible that there exists a construction $c$ such that $c$ is a warrant for $P$ that increases the probability that $P$ is true, other things being equal ($TP \rightarrow \exists c V_{weak}cP$).

Verification Constraint (with collapsed modal)
If $P$ is true, then there exists a construction $c$ such that $c$ is a warrant for $P$ that increases the probability that $P$ is true, other things being equal ($TP \rightarrow \exists c V_{weak}c$).

Given these interpretations of “...V”, we can inquire whether $\neg VP \rightarrow V\neg P$ is problematic, on a classical interpretation of the logical operators involved.

The reading without the collapsed modal give us the following thesis.

Wright’s First Result
If it is not possible that there exists a construction $c$ such that $c$ is a warrant for $P$ that increases the probability that $P$ is true, other things being equal, then it is possible that there exists a construction $k$ such that $k$ is a warrant for $\neg P$ that increases the probability that $\neg P$ is true, other things being equal ($\neg V_{weak}cP \rightarrow (\exists k V_{weak}k\neg P)$).

This is clearly non-problematic, given that the antecedent of the conditional is so strong, and that the consequent so weak. The condition merely states that if there is no possible world where weak evidence for $P$ exists, then in some possible world, weak evidence for $\neg P$ exists. Wright gives no argument against such a claim. The
kinds of neutral states of information he considers are ones where we in fact possess no information one way or the other about a claim, but nothing about the above conditional rules this out.

Perhaps this is a point where the collapsing modal does some work for the revisionist. With the collapsed modal, the antecedent of the above conditional becomes weaker, while the consequent becomes stronger. That is, the conditional does assert something more contentious than the version without the collapsed modal. Thus, with the collapsed modal the result becomes.

Wright’s First Result (with collapsed modal)

If there does not exist a construction c such that c is a warrant for P that increases the probability that P is true, other things being equal, then there exists a construction k such that k is a warrant for ¬P that increases the probability that ¬P is true, other things being equal.

\[ \neg(\exists c V_{weak} c P) \rightarrow (\exists k V_{weak} k \neg P). \]

This is somewhat better for Wright, as it is a priori somewhat less plausible than the other result, given that it is logically weaker than Wright’s First Result. However, it is still completely unclear why the verificationist should balk at this claim with the classical interpretations of the logical operators. The statement merely says that if there is no very weak warrant for a claim in this world, then there is a very weak warrant for the claim’s negation in this world (albeit, the warrants needn’t ever be discovered by anyone). Again, the fact that the warrants need only increase the probability of the claim in question renders the antecedent so strong, and the consequent so weak, that it is not at all clear why a verificationist would balk at asserting the claim.

In any case, as I argued in the last chapter, no anti-realist has given an argument for collapsing the modal in the empirical case. Given this, we were led to conjecture that the modal was collapsed in anti-realists’ writings because of a false analogy with mathematical claims, where one might, given the supposed necessity of mathematical claims, be able to defend the claim that the modals do collapse.
To evaluate the second argument we need to ask whether a classicist's commitment to \((VP \land V\neg P)\) is a problem. Again, we must examine what such an assertion amounts to, given the strongest reasonable interpretations of the \(V\) predicate of which the Dummettian can avail herself when talking about empirical sentences with defeasible warrants. So, once again, we get two possible interpretations, one without collapsed modals and one with. With the \(V\) predicate as defined above these two interpretations can be represented in the following manner.

**Wright's Second Result**

It is possible that there exists a construction \(c\) such that \(c\) is a warrant for \(P\) that increases the probability that \(P\) is true, other things being equal, or it is possible that there exists a construction \(k\) such that \(k\) is a warrant for \(\neg P\) that increases the probability that \(\neg P\) is true, other things being equal \((\exists^0 c V_{weak} c P) \lor (\exists^0 k V_{weak} k P)\).

**Wright's Second Result (with collapsed modal)**

There exists a construction \(c\) such that \(c\) is a warrant for \(P\) that increases the probability that \(P\) is true, other things being equal, or there exists a construction \(k\) such that \(k\) is a warrant for \(\neg P\) that increases the probability that \(\neg P\) is true, other things being equal \((\exists^0 c V_{weak} c P) \lor (\exists^0 k V_{weak} k P)\).

While I agree with Wright that this would be implausible if the sentence were an exclusive disjunction, ("the scales of in-principle evidence must tilt, sooner or later, one way or the other..."), the sentence being an exclusive disjunction only follows from taking either \((\exists^0 c V_{weak} c P)\) or \((\exists V_{weak} c P)\) to be sufficient conditions for the truth of \(P\), and clearly neither of them is sufficient for truth.\(^6\) Thus, again, Wright's troublesome conclusions are only troublesome if one adopts a kind of verifiability which, by the discussion in Chapter II, is unmotivated anyway.

I've suggested, and slightly fleshed out, a strategy forced upon the non-revisionary verificationist by Wright's arguments. The trick is to weaken the concept of verifiability.

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\(^6\)One might think, motivated by the thought that there may exist a sentence \(P\) such that no amount of evidence would ever bear in favor (or against) it, that there is no reason an inclusive reading is any more plausible. This can't be Wright's thought though, as this thought is intuitionistically inconsistent with the necessity of verifiability for truth!
iability so that: (1) verifiability can be taken as a necessary condition upon the truth of a claim in a manner which does not run afoul of Dummettian dialectic, and (2) Wright’s unsettling conclusions that follow from a non-intuitionistic interpretation of the logical operators, as well as from the validity of non-intuitionistic inferential patterns, no longer strike one as so unsettling. Thus, in a sense, Wright’s arguments place a burden of proof upon the non-revisionary verificationist to argue that her interpretation of the \( V \) predicate is the proper one. However, in another sense, Wright’s arguments also place a burden of proof upon the revisionist. In the context of Wright’s proofs, the revisionist needs to say why we should adopt an understanding of the verificationist principle on which these results are unsettling.

§ 3.2 Dummett’s Single Sentence Arguments

In this section we shall see that Dummett’s more complicated revisionary argument fails for the same reason that Wright’s arguments failed. Again, I shall be able to show that Dummett’s argument is sound only to the extent that an implausible formulation of verifiability is adopted in the Verification Constraint, though it will take some work to show this. However, explicating Dummett’s argument clearly and charitably is no small task.

§ 3.2.1 Some Other Important Distinctions

To evaluate Dummett’s arguments, we must be very careful to divorce the effective undecidability of a sentence from its in-principle undecidability, and to divorce both kinds of undecidability from the undecidability of a theory. The first argument we shall consider concerns itself with the possible in-principle undecidability of a sentence.

Since in-principle undecidability is a difficult notion we shall proceed by first characterizing the other two types of undecidability. Take a theory to be undecidable if, and only if, there is no correct decision procedure to determine of any given
sentence of the language, whether it is in the theory. Using Church's Thesis, we can restate the definition this way; a theory is undecidable if, and only if, the set of true sentences of the theory is not recursive. (Actually Church's Thesis gets us that if a set of sentences is not recursive, then it is not decidable. The other direction is completely unproblematic.) However, Church's Thesis and a mathematical characterization of recursiveness should not lead us to think that this kind of decidability and undecidability is a philosophically simple, if formally complicated, issue. For example, clarity about this kind of decidability requires an explication of the status of "truth" mentioned in the definiens. If one held that all sentences are true, and the syntax of the theory in question were recursive, then one would have a trivial decision procedure for all sentences. Moreover, it is not clear without a great deal of explication how mathematical results in recursion theory are relevant to any but the mathematical contexts they occur in.

For present purposes, take a sentence $P$ to be effectively undecidable if, and only if: (1) neither $P$ nor $\neg P$ is verified, and (2) neither $P$ nor $\neg P$ has been shown to be a member of a decidable true theory. The effective undecidability of a sentence can, prima facie, change. For example, Fermat's last theorem, which states, "For all natural numbers $x, y, z$, and all $n > 2$, it is not the case that $x^n + y^n = z^n$," was effectively undecidable, but now isn't. Some first order sentences involving only zero, successor, less than, and addition, sentences of what is now called Presburger Arithmetic, were once effectively undecidable, but now aren't. Goldbach's Conjecture, which states that every even natural number is the sum of two primes, however, remains effectively undecidable, because it has neither been proved nor disproved, nor shown to be a member of a decidable set of true sentences.

Take a sentence $P$ to be in-principle undecidable if and only if there does not, and in a sense to be contemplated, cannot, exist a verification of the sentence or of its negation. Candidates for this kind of undecidability are hard to come by. In

\[\text{See (Presburger, 1929).}\]
the context of mathematics, how could we ever know that a sentence can never be proved short of proving its negation? For example, we know that there does not exist a proof of the continuum hypothesis from the axioms of Zermelo-Fränkel set theory. However, we do not know that there does not and cannot exist a proof of the continuum hypothesis. Whatever such a condition would amount to depends heavily on auxiliary mathematical and philosophical hypotheses.

Since the first argument that I will present maintains that there can be no such in-principle undecidable sentences, we will need to begin with some sort of rough and ready characterization of this form of undecidability, and then attempt to improve upon this characterization as best we can as we proceed. In what follows, a sentence $P$ is considered in-principle undecidable if and only if it is not possible to know that $P$ and it is not possible to know that $\neg P$, given any finite extension of our conceptual resources. Thus the claim that there exists an in-principle undecidable sentence will be formulated this way;

$$\exists X (\neg kX \land \neg k\neg X)$$

where the domain of quantification is taken to be all sentences that are in some idealized sense understandable, and "$kP" means "it is possible to know that $P."

§ 3.2.2 Dummettian Realism and its Denial

In his earlier writings Dummett considers a certain stance towards effectively undecidable sentences to be constitutive of realism. In a representative passage he writes,

Realism I characterize as the belief that statements of the disputed class [effectively undecidable sentences] possess an objective truth-value, independently of our means of knowing it: they are true or false in virtue of a reality existing independently of us. (Dummett (1959, p. 14))

Where "$EuP" means that $P$ is effectively undecidable, "$TP" means that $P$ is true,
and "\(\Diamond P\)" means that it is possible that \(P\), we can formalize this position as,

\[
\text{Dummettian Realism} \\
(\exists X)(EuX \wedge ((TX \lor \neg TX) \wedge \Diamond(\neg kX \wedge \neg k\neg X))).
\]

Thus, Dummettian Realism says that there exists at least one effectively undecidable sentence which is determinately either true or false, and such that it is possible that the truth value of the sentence is in some absolute sense unknowable to us. After further explicating the relations that Dummett takes to obtain between classical truth-conditional semantics and Dummettian Realism, I will be in a better position to defend my characterization of a sentence "possessing its truth-value independently of our means of knowing it" as that sentence's truth value being possibly in-principle unknowable. I shall also be able to add some philosophical content to the above formalization. That is, the modal and epistemic notions occurring in the statement of Dummettian Realism will be fleshed out in a manner which will allow us to judge its truth as well as the validity of Dummett's arguments concerning them.

Both Dummett's realist and anti-realist accept that for any effectively decidable sentence \(P\), \(P\) is determinately either true or false and, moreover, that it is possible to know that \(P\) or possible to know that \(\neg P\). Then, since the negation of any instance of bivalence is intuitionistically contradictory, and since every sentence (at a given time) is either effectively undecidable or decidable, the disagreement between the Dummettian realist and anti-realist concerns whether or not it is possible that any sentence is in-principle undecidable. Thus, the verificationism that Dummett takes to save the meaning theorist needs to entail that it is not possible for any sentence to be unknowable. This rather complicated modal statement can be formalized as

\[
(\forall X)\neg \Diamond(\neg kX \wedge \neg k\neg X).
\]

\(^8\)In later writings Dummett has come to modify this view. In (Dummett (1982)) he argues that there are three individually necessary and jointly sufficient conditions for manifesting a realist stance towards a discourse: (1) knowledge of meaning is equated with knowledge of truth conditions, (2) every statement is taken to be determinately true or false, and (3) the truth of sentences in the discourse is secured via something like the standard Tarskian account of reference and satisfaction. This broader definition of Realism is taken into account in section 3.2.5.
In "The Philosophical Basis of Intuitionistic Logic" Dummett throws the gauntlet down. He writes,

This (the realists') conception violates the principle that use exhaustively determines meaning; or, at least, if it does not, a strong case can be put up that it does, and it is this case which constitutes the first type of ground which appears to exist for repudiating classical in favor of intuitionistic logic for mathematics. For, if the knowledge that constitutes a grasp of the meaning of a sentence has to be capable of being manifested in actual linguistic practice, it is quite obscure in what the knowledge of the condition under which a sentence is true can consist, when that condition is not one which is always capable of being recognized as obtaining. (Dummett (1973, 224))

In this passage Dummett can be understood as arguing that unless the principle, 

\((\forall X)(TX \rightarrow kX)\)

holds, it is quite obscure in what knowledge of meaning can consist. This is Dummett's challenge, sketched in Chapter 1, to specify what knowledge of meaning does consist in, in terms of practical abilities that can be correctly attributed to a competent language user. As was discussed in Chapter 1 and Chapter 2, and as stated in the above quote, Dummett holds that a verificationist, committed to 

\((\forall P X)(TX \rightarrow kX)\),

can provide a theory of competence for a language. The argument we are concerned with attempts to show that the truth conditional semanticist cannot use a verificationist theory of competence to accomplish this goal.

With the schematic \((TP \leftrightarrow P)\), it is trivial to show that \((\forall X)(TX \rightarrow kX)\) is equivalent to \((\forall X)((TX \rightarrow kX) \land (\neg TX \rightarrow k\neg X))\). Unfortunately, to contradict Dummettian Realism, Dummett needs the stronger principle that it is necessarily the case that for all understood sentences \(P\), if \(P\) is true then it is possible to know that \(P\), and if \(P\) is not true then it is possible to know that it is not the case that \(P\). Where "\(\Box_1\)" means "it is necessarily the case that," we can formalize this stronger principle as,

Premiss i.

\(\Box_1(\forall X((TX \rightarrow k_1X) \land (\neg TX \rightarrow k_1\neg X)))\).

Given some interpretations of the necessity operator, Dummett does clearly intend
something like this. A verificationist meaning theorist does not consider \((\forall X ((TX \rightarrow k_1X) \land (\neg TX \rightarrow k_1\neg X)))\) to be a contingent state of affairs about the set of sentences that are understandable to us (the domain of quantification of the embedded sentence). Thus, in some sense, he holds that it is necessary that all truths are knowable. That the necessity box in Premiss i. can be so interpreted that Premiss i. is both a reasonable assertion and dialectically efficacious in Dummett’s revisionary argument still needs to be shown, however.

§ 3.2.3 First Try and Refutation

While it is clear that Dummett’s verificationism does entail that it is necessary that no sentence is in-principle undecidable, it is not clear how this causes a problem for uses of classical semantics. This very difficulty has recently caused Neil Tennant, to argue that one might be forgiven for thinking that Dummett’s manifestation argument, as an argument against bivalence, “is a non-sequitur of numbing grossness.” (Tennant (1997, p. 160))

While Tennant agrees that Dummett has sufficiently motivated the kind of verificationism stated in Premiss i., and thus refuted Dummettian Realism, Tennant holds that the contrast between the verificationist and Dummettian Realist is properly orthogonal to the issue between the classicist and the intuitionist. That is, according to Tennant, Dummett systematically conflates adherence to classical principles with the denial of verificationism, and thus ignores the possibility of coherently accepting both Premiss i. and classical logic. For example consider the following passage,

What is a realistic interpretation of statements of some given class? It is, essentially, the belief that we possess a notion of truth for statements of that class under which every statement is determinately either true or not true, independently of our knowledge or our capacity for knowledge. [emphasis and italics added] (Dummett, (1976c, p. 274))

Dummett does seem to be conflating determinate truth or falsity with recognition, or knowledge, transcendence. Moreover, such passages are not at all rare in Dummett’s
The position which Tennant claims Dummett overlooks, the combination of the verificationism stated in Premiss i. and adherence to classical semantics, is a form of optimism, as the law of bivalence, combined with Premiss I. yields,

\[ \Box_1 (\forall X (k_1 X \lor k_1 \neg X)). \]

Gödelian Optimism is optimistic because it holds that any sentence which we understand can ultimately be known to be true or known to be false.

It should be noted that Gödelian Optimism follows from Premiss i. and bivalence, since bivalence is logically necessary, and since the necessity formalized by "\( \Box_1 \)" will in some sense be of a weaker sort. To prove this we will need to utilize the following standard inference in modal logic, where "\( \Box_L P \)" means that \( P \) is logically necessary.

Transformation Rule:

\[ K. P, \text{ therefore } \vdash \Box_L P \]

This just says that if \( P \) is provable from no premisses, then \( P \) is logically necessary. Then the first part of the proof will prove that it is logically necessary that if verificationism and bivalence are true, then every sentence is such that it is either

\[ ^9 \text{The term "Gödelian Optimism" comes from (Shapiro, 1993).} \]
knowable to be true or knowable to be false. Consider.

Claim:
\[ \Box_L((\forall X(TX \lor T\neg X) \land \forall X((TX \rightarrow k_1X) \land (T\neg X \rightarrow k_1\neg X))) \rightarrow \forall X(k_1X \lor k_1\neg X)) \]

Proof:
1. \[ \forall X(TP \lor T\neg P) \land (\forall X((TX \rightarrow k_1P) \land (T\neg X \rightarrow k_1\neg X))) \]
   Assumption for \( \rightarrow \) intro.
2. \[ \forall X(TX \lor T\neg X) \]
   1 \( \land \) elimination
3. \[ \forall X((TX \rightarrow k_1X) \land (T\neg X \rightarrow k_1\neg X)) \]
   1 \( \land \) elimination
4. \[ (TP \lor T\neg P) \]
   2 \( \forall \) elimination
5. \[ (TP \rightarrow k_1P) \land (T\neg P \rightarrow k_1\neg P) \]
   3 \( \forall \) elimination
6. \[ (TP \rightarrow k_1P) \]
   5 \( \land \) elimination
7. \[ (T\neg P \rightarrow k_1\neg P) \]
   5 \( \land \) elimination
8. \[ TP \]
   Assumption for \( \lor \) elimination
9. \[ k_1P \]
   6,8 \( \land \) elimination
10. \[ (k_1P \lor k_1\neg P) \]
   9 \( \lor \) introduction
11. \[ T\neg P \]
   Assumption for \( \lor \) elimination
12. \[ k_1\neg P \]
   7,11 \( \rightarrow \) elimination
13. \[ (k_1P \lor k_1\neg P) \]
   12 \( \lor \) introduction
14. \[ (k_1P \lor k_1\neg P) \]
   4,8–10,11–13 \( \lor \) elimination
15. \[ \forall((k_1X \lor k_1\neg X)) \]
   14 \( \forall \) introduction
16. \[ ((\forall X(TX \lor T\neg X) \land \forall X((TX \rightarrow k_1X) \land (T\neg X \rightarrow k_1\neg X))) \rightarrow \forall X(k_1X \lor k_1\neg X)) \]
   1–15 \( \rightarrow \) introduction
17. \[ \Box_L((\forall X(TX \lor T\neg X) \land \forall X((TX \rightarrow k_1X) \land (T\neg X \rightarrow k_1\neg X))) \rightarrow \forall X(k_1X \lor k_1\neg X)) \]
   15 K.

To finish this proof we will need to include our premisses and two rules of inference, given here.

Premisses:

i. \( \Box_L((TX \rightarrow k_1X) \land (\neg TX \rightarrow k_1\neg X)) \)

Biv. \( \Box_L(\forall X(TX \lor T\neg X)) \).

Rules of inference:

\( \Box \) dist. \( \Box_x(P \rightarrow Q) \), therefore \( (\Box_xP \rightarrow \Box_xQ) \)

\( \Box \) dist.' \( (\Box_xP \land \Box_xQ) \), therefore \( \Box_x(P \land Q) \)

\( \Box >. \) \( \Box_LP \), therefore \( \Box_yP \) (If \( P \) is logically necessary, then \( P \) is necessary in any other modality)

Again, these rules of inference are standard rules of modal logic that clearly retain their validity in the present context. With them we can prove our desired result in 136.
a few more steps. Rather than referring to the above proof as a lemma, I will just continue it.

Claim:

\[ \Box \forall X (k_1 X \lor k_1 \neg X) \]

Proof:

18. \[ \Box (\forall X (T X \lor T \neg X) \land \forall X ((T X \rightarrow k_1 X) \land (T \neg X \rightarrow k_1 \neg X))) \rightarrow \forall X (k_1 X \lor k_1 \neg X) \]

17 \[ \Box \rightarrow . \]

19. \[ \Box (\forall X (T X \lor T \neg X) \land \forall X ((T X \rightarrow k_1 X) \land (T \neg X \rightarrow k_1 \neg X))) \rightarrow \Box \forall X (k_1 X \lor k_1 \neg X) \]

18 \[ \Box \text{dist.} \]

20. \[ \Box \forall X ((T X \rightarrow k_1 X) \land (\neg T X \rightarrow k_1 \neg X)) \]

21. Premiss i.

22. \[ \Box (\forall X (T X \lor T \neg X)) \]

21 Biv. \[ \Box \rightarrow . \]

23. \[ \Box (\forall X ((T X \rightarrow k_1 X) \land (\neg T X \rightarrow k_1 \neg X))) \land \Box (\forall X (T X \lor T \neg X)) \]

20, 22 \[ \land \text{introduction} . \]

24. \[ \Box (\forall X ((T X \rightarrow k_1 X) \land (\neg T X \rightarrow k_1 \neg X))) \land (\forall X (T X \lor T \neg X)) \]

24 \[ \Box \text{dist.'} \]

25. \[ \Box \forall X (k_1 X \lor k_1 \neg X) \]

19, 24 \[ \rightarrow \text{introduction} \]

Thus does Gödelian Optimism follow from bivalence and verificationism. We shall not need to discuss whether or not Dummett is guilty of equivocation in ignoring it, as Tennant wonders. This is because of our restriction to empirical sentences.\(^\text{10}\)

In Chapter 6 of *The Taming of the True*, Tennant strives to discern a watertight, convincing, and non-equivocating argument from verificationism to logical revision in Dummett’s work, and is unable to do so. Where there is not simple conflation, an embarrassing logical fallacy seems to occur. Dummett often seems to say that we all agree that there are effectively undecidable sentences in a language, and that the existence of such undecidable sentences, in conjunction with bivalence and verificationism, leads to a contradiction. This seems to be the substance of many passages

\(^{10}\)Gödelian Optimism just is the problematic conclusion of Wright’s second argument. However, our way around the problem, by weakening the notion of “knowability” that occurs in the statement of Optimism, does not properly yield Gödelian Optimism. One only gets the Gödelian position if the “knowability” operators are taken to be sufficient for truth, and with empirical sentences there is no reason to do so.
of Dummett. In a representative one he writes,

> It is when the principle of bivalence is applied to undecidable statements
> that we find ourselves in the position of being unable to equate an
> ability to recognize when a statement has been established as true or
> as false with a knowledge of its truth-condition, since it may be true in
> cases when we lack the means to recognize it as true or false when we
> lack the means to recognize it as false. (Dummett, (1976b, p. 63))

Thus the argument seems to state that if we subscribe to verificationism, then bi-
valence will force us into a kind of Gödelian Optimism, which then contradicts the
existence of effectively undecidable sentences.

This interpretation of Dummett’s argument can easily be formulated in a Fitch
style proof system. In formulating it thus, the fallacious nature of the argument is
immediately apparent. Moreover, to see what is wrong with this proof, we need not
include the modal in front of the statement of verificationism.

Premisses:

i. $\forall X((TX \rightarrow k_1 X) \land (\neg TX \rightarrow k_1 \neg X))$ (verificationism)

ii. $\exists Y(\neg k_2 Y \land \neg k_2 \neg Y)$ (the existence of an
undecidable sentence)

iii. $\forall Z(TZ \lor \neg TZ)$ (bivalence)

The proof of the joint inconsistency of these premisses will proceed in two stages.
First I shall show that the first and third premisses entail that for every sentence
$X$, either $X$ or its negation is knowable (this is essentially the same kind of thing
Lemma 1: i. iii. \( \vdash (\forall X)(k_1X \lor k_1\neg X) \)

Proof:
1. \( \forall X((TX \rightarrow k_1X) \land (\neg TX \rightarrow k_1\neg X)) \)  
   Premiss i.
2. \( (TP \rightarrow k_1P) \land (\neg TP \rightarrow k_1\neg P) \)  
   1 \( \forall \) elimination
3. \( (TP \rightarrow k_1P) \)  
   2 \( \land \) elimination
4. \( (\neg TP \rightarrow k_1\neg P) \)  
   2 \( \land \) elimination
5. \( \forall X(TX \lor \neg TX) \)  
   Premiss iii.
6. \( (TP \lor \neg TP) \)  
   5 \( \forall \) elimination
7. \( | TP \)  
   Assumption for \( \lor \) elimination
8. \( | k_1P \)  
   3, 7 \( \rightarrow \) elimination
9. \( | k_1P \lor k_1\neg P \)  
   8 \( \lor \) introduction
10. \( | \neg TP \)  
    Assumption for \( \lor \) elimination
11. \( | k_1\neg P \)  
    4, 10 \( \rightarrow \) elimination
12. \( | k_1P \lor k_1\neg P \)  
    11 \( \lor \) introduction
13. \( k_1P \lor k_1\neg P \)  
    6, 7–9, 10–12 \( \lor \) elimination
14. \( \forall X(k_1X \lor k_1\neg X) \)  
    13 \( \lor \) introduction

Now I show that this result is inconsistent with Premiss ii. Thus, by the transitivity of deduction, we shall have proved that the above three premisses (verificationism, the existence of an undecidable sentence, and bivalence) are inconsistent with one another.

Lemma 2: \( \forall X(k_1X \lor k_1\neg X) \), ii. \( \vdash \bot \)

Proof:
1. \( \exists Y(\neg k_2Y \land \neg k_2\neg Y) \)  
   Premiss ii.
2. \( | \neg k_2P \land \neg k_2\neg P \)  
   Assumption for \( \exists \) elimination
3. \( | (\forall X)(k_1X \lor k_1\neg X) \)  
   Conclusion of lemma 1
4. \( | k_1P \lor k_1\neg P \)  
   3 \( \forall \) elimination
5. \( | \neg k_2P \)  
   2 \( \land \) elimination
6. \( | \neg k_2\neg P \)  
   2 \( \land \) elimination
7. \( | | k_1P \)  
   Assumption for \( \lor \) elimination
8. \( | | k_1P \land \neg k_2P \)  
   5,7 \( \land \) introduction
9. \( | | \bot \)  
   8 \( \neg \) elimination
10. \( | | k_1\neg P \)  
    Assumption for \( \lor \) elimination
11. \( | | k_1\neg P \land \neg k_2\neg P \)  
    6,10 \( \land \) introduction
12. \( | | \bot \)  
    11 \( \neg \) elimination
13. \( | \bot \)  
    4, 7–9, 10–12 \( \lor \) elimination
14. \( \bot \)  
    2–13 \( \exists \) elimination

Then by the transitivity of deduction it follows from Lemma 1 and 2 that Pre-
misses i. ii. and iii. are jointly inconsistent. This proof does seem faithful to what Dummett often writes. At this point the Dummettian anti-realist concludes that Bivalence caused the contradiction, and rests content with asserting Premisses i. and ii. (eschewing bivalence but still retaining commitment to verificationism and the existence of undecidable sentences). On the other hand, the Dummettian realist concludes that Premiss i. (verificationism) is the root of the evil and continues to assert Premisses ii. and iii. (bivalence and the existence of an undecidable sentence). The Gödelian Optimist decides to assert Premises i. and iii. (verificationism and bivalence) and eschews Premiss ii. (the existence of an undecidable sentence). Right? Wrong. ¹¹

We can prescind for now from the question of whether or not the two knowability operators really are comparable in the contradictions arrived at in lines 8. and 11. of the above proof. Even if they are comparable, the proof miserably fails to achieve its objectives, as the occurrence of bivalence in the proof is completely superfluous. As the following, intuitionistically valid, proof shows, premisses i. and ii. above can

¹¹After showing him the above proof as an explication of Dummett’s argument, Neil Tennant pointed out to me the problem I go on to discuss with it (Tennant, (p.c., 1995)).
be shown to be inconsistent without any dependence upon the notion of bivalence.

Claim: \( \forall X ((TX \to k_1X) \land \neg(TX \to k_1 \neg X)) \to \neg \exists Y (\neg k_1Y \land \neg k_1 \neg Y) \)

Proof:
1. \( \forall X ((TX \to k_1X) \land \neg(TX \to k_1 \neg X)) \) Assumption for \( \to \) introduction
2. \( \exists Y (\neg k_1Y \land \neg k_1 \neg Y) \) Assumption for \( \exists \) introduction
3. \( (k_1P \land \neg k_1 \neg P) \) Assumption for \( \exists \) elimination
4. \( (TP \to k_1P) \land (\neg TP \to k_1 \neg P) \) 1 \( \forall \) elimination
5. \( TP \to k_1P \) 4 \( \land \) elimination
6. \( \neg k_1P \) 3 \( \land \) elimination
7. \( TP \) Assumption for \( \neg \) introduction
8. \( k_1P \) 5, 7 \( \to \) elimination
9. \( k_1P \land \neg k_1P \) 6, 8 \( \land \) introduction
10. \( \bot \) 9 \( \neg \) elimination
11. \( \neg TP \) 7–10 \( \neg \) introduction
12. \( \neg TP \to k_1 \neg P \) 4 \( \land \) elimination
13. \( k_1 \neg P \) 11, 12 \( \to \) elimination
14. \( \neg k_1 \neg P \) 3 \( \land \) elimination
15. \( k_1 \neg P \land \neg k_1 \neg P \) 13, 14 \( \land \) introduction
16. \( \bot \) 15 \( \neg \) elimination
17. \( \bot \) 3–16 \( \exists \) elimination
18. \( \neg \exists Y (\neg k_1Y \land \neg k_1 \neg Y) \) 2–17 \( \neg \) introduction
19. \( \forall X ((TX \to k_1X) \land (\neg TX \to k_1 \neg X)) \to \neg \exists Y (\neg k_1Y \land \neg k_1 \neg Y) \) 1–18 \( \to \) introduction

Thus (on the assumption that the knowability operators in the two premisses are comparable), we simply have that Premiss i. (verificationism) and Premiss ii. (the existence of an undecidable sentence) are logically contradictory, and with this discovery we reach another possible stumbling block the Dummett interpreter must circumnavigate.

The initial proof attempted to show that bivalence, verificationism, and the existence of effectively undecidable sentences were jointly contradictory. Then the above proof showed that the assumption of bivalence in the initial proof was superfluous. But this seems to show that verificationism and the existence of effectively undecidable sentences are already jointly contradictory. Since no one would deny the existence of effectively undecidable sentences, we seem to have a short and nasty refutation of verificationism!
Fortunately the above proof needn't be interpreted in this manner. Perhaps the proof just shows us that the modal \( k_2 \) in the statement of the existence of the undecidable sentence \( ((\exists Y) (\neg k_2 Y \land \neg k_2 \neg Y)) \) needs to be sufficiently idealized, so that the sentence says that there exists a sentence such that it is not possible, given any finite extension of our conceptual resources, to know whether the sentence is true or false. But then the above proof just states that verificationism entails that there does not exist an in-principle undecidable sentence, which should not surprise anyone. This is perfectly of a piece with classical intuitionism. Indeed, the double negation of any instance of the law of excluded middle \( \neg \neg (P \lor \neg P) \) is a theorem of intuitionistic logic. By the standard Heyting Semantics interpretation of the logical operators, an instance of this is interpreted as saying that the assumption \( P \) is neither provable nor disprovable is absurd. This does raise questions concerning whether the proof forces an interpretation of the epistemic modal \( k_1 \) in the statement of verificationism upon us, which will be discussed after discerning a more charitable proof. In any case, we still don't have an argument for logical revision.

§ 3.2.4 Second Try and Refutation

Tennant recognizes that there is a reading of Dummett that leads to a reconstruction of his original argument, where he neither simply conflates acceptance of classical semantics with denial of verificationism nor superfluously employs classical principles. Perhaps, so the suggestion goes, Dummett can best be read as arguing that bivalence \( \forall X (TX \lor \neg TX) \) somehow implies the existence of an in-principle undecidable sentence. Then, given that verificationism entails that no such sentence exists, accepting verificationism forces the denial of bivalence upon us.

It should also be noted that if one were to assert \( \neg \forall X (TX \lor \neg TX) \) then one would need to reject classical logic; for if one utilizes classical logic one can prove \( \exists X (TX \land \neg TX) \) from \( \neg \forall X (TX \lor \neg TX) \). Unless one gives up the schematic biconditional \( TP \leftrightarrow P \), then one has a proof of \( \exists X (X \land \neg X) \), which is clearly an
unacceptable result. However, \( \neg \forall X (TX \lor \neg TX) \) is intuitionistically consistent, assuming that the intuitionist would still eschew the transition from “It is not the case that for all \( x \ldots \)” to “There exists and \( x \) such that it is not the case that...” when quantifying over sentences. This seems an entirely reasonable assumption. It should be noted that the intuitionist would probably make the transition when the domain of quantification was a set of sentences that composed a decidable theory. However, for decidable theories they would not assert that bivalence over the theory entailed the existence of an in-principle undecidable sentence.

Unfortunately, as Tennant points out, Dummett does not give any good evidence for the claim that bivalence implies the existence of an in-principle undecidable sentence. What evidence could there be for such a claim?

Mathematical independence results do not help. Mathematical independence results are always relative decidability results. For example, we know that Zermelo-Fraenkel set theory does not decide the axiom of choice, and that ZF set theory plus the axiom of choice does not decide the continuum hypothesis. Thus, the continuum hypothesis is undecidable relative to ZFC. This does not mean that it is in-principle undecidable; perhaps a new set of intuitive axioms for set-theory will be discovered which do decide the continuum hypothesis.12

Appeal to stronger limitation results such as Gödel's theorem are of no help here. Gödel's results entail that for any well behaved formal theory of sufficient strength there will exist a sentence which is undecided by that theory. Does this provide evidence for the claim that there is an in-principle undecidable sentence, that is one that no intuitively correct formal system will decide? No. To infer this from

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12I'm a little uncomfortable with the philosophy of mathematics that seems to be lurking behind many of these examples. Suppose a new set-theory with many of the desirable features that mathematicians value decided the continuum hypothesis negatively. Call this set theory HF. It would be quite possible for mathematicians to utilize and study both ZFC and HF in different contexts. Though they wouldn't assert tout-court that the continuum hypothesis were true or false, they could still use classical principles when utilizing and reasoning about ZFC and HF. This suggests one reply to Dummett's argument that I will briefly discuss. One might reasonably claim that use of classical semantics does not commit one to bivalence.
Gödel-type results is a gross quantifier switch fallacy of exactly the kind students of introductory logic courses are disabused of. That is, inferring that there exists a sentence undecided by any interesting formal system from the claim that every interesting formal system fails to decide a sentence is just as invalid as inferring that there exists a person that loves everyone does not follow from everyone loving someone.\(^{13}\)

§ 3.2.5 Third, and Final, Try

I think there is a more charitable construal of Dummett’s argument, albeit one that has eluded Dummett’s interpreters. However the construal of the argument I shall present is strongly supported by certain passages in Dummett’s corpus. The argument I shall present requires construing Dummettian Realism in the way I did above, so that the disagreement between the Dummettian realist and anti-realist concerns the possibility of an in-principle undecidable sentence. If the anti-realist is denying that such sentences are possible, and this denial is supposed to lead us to reject classical semantics and reasoning, then it is plausible to think of Dummett’s claim as being that somehow classical principles imply the possibility of the existence of such a sentence.

Moreover, a careful study of Dummett’s texts reveals that he does not take the principle of bivalence to imply that it is possible that an in-principle, undecidable sentence exists. He takes classical model-theoretic semantics, when used as part of a semantics for natural language or mathematics, to imply the possibility of such a sentence. Realizing this point is essential to motivating Dummett’s argument, as Tennant’s considerations show that it is entirely unclear how the principle of bivalence itself is supposed to imply anything about the epistemology of linguistic understanding.

\(^{13}\)See *The Taming of the True* pp. 185–194 for a discussion of the quantifier switch fallacy and the irrelevance of Gödel type results in this context.
Thus, I take Dummett's revisionary argument to crucially rest upon the following premiss,

Premiss ii'.
\[ TCS \rightarrow \Diamond_2 \exists X (\neg k_2 X \land \neg k_2 \neg X) \]

where "TCS" means, roughly, "Classical model-theoretic semantics is the correct semantics for the logical operators, and is a part of the correct semantics for natural language and mathematics." Dummett does, in fact, argue for this principle.

Before giving the argument for Premiss ii' we must make precise what Dummett means by $TCS$. Dummett does take $TCS$ to entail that every sentence is either true or not true. As far as I know, not one commentator who pays attention to the properly semantic nature of Dummett's arguments questions this assumption, though it does not follow without further argumentation. Classical model-theoretic semantics defines truth in a model, not truth simpliciter. Thus, classical semantics simply tells us that a sentence is either true or false in any interpretation of the sentence. It is only if one takes truth in an intended interpretation to be a good model of truth simpliciter, that one gets bivalence from classical semantics. One position not explored in the anti-realism dialectic is to utilize classical semantics to justify classical inference but not to utilize truth in an intended interpretation as a notion of truth simpliciter. However, for the present I will not explore this avenue, but will rather assume that $TCS$ includes the assumption that, to borrow a phrase from Stewart Shapiro, truth in a model is a good model of truth.

In any case, one might rightly object to the above possible position that classical semantics does legitimate $(P \lor \neg P)$ for all $P$ in all models, and hence, if we are committed to the inference that $P$ is true if and only if $P$, and if we are committed to the truth predicate distributing over logical constants in reasonable ways, we would get bivalence from classical semantics. However, it is still the case that bivalence on its own would not warrant the inference to the possible existence of an undecidable sentence.
What is constitutive, for Dummett’s purposes, of TCS is that the truth predicate attaches correctly to a sentence in virtue of the referential relations of the subsentential units of the sentence. Thus, Dummett writes,

To have a realistic view, it is not enough to suppose that statements of the given class are determined, by the reality to which they relate, either as true or as false; one has also to have a certain conception of the manner in which they are so determined. This conception consists essentially in the classical two-valued semantics: and this, in turn, embodies an appeal to the notion of reference as indispensable.

(Dummett (1982, p. 231))

On this conception a simple atomic sentence like “Fred is envious” is true if, and only if, the entity referred to by “Fred” is in the extension of the set of entities referred to by “envious.” For our purposes, we do not need to question this picture of the way truth-values of such logically simple sentences are determined.

For Dummett it is the way the truth of logically complex statements are determined in classical model theory that is important. For example, he writes,

The truth value of a quantified statement is, on this conception, determined by the truth-values of its instances, so that the instances stand to the quantified statement just as the constituent subsentences of a complex sentence whose principal operator is a sentential connective stand to the complex sentence: the truth-value of the quantified statement is a truth-function of the truth-values of its instances, albeit an infinitary one if the domain is infinite. The truth-value of a universally quantified statement is the logical product of the truth-values of its instances, that of an existentially quantified statement the logical sum of the truth-values of its instances. These operations, these possibly infinitary truth-functions, are conceived of as being everywhere defined, that is, as having a value in every case: in other words, the application of the operation of universal or of existential quantification to any predicate that is determinately true or false of each object in the domain will always yield a sentence that is itself determinately either true or false, independently of whether we are able to come to know its truth-value or not.

(Dummett (1982, pp. 231-232))

As this passage illustrates, yet another step is required to move from the independent determination of the truth value to the possibility of the existence of an in-principle
undecidable sentence. The set of truths in question must be complex enough to be
undecidable. From the perspective of truth-conditional semantics, this entails at
the very least that some models of the theory in question will be infinite. It then
becomes plausible to think that the uncountable number of classical truth conditions
might make true a sentence which does not admit a finite proof.\footnote{Since any subset of the domain will trivially be the extension of a possible predicate, and the set
of subsets of an infinite set $X$ always has a greater cardinality than $X$, the set of truth-conditions
will be uncountable.}

One of the main problems with interpreting Dummett as talking about mathe-
matics in this context is that, since many take mathematical truths to be necessarily
true, if true, and necessarily false, if false, and also equate the knowability of math-
ematical truths with their provability,\footnote{This position requires that an axiom is known by its self evidence, and hence is its own proof.} it follows that the possible unknowability
of a mathematical truth would amount to its being true while being absolutely un-
provable. But, given the modal and proof-theoretic intuitions about mathematics
that perhaps best characterize the silent majority’s thinking about these things, it
is very difficult to interpret the claim that it is possible there exists an absolutely
unprovable mathematical truth.

Could a mathematical truth be possibly absolutely unprovable without being
actually absolutely unprovable? If there could not be such a thing, then Premiss
ii. simply reduces to the claim that truth conditional semantics entails that there
exists an unprovable mathematical truth. But then we return to the sorts of problems
Tennant claims Dummett faces. Rather than get bogged down in all of the salient
issues in the philosophy of mathematics, we would do better to just focus on empirical
sentences, where Premiss ii. can be easily and sufficiently motivated.

Consider a possible world containing an infinite sequence of red and green objects.
Now consider the sentence, “There are infinitely many red objects.” If classical model
theory gave the correct interpretation of this sentence, then the sentence would be
made true or false in the way Dummett describes above. Yet there would be no way
for the inhabitants of such a world to tell whether or not the sentence were true or

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false, since they couldn’t simply survey the infinite domain. Thus in the described possible world, there does exist an in-principle undecidable sentence.

In the case of decidable, yet contingent, empirical predicates such as “red” it is easy to generate such scenarios given the quantificational apparatus of classical logic, and the attendant model-theoretic interpretation of those quantifiers. I take it that Dummett thinks that the red-green world described above is similar to the structures the classical mathematician trades in. On this view the classical mathematician is committed to the view that it could be the case that the natural number structure is such that for example, every even number is the sum of two primes, while no finite proof of such a claim exists. If the classicist thinks of the number structure as existing in Plato’s heaven then does not seem wholly implausible. However, given the problem of interpreting the modals in such a claim, I will stick to empirical sentences in what follows.

Given Premiss i. (which is equivalent to the claim that necessarily, all true sentences are knowable), Premiss ii.‘ (which asserts that $TCS$ implies the possible existence of an in-principle undecidable sentence), and $TCS$ one can deduce absurdity. As the following Fitch-style deduction shows, this is actually a quite simple argument.

Premisses:

i. $\square_1 \forall X ((TX \rightarrow k_1 X) \land (\neg TX \rightarrow k_1 \neg X))$

ii.‘ $TCS \rightarrow \diamond_2 \exists X (\neg k_2 X \land \neg k_2 \neg X)$

Transformation rules:

K. $\vdash P$, therefore $\vdash \square L P$

Rules of inference:

$\Box$ EQ. $\Box \neg P$, therefore $\neg \diamond x P$

$\Box$ dist. $\Box (P \rightarrow Q)$, therefore $(\Box P \rightarrow \Box Q)$

$\Box >$. $\Box L P$, therefore $\Box \neg P$ (If $P$ is logically necessary, then $P$ is necessary in any other modality)

[Note: From the proof immediately preceding this one we have that verificationism entails that there does not exist an undecidable sentence. Instead of using the pre-
ceding proof as a lemma though, I just present the following as a continuation of it.

Thus, line 1–19 of the proof are the same as the proof on page 141.

Claim: i. and ii.' $\vdash \neg TCS$

Proof:

20. $\Box_L(\forall X((TX \to k_1X) \land (\neg TX \to k_1\neg X))$
   $\to \neg \exists Y(\neg k_1Y \land \neg k_1\neg Y))$

21. $\Box_1(\forall X((TX \to k_1X) \land (\neg TX \to k_1\neg X))$
   $\to \neg \exists Y(\neg k_1Y \land \neg k_1\neg Y))$

22. $\Box_1\forall X((TX \to k_1X) \land (\neg TX \to k_1\neg X))$
   $\to \neg \exists Y(\neg k_1Y \land \neg k_1\neg Y)$

23. $\Box_1\forall X((TX \to k_1X) \land (\neg TX \to k_1\neg X))$
   Premiss i.

24. $\neg \exists Y(\neg k_1Y \land \neg k_1\neg Y)$

25. $\neg \Box_1\exists Y(\neg k_1Y \land \neg k_1\neg Y)$
   $\Box \Box \neg \exists Y(\neg k_1Y \land \neg k_1\neg Y)$

26. $TCS \to \Box_2\exists X(\neg k_2X \land \neg k_2\neg X)$
   Premiss ii.'

27. $\neg TCS$
   For $\neg$ introduction

28. $\neg \Box_2\exists X(\neg k_2X \land \neg k_2\neg X)$
   $\Box \Box \neg \exists X(\neg k_2X \land \neg k_2\neg X)$

29. $\neg \Box_2\exists X(\neg k_2X \land \neg k_2\neg X) \land \neg \Box_1\exists Y(\neg k_1Y \land \neg k_1\neg Y)$
   $\Box \Box \neg \exists Y(\neg k_1Y \land \neg k_1\neg Y)$

30. $\neg TCS$
   $\Box \Box \neg \exists Y(\neg k_1Y \land \neg k_1\neg Y)$

This proof immediately suggests a novel response which the non-revisionist verificationist can make. The final step of the proof is only valid if the modal in the statement of verificationism ($\Box_1$) is in some sense as strong as the modal in the statement of the possible existence of an undecidable sentence ($\Box_2$). Otherwise line 29. wouldn't be a contradiction, and license the conclusion on line 30.

For example, if a possible worlds interpretation of the quantified, modal, epistemic logic could be given, we would say that the set of possible worlds relevant to evaluating the truth of $\Box_2(\exists X)(\neg k_2X \land \neg k_2\neg X)$ must be a subset of the set of possible worlds relevant to evaluating the truth of $\neg \Box_1\exists Y(\neg k_1Y \land \neg k_1\neg Y)$ for the final step of the proof to be correct. By subscripting the modals the way we have, we can then see that the validity of the proof depends upon the possible worlds relevant to the evaluation of Premiss ii.' being relevant also to the evaluation of Premiss i. For the proof to be valid, the modal in Premiss i. must be at least as strong as the modal in Premiss ii.' (in the sense that we would say that logical necessity is
stronger than conceptual or physical necessity). That is we must have $\Box_1 P \vdash \Box_2 P$ and $\Diamond_2 P \vdash \Diamond_1 P$ for all sentences $P$.

Unfortunately it is not immediately clear what is involved in either affirming or denying this claim about the relevant strengths of the modals. Assume that the non-revisionary verificationist realizes that the proof forces her to assert that the modals in Premiss ii.' are strictly stronger than the modals in Premiss i. (that not all possible worlds relevant to evaluating Premiss ii.' are relevant to evaluating Premiss i). This would be analogous to saying that it is physically necessary that all truths are knowable, and that the correctness of truth conditional semantics entails that it is conceptually possible that there exists an in principle unknowable sentence.

Dummett's original arguments for the two premisses are of little help here. The necessity involved in his verificationism in some sense holds in virtue of what he takes to be our best account of the epistemology of linguistic understanding. The possibility adverted to in the possible undecidable sentence implied by the correctness of truth conditional semantics seems to be, in the case of mathematics, probably best taken to be synonymous with "it is plausible to think that;" in the case of empirical truths it holds in virtue of the classical interpretation of the quantifiers. Since the non-revisionary verificationist (who would assent to all of the above premisses while holding that the wide scope modals are incomparable) takes truth conditional semantics to be true in the actual world, she is committed to the possibility of an in-principle undecidable sentence. Yet this sentence, with its in-principle undecidability, must not exist in any world where her verificationism is true.

Thus, for this response to be compelling much more would need to be said, by way of clarifying the modals involved, for the denial of the validity of step 28. of the above proof to be reasonable. For the time being we can assume that no equivocation in the initial modals has taken place, and turn to examine another possible equivocation in the epistemic operators, one that very quickly shows Dummett's argument to be unsound.
§ 3.2.6 Refutation of the Third Try

With the stage setting in the previous sections, it is easy to show that Dummett’s argument succumbs to the same considerations that undermine Wright’s arguments. In presenting Dummett’s arguments, I did not make an issue of the correct interpretation of the knowability operators in either of the premisses; however, once we subject these to scrutiny it becomes clear that it is only by adopting an unreasonable notion of “knowability” in the first premiss that the final contradiction is achieved. Again, Premiss i. stated that

$$\Box_1 \forall X (TX \rightarrow k_1 X) \land (\neg TX \rightarrow k_1 \neg X).$$

It was motivated by the logically equivalent formula,

$$\Box_1 \forall X (TX \rightarrow k_1 X),$$

which is just a formulation of the Verification Constraint, with the necessity box put in front of it. Here I will not gripe about the necessity box, as I argued that it is motivated by Dummett’s taking his verificationism not to be a contingent matter. Rather, if we return to the two Verification Constraints left on the table after our discussion in Chapter II, we get a better picture of how the knowability operator $k_1$ is to be interpreted in Premiss i. These were

Verification Constraint

If $P$ is true, then it is possible that there exists a construction $c$ such that $c$ is a warrant for $P$ that increases the probability that $P$ is true, other things being equal ($TP \rightarrow \exists c V_{weak} c P$),

or, if the modal is collapsed,

Verification Constraint (with collapsed modal)

If $P$ is true, then there exists a construction $c$ such that $c$ is a warrant for $P$ that increases the probability that $P$ is true, other things being equal ($TP \rightarrow \exists c V_{weak} c P$).

Now we are in a better position to evaluate Dummett’s argument. With the above two forms of verifiability, we have the following two possibilities for interpreting the
knowability operator,

Definition of "k_1"
\[ k_1 P = \text{def } \begin{cases} \text{it is possible that there exists a construction c such that c is a warrant for P that increases the probability that } P \text{ is true, other things being equal } (k_1 P = \exists c V_{\text{weak}} c P). \end{cases} \]

Then, line 25. of the above proof \( \neg \Box_1 \exists Y(\neg k_1 Y \land \neg k_1 \neg Y) \) can be given in a slightly less abbreviated manner in one of the following two ways.

Line 25.
\[ \neg \Box_1 \exists Y(\neg \exists c V_{\text{weak}} c Y \land \neg \exists c V_{\text{weak}} c \neg Y) \]

Line 25. (with collapsed modal)
\[ \neg \Box_1 \exists Y(\neg \exists c V_{\text{weak}} c Y \land \neg \exists c V_{\text{weak}} c \neg Y) \]

No problem so far.

However, we now need to examine the knowability operator in Premiss ii.' to see if line 28. in the above proof really does contradict either of the line 25.s here. Again, Premiss ii.' states that the appropriateness of truth conditional semantics when used as part of a semantics for natural language entails the existence an undecidable sentence. Formally we chose to render this statement in the following manner.

\[ TCS \rightarrow \Box_2 \exists X(\neg k_2 X \land \neg k_2 \neg X). \]

Now we ask ourselves, has Dummett or anybody else argued for this principle, when the knowability operator is understood in as weak a manner as it is understood in Premiss i. (or line 25.) For the proof to work line 28. \( \Box_2 \exists X(\neg k_2 X \land \neg k_2 \neg X) \) must contradict line 25. \( \neg \Box_1 \exists X(\neg k_1 X \land \neg k_1 \neg X) \); but this forces us to read Premiss
in one of the following manners.

**Premiss ii.'**
\[ TCS \rightarrow \Diamond_2 \exists X (\neg \exists^0 c V_{weak} c X \land \neg \exists^0 c V_{weak} c \neg X) \]

**Premiss ii.' (with collapsed modals)**
\[ TCS \rightarrow \Diamond_2 \exists X (\neg \exists c V_{weak} c X \land \neg \exists c V_{weak} c \neg X) \]

Expanding these by replacing the \( V \) variables with their definitions given above illustrates just how implausible the premiss has been rendered.

**Premiss ii.'**
\[ TCS \rightarrow \Diamond_2 \exists X ([\text{it is not possible that there exists a construction } c \text{ such that } c \text{ is a warrant for } X \text{ that increases the probability that } X \text{ is true, other things being equal}] \land [\text{it is not possible that there exists a construction } c \text{ such that } c \text{ is a warrant for } \neg X \text{ that increases the probability that } \neg X \text{ is true, other things being equal}]). \]

**Premiss ii.' (with collapsed modals)**
\[ TCS \rightarrow \Diamond_2 \exists X ([\text{there does not exist a construction } c \text{ such that } c \text{ is a warrant for } X \text{ that increases the probability that } X \text{ is true, other things being equal}] \land [\text{there does not exist a construction } c \text{ such that } c \text{ is a warrant for } \neg X \text{ that increases the probability that } \neg X \text{ is true, other things being equal}]). \]

Clearly the version without the collapsed modals is unsustainable. That is, there is no reason why the classicist couldn't affirm \( TCS \) and deny the consequent of the claim. This would involve commitment to the claim that necessarily, for all sentences \( X, X \) is verifiable, or \( X \) is falsifiable, in the extraordinarily weak sense of verifiability explicated here. But this is just the conclusion that Wright's argument was supposed to have us demur at. Likewise for the second version, where the classicist allows the anti-realist to collapse the modal. Once again, denying this claim commits one to the result Wright found inadmissible. However, no argument has been given that such a claim is inadmissible for the classicist. Therefore, the non-revisionary verificationist only needs to defend the consistency and plausibility of the correctness of truth conditional semantics conjoined with the denial that it is possible there exists an empirical sentence which is neither verifiable nor falsifiable in the extraordinarily
weak sense above. But there are no arguments that such a position is inconsistent, and it is not at all clear why such a position is any less plausible than revisionary verificationism.

The defender of Dummett is likely to respond here that the red-green world presented above does seem to show that truth conditional semantics entails the possibility of such a sentence. However, the non-revisionary verificationist does have a principled way to block such sentences. If she could argue that the necessity operator in front of the statement of verificationism didn’t range over such pathological worlds, then the fact that such a world’s existence is entailed by the correctness of truth-conditional semantics would not rationally compel us to abandon truth-conditional semantics. I think she can muster good reasons for this.

If the necessity box in modal, epistemic, quantified propositional logic used in the above proofs is interpreted as a universal quantifier over possible worlds, then we are really dealing with two domains of quantification for any given sentence. For example, in Premiss i., $\Box_1 \forall X((TX \rightarrow k_1 X) \land (\neg TX \rightarrow k_1 \neg X))$, the universal quantifier ranges over the set of sentences understandable to a competent language user, and the necessity operator ranges over the set of worlds which include competent language users. Thus, for our red-green world to count as a counterexample to Premiss i., it would have to have competent language users in it, and at that world $\forall X((TX \rightarrow k_1 X) \land (\neg TX \rightarrow k_1 \neg X))$ would have to be false. However, if the classicist refuses to collapse the modal in the Verification Constraint, she can interpret “$k_1 P$” in terms of the possible existence of a warrant which increases the probability of $P$. But then, it is not unreasonable to argue that “there are an infinite number of red things” is verifiable to denizens of the red-green world. For all that is required is that if their technologies and capacities were finitely extended they could tell whether or not the claim was probable.

Since, by assumption, the denizens are competent language users they can imagine (or could with finitely extended capacities) all sorts of ways in which the claim
can be made more probable. They can imagine their best physics telling them that
the universe would collapse on itself if there were not an infinite number of red
things. If their best physics told them this, then the truth of the claim would be
made much more probable. In this sense, the description of the red-green world does
not violate the kind of verificationism one gets if one does not collapse the modal.
If the denizens’ capacities were extended then they could recognize that the claim
was probable.

The Dummettian might respond by offering a positive argument for collapsing
the modal. Then the classicist would be committed to the existence of a warrant
for or against the claim in the red-green world. That is, without the collapse, in
possible worlds terminology, there need only be a possible world accessible from
the red-green world where the denizens’ physics renders the claim more probable.
That the red-green denizens, with extended capacities, can cognize such a world
shows that they grasp what it would be for the sentence to be true, in terms of its
verification conditions. However, no direct argument has been given by Dummett,
Wright, or Tennant for collapsing the modal, when the Verification Constraint is
applied to empirical sentences.

Moreover, even if the modal is collapsed, no reason has been given which would
prohibit the classicist from further restricting the necessity box in front of the state-
ment of verificationism so that only non-pathological worlds are considered. As long
as the actual world is suitably non-pathological, there is no reason such a restriction
would be ill-motivated. Is the actual world pathological in this sense? Can the
classicist say that for all claims $P$ a warrant exists which increases the probability
of $P$, or a warrant exists which decreases the probability of $P$? If the intuitionist
has a problem with this, then she needs to say why this is problematic, and why
there is no problem with the intuitionistically weaker claim (which is intuitionisti-
cally entailed by the Verification Constraint), “There does not exist a claim $P$ such
that neither does there exist a warrant which increases the probability of $P$, nor
does there exist a warrant which increases the probability of \( \neg P \)."\(^{16}\) It is not at all clear why one should find the intuitionistically weaker claim O.K. and the stronger claim bad. Other than Wright's argument, which assumed that the warrants were sufficient for truth, nobody has presented an argument which shows the intuitionist is on any safer ground than the classicist here.

\section*{§ 3.3 Taking Stock}

Before explicating Tennant's arguments, it behooves us to step back and take stock of the substantive results of the above sections and to make some generalizations based on them. First, we need to reexamine the logical relations made explicit in our discussion. First, Wright's two results can be represented in this manner.

\begin{quote}
Wright's first result
The Verification Constraint intuitionistically entails Negation Equivalence, or
\[ \forall X(TX \to VX) \vdash_I \forall X(\neg VX \to V \neg X). \]

Wright's second result
The Verification Constraint classically entails Gödelian Optimism, or
\[ \forall X(TX \to VX) \vdash_C \forall X(VX \lor V \neg X). \]
\end{quote}

Then, along the way to presenting Dummett's argument we managed to prove the following.

\begin{quote}
Result proved along the way
The Verification Constraint intuitionistically proves that there are no undecidable sentences (as long as the two senses of knowability are comparable in the manner given by the first premiss), or
\[ (\forall X)(k_2 X \to k_1 X), (\forall X)(TX \to k_1 X) \vdash_I \neg(\exists Y)(\neg k_2 Y \land \neg k_2 \neg Y). \]
\end{quote}

I have restated the above result to illustrate the kind of commensurability of knowability operators required but not achieved by Dummett, and in the following repre-

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\(^{16}\)One might respond (as Tennant has (Tennant, p.c, 1997)) by arguing that the reason there doesn't exist such a claim is that for intuitionists the nonexistence of a warrant for a claim ipso facto is a warrant for its negation. Thus, the lack of warrant for a claim and its negation ends up entailing the existence of warrants for both the negation and double negation of that claim. In Chapter II I argued that defeasible warrants are exactly like this.
sentation of Dummett's argument again state the kind of commensurability required as a specific premiss. Thus, at the end of our exegetical investigations, we saw that Dummett's argument could be represented by the following.

Dummett's argument
The Verification Constraint, and Dummett's claims about classical truth conditional semantics, intuitionistically prove that classical truth conditional semantics is incorrect (as long as the modals and sense of knowability are comparable in the manner given by the first premiss), or
\[
\neg (\Box_2 \exists X (\neg k_2 X \land \neg k_2 \neg X) \land \neg \Box_1 \exists Y (\neg k_1 Y \land \neg k_1 \neg Y))
\]
\[
\Box_1 \forall X (TX \to k_1 X)
\]
\[
TCS \to \Box_2 (\forall X)(\neg k_2 X \land \neg k_2 \neg X)
\]
\[
\neg TCS.
\]

In each case we saw that, where the relevant sense of verifiability, or knowability, was weakened enough to yield a reasonable formulation of the Verification Constraint (in light of the discussion in Chapter II), the revisionary arguments failed. For Wright's two arguments, and the result proved along the way, we saw that none of the conclusions were troubling when the predicate "V" ("k_1" in the case of the result proved along the way) was interpreted as weak verifiability in Ayer's sense.

For Dummett's argument we saw that once the verifiability predicate ("k_1") occurring in the Verification Constraint was interpreted in the manner suggested by Chapter II, it was too weak to contradict the claim that it is possible that there exists an unverifiable sentence, in the sense which Dummett argued follows from the correctness of truth conditional semantics. This rendered the suppressed premiss about the comparability of the modals involved false, and Dummett's argument unsound.

It is useful to represent Dummett's argument in slightly less formal terms so that we can be very sure that we have drawn the correct conclusions. We can proceed even more informally than above, while still subscripting the relevant senses of verifiability to show the crucial equivocation that renders Dummett's argument unsound. Thus, Dummett's main revisionary argument can be represented by the following.
1. Necessarily, if a sentence is true, then it is verifiable.

2. If classical truth conditional semantics is correct, then it is possible that there exists a sentence which is neither verifiable, nor falsifiable.

3. Therefore, classical truth conditional semantics is incorrect.

A very strange thing has happened. To the extent that either of these premisses is plausible, given an interpretation of the notion of verifiability, the other one is not.

Returning to the jargon of Ayer, consider the following two disambiguations of the notions of verifiability as it occurs in the premises of Dummett’s argument.

1. Necessarily, if a sentence is true, then it is verifiable,

2. If classical truth conditional semantics is correct, then it is possible that there exists a sentence which is neither verifiable, nor falsifiable.

3. Therefore, classical truth conditional semantics is incorrect.

This argument, given the formal explication of it above, would be valid, but unsound, because the second premiss would be false. Likewise, the following version would be valid and unsound.

1. Necessarily, if a sentence is true, then it is verifiable,

2. If classical truth conditional semantics is correct, then it is possible that there exists a sentence which is neither verifiable, nor falsifiable.

3. Therefore, classical truth conditional semantics is incorrect.

Here the argument is unsound because the first premiss is false. In so far as anyone has provided arguments for the premisses, the correct version of the argument would be this.

1. Necessarily, if a sentence is true, then it is verifiable,

2. If classical truth conditional semantics is correct, then it is possible that there exists a sentence which is neither verifiable, nor falsifiable.

3. Therefore, classical truth conditional semantics is incorrect.

However, this argument is invalid, because it involves crucial equivocation on the senses of verifiability occurring in the premisses.
Thus, I conclude that none of the arguments for logical revision considered thus far, when our attention is restricted to empirical claims with defeasible warrants, are sound. However, this is not the point where the realist should pack her bags and declare victory. We have still not addressed the possibility of constructing a non-verificationist, Dummettian theory of sense isomorphic to a truth conditional semantics for natural language. Nor have we evaluated Dummett’s arguments to the conclusion that such a theory is both possible and desirable. This task we undertake in the next chapter. Before undertaking this challenge, consideration of Neil Tennant’s recent discussion of revisionary arguments is well motivated, since discerning a principled answer to Tennant motivates criticisms of Dummett’s projected theory of sense, as well as a principled defense of what might be called a roughly Davidsonian approach to some of these issues.

§ 3.4 Tennant’s Discourse Argument & Challenge

After criticizing what he takes to be Dummett’s revisionary arguments in the previous chapter, in Chapter 7 of The Taming of the True, Neil Tennant presents a new revisionary argument. Tennant’s argument is one which, by his own admission, suffers drawbacks very similar to the ones I have shown Dummett’s argument to suffer. However, in spite of the drawbacks, Tennant is able to show that his argument winnows down the dialectical space, so that the only positions left on the table are what he calls Modest Anti-Realism, M-Realism, Gödelian Optimism, and Orthodox Realism. Then the rest of the chapter attempts to show that Tennant’s Modest Anti-Realism is the most plausible of the surviving contenders. Here I will discuss both Tennant’s revisionary argument, as well as the considerations he takes to motivate Modest Anti-Realism. From his discussion I shall be able to extricate a fairly compelling criticism of the non-revisionary verificationism I have been defending as coherent.
§ 3.4.1 Tennant’s Discourse Argument

In this section I will explicate Tennant’s new revisionary argument, showing how it fails, by Tennant’s admission, on its own, to provide a knock-down argument against either the correctness of using classical semantics while modeling natural language, or the correctness of classically valid, but intuitionistically invalid, modes of reasoning.

First we must attend to a matter of terminology, as Tennant’s terminology is at a slight variance from that used above. Instead of a Recognition Thesis, he defends what he calls a Manifestation Requirement, which is easy to show equivalent to the Recognition Thesis. The general form of the Manifestation Requirement is given in the following manner.

(MR) Manifestation Requirement
Any speaker should be able fully to manifest his grasp of the meaning of any sentence $\Phi$ that he understands, by a suitable exercise of salient recognitional capacities in connection with $\Phi$.

(Tennant (1997, p. 228))

In the context of his discussion, Tennant discusses two ways to explicate the “salient recognitional capacities” adverted to in the Manifestation Requirement. These are

(saM) Hawkish (or strong) Manifestationism
A speaker $X$ understands a sentence $\Phi$ if, and only if, $X$ can discover whether or not $\Phi$ is true.

(wpM) Conservative (or weak) Manifestationism
A speaker $X$ understands a sentence $\Phi$ if, and only if, were $X$ presented with a proffered verifier $c$ for $\Phi$, $X$ would be able, in principle, to recognize whether $c$ verifies $\Phi$.

Tennant does not distinguish between Ayer’s strong and weak verificationism at this point in the text. However, it is clear that he is talking about proofs which provide indefeasible evidence for claims. Moreover, it is clear that Tennant’s Conservative Manifestationism is equivalent to what I have called the strong version of the Recog-
nition Thesis, given in this manner.

Recognition Thesis (strong, de re)

\(X\) understands \(P\) if, and only if, were \(X\)'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were \(X\) then presented with a construction \(c\) (such that \(\forall Q(c \text{ verifies } Q \rightarrow V_{\text{strong}}cQ)\)), i.e. for all \(Q\), if \(c\) verifies \(Q\), then \(c\) makes the probability of \(Q\) equal to one), then \(X\) could recognize whether \(c\) verifies \(P\) and whether \(c\) verifies \(\neg P\).

As I argued in Chapter II, when concerning ourselves with empirical claims with defeasible warrants, the strongest version of the Recognition Thesis defensible is the following.

Recognition Thesis (weak, de re)

\(X\) understands \(P\) if, and only if, were \(X\)'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were \(X\) then presented with a construction \(c\) (such that \(\forall Q(c \text{ verifies } Q \rightarrow V_{\text{weak}}cQ)\), i.e. for all \(Q\), if \(c\) verifies \(Q\), then \(c\) increases the probability of \(Q\), other things being equal), then \(X\) could recognize whether \(c\) verifies \(P\) and whether \(c\) verifies \(\neg P\).

The primary point of this section is not to show that Tennant's argument is invalidated because of its commitment to unreasonably strong versions of verificationism (though this will be clear enough from my discussion). Rather, I present the differences because Tennant presents a challenge (discussed in the next section) which properly grows out of his revisionary argument, and (as we shall see) it is easier to explicate Tennant's challenge by being clear about the differences between the notion of verifiability he uses and the one I have defended as being more reasonable.

Tennant is also committed to a principle he called the "Knowability Thesis" which he gives in this manner,

\((KT)\) \(\forall x\forall \Phi (Ux\Phi \rightarrow (\Phi \text{ is true } \rightarrow \text{it is possible for someone to know that } \Phi \text{ is true}) \wedge (\neg (\Phi \text{ is true}) \rightarrow \text{it is possible for someone to know that } \neg (\Phi \text{ is true})))\) (Tennant, (1997, p. 204)).

However, given that, for any sentence \(P\), it is not the case that \(P\) is true if, and only
if, it is true that it is not the case that $P$, and that it is reasonable to hold that if a person understands a sentence then they understand its negation, $KT$ is equivalent to the simpler.

$$(KT') \forall x \forall \Phi (Ux\Phi \rightarrow (\Phi \text{ is true} \rightarrow \text{it is possible for someone to know that } \Phi \text{ is true}).$$

Moreover, on the assumption that all sentences in the domain of quantification are understandable this is equivalent to the even simpler,

$$(KT'') \forall \forall (\Phi \text{ is true} \rightarrow \text{it is possible for someone to know that } \Phi).$$

From Tennant's broader discussion, it is clear that he intends this to be equivalent to the stronger forms of Verificationism which Ayer argued are unsustainable for empirical discourses with defeasible warrants. That is, where constructions are understood as being humanly recognizable, $KT''$ is equivalent to the following.

Verification Constraint (strong)

If $P$ is true, then it is possible that there exists a construction $c$ such that $c$ is a warrant for $P$ that makes the probability of $P$ equal to 1 ($TP \rightarrow \exists^0 c V_{\text{strong}cP}$).

Again, as I argued in the last chapter, when restricted to empirical claims with defeasible warrants, Dummett's dialectic at best provides evidence for this weaker form of verificationism.

Verification Constraint (weak)

If $P$ is true, then it is possible that there exists a construction $c$ such that $c$ is a warrant for $P$ that increases the probability of $P$, other things being equal ($TP \rightarrow \exists^0 c V_{\text{weak}cP}$).

Thus, we have interpretations of the substantive claims at work in Tennant's revisionary arguments in the terms appropriate to our discussion of Dummett's dialectic.

With one more distinction on the table we shall be able to proceed to present Tennant's revisionary argument with minimal confusion. Where "D" refers to a

\[17\]From Tennant's discussion it is clear that he intends the modal to collapse in the manner discussed in Chapter II. Thus, for Tennant, the possible constructions are just understood as warrants that objectively exist in this world, but have yet to be discovered. Since nothing depends on this for the purpose of our current discussion. I charitably refuse to collapse the modal here.
discourse (for our purposes we can ignore difficult questions about discourse individuation in our discussion), and to say that a sentence is "constructively true" is to commit oneself to "the constructive existence of an effectively checkable truth maker" for the sentence, Tennant distinguishes between the following two statements of bivalence.

\[(cBivD)\]
\[\forall \Phi (\Phi \text{ is in } D \rightarrow (\Phi \text{ is [constructively] true} \quad \text{or} \quad \neg(\Phi \text{ is [constructively] true}))\],

\[(nBivD)\]
\[\forall \Phi (\Phi \text{ is in } D \rightarrow (\Phi \text{ is [possibly non-constructively] true} \quad \text{or} \quad \neg(\Phi \text{ is [possibly non-constructively] true}))\].

Now we have enough on the table to explicate Tennant’s arguments.

Tennant’s substantive claim is that the person who equates truth with "the constructive existence of an effectively checkable truth-maker," cannot subscribe both to bivalence and to the weak Manifestation Requirement. In effect he will argue that commitment to the two entails a violation of Church’s thesis (which states that a set is decidable if and only if it is recursive). Tennant argues that if constructive bivalence and weak manifestationism hold over any arbitrary discourse recursive or not, then it follows that that discourse is decidable. But by Church’s Thesis we

\[18\text{William Taschek (Taschek (p.c, 1996)) has stressed how difficult it is to arrive at informative identity conditions for discourses, as well as how important the issue is for many schools of philosophy, including those under discussion.}\]

\[19\text{Tennant does not countenance the possibility of somebody defending constructive bivalence for a discourse, while still holding that some sentence can fail to be constructively true and still be classically true. From the argument he presents it is clear that Tennant intends the defender of (cBivD) to be committed to the inference that } P \text{ fails to be constructively true if, and only if, } \neg P \text{ is constructively true. While this might be a strategy for criticizing Tennant’s argument, I will not pursue it here.}\]

\[20\text{It might have been better to replace "} \Phi \text{ is [possibly non-constructively] true} \text{" with "} \exists \Pi (\Pi \text{ makes } \Phi \text{ true}) \text{, where } \exists \text{ is understood non-constructively. Then a possibly important ambiguity, not raised by Tennant, is revealed. If may then be understood either as a constructive truth maker or as non-constructive truth maker, independently of the interpretation of the existential quantifier. The same ambiguity exists for } nBivD.\]
know that some sets of true sentences are not decidable. Thus, given the distinctions made above, his argument reduces to the following claim, where the premisses are understood to be such that constructive bivalence and weak manifestationism is stated to hold for all discourses $D$, and hence for all undecidable discourses.\footnote{Tennant’s argument interestingly suggests a hybrid position which involves affirming Bivalence for all decidable discourses and rejecting it for undecidable discourses. This might have the strange result of forcing the intuitionist to accept classical propositional logic.}

$$(cBiv_D), (wpM) \vdash \bot$$

Since the structure of his argument requires us to restrict the “discourse” in question to theories with sets of truths which are provably non-recursive, I will let $D$ be equal to first order elementary number theory (the theory of first order sentences formulable with plus and times), which is provably non-recursive (and thus by Church’s Thesis undecidable) when closed under either classical or intuitionistic entailment.

Before considering the mechanics of Tennant’s argument, we do well do consider a simplified, schematic version of it here (on the assumption that the discourse in question is a provably non-recursive one).

1. Constructive bivalence and weak manifestationism (restricted to a discourse $D$) entail that there exists a correct decision procedure for $D$.
2. By Church’s Thesis, there exists a correct decision procedure for $D$ if, and only if, the set of truths in $D$ are recursive.
3. By assumption, $D$ is equal to first order elementary number theory.
4. Then, by 2. and 3., the set of truths of first order number theory is recursive.
5. But the set of truths of first order number theory is provably not recursive.
6. Lines 4. and 5. contradict one another.
7. Therefore, by lines 1 and, 6 constructive bivalence and weak manifestationism cannot both be correct for first order number theory.
8. Hence if we maintain weak manifestationism then constructive bivalence cannot hold unrestrictedly for for all discourses.

The only clearly contentious claim in this argument is the first one. Just from inspection it is not at all clear how commitment to both constructive bivalence and
weak manifestationism for a set of sentences entails that there is a correct decision procedure for that set of sentences. Tennant does, of course, provide an argument for this claim, though not one without problems.

He does this by using constructive bivalence and weak manifestation to describe a decision procedure $\mu$, which applies to sentences of a discourse $D$, such that for all $\Phi$ in $D$ we have

1. $\mu$ is total;
2. $\mu$ is effective; and
3. $(\mu(\Phi) = T \rightarrow \Phi$ is true) and $(\mu(\Phi) = F \rightarrow \neg(\Phi$ is true)).

To defend these claims, Tennant first gives a general method and then argues specifically that the general method satisfies conditions one through three. He writes,

Given any sentence $\Phi$ in $D$, find a speaker who understands it. *Ex hypothesi* we can do this. Set the speaker the recognitional task, with our presentational help, of telling whether $\Phi$ is true. (This is the first step of the tandem method.) If the speaker says that $\Phi$ is true, record him as delivering the verdict $T$. If he says that it is not the case that $\Phi$ is true, record him as delivering the verdict $F$. Take the speaker's verdict as the output of $\mu$ on $\Phi$. (This is the second step of the tandem method) (Tennant, (1997, p. 207))

From this it is not at all clear that the method in question is either total or effective. Weak manifestationism just says that if people are presented with proofs or disproofs for claims, then they will be able to correctly recognize these as proofs or disproofs for claims. For the method Tennant describes to be total and effective on $D$, it has to be the case that “our presentational” help is such that we (the presenters) have a total and effective method by which we can find for any sentence $\Phi$ a proof or disproof to present to any speaker who understands $\Phi$.

Tennant in fact attempts to argue that constructive bivalence itself entails that we would have such method, when he provides a justification for the claim that $\mu$ is
total. He writes,

Ad (1): By \((cBi\nu_D)\),

\[ \Phi \text{ is true or } \neg(\Phi \text{ is true}) \]

Assume first that \(\Phi\) is true. Thus there is (constructively) some truth-maker \(\Pi\) for \(\Phi\). Find it, and present it to the speaker. By \((wpM)\), the speaker is able to recognize \(\Pi\) as showing that the truth-condition for \(\Phi\) obtains, or at least is able to get himself into a position where he can so recognize. That is, the speaker will be able to return the verdict \(T\) on \(\Phi\). Therefore

\[ \Phi \text{ is true } \rightarrow \mu(\Phi) = T \]

Now assume that it is not the case that \(\Phi\) is true. Thus there is (constructively) some falsity-maker \(\Sigma\) for \(\Phi\). Find it, and present it to the speaker. By \((wpM)\) again, the speaker is able to recognize \(\Sigma\) as showing that the truth-condition for \(\Phi\) does not obtain, or at least is able to get himself into a position where he can so recognize. That is, the speaker will be able to return the verdict \(F\) on \(\Phi\). Therefore

\[ \neg(\Phi \text{ is true}) \rightarrow \mu(\Phi) = F \]

It now follows by \((cBi\nu_D)\) that \(\mu\) as defined is total.

\text{via Tennant, (1997, pp. 205-206)}

This passage shows that Tennant requires constructive truth (and falsity) to be such that, if a constructive truth (or falsity) maker for a claim exists, then we can find it. This is because Tennant is reading the existential in the claim that “there exists a constructive truth (or falsity) maker” in a constructive manner.

Consider the standard Heyting Semantics clause for the existential, (which identifies the truth of a claim with the existence of a construction verifying it),

If \(\Phi\) is an existential, \(\Phi = (\exists x \Phi_1(x))\), then \(k\) verifies \(\Phi\) precisely when \(k\) determines for which object \(a\ \Phi_1(a)\) holds and yields a construction \(k_1\) verifying \(\Phi_1(a)\).

So if Tennant interprets the existential in the claim “There exists an \(x\) such that \(x\)
is a truth maker for \( \Phi \)" constructively, he is interpreting the claim in this manner,

there is some \( k \) which determines for which truth maker \( a \), "\( a \) is a truth maker for \( \Phi \)" holds and yields a construction \( k_1 \) verifying "\( a \) is a truth maker for \( \Phi \)."

A reasonable interpretation of this does provide evidence for the inference Tennant makes from the claim that \( \Phi \) is constructively true, to the conclusion that the presenter can find the truth-maker for \( \Phi \).

However, once "constructive truth" is understood in a manner which licenses Tennant's inference, "constructive bivalence" itself is inconsistent with Church's thesis and the existence of provably non-recursive theories. Tennant assumes that a claim is constructively true (false) if, and only if, there is some construction verifying (falsifying) the claim which can be grabbed out of the hat in the manner of the presenter in his argument. But then the presenter herself is, via her ability to grab the truth-makers and falsity-makers, (with bivalence) able to decide any sentence of the theory in question. It is clear from the above quote that Tennant's argument requires the presenter herself to instantiate an effective, total, and correct decision procedure on the discourse in question. But then weak manifestationism contributes nothing to the argument. Rather, Tennant's argument at best shows that constructive bivalence is inconsistent with Church's thesis and the existence of provably non-recursive theories.

Given the larger dialectical role Tennant's intended argument plays, it is not clear to me how significant my criticism is. In Tennant's discussion he seeks to establish

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22 One could invalidate the argument by somehow attempting to read the existential in "there exists a constructive truth maker for \( P \)" non-constructively, but as still ranging over constructive truth-makers. I don't have any intuitions about how plausible or interesting such a position would be, so I'm sympathetic to the inference Tennant makes.

23 I will not discuss Tennant's argument for the claim that \( \mu \) is effective, other than to point out here that he admits one might not view the kind of decision procedure envisioned by the argument as one which violates Church's Thesis, and cites (Shapiro, 1994) as a source relevant to disambiguating "psychological computability" from the relevant sense of "effective computability" Church's Thesis is concerned with. If the two claims can be pulled apart in this manner, then one could argue that Tennant has just shown that constructive bivalence and weak manifestationism entail psychological computability, and hence don't violate Church's Thesis.
the following claims.

\[(cBiv_D), (wpM) \vdash (Dec_D)\]
\[(nBiv_D), (saM) \vdash (Dec_D)\]

That is, Tennant attempts to establish both that constructive bivalence and weak manifestationism over a discourse \(D\) entail that \(D\) is decidable, as well as that non-constructive bivalence and strong manifestationism entail the decidability of the discourse in question. I have decided not to present Tennant’s argument for the second result because Tennant himself argues that strong manifestationism is implausible.

Tennant’s results are an interesting parallel with my own results (discovered independently) about Dummett’s proofs. I argued that Dummett’s proofs are valid only if one accepts too strong a form of verificationism in the Verification Constraint, or if one accepts that the correctness of truth conditional semantics entails that a sentence is undecidable in an unacceptably strong manner. Independently of the problem I have isolated with Tennant’s first argument, the anti-anti-realist is likely to view Tennant’s arguments in a similar manner. They are only valid to the extent that one either accepts an unreasonable form of manifestationism, or asserts an unreasonable form of bivalence.

Thus, since the first argument is the dialectically efficacious one for Tennant, and since he defends weak Manifestationism, its role is to isolate a possible position Tennant must argue against. Tennant himself claims that

\[\neg((nBiv_D), (wpM) \vdash (Dec_D))\]

That is, weak manifestationism and a statement of bivalence with a “neutral” embedded notion of truth does not entail that the discourse under consideration is decidable. Thus, Tennant’s task in the latter part of Chapter 7 of The Taming of the True is to attempt to undermine the combination of weak manifestationism with non-constructive bivalence. Crucially, Tennant’s revisionary arguments work in the chapter to narrow down the options, so that he can go on to criticize the combination just mentioned on independent grounds. Once he has used his discourse arguments
to discard the possibilities of being committed to weak manifestationism and con­structive bivalence or to strong manifestationism and non-constructive bivalence, as well as presented independent evidence against strong manifestationism, it follows that the classicist who wants to commit to bivalence is forced both to commit to either weak manifestationism or something more liberal, and to avoid commitment to a constructive account of truth.

Given the parallels between Tennant’s discussion and my discussion of Dummett and Wright, we shall see in the next section that Tennant’s attempt to undermine positions which combine manifestationism with classical logic yields a criticism of the position which I have argued is consistent, namely the position derived by combining a very weak verification constraint and commitment to classical truth conditional semantics. However, before exploring this issue, we must examine the extent to which my criticism of Tennant’s proof undermines his attempt to narrow down the options.

In fact, my criticism does not undermine the use to which Tennant puts his dis­course argument. Since the ultimate conclusion is that the classicist cannot commit to bivalence along with a constructive account of truth and a reasonable form of the manifestation requirement, Tennant’s hand is only strengthened by an argument which shows that bivalence and a constructive account of truth are themselves jointly unsustainable. Thus, in spite of my criticism, Tennant is at least provisionally successful (despite the forebodings expressed in the footnotes) at narrowing down the classicist’s options.

§ 3.4.2 Tennant’s Challenge

Tennant goes on to consider four views: Moderate Anti-Realism, Orthodox Real­ism, M-Realism, and Gődelian Optimism. He holds that all four of these are left on the table after the proper morals are drawn from his revisionary arguments. Thus, he need only show that Moderate Anti-Realism is the most plausible of the remain-
ing positions. It is from this attempt that we shall be able to extract a challenge to the non-revisionary verificationist.

The first is the position which Tennant himself would like to defend.

**Moderate Anti-Realism**
1. Bivalence is either false or not asserted,
2. All truths are knowable, and
3. Truth is constructive (i.e. cannot transcend proof).

The second position is the one most similar to Dummett’s characterizations of the Realist.

**Orthodox Realism**
1. Bivalence is true,
2. It is not the case that all truths are knowable, and
3. Truth is non-constructive (i.e. can transcend proof).

The third is a position presented and defended as coherent by John McDowell in “Truth Conditions, Bivalence and Verificationism.”

**M-Realism**
1. Bivalence is either false or not asserted,
2. It is not the case that all truths are knowable, and
3. Truth is non-constructive (i.e. can transcend proof).

The final position is one Stewart Shapiro credits Kurt Gödel with.

**Gödelian Optimism**
1. Bivalence is true,
2. It is the case that all truths are knowable, and
3. Truth is non-constructive (i.e. can transcend proof).

Since Tennant wants to defend Moderate Anti-Realism, it is incumbent upon him to criticize the other three options. I will not explicate all of his discussion in this regard, as much of it is properly orthogonal to our dialectical context.

Tennant essentially argues that, while M-Realism may be consistent in the manner presented above, it is wholly unmotivated. If the M-Realist adopted a con-

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24See (McDowell, (1976)).
A constructive account of truth, then they could argue that their position involved some form of "epistemic gain" to motivate it; however, denial of the knowability principle precludes adoption of a constructive account of truth. Thus, there is no very good reason to be an M-Realist. All the M-Realist achieves is a defense of the claim that one can eschew use of strictly classical principles. This is neither surprising nor interesting. One can refrain from using modus ponens if one wants to badly enough. Things would only be interesting if one had a proof that people should so refrain. What one wants from the M-Realist is an argument that one should eschew use of strictly classical principles, and the M-Realist's own position prevents them from discerning such an argument. Moreover, as good Aristotelians, we should treat the fact that most experts use classical logic as some (albeit defeasible) evidence that classical logic is correct. So a position such as MacDowell's which is committed to intuitionist logic but that precludes arguments for the correctness of intuitionist logic, should certainly be rejected.\textsuperscript{25}

As far as I can tell, Tennant takes Orthodox Realism to be confuted by the Manifestation Requirement. That is, his Manifestation Requirement provides evidence for the Knowability Thesis, which is inconsistent with Orthodox Realism. In our terminology, the claim is that a strong Recognition Thesis provides evidence for a strong Verification Constraint, which is inconsistent with Orthodox Realism. Thus, if we are committed to a strong Recognition Thesis, then we should eschew Orthodox Realism. Since Tennant is committed to a strong Recognition Thesis (shown to be equivalent to weak manifestationism above) he does eschew it.

Tennant has two criticisms of Gödelian Optimism. The first is that it is not sufficiently motivated. What possible argument could somebody give for the claim

\textsuperscript{25}Admittedly this is not very nuanced. If intuitionist logic is fruitful or even just intellectually stimulating in its applications, then it should pursued, but certainly not to the exclusion of using other logics for applications suited to them. The broader notion of "the correct logic" doesn't really make sense in this regard. I have focused on applications involving natural language semantics, and argued that no one has provided a compelling argument to abandon classical model theory in favor of some rigorization of Heyting Semantics for intuitionistic logic.
that every sentence or its negation is knowable (a claim entailed by Gödelian Optimism)? This is very similar to Wright's animadversions about the claim that "either evidence is available for $P$ or evidence is available for $\neg P$." With Wright, Tennant is here arguing that the burden of proof lies with the person making such a strong claim.\(^{25}\)

Tennant's second argument against Gödelian Optimism is frustratingly brief, given that properly understood, it illustrates the substantive issue which is the proper locus of disagreement between the anti-realist and the anti-anti-realist. When considering whether or not the Gödelian Optimist can subscribe to weak manifestationism, Tennant writes

> The trouble now, however, is that it is entirely unclear how one might adjudicate the question of whether a given speaker meets the demand posed by this more God-fearing version of the manifestation requirement. The worry stems from the fact that such future, classical truth-maker as $\Phi$ might enjoy the fulfilment of the Gödelian Optimism here indulged could well involve new methods and rules of inference governing the constituent expressions in $\Phi$. These new methods and rules, in making good the known logical shortcomings of any present formal system, cannot be guaranteed not to have changed the meaning of the sentence $\Phi$. And we cannot say of our speaker at present that he would necessarily be able to grasp that future, changed meaning of $\Phi$, even were he adequately to grasp its present meaning. He just might not be inclined to recognize those new methods and rules as licit. And indeed why should he? Indeed how can the Gödelian Optimist make so bold as to suggest not only that every one of the alleged truths to be found in any given pair $\Phi$, $\neg \Phi$ will in principle admit of finitary (albeit possibly classical) proof, but also that in doing so the new methods of proof would be justified by appeal only to the present meanings of $\Phi$ and its constituent expressions?\(^{26}\) (Tennant, (1997, p. 201))

Prior to saying what I think is right about this criticism, and why it does isolate the proper locus of disagreement between the anti-realist and anti-anti-realist, I must

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\(^{25}\) In (Shapiro, 1993) the author countenances bivalence working more as a sort of Kantian (Veihingerian?) regulative principle for the optimist. In this manner the optimist might not, upon reflection, seriously be asserting that every sentence or its negation is knowable, but rather be prepared to act as if every sentence or its negation is knowable.
address serious reservations I have about how the criticism is stated. Tennant's argument, in brief, can be given in this manner.

1. By weak manifestationism, for all sentences $\Phi$, a competent speaker can (if suitably idealized) recognize of any construction $c$ whether or not $c$ is a proof of $\Phi$.

2. By Gödelian Optimism, for all $\Phi$, $\Phi$ is knowable or $\neg\Phi$ is knowable.

3. In mathematics this entails that either there exists a construction $c$ which is a proof of $\Phi$ or there exists a construction $d$ which is a proof of $\neg\Phi$.

4. Therefore (in mathematics), for all sentences $\Phi$ and competent speakers $S$, there exists a construction $c$ which is a proof for $\Phi$ such that $S$ can recognize that $c$ is a proof of $\Phi$ or there exists a construction $d$ which is a proof of $\neg\Phi$ such that $S$ can recognize that $c$ is a proof of $\neg\Phi$.

5. But we have no guarantee that the construction $c$ or $d$ won't involve resources irrelevant to the meaning of $\Phi$.

6. If $c$ or $d$ involve resources irrelevant to the meaning of $\Phi$ then they would be irrelevant to $S$'s grasp of the meaning of $\Phi$, and hence the manifestation requirement would be too strong, requiring speakers to grasp resources irrelevant to the meaning of $\Phi$ in order to understand $\Phi$.

Thus, the first worry is that the resources involved in a future proof of some presently undecided sentence might involve resources not strictly relevant to the present meaning of the sentence. The second worry from the above quote is an extension of this one, concerning whether these resources might so far extend the present meaning of the sentence that we would be rationally constrained to say the meaning of the sentence has changed. Thus, the argument continues in this manner.

7. Furthermore, we have no guarantee that such resources won't change the meaning of $\Phi$, and again make the manifestation requirement too strong, requiring speakers to grasp resources irrelevant to the present meaning of $\Phi$ in order to understand $\Phi$ with its present meaning.

8. Therefore, Gödelian Optimism, renders weak manifestationism implausible.

My first problem with this argument is similar to my problem with Tennant's revisionary argument. It seems that Gödelian Optimism really isn't doing any work, but rather that Tennant has provided a strong argument against weak manifestationism.
In mathematics proper interesting proofs sometimes do involve what can fairly be described as “resources irrelevant to the meaning” of the theorem proved.\textsuperscript{27} Thus, we needn’t bother about future proofs of mathematical claims to make this point. Moreover, we have some inductive evidence that such proofs are not provable without using extended resources. Many a math graduate student has been told not to waste their time trying to prove Fermat’s Last Theorem, both because it can consume one, and because so many extraordinarily gifted mathematicians have failed to prove it using obvious resources clearly related to number theory. Wiles only managed to find a proof of it because he went far, far outside the realm of such resources. Thus, if the manifestation requirement really requires a speaker to recognize of any construction c whether it proves a claim, then it is demanding too much. This point on its own is not too surprising. This is one of the reasons why latter-day intuitionists have to give accounts of “canonical constructions,” where the domain of quantifications, when quantifying over “any construction c” is really taken to consist of canonical constructions. After all, there is no logical system where every logical truth has only one proof.\textsuperscript{28} When anti-realists talk about constructions, in the Recognition Thesis as I have given it, or in the Manifestation Requirement, as Tennant has given it, they have to be referring to “canonical constructions” to limit what is required of the speaker.

Moreover, given that logically equivalent claims are such that any proof of one can be transformed into a proof of the other, if anti-realists did not utilize a notion of canonical proofs, they would not be able to use such constructions to individuate the meanings of claims. Thus, when I have been talking about “constructions” in the Recognition Thesis, I have not been assuming that a speaker needs to be charged

\textsuperscript{27}Stewart Shapiro (p.c., (1996)) first pointed this out to me as an independent argument against Dummett.

\textsuperscript{28}Neil Tennant is presently developing a system of “skeletal logic” where proofs might have this property. Bob Hale and I (p.c., 1998) conjectured that there may be a limitation result lurking in this area which would prevent successful completion of skeletal logic for any logic with an undecidable set of logical truths.
with recognizing any old proof for a claim. While the notion of a canonical proof in the writings of Prawitz, Tennant and others is both fascinating and problematic, we do not need to dig too deeply to see why phenomena such as Fermat’s Last Theorem cause problems for the philosophical uses the anti-realist puts it to. Again, canonical proofs are both supposed to allow the Recognition Thesis to individuate meanings, and to allow it to not require the speaker to recognize verifications which involve resources irrelevant to the meaning of a claim. Good so far. However, as I claimed, we have very good inductive evidence that there is no proof for Fermat’s last theorem which only involves number-theoretic resources.

But if this is the case for Fermat’s last theorem, or any mathematical claim proved with “outside resources,” then Tennant’s argument against Gödelian Optimism can be given as an argument against weak manifestationism when applied to mathematics. The argument, in brief, can be given in this manner.

1. By weak manifestationism, for all sentences $\Phi$ a competent speaker can (suitably idealized) recognize of any construction $c$ whether or not $c$ is a proof of $\Phi$.

2. There exist some mathematical claims $\Phi$, which do have proofs involving resources irrelevant to the meaning of $\Phi$.

3. If these proofs are canonical (such that they count as constructions bound by the quantifier in 1.), then speakers who understand $\Phi$ can recognize any proof of $\Phi$ as a proof of $\Phi$. But then, the manifestation requirement would be too strong, requiring speakers to grasp resources irrelevant to the meaning of $\Phi$ in order to understand $\Phi$.

4. If these proofs are not canonical, given that they involve resources irrelevant to the meaning of $\Phi$, then for some such claims like Fermat’s last theorem, we have very good evidence that mathematical claims can be true without canonical proofs for them. But then, by the sense of verifiability encoded by claiming a sentence is verifiable just in case a canonical proof for it exists, it follows that we have very good evidence that some claims are true, yet not verifiable, rendering Tennant’s Knowability Requirement (which follows from weak manifestationism) false.

This proof leaves the manifestationist with several unpalatable choices. She can say that any non-canonical proof does not establish the truth of a claim. This is
bad; constructivism is only defensible if it can be argued that mathematics won’t be
completely mutilated by “going constructive.” To refuse to recognize proofs which
extend the resources appropriate to the meaning of the theorem proved would ar­
guably mutilate mathematics.

Perhaps the manifestationist should say that the proof of Fermat’s last theorem
by Wiles and his associate can be transformed into a canonical proof that only
involved the meanings of the words in the theorem. This is also bad, because there
is not only no evidence for such a claim, but some inductive evidence against it. We
shouldn’t claim that mortals can skip along a path which has felled angels. That
is, if so many mathematical geniuses failed to find such a canonical proof for over
a century, and the geniuses who finally did find a proof did so by going outside of
number theory, this is at least some inductive evidence that there is no proof in
number theory.

As a last recourse, the manifestationist might say that, if the new proof is valid,
then it can be transformed into such a canonical proof. Again, I am very uncom­
fortable with this as a general strategy for accepting proofs whose resources do not
conservatively extend the resources appropriate to the meaning of the claim. To
work it would have to successfully negotiate between the Scylla of the mutilation
of mathematics and the Charybdis of grotesque optimism about the existence of
canonical proofs.

Since this monograph is not about anti-realism in mathematics, I will not further
develop this criticism. However, it is necessary to get it on the table, so that we can
later transform Tennant’s criticism of Gödelian Optimism into the proper bone of
contention between the anti-realist and the anti-anti-realist. At this point, I should
point out that at other places in the text Tennant seems to be aware of the kind
of charge I’m making against the constructivist. In a discussion of possible realist

\[^{29}\text{Tennant (Tennant, (p.c, 1998)) has responded that this argument would only be really persu­}
\text{asive after a thorough examination of relevant conservative extension results.}\]
reconstruals of manifestationism he writes,

The linguistic competent need not be a scientific genius, or inspired mathematician. He need not be able to take any sentence $\Phi$ and settle for himself (except in the grammatically simplest and epistemically most obvious cases, which would be criterial for grasp of the constituent terms of $\Phi$) what the truth-value of $\Phi$ is or would be. But should he not be able to respond appropriately to someone else’s suggestion of grounds for believing or disbelieving $\Phi$? Should he not be able to appraise purported proofs or disproofs of $\Phi$, and pronounce correctly on their probative quality? This seems plausible, especially in those cases where the proofs and disproofs in question are easily surveyable and suitably ‘predicative’—that is, where they do not involve sentences containing terms from stretches of discourse with which the speaker is unfamiliar. The latter might be the case with a proof of Fermat’s Last Theorem that involved long excursus into the arcane upper reaches of set theory and algebraic topology. If the speaker were being tested for grasp of meaning simply as an arithmetician, it would be asking too much of him that he be able to appraise such a difficult ‘theoretically impredicative’ proof correctly.

One could maintain that one understood full well what was meant by Fermat’s famous arithmetical claim that for no $n > 2$ is it the case that there are natural numbers $a, b, c$ such that $a^n + b^n = c^n$; while yet confessing a complete inability to assess the recent proof of that theorem offered by Wiles and his associate. Of course ideally the competent understander should be able to absorb the extra criterial axioms needed to furnish senses for the terms of the arcane theoretical extension within which the proof makes its excursus, but for understanding as we usually understand it, this would be to ask too much of the understander of Fermat’s Last Theorem. (Tennant, (1997, p. 225))

Then Tennant immediately concludes from this, not that the manifestation requirement is false, but rather that we need to be careful “stipulate that it is only ‘theoretically predicative’ and fully formalized proofs that the understander should be able to recognize as such.” (Tennant, (1997, p. 225)) Again, since this monograph is not about the epistemology of understanding mathematical claims, I will not further pursue criticism of this tack, other than to point out that it seems to involve Tennant in a kind of optimism about the existence of “theoretically predicative” proofs which is not clearly any more warranted than Godelian Optimism.

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My second caveat concerns Tennant's use of the notion of "change of meaning" in his criticism of Gödelian Optimism. He seems to be countenancing a situation where the proof procedures so outrun what we took to be the resources appropriate to the meaning of the theorem proved, that we are forced to say that the meaning of the theorem has changed. If this is possible then how can the Gödelian Optimist be sure that any claim or its negation is provable, with the meanings we currently attach to them (which is the claim she needs to defend if she wants the proofs in question to be the constructions mentioned in the Manifestation Requirement)?

To evaluate this claim we need some convincing historical examples where Tennant is willing to argue that the meaning of a mathematical claim did change due to proofs which extended the resources appropriate to the (earlier) meaning of the claim in question. The best such examples would involve proofs where widely held mathematical beliefs were overturned. For example, Euclid believed that the parallel postulate was true, but centuries later Riemann and Lobachevski's independence results showed that it was not in general true. Did the meaning of the parallel postulate change at that point? It's hard to say because the parallel postulate is still true over Euclidean space, and provably so since it is an axiom of Euclidean geometry. For another example, Leibniz believed, "The infinite sum $1 - 1 + 1 - 1 \ldots$ is equal to one half," which we know to be false today. Did Leibniz mean something different by any of the subsentential units in this claim, or did he just have a false belief about infinite sums? In the next chapter I will argue that, in so far as meaning does not transcend use, we are not rationally constrained to characterize Leibniz and others in his situation in one way or the other. Thus, it's hard to adjudicate Tennant's claim about future conceptual resources changing the meaning of mathematical claims.

However, to the extent that future conceptual resources could change the meaning of a mathematical claim, this again seems to be a problem for the manifestationist generally, not specifically for the Gödelian Optimist. Consider the following argu-
ment.

1. By weak manifestationism, for all sentences $\Phi$ a competent speaker can (suitably idealized) recognize of any construction $c$ whether or not $c$ is a proof or a disproof of $\Phi$.

2. But we have no guarantee that the construction $c$, if discovered, won’t involve resources so irrelevant to the meaning of $\Phi$, that it changes the meaning of $\Phi$.

3. If $c$ involves resources so irrelevant to the meaning of $\Phi$, that it changes the meaning of $\Phi$ then recognition of $c$ would be irrelevant to $S$’s grasp of the meaning of $\Phi$ with its current meaning, and hence the manifestation requirement would be too strong, requiring speakers to grasp resources irrelevant to the current meaning of $\Phi$ in order to understand $\Phi$ with its current meaning.

Again, this is an all too-quickly stated challenge to the anti-realist about mathematics, which is not the topic of discussion. However, the issue discussed will be recapitulated by the next chapter, as an issue about grasp of empirical claims with defeasible warrants. Essentially Tennant has argued that Gödelian Optimism renders understanding of mathematical claims too holistic to be able to subscribe to even the weak Manifestation Requirement. I have responded by suggesting that the Manifestation Requirement itself renders grasp of mathematical claims too holistic, and then raised the possibility that attempts to reign it in by characterizing mathematical meaning in terms of canonical proof conditions, either ends up mutilating mathematics or entailing an unreasonable optimism about what the anti-realist’s bearers of meaning (canonical proof conditions) can do.

After considering Tennant’s discussion of further weakenings of the Manifestation Requirement we shall see that the anti-realist will be able to make an argument very similar to Tennant’s argument against Gödelian Optimism to the conclusion that the extraordinarily weak Recognition Thesis I have defended in Chapter II (as being the most plausible form of Recognition Thesis for empirical claims with defeasible warrants) renders understanding too holistic. While I will agree with Tennant that it does render understanding too holistic, I will not conclude from this
that we need a stricter form of the Recognition Thesis, but rather will use claims about the underdetermination of meaning (similar to the claim made above about Leibniz and sums) in the next chapter to argue that the fault lay with Dummettian verificationism itself, not with any specific species of it.

Tennant himself considers even weaker versions of his Manifestation Requirement. In addition to Hawkish (strong) Manifestationism, which requires a speaker to be able discover verifications of sentences in order to understand them, and Conservative (weak) Manifestationism, which requires a speaker to be able to recognize verifications of sentences in order to understand them (equivalent to what I have called a strong Recognition Thesis) Tennant considers the following two positions.

**Moderate Manifestationism**

[A speaker $X$ understands a sentence $\Phi$ if and only if $X$ can recognize] of any local aspect of a given construction (no matter how long or complex) whether it... [is] in order as needed for such a construction ultimately to count as a... proof or a disproof of $\Phi$.  

(Tennant, (1997, p. 231))

**Radical Manifestationism**

[A speaker $X$ understands a sentence $\Phi$ if and only if $X$ can manifest such understanding by any capacities which would] suffice to justify one in attributing to the speaker,

1. a knowledge of how $\Phi$ is constructed from its constituent expressions;
2. a knowledge of what canonical inferential and/or computational contexts govern those constituent expressions; and
3. a knowledge of what would count as a proof or a disproof of $\Phi$ (if it is relatively short and uncomplicated), or of various tractable sub-sentences of $\Phi$ or of various other tractable sentences that could be constructed from the constituent expressions occurring in $\Phi$.

(Tennant, (1997, p. 231))

Both of these are different from the weak Recognition Thesis I defended earlier as the strongest Recognition thesis both plausible and motivated by Dummett's writings. However, Tennant's criticism, to the extent that it works against the attempt to simultaneously defend bivalence while subscribing to either of these positions, or weak manifestationism, also applies against the weak Recognition Thesis. Tennant
writes,

The next three readings—conservative, moderate, and radical, all suffer from another drawback. This is that they undermine the realist’s imagined right to assert bivalence across the board, even if they are not provably inconsistent with it. These three readings of (MR) do not provide rich enough meanings for our sentences to guarantee their bivalence... The anti-realist insists on a constructive notion of truth, and expouses a plausibly weak form of manifestationism ((wpM) or something even weaker). In response to the undecidability of certain discourses, he is prepared to give up bivalence.

(Tennant, (1997, p. 234))

Unfortunately, Tennant does not say much about why weaker readings of his Manifestation Requirement undermine a right to assert bivalence, but given his discussion of Godelian Optimism I take him to be arguing that the weaker versions of the Manifestation Requirement do not individuate meanings enough for the realist to be able to assert that every sentence is either true or false, with its current meanings. If this is his argument, it again seems properly orthogonal to the issue of bivalence. That is, even if weaker forms of manifestationism do not individuate meanings enough, then they still don’t, even if bivalence is false.

However, Tennant’s discussion of Godelian Optimism does suggest one final argument, one that is consonant with Dummett’s work. Perhaps Tennant can be understood as producing independent argumentation to the conclusion that a strong form of the Recognition Thesis is correct. Given that, in the last chapter, we saw that one could argue that a strong Recognition Thesis entails that a constructive account of truth is correct (remember that the strong Recognition Thesis is equivalent to Tennant’s weak Manifestation Requirement) if Tennant had an argument against any weaker Recognition Thesis, then he could plausibly claim to have motivated a constructive account of truth as correct.

If the verification conditions mentioned in the Recognition Thesis are to individuate meanings, then one can make an argument that a weak Recognition Thesis is
incorrect. The weak Recognition Thesis, again, is

**Recognition Thesis (weak, de re)**

$X$ understands $P$ if, and only if, were $X$'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$ (such that $\forall Q (V_c Q \rightarrow V_{\text{weak}} c Q)$), i.e. for all $Q$, if $c$ verifies $Q$, then $c$ increases the probability of $Q$, other things being equal), then $X$ could recognize whether $c$ verifies $P$ and whether $c$ verifies $\sim P$.

Now we can ask whether such warrants plausibly could individuate the meanings of claims. That is, can warrants which only increase the probability of claims be canonical in the manner which renders the Dummettian Canonicity Requirement plausible? Rather, is it at all plausible to affirm something like the following?

Two sentences $A$ and $B$ have the same meaning if, and only if, for all constructions $c$, $V_{\text{weak}} c A \leftrightarrow V_{\text{weak}} c B$, and $V_{\text{weak}} c \sim A \leftrightarrow V_{\text{weak}} c \sim B$.

If a coherent argument can be given to the conclusion that weak verifiers cannot be canonical in this manner, and that strong verifiers can be, then the anti-realist would have good evidence that non-revisionary verificationism is not adequate for answering Dummett's challenge.

I shall not attempt to defend non-revisionary verificationism from this charge, but shall rather go on to try to undermine Dummett's arguments that give rise to verificationism in the first place. Moreover my attempt, in the next chapter, will revolve around this very issue. In brief, I shall argue that "meaning" and "understanding" are best understood as Wittgensteinian family resemblance terms in a manner which undermines Dummett's challenge. Thus, the desire to individuate meanings by dispositional states of speakers who grasp meanings will be undermined.

§ 3.5 Conclusion

In Chapter II, I argued that the strongest plausible forms of Recognition Thesis and Verification Constraint defensible for empirical claims with defeasible warrants
were the following,

**Recognition Thesis (weak, de re)**

\( X \) understands \( P \) if, and only if, were \( X \)'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were \( X \) then presented with a construction \( c \) (such that \( \forall Q (c \text{ verifies } Q \rightarrow V_{\text{weak }} cQ) \)), i.e. for all \( Q \), if \( c \) verifies \( Q \), then \( c \) increases the probability of \( Q \), other things being equal), then \( X \) could recognize whether \( c \) verifies \( P \) and whether \( c \) verifies \( \neg P \).

**Verification Constraint**

If \( P \) is true, then it is possible that there exists a construction \( c \) such that \( c \) is a warrant for \( P \) that increases the probability that \( P \) is true, other things being equal (\( TP \rightarrow \exists^0 cV_{\text{weak }} cP \)).

I also argued that these theses on their own do nothing to motivate adopting a constructive truth predicate. This realization prompted the extensive explication and evaluation of the revisionary arguments presented by Wright, Dummett, and Tennant. For, if the anti-realist can argue that non-constructive truth is unsustainable in light of the above form of verificationism, then she can conclude that we should adopt a constructive truth predicate.

In this chapter I extensively discussed the relevant revisionary arguments. In each case we saw that the arguments are only valid to the extent that they assume an unwarrantedly strong Verification Constraint. Thus, I conclude that a convincing argument hasn't been made against using classical model theory, in modelling the meaning of empirical claims with defeasible warrants.

In our discussion of Tennant's argument we were able to read a challenge to the non-revisionary verificationist lurking just under the surface of Tennant's discussion. Tennant can be understood as challenging the non-revisionary verificationist to show that the verification conditions in her Recognition Thesis can be canonical verification conditions. Rather than picking up this gauntlet in the next chapter, I will turn to the broader challenge of which it is a part. Thus, instead of trying to argue that the non-revisionary verificationist can use her Recognition Thesis to individuate meanings, I will argue that she shouldn't. Moreover, once it is clear
why she shouldn't, the Dummettian impetus for being a verificationist at all will be weakened.
In this chapter I will attempt to establish that Dummettian Manifestationism involves illicit use of the analytic-synthetic distinction, as well as commitment to what can be called “the extricability of meaning” thesis. First, however, it behooves us to step back and take stock. In the last chapter I presented arguments by Wright, Dummett, and Tennant. Where “Strong Verificationism” states that if a sentence is true then there exists a construction indefensibly verifying the sentence, Wright’s first argument can be presented in this manner.

Wright’s first argument

\[
\text{Strong Verificationism} \\
\text{Negation Equivalence} \quad \text{Plausibility Considerations}
\]

The logical operators should be interpreted intuitionistically.

Then, when “Gödelian Optimism” states that for all sentences \( P \), \( P \) either is indefensibly verifiable or \( \neg P \) is, Wright’s second argument can be presented schematically in this manner.

Wright’s second argument

\[
\text{Strong Verificationism} \quad \text{Bivalence} \\
\text{Gödelian Optimism} \quad \neg (\text{Gödelian Optimism}) \\
\neg (\text{Bivalence})
\]

We also saw that, where “TCS” states that classical truth-conditional semantics
are used to explain the meanings of logical forms in natural language, Dummett’s revisionary argument is best represented in this manner.

Dummett’s Argument

\[ \text{TCS} \]
\[ \vdash \neg(\text{Strong Verificationism}) \equiv (\text{Strong Verificationism}) \]
\[ \neg(\text{TCS}) \]

Finally, where Church’s Thesis states that a set of sentences is computable if, and only if, it is recursive, and where the Strong Recognition Thesis charges understanders of sentences with being able to recognize indefeasible verifiers and falsifiers for those sentences when presented with them, Tennant’s revisionary argument can be given in this manner,

Tennant’s Argument

\[ \text{Strong Recognition Thesis} \]
\[ \vdash \text{Strong Verificationism} \]
\[ \text{Gödelian Optimism or Truth is Constructive} \equiv \text{Bivalence} \]
\[ \neg(\text{Gödelian Optimism}) \equiv \neg(\text{Church’s Thesis}) \equiv \neg(\text{Bivalence}) \]

Since in Chapter 2, I showed that Strong Verificationism is neither plausible nor motivated by Dummett’s dialectic as a claim about empirical sentences with defeasible warrants, I was able to conclude that intuitionistic revision of logic or semantics has not been adequately defended when we are concerned with empirical claims with defeasible warrants. Thus, from both Chapters 2 and 3, one could read into my discussion a defense of non-revisionary anti-realism.
However, from Tennant’s discussion we were able to read a possible challenge to the person who defends weak verificationism and bivalence while simultaneously accepting Dummett’s desiderata for a theory of meaning. Before stating Tennant’s challenge it is worthwhile to restate the Dummettian desiderata. In Chapter 1 I presented Dummett’s arguments for the claim that, in addition to providing a compositional semantics, the meaning theorist must provide a theory of sense to explain tacit knowledge of the compositional semantics.

Dummettian “manifestationism” is the insistence that such a theory be given in terms of sets of dispositions, necessary and sufficient for understanding of the sentences they (the dispositions) are correlated with. The position can be given in the following tripartite way.

**Manifestationist View**

(A) A speaker $S$ is competent in the language $L$ if, and only if, $S$ has tacit knowledge of a correct theory of reference for $L$ (we call this theory $R_L$).

(B) The theory of meaning for $L$ (which we will call $M_L$) is identical to $R_L$ plus a theory of sense (which we call $S_L$), which correlates sets of dispositions with the truth conditions generated by $R_L$.

(C) A person can correctly be attributed tacit knowledge of $R_L$ if, and only if, she possesses the dispositions correlated with the truth conditions of $R_L$ by $S_L$.

Then, we considered what arguments Dummett gives for the following two theses, labelled by us “the manifestation constraints.”

**The Entailment of Identity of Speaker’s Meaning from Identity of Use**

If two speakers agree in all possible correct uses of a sentence, then they agree about the meaning of the sentence.

**The Equivalence of Correct Use and Grasp of Meaning**

One can correctly use a sentence if and only if one correctly grasps its meaning.

Finally, in Chapter 2, we saw how the notions of correct use mentioned in the above two constraints must be understood as capable of individuating the meaning of sentences. Thus, we were led to attribute the following further constraint to the
Dummettian.

**Canonicity Requirement**
The dispositions to correctly use a sentence mentioned in the above two requirements must individuate the meanings of sentences.

In so far as these theses are independently plausible, they can be thought of as providing evidence for Dummett’s dispositionalist approach to the theory of sense, what I have described as his “Manifestationism.” In so far as Manifestationism is plausible, these theses set reasonable success conditions for the theory of sense provided.

Thus, Dummett can be thought of as challenging the classical semanticist (who takes a speaker’s competence with a natural language to consist in that speaker’s tacit knowledge of a classical semantics for that language) to provide a theory of sense satisfying the above requirements. Dummett’s positive neo-positivistic program requires attempting to show how use of constructive, intuitionistic accounts of truth and inference can satisfy all of the above requirements. In this manner, the Verificationist Manifestationist defends the following theses.

**Publicity Requirement**
If two speakers agree, for all possible constructions c, whether c verifies P and whether c verifies ¬P, then they agree about the meaning of P.

**Recognition Thesis**
X understands P if, and only if, were X presented with a construction c, then X could recognize whether c verifies P and whether c verifies ¬P.

**Verificationist Canonicity Requirement**
The constructions quantified over in the Publicity Requirement and Recognition Thesis must be such that two sentences A and B have the same meaning if, and only if, for all constructions c (from the same domain of constructions quantified over in the Publicity Requirement and Recognition Thesis) c verifies A if, and only if, c verifies B, and c verifies ¬A if, and only if, c verifies ¬B.

These motivate, for Dummett, use of a constructive semantics, which actually corre-
late verification conditions with natural language sentences. Thus, for the Dummettian verificationist, the tripartite definition of the “Manifestationist View” is filled out in this manner.

**Verificationist Manifestationist View**

(A) A speaker $S$ is competent in the language $L$ if, and only if, $S$ has tacit knowledge of a correct constructive theory of reference for $L$ (we call this theory $R_L$), which compositionally determines canonical verification conditions of the sentences of $L$.

(B) The theory of meaning for $L$ (which we will call $M_L$) is identical to $R_L$ plus a theory of sense (which we call $S_L$), which correlates sets of dispositions with the truth conditions generated by $R_L$, by stating that speaker $X$ understands the sentence $P$ if, and only if, were $X$ presented with a construction $c$, then $X$ could recognize whether $c$ verifies $P$ and whether $c$ verifies $\neg P$.

(C) A person can correctly be attributed tacit knowledge of $R_L$ if, and only if, she possesses the dispositions correlated with the truth conditions of $R_L$ by $S_L$, as described in (B) above.

We saw at the end of the last chapter that Tennant could be thought of as challenging the non-revisionary weak verificationist, arguing that she cannot satisfy the Canonicity Requirement. In Chapter II, weak verificationism was shown to follow from the following weak Recognition Thesis.

**Recognition Thesis (weak, de re)**

$X$ understands $P$ if, and only if, were $X$’s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$ (such that $\forall Q (c$ verifies $Q \rightarrow V_{weak}cQ)$, i.e. for all $Q$, if $c$ verifies $Q$, then $c$ increases the probability of $Q$, other things being equal), then $X$ could recognize whether $c$ verifies $P$ and whether $c$ verifies $\neg P$.

Then, by the Canonicity Requirement, the weak verificationist is committed to the
following claim about the identity of meaning.

Weak Verificationist Canonicity Requirement

Two sentences A and B have the same meaning if, and only if, for all constructions c (such that $\forall Q(c \text{ verifies } Q \rightarrow V_{\text{weak}}cQ)$, i.e. for all $Q$, if $c$ verifies $Q$, then $c$ increases the probability of $Q$, other things being equal) $c$ verifies $A$ if, and only if, $c$ verifies $B$, and $c$ verifies $\neg A$ if, and only if, $c$ verifies $\neg B$.

Tennant can be understood as challenging the non-revisionary weak verificationist to motivate this principle.

The strong Recognition Thesis which Tennant defends (his "weak manifestation-ism") can be given in this manner.

Recognition Thesis (strong, de-re)

$X$ understands $P$ if, and only if, were $X$'s cognitive capacities and technologies finitely extended (in an appropriate manner), and were $X$ then presented with a construction $c$ (such that $\forall Q(c \text{ verifies } Q \rightarrow V_{\text{strong}}cQ)$, i.e. for all $Q$, if $c$ verifies $Q$, then $c$ makes the probability of $Q$ equal to one), then $X$ could recognize whether $c$ verifies $P$ and whether $c$ verifies $\neg P$.

This, of course, is the very principle I criticized in Chapter 2.

Tennant’s reasoning can be represented in this manner.

Tennant

<table>
<thead>
<tr>
<th>Weak Recognition Thesis</th>
<th>Strong Recognition Thesis</th>
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<tbody>
<tr>
<td></td>
<td>Fulfillment of</td>
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<tr>
<td></td>
<td>Strong Verificationism</td>
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<tr>
<td></td>
<td>Canonicity Requirement</td>
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<td>Possible Failure</td>
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<td>To Fulfill</td>
<td>Constructive Truth</td>
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<tr>
<td>Constructive Truth</td>
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<tr>
<td>Logical Revision</td>
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Therefore, since by reflective equilibrium considerations, it is better to revise logic and fulfill the Canonicity Requirement, it is better to adhere to a Strong Recognition Thesis and revise logic, and deny the Weak Recognition Thesis.

To see the a poria we have reached, consider the schematic representation of my
reasoning, which can be similarly represented.

Cogburn

\[
\begin{array}{c}
\text{Ayer's concerns in Ch. 1 of Language, Truth, and Logic} \\
\text{Strong Recognition Thesis} \\
\neg(\text{Strong Verificationism}) \text{ Strong Verificationism} \\
\neg(\text{Strong Recognition Thesis}) \text{ in Ch. II} \\
\text{Contradiction} \\
\neg(\text{Strong Recognition Thesis})
\end{array}
\]

Thus, one response I could make would be to attempt to match Tennant's challenge, and give a compositional account of canonical constructions on which they provide evidence well short of what is sufficient to determine with certainty that a claim is true. This, I take it, is what the non-revisionary verificationist who subscribes to Dummett's desiderata for the theory of meaning needs to do.

However, in light of the issues raised in the last section of Chapter 3, where I pointed out that the anti-realist faces problems maintaining the Canonicity Requirement in the case of mathematics, where she can more plausibly uphold a strong Recognition Thesis, I do not want to attempt this strategy. Another response is to attempt to shift the burden of proof to Tennant, in effect saying, "You need to provide a compelling answer to Ayer's considerations and to the considerations I presented in Chapter 2, or at least provide more evidence for the claim that, in light of the arguments of Chapter II, reflective equilibrium considerations really do decide in favor of maintaining Dummettian Manifestationism at the expense of the problems isolated in Chapter 2." Since shifting the burden of proof is so often the last refuge of a scoundrel I do not want to attempt this strategy.

Instead, I shall attempt to criticize the Dummettian manifestation constraints all together. That is, I will show that meaning is not equal to use in the sense enshrined by the Manifestation Constraints. This claim will be defended in a two-fold manner.
First I will show how the Dumettian verificationist is committed to (i) a certain kind of use of the analytic-synthetic distinction (to explain word meaning and grasp of it) as well as (ii) what I will call the Extricability Thesis. Then, I will present a variety of philosophical, psychological, linguistic, and lexicographic considerations to argue that an analytic-synthetic distinction cannot do the philosophical work which Dummett requires of it. Moreover, attention to this data suggests ways in which grasp of word meaning can be construed in a non-Dummettian fashion. Though these arguments will only directly engage the Verificationist Manifestationist, in the conclusion of this chapter I will suggest that they provide good grounds for holding that the Manifestation constraints themselves are to blame. The second part of my demonstration that meaning is not equal to use in Dummett’s manner involves sketching an account of how we can be said to have tacit knowledge of the truth conditions generated by a semantics without subscribing to the manifestation constraints. This task will form the substance of Chapter V.

§ 4.1 WHY THE DUMMETTIAN NEEDS ANALYTICITY AND EXTRICABILITY

When I talk about a sentence being analytic, I do not merely mean that it is true in virtue of meaning. I also mean that its truth in virtue of meaning entails that two speakers who disagree about the truth value of the sentence must disagree about the meaning of it. Thus the working definition of analyticity which will be used throughout our discussion is the following.

**Analyticity**

A sentence $P$ is analytic if, and only if, it is true solely in virtue of its meaning; whence, two speakers disagree about the truth value of $P$ only if they disagree about the meaning of $P$.

Then, the person I will call the analyticity theorist is committed to the claim that meanings of many words can be individuated by reference to the set of analytical truths the words occur in. Again, I represent this individuation not just as a thesis about word meaning, but simultaneously as a thesis about grasp of word meaning.
In this manner, the position can be given as entailing commitment to the existence of a robust set of what I will call analytic words, which can be defined in the following manner.

**Analytic Word**

A word $Q$ is an analytic word if, and only if, a set $\Phi$ of analytic sentences involving $Q$ is sufficient to individuate the meaning of $Q$ from that of other words in the language; (on the assumption that they agree about the meanings of the words in the sentences in $\Phi$ other than $Q$) two speakers disagree about the truth value of any of the sentences in $\Phi$ only if they disagree about the meaning of $Q$.

The anti-Quinean thesis that meaning is extricable from background knowledge is again given epistemelologically, more appropriately as the claim that grasp of a word’s meaning can be extricated from background knowledge about the world. Thus, the Extricability Thesis can be given in this manner.

**The Extricability Thesis**

1. For any utterance $P$, $P$’s truth or falsity is a function of the meaning ($M$) of the sentence and the way the world is ($W$).

2. The $M$ component bifurcates from the $W$ component in such a way that, if $P$ is true given $M$ and $W$, and a person $S$ takes $P$ to be false, then there is some fact of the matter which determines whether or not $S$ attaches a deviant meaning $M'$ distinct from $M$ to $P$ and possibly has a correct belief about $W$, or attaches the right $M$ to $P$ and has an incorrect belief about $W$.

In this section I seek to establish that the Manifestationist Verificationist is committed both to the claim that many words in natural language are analytic words and to the Extricability Thesis.¹

Establishing this requires attending to Dummett’s distinctions between atomistic, molecularist, and holistic meaning theories. I must confess that I don’t understand the distinction Dummett tries to draw between atomistic and molecularist meaning theories, since the way he describes a molecularist meaning theory *prima facie* ren-

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¹I intend the first claim in the statement of the Extricability Thesis to be one we can all agree on. Thus, the meaning component of the utterance might be determined by a variety of contextual, holistic and conventional factors, depending upon the theoretical context in which such meanings are being posited.
determines it atomistic as well. Here is a representative passage.

If a theory correlates a specific practical capacity with the knowledge of each axiom governing an individual word, that is, if it represents the possession of that capacity as constituting a knowledge of the meaning of that word, I shall call it atomistic; if it correlates such a capacity only with the theorems which relate to whole sentences, I shall call it molecular. (Dummett, 1976, p. 38)

This is extremely confusing. Given that the theory of meaning is supposed to be compositional it is not at all clear how a Dummettian theory of grasp of meaning could be molecular and fail to be atomistic at the same time.

Luckily, we shall not have to utilize the supposed distinction between a molecular meaning theory and an atomistic one. For Dummett, the important distinction is between the molecularist or atomistic view on the one hand, and the holistic view on the other. In “What is a Theory of Meaning? (I)” he distinguishes between the molecularist and holistic view in the following manner,

The difference between a molecular and a holistic view of language is not that, on a molecular view, each sentence could, in principle be understood in isolation, but that, on a holistic view, it is impossible fully to understand any sentence without knowing the entire language, whereas, on a molecular view, there is for each sentence, a determinate fragment of the language a knowledge of which will suffice for a complete understanding of that sentence. Such a conception allows for the arrangement of sentences and expressions of the language in a partial ordering, according as the understanding of one expression is or is not dependent upon the prior understanding of another. (That it be, or approximate to being, a partial ordering, with minimal elements, seems to be required if we are to allow for the progressive acquisition of a language. On a holistic view, on the other hand, the relation of dependence is not asymmetric, and in fact obtains between any one expression and any other: there can be nothing between not knowing the language at all and knowing it completely.) (Dummett, 1976, p. 44)

If one had to chose between molecularism and holism in these two senses then molecularism would clearly be more palatable. However, the dichotomy Dummett presents here is so clearly a false one that nothing need be said to show it so.
In the passage Dummett does not address the possibility of holding an intermediate position, one which eschews the Dummettian molecular claim that “there is for each sentence, a determinate fragment of the language a knowledge of which will suffice for a complete understanding of that sentence” and instead is committed to the weaker position that “there is for each sentence, an indeterminate fragment of the language a knowledge of which will suffice for the correct attribution of understanding of that sentence in many contexts.” In section 4.3 I will suggest that this intermediate position is correct, and then in the next chapter further explicate and defend it. For now it is enough to point out that Dummettian molecularism and holism in no way exhaust the dialectical space of possible positions. What we might call “fuzzy molecularism” is a coherent place in the dialectical space.

In some passages Dummett characterizes molecularism in a stricter manner than he does in the above. One such passage might be taken to be the statement in “The Philosophical Basis of Intuitionistic Logic” where he writes,

> On any molecular view of language- any view on which individual sentences carry a content which belongs to them in accordance with the way they are compounded out of their own constituents, independently of other sentences of the language not involving those constituents...

(Dummett, (1973, p. 222))

Given the restriction to components of the sentence in question this statement of molecularism may seem to require more than the previous one. The previous statement of molecularism contained no such restriction. However, there is an important difference between the two which might render this statement less strict. This version of molecularism may not actually be more demanding, as it is given as a thesis about the meaning of sentences themselves, not what is required for grasp of meaning of sentences. One might plausibly hold that the meaning of a sentence is solely a function of the meaning of its constituents and the way they are put together (this

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2Dummett came to attribute a position such as this to Davidson. See the appendix to (Dummett, 1976a) and Chapter 4 of (Dummett, 1991).

3Neil Tennant came up with this title.
is really just compositionality), and deny that our grasp of meaning of a sentence is solely a function of our grasp of the meaning of its constituents and the way they are put together.

In any case, Dumett does sometimes state the molecularism as a very demanding thesis about grasp of meaning. In “The Justification of Deduction” he writes,

[The molecularist conception of language] requires that we can imagine each sentence as retaining its content, as being used in exactly the same way as we now use it, even when belonging to some extremely fragmentary language, containing only the expressions which occur in it and others, of the same or lower levels, whose understanding is necessary to the understanding of these expressions: in such a fragmentary language, a sentence of greater logical complexity than the given one would not occur. (Dummett, (1973, p. 302))

If this is what molecularism comes to, then molecularism is clearly false. Could I possibly understand the sentences, “Aleph naught is the smallest transfinite cardinal,” “Omega exists,” “Mugwumps have no liver,” “There is indeed the mystical,” “Electrons are negatively charged” by only understanding sentences of less logical complexity? Such a position, as stated, is so absurd that it needn’t be considered, though at the end of this section I will suggest that it is not as absurd as it might seem, given an interpretation of the claim which is exegetically plausible, once one considers the milieu in which Dummett was writing. Before this it seems that the Dummettian molecularist would do best to subscribe to the weaker sense of molecularism sketched above. The most obvious way to do this would be to be an analyticity theorist, that is, to hold that grasp of words in sentences is gained either by grasp of analytical sentences involving those words, or perhaps by ostensive abilities with a limited part of vocabulary.

Therefore, Dumett’s actual writings on atomism versus molecularism versus holism aren’t that helpful in determining the extent to which he is committed to being an analyticity theorist or committed to the inextricability thesis. So we must discern: (1) what kind of molecularist the verificationist manifestationist is rationally
constrained to be, and (2) the extent to which this kind of molecularist is rationally constrained to be either an analyticity theorist or a subscriber to the Extricability Thesis. However, it is easier first to provide an argument for the conclusion that Dummett is committed to the Extricability Thesis. Then we shall be in a position both to see why he should subscribe to some form of molecularism and to canvass the forms of molecularism open to him.

Consider the sentence “Neutrinos have mass.” Now say that two people, Jones and Smith, disagree about the truth value of this sentence. Jones says “Neutrinos have mass” and Smith says “It is not the case that neutrinos have mass.” Now, being good Dummettians, we know that the meaning of “Neutrinos have mass” in Jones’ and Smith’s respective idiolects is determined by the set of possible canonical constructions which verify or falsify them. Given this, and the Canonicity Requirement, it follows that the sentence in each person’s idiolect has the same meaning if, and only if, the same set of possible constructions verify and falsify each sentence.

Where \( P_j \) is “Neutrinos have mass” in Jone’s idiolect, and \( P_s \) is the sentence in Smith’s idiolect, and the function \( m \) takes a sentence in an idiolect as an argument and gives that sentence’s meaning as output, this can be stated formally in the following manner.

\[
m(P_j) = m(P_s) \text{ if, and only if, } \\
\forall \diamond c ((VcP_j \iff VcP_s) \land (Vc\neg P_j \iff Vc\neg P_s)).
\]

This is classically equivalent to the claim that the two sentences are different in meaning if and only if there exists a possible construction which verifies one and not the other, or falsifies one and not the other, which can be given formally in this manner.

\[
(m(P_j) \neq m(P_s)) \text{ if, and only if, } \\
\exists \diamond c ((VcP_j \land \neg VcP_s) \lor (\neg VcP_j \land VcP_s)) \lor \\
(Vc\neg P_j \land \neg Vc\neg P_s) \lor (\neg Vc\neg P_j \land Vc\neg P_s))
\]

Thus, Jones and Smith disagree about the meaning of the claim, if and only if, a possible construction exists which verifies or falsifies one of their claims and not the
other. But then, Dummett is committed to the extricability thesis, repeated here.

The Extricability Thesis

(A) For any utterance $X$, $X$'s truth or falsity is a function of the meaning $(M)$ of the sentence and the way the world is $(W)$.

(B) The $M$ component bifurcates from the $W$ component such that, if $X$ is true given $M$ and $W$, and a person $P$ takes $X$ to be false, then there is some fact of the matter which determines whether or not $P$ attaches a deviant meaning $M'$ distinct from $M$ to $X$ and possibly has a correct belief about $W$, or attaches the right $M$ to $X$ and has an incorrect belief about $W$.

Assume both that we are talking about Jones' idiolect, and that in Jones' idiolect "Neutrinos have mass" is true. Then, the question concerns Smith's sincere statement of "Neutrinos do not have mass." If "Neutrinos have mass" in Smith's idiolect is verified and falsified by the same constructions that respectively verify and falsify "Neutrinos have mass" in Jones' idiolect, then Smith does not attach a deviant meaning to "Neutrinos have mass," and he (Smith) has a false belief about the world. If there exists a construction which verifies or falsifies "Neutrinos have mass" in one of their idiolects and not the other, then Smith attaches a deviant meaning to the sentence and possibly has a true belief about the world. Thus, the Dummettian anti-realist, committed to the Canonicity Requirement, is also committed to the Extricability Thesis.

Now we turn to Dummett's molecularism and the analytic-synthetic distinction. Consider the sentence "Electrons have negative charge." By Dummettian Manifestationism somebody understands this sentence just in case they can recognize verifiers and falsifiers of it as verifiers and falsifiers of it. But wait a minute. Possession of such recognitional capacities requires understanding a great many other sentences in physical theory. In fact, even the non-Dummettian should want to say that understanding of "Electrons have negative charge" requires understanding of a great deal of physical theory.

This is potentially very dangerous for the Dummettian. There is a kind of circularity worry that Dummett must confront, give his broader commitments, and the
fact that grasp of most words' meanings depends upon grasp of sentences involving the words. But then, if understanding of complex sentences is a function of understanding the meanings of the words in those sentences and the syntactico-semantic principles of combination involved, and if it is also true that understanding of words requires understanding many very complex sentences, then it seems Dummett's proposal is essentially that one understands a complex sentence if, and only if, one understands many of other complex sentences.

Second, unless one defends some variant of Molecularism, the problem with words whose grasp presupposes grasp of a great deal of background theory undermines the Extricability Thesis. Dummett holds that the meaning of a sentence is determined by the meanings of the words in the sentence and the way they are put together. For Dummett, the set of possible verifiers and falsifiers of a sentence is likewise supposed to be determined by the meanings of the words in the sentence and the way they are put together. Thus, predicates must uniformly contribute to the verification conditions they occur in (and hence also the verification conditions of negations of these sentences). For Heyting Semantics of first order logic, this is accomplished by understanding a predicate’s meaning to be not only the set of things the predicate holds of, but canonical criteria for determining of arbitrary objects whether or not they fall under the extension of the predicate. In this manner these criteria can contribute to determining the verification conditions of arbitrary sentences in which the predicate occurs.

So, for the Dummettian anti-realist, to understand the word “electron” is to grasp a method of determining for any possible arbitrary objects whether or not they are electrons. Of course being able to do so will require extensive understanding of background physical theory. However, given compositionality and the Canonicity Requirement, it will have to be the case that this theory determines the set of possible objects recognizable as electrons by those who understand the word “electron.” Therefore, if the verification conditions for “x is an electron” can be plugged into a
compositional theory of verification conditions, and if grasp of the verification conditions in question is necessary and sufficient for grasp of the meaning of "electron," it must be the case that any background theory necessary for understanding the word "electron" must determine a unique set of possible objects.

But this is just to say that "electron" is an analytic word, in the sense given above, repeated here.

**Analytic Word**
A word $Q$ is an analytic word if, and only if, a set $\Phi$ of analytic sentences involving $Q$ is sufficient to individuate the meaning of $Q$ from that of other words in the language; (on the assumption that they agree about the meanings of the words in the sentences in $\Phi$ other than $Q$) two speakers disagree about the truth value of any of the sentences in $\Phi$ only if they disagree about the meaning of $Q$.

Moreover, it seems clear that any "non-observational term," that is, a term whose grasp can't be manifested simply by ostension, must be an analytic word for the Dummettian. Otherwise the chunks of background theory necessary for understanding of the word could determine different sets of intensions, and the word's meaning (for Dummett, where $N$ is the theoretical word in question, the verification conditions of "$x$ is an $N$") would fail to make a uniform contribution to the verification conditions of sentences it occurs in.\footnote{Nothing I say here commits me to the claim that two words have the same meaning if, and only if, they have the same intensions (construed as extensions across possible words). Rather my discussion merely presupposes the weak assumption that if two words (or two senses of the same word) have different intensions then they have different meanings.} This is why Dummett needs some sort of molecularist thesis, at least strong enough to guarantee that words whose grasp can't be manifested by ostension are analytic words in the above sense.

Now we are in some position to understand Dummett's peculiar claims that the set of sentences grasp of which is sufficient for grasp of a given sentence $P$ should: (1) be no longer than $P$, and (2) only include the vocabulary of $P$. I conjecture that Dummett is thinking about formulations of mathematical theories in first order logic when he made this claim. Thus, the sentence "omega exists," is, in some sense,
really an incredibly long sentence of first order logic stating the set theoretic axiom of infinity.

\[ \exists x ((x = \omega) \land (0 \in x) \land \forall y (y \in x \rightarrow (\text{successor of } y)) \in x) \]

To get what Dummett really has in mind, one would have to make this longer by replacing “successor of” with its definition. In any case, if “Omega exists” is understood to really have the logical form \( \exists x ((x = \omega) \land P[x]) \) (or possibly just \( \exists x (P[x]) \), where \( P \) gives the definition of “omega,”) then Dummett’s claim starts to make more sense. If everything in the expanded sentence was explicitly defined with a minimum of non-logical primitives, then grasp of sentences shorter than the expanded sentence, including only vocabulary from the expanded sentence, might be all that is necessary for understanding the sentence (and being able to recognize proofs of it . . . in this case, I guess, recognizing the axiom of infinity as correct).

Moreover, the period when Dummett was writing his classic pieces was during the heyday of cross-disciplinary work in linguistics and the philosophy of language. It was also the heyday of deep structures and rampantly proliferating transformations to derive natural language sentences from the deep structures in question. During that time it was not too unreasonable to think of natural language the way I suspect Dummett thought of mathematical language. In fact, the Generative Semanticists and Davidsonians did think that deep structures would be sentences of a logical notation like first order logic, which would then suffer transformations (thought not to change meaning) to become natural language sentences.

It was also in this period that people were taking seriously the idea that deep structures might involve a semantically primitive vocabulary which decomposed normal words of natural languages by their definitions.\(^5\) Thus, the transformations would not only get the word order right, but would also generate words from basic lexical entries. Thus, one could have believed that “Bachelors exist” had a deep

\(^5\)See (Katz and Postal, 1964) for the classic instance of lexical-decomposition-as-semantics. For a very good criticism of this kind of thing see (Bolinger, 1965).
structure of the form \( \exists x (\neg \text{married}(x) \land \text{male}(x)) \)" or perhaps something even more exotic. Under this program the transformations were to generate the natural language sentence. Thus, I suspect that Dummett was thinking of what many people then thought of as deep structures during the time he characterized molecularism the way he did. If this is the way Dummett thought of a grammar during his writings, then it is even clearer that he needs many\(^6\) natural language predicates to be analytical words. For it was meaning-specifying definitions which were to govern the lexical decomposition (e.g. from \("\text{bachelor}(x)\)" to \(\neg \text{married}(x) \land \text{male}(x)\)”) on this picture. But then these definitions are analytic truths, and the words which required decomposition are analytical words in the above sense.

\[4.2 \text{ Why Dummett Can’t Have Analyticity} \]

In this section we shall have to take a step back in order to take two steps forward. I want to show that there is enough evidence from a variety of areas to support the claim that lexical meaning is not secured by one or more analytic truths involving the words in question. Given that in the last section I showed that Dummett was committed to such a view of lexical meaning, the evidence we shall assess will be evidence against the truth of Dummett’s view.

In short, I will attempt to show that Quine was right, in so far as he is correctly interpreted as having argued that the view of lexical meaning I have attributed to Dummett is mistaken.\(^7\) After showing this, I will not follow the famous generative semanticist who (right before the field abandoned Generative Semantics \textit{en masse}) stood back and said “Nyahl! Nyahl!” If the Dummettian view were correct, linguistics, psychology, and lexicography would be a lot easier to do. Moreover, the need for an integrated and explanatorily robust model of grasp of meaning is no

\(^6\)I want to say he needs \textit{most} natural language predicates to be analytical words. Only one who thinks that most words are such that ostensive abilities regarding them are sufficient for their grasp would quibble with this.

\(^7\)Not to be taken as an endorsement of Quine’s behaviorism, peculiar brand of naturalism, or pyrrhonism about meaning and anything related to it.
less pressing today than it was in Dummett's day, or if it is less pressing, this is only because the problems I call attention to are practical problems for giving an account of word and sentence meaning themselves, never mind discerning a proper philosophical interpretation of such an account.

That is, in spite of impressive progress in these areas since Dummett's time, one of the upshots of my discussion is that the pressing interesting and difficult questions are psychological and linguistic ones concerning what we might still call "the theory of meaning" itself, rather than philosophical problems about such a theory. As I have tried to show above in the case of molecularism, reifying and then philosophizing about programmatic suggestions concerning such a theory was not so irresponsible in Dummett's time. Given that a large body of linguistic researchers thought first order logic could provide the deep structure of sentences, and that transformations and backwards lexical decomposition could yield natural language sentences, it was not so irresponsible to philosophize about the representation of mathematical theories in first order logic as if we were talking about grasp of natural language.

Given the role of lexical decomposition in the early days of transformational grammar it was not irresponsible to assume that analytic sentences could individuate the meanings of words. I will not here go through the history of the failure of first order logic as deep structure (or even the failure of first order logic as logical forms, for that matter); nor shall I go through the history of the failure of the view that transformations (in so far as they are plausible at all) cannot be a priori restricted so as to preserve meanings.® But I will attempt to show why lexical decomposition, licensed by definitions which specify the necessary and sufficient conditions for the application of words, is bankrupt as a position that would hold for more than the most minimal fragment of natural language sentences, and in the process show why analyticity cannot do the work Dummett intended it to do.®

®When someone does this in a way accessible to current philosophers of language and mind, as well as meta-ethicists, I think the resulting hammer will shatter many a contemporary idol.
®I am not talking about the important decompositional analysis of aspect and aktionsart in (Dowty, 1979), which is immune from my criticism. Dowty's lexical decomposition captures im-
Before doing this I need to make clear that my discussion is not meant to justify pyrrhonism about meaning; nor is it meant as a criticism of branches of science where idealizing assumptions, inconsistent with the data presented, are utilized. In any branch of science such assumptions are a necessity, and there is no reason why psychology and linguistics should be any different. However, as I will try to show, some of the most exciting new work in these fields concerns what happens when these idealizations are relaxed.

Since I want to gather empirical evidence against the analyticity theorist from diverse fields, I shall have to talk initially about "concepts" rather than word meanings, given that some of the most trenchant criticism of the analyticity theorist arises once we take seriously the psychological work on concepts from the last thirty years. It will be clear from the following discussion that "the classical model" of concepts is Dummett’s model of word meaning, if we assume, with most of the researchers on concepts and word meanings, that word meanings can correctly be correlated with concepts without too much idealization.

§ 4.2.1 The Classical Model of Concepts

In this section I will draw together a number of positions to sketch what can fairly be called "the classical model" of concepts. Though the resulting picture may be something of a cartoon, it both represents a good foil for post-classical positions, and is similar enough to actual views to which post-classical theorists have been reacting. Thus, it is fair to lump the views together into one model.

Many of these views have quite a long and distinguished philosophical history, extending at least from Plato through the classical empiricists and rationalists all the way to Frege, and arguably in a revised version through to contemporary "externalist" views of concepts deriving largely from an influential paper by Hilary Putportant syntactic and inferential regularities in classes of English verbs.
Prior to discussing each individual view composing in the classical model, I first present them together.

(i.) **Intensionality**: A concept possesses an intension, which, given the way the world is, determines its extension.

(ii.) **Graspability**: Concepts can be grasped by human beings. One who grasps a concept can, *ceteris paribus*, recognize which objects fall under that concept and which objects don’t fall under that concept.

(iii.) **Expressibility**: Many, and perhaps all, concepts are expressible in language. For many a word, its meaning can be identified with a concept.

(iv.) **Determinacy**: For any concept $C$, and any (possible or actual) object $o$, it is determinate whether or not $o$ is a paradigm instance of $C$, a paradigm instance of something that is not $C$, or neither.

(v.) **Publicity**: If two people grasp the same concept then it is possible for someone to discover that they do, and if two people grasp different concepts, then it is possible for someone to discover that they do.

(vi.) **Bifurcation**: Concepts are either basic or nonbasic. Nonbasic concepts are those whose grasp presupposes grasp of other concepts.

(vii.) **Summary Representability**: When we determine that an object falls under a concept, we do this by determining that the object possesses features shared with all other objects in the extension of the concept. A theory of concepts should specify what these features are for nonbasic concepts.

(viii.) **Definability**: Nonbasic concepts can be defined with individually necessary and jointly sufficient conditions.

(ix.) **Inheritability of Features**: If the extension of a concept $X$ is always a subset of the extension of a concept $Y$, then the defining features of $Y$ hold of all of the objects in the extension of $X$.

Again, given the diversity of views within the philosophical, psychological, and linguistic traditions, my discussion will of necessity present something of a caricature.

The first traditional property of concepts is properly a metaphysical one. We can state it in this manner:

(i.) **Intensionality**: A concept possesses an intension, which, given the way the world is, determines its extension.

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10See (Putnam, 1975), (Burge, 1979), (Burge, 1982), (Burge, 1986a), and (Burge, 1986b).
derstanding the intensional nature of concepts only requires understanding what intensions do. This can be done by assuming, for \textit{reductio ad absurdum}, that the conceptual content of a word is in some sense a function of the set of objects that fall under the concept. On this assumption the conceptual content of \textsc{red} is a function of the set of existing red things. But if this were true it would not be possible that there existed a universe just like ours except where all Coca-Cola cans are green. But this would clearly be absurd. Or consider the concepts \textsc{renate} and \textsc{cordate}. Every object in our world that is one is the other. If conceptual content were a function of the actually existing renates and cordates, then the concepts \textsc{renate} and \textsc{cordate} would be identical, which is also absurd.

These considerations, as well as other perhaps more properly linguistic ones, have led many to identify intensions with extensions over possible worlds. However, the difficulty of imagining a possible world where one thing is triangular and another is trilateral, or where the square root of nine is not equal to three, suggest a more abstract approach to intensions. The best way to characterize intensions, given the linguistic, philosophical, and psychological uses to which they are put, need not concern us here however.\footnote{Throughout, words denoting concepts will be all caps and words denoting words will either be italicized, quoted, or neither (when use versus mention is clear from context, such as when talking about variables in a formal system, I will continue to follow the policy of allowing a word or variable to denote itself).} What concerns us is what is minimally being affirmed by \textit{(i.)}, and that is that a concept must be such that it makes sense to hold both that distinct concepts have (perhaps necessarily) identical extensions, and to hold that many perfectly reasonable ways in which the world might have been different involve objects falling in the extension of different concepts (such as the world where my shirt is not red, but green).

While the first property we have considered is a metaphysical one, the second property is more properly considered an epistemological one, as it is a traditional

\footnote{See (Bach, 1989) for a very nice, accessible discussion of linguistic motivations for characterizing intensions in various ways.}
presupposition concerning what properly constitutes possession of the concept by a human being. We can formulate this property in the following manner.

(ii.) 
**Graspability:** Concepts can be grasped by human beings. One who grasps a concept can, *ceteris paribus*, recognize which objects fall under that concept and which objects don’t fall under that concept.

Thus, if someone grasps the concept RED they can, in normal circumstances, recognize whether or not something is red. How extensive this recognitional capacity needs to be for someone to grasp a concept is one of the primary issues we shall discuss in what follows. Philosophers of language and mind who defend an “externalist” view of concepts typically attempt to show that people can grasp concepts without having the recognitional capacity adverted to above, though even externalists typically assume that the concept can be grasped by some person in the manner presented above.

It is somewhat unfortunate that, in much of the literature on concepts, the extent to which the following property holds is not openly discussed.

(iii.) 
**Expressibility:** Many, and perhaps all, concepts are expressible in language. For many a word, its meaning can be identified with a concept.

Lack of debate about this is unfortunate because, on the one hand, it might be optimal to describe people as grasping concepts which are not expressible, at least for them. For example, a person’s (or non-human animal’s, for that matter) competent ability to make color discriminations might be taken as evidence that they grasp the concepts HUE and SATURATION even if they are incapable of expressing these concepts. Also, some of the most vitriolic criticisms of post-Montague semantics assume, with very little or no argumentation, that the account of word meaning given by the semantics is simultaneously a complete account of concepts.\(^{13}\) On the other hand, expressibility and graspability jointly seem to entail that one who grasps the meaning of a word should be able to recognize which objects are described by that word, and it is not clear that this is the case. I competently grasp the meaning

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\(^{13}\)For example see (Lackoff, 1987).
of the word "gold" well enough to communicate with it in many situations, but I would have to consult an expert to determine whether or not my grandfather's ring is genuine.

This chapter is not the proper place to critique externalist accounts of concept possession, though it should be pointed out that acceptance of the following property of concepts by externalists might be good reason for characterizing philosophical externalism as a neo-classical variant of the classical view, rather than as a rejection of it.

(iv.) **Determinacy:** For any concept $C$, and any (possible or actual) object $o$, it is determinate whether or not $o$ is a paradigm instance of $C$, a paradigm instance of something that is not $C$, or neither.

Notice that determinacy does not prohibit concepts from being vague. Also notice that determinacy does not follow from the intensionality of concepts.

Concepts are typically assumed to be such that it is possible to determine whether or not someone possesses them. More precisely, this requirement can be given in the following manner.

(v.) **Publicity:** If two people grasp the same concept then it is possible for someone to discover that they do, and if two people grasp different concepts, then it is possible for someone to discover that they do.

With the assumption of expressibility, the publicity of concepts entails that word meaning is also public. In particular, this would entail that if two people mean something different by a word, then it should be the case that they can discover this fact.

Finally, it is often assumed that concepts can be divided into basic and nonbasic concepts.

(vi.) **Bifurcation:** Concepts are either basic or nonbasic. Nonbasic concepts are those whose grasp presupposes grasp of other concepts.

Thus, for example, one might want to say that RED is a basic concept, because one can possess the recognitional capacity appropriate to grasping RED (the ability to recognize of red things that they are red and non-red things that they are not).
without having to grasp any other particular concepts. One might want to say that JUSTICE, for example, is non-basic, since the recognitional capacity appropriate to grasping JUSTICE requires a rich conceptual background. Note that bifurcation on its own does not entail that nonbasic concepts are decomposable in any neat way. Indeed, the interesting recent holistic accounts of concepts are best represented as accepting bifurcation and rejecting traditional characterizations of the decomposability of concepts.\textsuperscript{14}

The three further properties of concepts which I wish to discuss are perhaps more properly thought of both as properties of concepts and constraints on an adequate description of concepts. In Smith and Medin's \textit{Categories and Concepts} they are given as summary representability, definability, and inheritability of features. These are characterized as the three properties of concepts which properly compose the classical view. Summary representability can be defined in this manner:

(vii.) \textit{Summary Representability}: When we determine that an object falls under a concept, we do this by determining that the object possesses features shared with all other objects in the extension of the concept. A theory of concepts should specify what these features are for nonbasic concepts.

Thus, for example, when we determine that something is a square we determine that it has four sides of equal length and interior angles of ninety degrees.

The next property stipulates a constraint on the specification of these features. Smith and Medin state that "the heart of the classical view" lay with this principle:

(viii.) \textit{Definability}: Nonbasic concepts can be defined with individually necessary and jointly sufficient conditions.

Again, the geometrical concept SQUARE illustrates this condition. All and only squares possess these features: closed planar figure, four sided, equilateral, and equiangular. It is interesting to note that the property of definability underlies traditional lexical decomposition approaches to word meaning. If definability is a property of concepts, and if traditional lexical decomposition were a good lexical

\textsuperscript{14}See (Murphy and Medin, 1985) and (Keil, 1989).
semantics strategy, then, by the expressibility constraint above, lexical semantics could easily be incorporated into a theory of concepts.

The final traditional property of concepts widely discussed in the psychological literature can be called inheritability of features.

(ix.) Inheritability of Features: If the extension of a concept $X$ is always a subset of the extension of a concept $Y$, then the defining features of $Y$ hold of all of the objects in the extension of $X$.

Again, we can illustrate this property with geometrical figures. Since all squares are necessarily rectangles, the above property predicts that all (possible) squares will possess the defining features of rectangles. Rectangles have the defining features: closed planar figure, four sided, and equiangular. These features are also possessed by all (possible) squares.

§ 4.2.2 Criticisms of the Classical Model

In this section I will present what I take to be the main evidence for the claim that the conjunction of (i.) through (ix.) above is untenable. This evidence comes from a variety of sources, and I have subdivided this section into philosophical, psychological, linguistic, lexicographic, and developmental criticisms.

It should be noted in advance that a defender of a variant of the classical view might simply dismiss the philosophical and linguistic evidence by engaging in a wholesale rejection of the property of expressibility. This is a possible dialectical move, as the following linguistic and philosophical evidence uniformly causes problems for accounts of word meaning which embody the above assumptions about concepts. So if one wanted to hold that word meanings in no way corresponded to concepts, then one might retain the classical account of concepts in light of the philosophical and linguistic evidence. On the other hand, one might respond to the psychological criticism by likewise denying expressibility. This would be one way to insulate the use of analytically true sentences in individuating word meanings from results in psychology that are prima facie in tension with such use.

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While this would be a possible dialectical move, it is not a very satisfying one. Almost all the psychological data on concepts involves the use of language by the test subjects. If we give up expressibility then we are left at a loss regarding how such data should be interpreted as bearing on concepts. This is not to say that it is not an open question to what extent concepts are expressible (and to what extent meanings are conceptual). But if we don't begin with the defeasible assumption that some such link exists, then it is unclear how to proceed either in forming a scientifically coherent psychological theory of concepts or, given the Chomskyan assumption that linguistic ability is a function of tacit knowledge of a grammar, in doing linguistics. Thus, the person who would reject expressibility wholesale incurs the task of producing a theory that explains both the psychological and linguistic data without assuming that concepts are expressible. Pessimism should be our default assumption here.

In what follows I shall assume that some form of expressibility holds of concepts, and thus both that philosophical and linguistic evidence is highly salient in the quest for an adequate theory of concepts, and that psychological evidence is highly salient in the quest for an adequate account of word meaning.

§ 4.2.2.1 *Philosophical Criticisms: Underdetermination and Inextricability*

The first philosophical criticism has its origin in Mark Wilson's "Predicate Meets Property." Wilson argues that many word meanings do not possess the property of determinacy given above. This is to say that for many words there are objects that are capable of being considered either as paradigm instances to which the word correctly applies or as paradigm instances to which the word does not apply.

It is not difficult to find examples which support Wilson's hypothesis. Consider painting. Prior to the advent of abstract expressionism the meanings of the word "painting" did not determine one way or the other whether or not abstract expressionist paintings should be considered as paintings. Indeed at every significant
juncture in the history of painting, there have been critics who argued that the paintings of the newer kind should not be considered paintings, in the aesthetically relevant sense of the word. Or consider "number." Today complex numbers are paradigm instances of "number," but this wasn't always the case. Indeed, from the time the Pythagoreans allegedly drowned the person who discovered that the square root of two is not expressible as a fraction, people who try to introduce new kinds of numbers have a hard time of it (the names of proposed new numbers are telling: "negative," "irrational," "imaginary," "non-standard," "surreal"...).

The relevant point is that nothing in the meaning of the word "number" uniquely determines whether or not the elements of many new kinds of structures get to be numbers. Most, though not all, mathematicians would say that transfinite numbers are numbers. Hamilton's quaternions, to his dismay, did not measure up to the title even though they behave like complex numbers in many respects. For many other set theoretic structures, the jury is still out.

While the property of underdetermination of word meaning is extremely important and often neglected in philosophical discussions, its relevance for psychological modeling of concept possession and linguistic modeling of word meaning is not immediately clear. For example, with successive models of atoms, from Democritus through Bohr through contemporary quantum theory, we can always ask whether the newer view is attributing properties to the same thing or talking about new things. There may be no fact of the matter about whether ideology or ontology is shifting as the world and our theory about it evolves. While this is of great philosophical interest it may not be of psychological or linguistic interest.

Psychological models of concept possession attempt to predict data concerning how people categorize and use concepts; linguistic models of word meaning attempt...
to predict correct inferences involving the words in a manner that allows the words’ meanings to compositionally contribute to the meanings of the sentences they occur in. It may be the case that underdetermination of word meaning may only affect psychological and linguistic behavior in degenerate cases which should not be predicted by the theory. Thus, the idealization of word meaning and conceptual content rendering both determinate may be well motivated, given the goals of the model-building in psychology and linguistics.\(^\text{16}\)

The other philosophical criticism of the classical view, suggested by the above reference to atomic theory, stems from some of the work of Quine.\(^\text{17}\) Quine argued that word meaning had the property of being inextricable from broader theory. This property is best understood to be a sort of limitation result for lexicography. A priori, it is plausible to hold both that dictionary definitions for words contain information specifying what the words’ meanings are, and that knowledge of a dictionary definition should be thus sufficient for grasp of the meaning of the word defined. However, we can ask how much information about a given word should be included in a dictionary. On the one hand, for easy nouns, one might just include the most relevant superordinate noun and conditions which individuate objects in the extension of the noun in question from other objects which are not in the extension of the noun but are in the extension of the superordinate. On the other hand, information of this sort is not necessarily sufficient for grasp of the meaning of the word defined. If a dictionary had to provide enough information to enable one who didn’t know what the word meant (but could understand the definition) to learn the word-meaning by consulting the definition, then each entry would begin to look more like an encyclopedia article. Then our intuition would be that information not strictly about the word’s meaning is being included in the entry.

\(^{16}\)I do think Wilson’s discussion is extraordinarily salient for formal pragmatics. In conversation we often strive to maximize shared meaning, if not always agreement on truth value.

\(^{17}\)See, in particular, Chapter II of (Quine, 1960). It might be the case that Hempel should be credited with the first sustained argument for the inextricability of word meaning from background theory. See (Hempel, 1951).
If word meaning is genuinely inextricable from background theory (or, more epistemologically, if grasp of word meaning is inextricable from background world knowledge), then it follows that there is no principled technique by which a lexicographer can determine, for each word, exactly how much information should be included in a dictionary definition. Again, it is unclear without further argumentation how moved psychologists and linguists should be by this problem. While it does express a practical problem which must be dealt with, the theoretical contexts in which psychologists, linguists, and lexicographers work certainly pick up much of the alleged Quinean slack. It should be intuitively obvious that the clearer the theoretical desiderata of a given model of concepts or word meanings are, the easier it is to make principled distinctions between word meaning, or conceptual content, and background theory.

Thus, the analyticity theorist might respond to the Wilsonian and Quinean philosophical criticisms by saying that the analytic-synthetic distinction is still very useful for explaining word meaning and grasp of word meaning in the range of cases which are relevant to the sciences of mind and language. However, in what follows I will provide a variety of reasons why one should think that this response to Wilson and Quine is not sanctioned by reasonable idealizations and appreciation of the explanatory reach of psychology and linguistics. Thus I will be able to provisionally conclude that there is no useful theoretical context in which the simple-minded property of definability, given as property (viii.) above, is salvageable from the Quinean critique.\textsuperscript{18} In fact our conclusion will be that some of the most interesting contemporary psychological work on concepts can be understood as being moved by something like

\textsuperscript{18}I'm not claiming that this evidence is decisive. Quine and Wilson are making empirical claims, and I think that consulting the relevant empirical theories does help in evaluating such claims.

There is a subtle burden-of-proof issue here. My opponent can always say that Dummett's positions don't have the empirical entailments I think they should... One way to get around this impasse is to view the evidence I present as merely suggesting how a non-Dummettian theory of meaning might proceed. Thus, rather than refuting Dummett's positive claims, I can be seen as presenting plausibility considerations for a non-Dummettian view. On one construal of Dummett's challenge to the truth-conditional semanticist, this is exactly what is needed anyhow.
these Quinean qualms. Holistic views of concepts attempt to model what would have traditionally been viewed as additional world knowledge, where this knowledge is taken as being particularly salient for concept possession. For example, to understand what a bird is you must not only know that birds have wings and that most birds fly, but also that the birds' movement of their wings is causally efficacious for flying. Thus, I would suggest that Quinean skepticism about principled necessary and sufficient conditions for concepts is helpful in discerning which other conditions (possibly neither necessary nor sufficient) are necessary for grasp of the concept.

§ 4.2.2.2 Psychological Criticisms: Simple Typicality Effects, Family Resemblance Data, and the Necessary Use of Non-necessary Features

Here I will briefly explicate some of the most salient early psychological data which cause problems for the classical view of concepts. These data are divided into three sets: simple typicality effects, family resemblance data, and the use of non-necessary features. I will argue that, while the classicist might have principled responses to any one of following data sets, all three together provide good evidence that the classical view is false.¹⁹

Many psychological studies have shown that, for a set of concepts (such as DOG, WHALE, etc.) all falling under a given superordinate concept (such as ANIMAL) people rank the subordinate concepts as being better or worse instances of the superordinate concept with a high degree of uniformity. For example, robins and sparrows are nearly uniformly characterized by test subjects as better instances of BIRD than chickens are. Then, in independent studies, the resulting typicality ratings are shown to be highly predictive in a number of ways. Much of these data concern reaction times. For example, robins and sparrows are categorized as birds more quickly than chickens are. Moreover, it has been shown that rank in a list of subordinate concepts (determined by asking different groups of subjects to list and rank a set number of

¹⁹Much of this discussion is taken from (Smith & Medin, 1981).
subordinate concepts) is a very good predictor of reaction time in categorization as well as errors in categorization (likelihood of wrong categorization increases the lower a subordinate concept is ranked). It has also been discovered that the more typical members of superordinate concepts are the ones learned first by children, as well as the ones most likely to be given first when subjects are asked for instances of the concept.

While these results do not necessarily show the classical view of concepts to be false, they do show that the classical view is insufficient for psychologically modeling categorization behavior with concepts. The data strongly suggest that when people decide whether or not a given concept is subordinate to another they do not merely consult the defining features of each concept and make a decision. By (ix.) above the defining features of the superordinate concept are shared by all of its subordinate concepts. Thus, one would expect the reaction time data to be different from what they are, if the classical view were correct.

The classical theorist might attempt to explain the above categorization and reaction time data in some way not inimical to the classical view. However, Rosch and Mervis proposed and verified an explanation for the data which causes further problems for the classicist. Rosch and Mervis hypothesized that better instances of a superordinate concept are those that have the most salient features in common with each other and the fewest salient features in common with objects not in the superordinate concept. Then, subordinate concepts ranked less typical are those with fewer salient features in common with each other and more salient features in common with concepts not subordinate to the superordinate concept.

Rosch and Mervis' hypothesis was amply verified by a number of interesting studies. For example, when they had subjects list features of concepts subordinate to the concept FURNITURE (such as CHAIR, SOFA, CUSHION, RUG, VASE, and TELEPHONE),

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20 The Chomskyan temptation to save one's theory from data by appeal to the performance-competence distinction begins to loom large. In section 4.2.2.3 I discuss this.

21 See (Rosch & Mervis, 1975).
they found that CHAIR had listed the greatest number of features that were common to other furniture concepts, and TELEPHONE had listed the least number common to other concepts subordinate to FURNITURE. From data such as these it is easy to construct an intuitive family resemblance measure, which is a function of how many times a feature gets listed as well as whether or not it is listed for the given subordinate concept. In the experiments, this measure ended up being a very good predictor of the typicality ratings that subjects give.

One of the main difficulties in insulating the classical theory from the family resemblance data stems from the fact that many of the features typically listed by subjects in the kinds of experiments considered are not necessary for an object falling under the extension of that concept. For example a robin with no wings is still a robin, even though it cannot fly. Moreover, when subjects are told to list salient properties of superordinate concepts such as BIRD, FRUIT, and TOOL the features they list are clearly not necessary conditions. However, the features they list do predict categorization times for categorizing subordinate concepts as subordinate to the superordinate concepts. That is, longer reaction times are correlated with fewer features being shared by the subordinate and superordinate concept.

At this point it is fairly clear that the classical model is insufficient as a psychological model of concept possession. At the very least, a theory constructed in the manner suggested by the classical model does not provide enough information to predict the observed data. Predicting the data would involve ranking features that are neither necessary nor sufficient for the occurrence of an instance of the concept in question, and a theory built on the classical model would only provide necessary and sufficient conditions for the occurrence of instances of the concept in question.

Moreover, if the philosophical problems discussed above are psychologically relevant, they might suggest that the concepts grasped are not things with necessary and sufficient conditions of the kind determined by the classical theory. One of the philo-

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22See, for example, (Hampton, 1979).
sophical virtues of Rosch’s work is that it suggests what features of concepts allow us to grasp them and, via expressibility, allow us to communicate using them, even if there are no necessary and sufficient conditions which fix determinate intensions.

However, the considerations we have covered thus far leave the classical theorist in psychology and linguistics, and the analyticity theorist in the philosophy of language, with a reasoned response to Rosch’s work. Perhaps Rosch’s work is only relevant to the graspability (property (ii.) above) of concepts. Perhaps Rosch and Mervis merely isolated heuristics used when people defeasibly identify objects as being instances of concepts, and perhaps the weighted feature structures that prototype theorists trade in are merely representations of such identification procedures.

If this is the case, the classical theorist could defend a two-factor view of concepts and word meanings, according to which a concept carried with it an (analytically true) definition, as well as an identification procedure. Presumably the analyticity theorist could still hold that the majority of words in a natural language are analytic words in the sense considered above, repeated here.

Analytic Word
A word $Q$ is an analytic word if, and only if, a set $\Phi$ of analytic sentences involving $Q$ is sufficient to individuate the meaning of $Q$ from that of other words in the language; (on the assumption that they agree about the meanings of the words in the sentences in $\Phi$ other than $Q$) two speakers disagree about the truth value of any of the sentences in $\Phi$ only if they disagree about the meaning of $Q$.

This kind of divorce between a word’s meaning (that which is described by the set of individuating analytical truths) and what is actually inside the head of most speakers who use the word (identification procedures) is not an uncommon view these days.\textsuperscript{23} However, the analyticity theorist who pursues this line might have to tell a story about whether it is possible for two speakers to possess different identification procedures for a word and still agree on the truth value of all of the

\textsuperscript{23}See (Putnam, 1975) for the first sustained argument to the conclusion that “meanings ain’t in the head.”
relevant analytical sentences involving the word. In any case, I will attempt to show in later sections that such a neo-classical two-factor view of word meaning and conceptual content is insupportable for other reasons.

§ 4.2.2.3 Linguistic Criticisms: Scalars and Hedges

One intuitive attempt to insulate linguistic theory from the Rosch-Mervis data involves a somewhat heavy handed use of the performance-competence distinction. In Chomskyan linguistics, facility with language is understood as holding in virtue of a speaker's tacit knowledge of a grammar of the language in question. It is a basic, and sound, methodological principle of this approach to hold that the grammar tacitly known does not in any straightforward way describe the language spoken and understood by speakers of the language. Rather, at best the grammar can be understood to describe the verbal behavior of an ideally competent speaker. Such a speaker is thought of as not being subject to any cognitive shortcomings whatsoever. For example, the ideally competent speaker is understood as being able to recognize as sentences strings of arbitrary length. The ideally competent speaker is also understood as immune to mistakes due to "noise" such as low blood sugar and inattentiveness. Of course no actual human speaker is ideally competent in this way. However, the behavior of the ideally competent speaker must be connected to the behavior of speakers of the language in question when performance limitations are minimized in such a way that it makes sense to attribute tacit knowledge of the grammar to ordinary speakers.

Without digging too deeply into the interesting philosophical and methodological issues raised by linguists' (essentially Aristotelian) use of the distinction, we can begin to see how it might provide a principled rebuttal to those who holds that the Rosch-Mervis data are relevant to accounts of word meaning. One could hold that these data are measuring performance limitations and that, given this, it is of no concern to the generative linguistic enterprise (understood as an attempt to discern
the correct competence theory). That is, one might hold that the classical theory of concepts, utilized in an account of word meaning, describes the conceptual architecture of the ideally competent speaker. With this train of thought the observed data merely measure how people cope with performance limitations. On this view, the real conceptual content of the word "bird" could be given by a definition specifying necessary and sufficient conditions, but real world humans' utilization of weighted feature sets in their attempts to determine which objects are birds is merely a matter of performance limitations.

Given the importance and fruitfulness of the performance-competence distinction in linguistics, such a view would not be wholly without principle. However, in this section I will argue that such a view is false. To do this I will present simple linguistic data which show that the classical view of concepts (or rather accounts of word meaning appropriate to it) is insufficient for describing phenomena within the realm of linguistics proper. That is, even the ideally competent speaker is such that the architecture of her lexicon is not the architecture suggested by the classical view of concepts.24

It is extraordinarily difficult to devise tests which segregate comparative adjectives from non-comparative adjectives. We want to say that words such as "heavy" and "red" are comparative, while words such as "unmarried" and "dead" are not comparatives. However if we say that an adjective $A$ is comparative if and only there exists a context where it can be truly said of two objects $x$ and $y$, that $x$ is more $A$ than $y$, then "married" and "dead" end up being comparatives. There are contexts where I can say that one person is more married than another person, and moreover, in the context my interlocutor understands what I'm saying. I'm saying that the first person has more of the salient features of married people than the second person does.

24Though the discussion that follows is limited to scalars and hedges, a fuller discussion would include metaphor and other non-literal facets of meaning, compound nominals generally, and quite possibly so-called theta-roles as well.
Such features cannot be the necessary and sufficient conditions specified in a theory implementing the classical model. If they were, then any time I said that Frank is more married than Sam it would have to be the case Sam is unmarried, since he would then be lacking features necessary for being married. But the sentence “Frank is more married than Sam” could be truly uttered if by it I meant to communicate that Frank behaves in ways more stereotypically associated with being married than Sam. Thus, at best the classical view would have to treat more married than as a primitive lexical string with a meaning of its own, unrelated to the meaning of married and more... than...

Clearly, such a treatment would be horrible methodologically, as primitive meanings are being multiplied without limit. Moreover, if the meaning of more married than is taken to be a function of the meanings of married and more... than... then there is some hope of revealing how such judgments work in a compositional semantics. Doing this would necessarily involve encoding the weighted features that Rosch and Mervis captured in the lexicon. Adequately generating the truth conditions of the comparative requires sensitivity to the family resemblance data that psychologists have gathered.

Another example of comparatives whose truth conditions can be predicted from the psychological data concerns judgments such as “A chair is more a piece of furniture than a telephone is.” Though this is slightly anomalous syntactically, most competent speakers recognize that it is true. Or consider the sentence “A chair is a better piece of furniture than a telephone.” Rosch and Mervis’ family resemblance measure, and the way it is derived, tells us exactly why these sentences are true. Again, the classical view lacks the resources to adequately model the truth conditions of the sentence in question.

Finally, consider the sentence, “This is a better apple than that is a fire engine.” Though the truth conditions of this sentence are surely vague, there are plenty of contexts in which the sentence could be taken to be truly uttered. Sentences such
as this suggest that there is some kind of common metric that enables judgments of relative prototypicality of instances of different properties, even when the properties are very dissimilar. Moreover, competent speakers can explain their judgments with respect to all of the above sentences by saying things such as “This is a better apple than that one, because this one is red and that one is brown.” It would clearly be advantageous if a lexical semantics could model such sentences in such a way that sentences such as “Red apples are better than brown apples” might come out as true (in a given context) in virtue of the meanings of the words involved.

Now we turn to the important phenomena which George Lakoff showed to be fairly pervasive in “Hedges: A Study in Meaning Criteria and the Logic of Fuzzy Concepts.” Hedges can be any of a number of natural language constructions (mostly adverbs in English) which, when occurring in phrases, are best understood as operating upon the kinds of weighted feature sets that Rosch and Mervis showed are used by people while categorizing. Here I will just discuss four such: *technically*, *loosely speaking*, -ly, and very. Consider the following four sentences

1. Technically, a whale is a mammal.
2. Loosely speaking, a whale is a fish.
3. Sally is grandmotherly.
4. That is a very good violinist.
5. That violinist is very good.
6. Frank is very good.

“Technically, a whale is a mammal,” seems to communicate that by some necessary and sufficient criteria for being a mammal, whales are mammals. It also implicates, in the Gricean sense, that whales are lacking in some prototypical properties associated with mammals. This is implicature, I think, because it would not be false to say “Technically a guppy is a fish” but rather infelicitous in most contexts. Most hearers assume that guppies satisfy necessary and sufficient criteria for being a fish in virtue of the fact that guppies possess the relevant prototypical properties.25

25Lakoff’s original article came out prior to wide circulation of Grice’s work. Thus, he assumes that the whale’s lacking prototypical properties of fishes is part of the truth conditions of the sentence in question.
However, prototypicality effects can not all be consigned to pragmatics.26 “Loosely speaking, a whale is a fish,” is true if and only if whales do possess a sufficient number of prototypical fish properties. The sentence also seems to communicate that whales in fact lack some of the necessary conditions for being a fish, but I am not sure whether or not this should be considered part of the truth conditions of the sentence, or whether it is again part of what is implicated by the sentence. “Loosely speaking a guppy is a fish” strikes me as false, so I am inclined to say that lack of some necessary conditions is part of the truth conditions of “loosely speaking” sentences. The truth value of “Sally is grandmotherly” is solely a function of whether or not Sally possesses the properties prototypical to grandmothers. Note that “My grandmother is not very grandmotherly” is grammatical, and could in fact turn out to be true.

Finally, “very” functions to, so to speak, “tighten up” the criteria for applying the embedded noun phrase. While the classical view may be able to make sense of this with standardly scalar adjectives, that “very” can apply to just about any adjective and yield a meaningful phrase highlights the same kinds of shortcomings in the classical view as did the previous discussion of scalars. If one says that Frank is very American, most people understand what is communicated. In many cases the relevant scale on which “very” operates will be determined by the kinds of weighted feature sets predicted by the psychological data. Thus, once again we see that the psychological data have relevance for generative linguistic theory.

With, as far as I can tell, the majority of linguists who tread in these waters, I think that this collection of data shows incontrovertibly that Rosch’s prototype phenomena have strong linguistic reflexes and need to be modeled by an adequate lexical semantics. However, it does not undermine the two-factor theory considered

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26By “consigned to pragmatics” I do not mean to be reinforcing the popular myth of the “pragmatic trash can” where phenomena which are not treated by a semantics can be thrown away and forgotten. Even if all of the phenomena I mention here, including my discussion of comparatives, ended up being more properly pragmatic rather than semantic, this would not affect my main point, which is that a classical architecture cannot be used to formally model these phenomena.
above. In fact, the hedge "technically" seems to provide evidence for the two-factor theory. It seems that "technically" works as a function from the identification function factor of meaning, to the definitional factor of meaning. Thus, it seems that there is an important role for the analyticity theorist, and our criticism thus far is just that the classical model is insufficient on its own as a model of concepts and word meaning. At the end of section 4.2.2.5 I shall show that this is the wrong moral to draw from "technically" and suggest an interpretation of the hedge distinct from Lakoff's original. However, at this point we have still not established that the analytic-synthetic distinction has no useful scientific role.

§ 4.2.2.4 Lexicographic Criticisms: Polysemy and Inextricability Revisited

One might think that the classical theorist, at least when worried about word meanings and not concepts, can take refuge in lexicography. Prima facie it is the case that all words of natural languages are analytical words in the sense specified above. This is because dictionaries provide criteria by which distinct senses of a word can be differentiated from each other and from senses of other words. That is, prima facie, dictionaries do provide necessary and sufficient conditions for (all of the different senses of) the words they define. Thus, the classical theorist about word meanings need only say that all dictionary definitions are analytically true, and in accordance with standard lexicographic norms, that the definitions individuate all of the different senses (meanings) a word has from all of the other different senses. This is important because one might be tempted to think that our troubles so far come from an inability to appreciate the rampant polysemy in natural languages. Perhaps the problem is one of overdetermination rather than Wilsonian underdetermination, and once word meanings are disambiguated Wilson's criticism of the classical view will be much less pressing, confined perhaps to interesting issues in the philosophy of mathematics and science, but appropriately insulated from psychological and linguistic theory.
I don't think so. If we attend to the two biggest practical problems facing lexicographers as they construct dictionaries, we see very quickly that dictionary definitions do not support the claims about competence that the analyticity theorist would have them. Typically lexicographers begin by doing large corpora searches over some body of text to find examples of usage of the words they are interested in. Then they attempt to differentiate the distinct senses of the words by putting the examples of usage into different groups of word senses. The definitions they give are each to reflect a different sense. Good so far. It looks like the lexicographic response to polysemy can defend the classical theory of word meaning, at least in-so-far as the Dummettian molecularist needs to avail herself of the analytic-synthetic distinction.

But this is mistaken. In fact, over two hundred years ago Samuel Johnson, after working some eight years, wrote as much in the Preface27 of his dictionary:

kindred senses may be so interwoven that the perplexity cannot be disentagled, nor any reason be assigned why one should be ranged before the other... The shades of meaning sometimes pass imperceptibly into each other, so that though on one side they apparently differ, yet it is impossible to mark the point of contact. Ideas of the same race, though not exactly alike, are sometimes so little different, that no words can express the dissimilitude, though the mind easily perceives it, when they are exhibited together; and sometimes there is such a confusion of acceptations, that discernment is wearied, and distinction puzzled, and perseverance herself hurries to an end, by crowding together what she cannot separate.

If the situation is any different today, this is just because attempts to build lexical knowledge bases from machine readable dictionaries has led to a robust verification of Johnson's claim. In “The Contribution of Lexicography” Sue Atkins shows how six different standard dictionaries (Collins Cobuild English Language Dictionary (1987), Collins English Language Dictionary (1986), Longman Dictionary of Contemporary English (1987), Webster's Ninth New Coeggiate Dictionary (1983), Oxford Advanced Learner's Dictionary (1989), and Webster's New World Dictionary

27Quoted in (Atkins, (1991, p. 61)).
each give distinct definitions of "acknowledge," "admire," "admit," "danger," "reel," and "safety." She shows how there is little systematicity in how the senses of these words are distinguished by each dictionary, and, indeed, within each dictionary.

Atkins concludes from her study and others that dictionaries should properly be understood as not stating "the meaning" of words in the sense of actually distinguishing all the senses of a word from each other and from the senses of other words, or in the sense of stating what a competent speaker needs to know about the usage of a word in order to understand it. For her and other lexicographers, this is because their experience with lexicography suggests that word meaning is not like that. Atkins writes,

Stock (1984)... points out that "not all citational evidence can be clearly disambiguated in terms of lexicographic senses." And therein, I believe, lies the heart of the matter. The traditional dictionary entry is trying to do what the languages simply will not allow. Word meaning cannot be sliced up into distinct bundles, labeled (however carefully) and packaged into a dictionary entry that will tell the truth, the whole truth, and nothing but the truth about the word. This of course is not news: but it did not matter so much when the only user of the dictionary was a human being, with a human being's innate knowledge of the way language works, of its secret passages from one sense to the next or one word to another, the ebbing and flowing of its word meanings, its flitting associations and known-but-not-known relationships. The advent of the machine-readable dictionary and the user-computer changes all that. (Atkins, (1993, p. 52))

Atkins is not attacking the very idea of the construction of a lexical data- and knowledge-base, or the very idea of using machine readable dictionaries to do so. In fact she goes on to contribute to this task further in the article. Nor should my discussion be read as an attack on the exciting recent work in lexical semantics, where generative principles for constructing one word sense from another (either generally or requiring contextual selection) are being investigated.

^28See (Hanks, 1979), (Hanks, 1987), (Hanks, 1994), (Krzesowski, 1990), and (Bolinger, 1965) for more evidence.
The main question we are concerned with is whether or not it is correct to think of a word’s meaning or, given polysemy, meanings, as being individuated by sets of analytical sentences the word occurs in, so that: (1) grasp of a set of such analytical sentences is necessary and sufficient for grasping the word sense in question, and (2) anyone who is committed to denying one of the analytical sentences or characterizing the word senses so that different intensions are determined has to mean something different by the word. A good explanation for why dictionary definitions of the same word differ so much from dictionary to dictionary is that most words don’t have such sets of analytical sentences which differentiate their different meanings from the meanings of other words. On the other hand, if philosophers who try to use the analytic-synthetic distinction to explain meaning individuation and grasp of meaning are right, then lexicographers and lexical semanticists are somehow inexplicably stupid for not being able to figure out in over two hundred years how to pick out the correct definitions of words. I don’t think that this is the case.

The second major practical problem which lexicographers face leads directly into a denial of the Extricability Thesis. Sometimes this problem is given in terms of how to differentiate a dictionary entry from an encyclopedia entry. Suppose we accept that in a dictionary each entry should give information necessary and sufficient for grasp of a word’s meaning. Then we are faced with the question of just how much information this is. I’d like to write, “of course, there is no principled distinction between the amount of information necessary for grasp of word meaning, and collateral information about the world” but the “of course” might be misplaced. Again, I will just say that there is widespread agreement among lexicographers that there is no principled division on this score.²⁹ Most include enough information to differentiate the different senses they have isolated in an act involving both creativity and discovery; these senses are to be of help to the intended user of the dictionary according to the manual that the dictionary’s publisher provides their lexicographers.

²⁹(Craig Roberts, (p.c. 1997)).
Deciding how much information to include is still then as much art as science, as well as relative to the purposes of the intended users of the dictionary in question.

Think of a word like “atom.” Most Americans learn Bohr’s model of the atom in high school and believe quantum reality to be like a lot of little solar systems. Do they have false beliefs about atoms that actually exist, or do they falsely think that something else exists? If they mean by “atom” what physicists mean, it would be the case that they have false beliefs about atoms that actually exist. If they don’t mean by “atom” what physicists mean, then their beliefs aren’t about what actually exist. Is this a difference that makes a difference? I’m not sure, but it does seem much more plausible than not to hold that there is no clear demarcation for how much one needs to know about atoms in order to be attributed grasp of the word in normal circumstances. Again, my reasoning is not just the result of thought experiments such as these. Rather, it is inductive. If lexicographers and linguists think there isn’t a principled distinction between meaning-specifying information, and collateral information, there probably isn’t one.

At this point we have begun to marshal evidence against the neo-classical two-factor theorist who would explain one part of word meaning and conceptual content with the classical model. If Atkins is correct, it is extremely misleading to think of dictionaries as giving different sets of sentences analytically true of (disambiguated) words. A dictionary definition simply does not do what the definitions of a classical theory of concepts or word meanings are supposed to do. Moreover, the fact that lexicographers are unable to get dictionaries to do this provides evidence for the claim that it can’t be done.

Rather than isolating all of the different senses of a word’s meaning and distinguishing these senses with definitions (which are analytically true in the sense considered above), dictionaries provide information concerning the usage of words with the purpose of helping readers use these words in a manner approximating predominant usage in the intended linguistic community. If the intended community is,
for example, a technical one, then the dictionary will begin to resemble an encyclo-
pedia. If the intended community includes fifth graders, much less information will
be included. That the lexicographers' task constitutively involves what might be
called spontaneity as well as receptivity in the way considered (isolation of senses,
and amount of information to include about a word's usage), is strong evidence that
word meanings and concepts, whatever they may be, are not analytic words in their
natural psycholinguistic home.

§ 4.2.2.5 More Psychological Criticisms: Acquisition of Concepts and Word Mean-
ings

The analyticity theorist is likely to get nervous at this point. It seems that our
considerations have forced us to affirm some sort of Quinean holism\(^{30}\) about word
meaning. Should we then proceed to replace a scientific approach to word meaning
and conceptual content with fuzzy metaphors of webs and Neurathian boats? Should
we just content ourselves with cautioning those who want to engage in productive
theorizing about word meaning that a word's meaning depends upon the meanings of
all of the expressions in the language the word occurs in? Is the Quinean limitation
result for lexicography a limitation result for cognitive science as well? If this were
the case one might reasonably hold that we should try a little harder to make a
two-factor theory of content and word meaning work. Fortunately, though, the
Quinean limitation result is not a limitation result for cognitive science, and is in fact
highly salient for philosophically understanding recent developments in the theory
of concepts.

A great deal of recent research on acquisition suggests that concepts and word
meanings are best thought of in a hybrid manner, as both answering to Rosch-
Mervis prototypicality effects, as well as effects specific to the theoretical domain in
which the concepts occur. In his recent paper, "Explanation, Association, and the

\(^{30}\) "Fuzzy molecularism" is a much better name for the position I defend.
Acquisition of Word Meaning" Frank Keil draws from his and others' research about acquisition to argue persuasively for the following three theses:

1. [The correct view of concepts] must ultimately characterize adult concepts as intrinsic mixes of two different sorts of relations: (a) those involving domain-general tabulations of frequencies and correlations, such as done by associative models and many connectionist systems, and (b) those involving domain-specific patterns of explanation, usually of a causal nature.

2. Empirical results from early cognitive development and first word meanings suggest that by the time the first words are acquired, most if not all concepts have this intrinsic mix even though changes in the nature of the mix can produce marked developmental change in apparent concepts, word meanings, and their use.

3. The intrinsic mix and its early appearance suggests a different kind of ambiguity in word meaning, wherein largely overlapping sets of instances have the same label applied to them but have different meanings, allow different patterns of induction, provide different categorizations of critical test cases. Unlike classical lexical ambiguities there is no dramatic change in the class of referents even as there is a discrete change in meanings.

(Keil, (1994, p. 170))

While part of the theory involving domain-specific patterns of explanation has, by Keil's own admission, not been adequately formalized, the evidence for the kind of hybrid view he is defending is compelling.

With the classical view of concepts children were typically understood as learning concepts by making inductions concerning which of the necessary and sufficient conditions were appropriate to the concept in question. Thus on the classical view, a child learning the concept SQUARE would, like a little scientist, need somehow to inductively discern that things which are not spatial (such as ideas, or sounds, or smells), not equiangular, not four sided, and not equilateral cannot be squares.

Much early research on learning, assuming the classical view of concepts to be correct, created somewhat artificial concepts such as BLIKS (e.g. large blue triangles) and recorded the inductive behavior of subjects with regard to the artificial concepts. However, the kind of research considered above led the majority of early post-classical researchers to attempt to model concepts essentially with sets of weighted features
which did not need to be either individually necessary, nor jointly sufficient for application of the concept.

Limitations of such prototype, or exemplar, models have led to the interesting recent concepts-in-theories view. Prototype views which explain concepts as lists of weighted features fail for two reasons. The first is that they do not adequately explain why certain features get selected as relevant in the learner's inductive practice.

In earlier research Keil realized that from kindergarten to around sixth grade the criteria children use to determine whether an object falls in the extension of a word shift drastically for many words. For words like "uncle," smaller children nearly uniformly thought that unrelated friendly, gift-giving, middle aged men were uncles while younger, meaner male siblings of their parents were not. At a certain age the criteria shift so that children make the correct categorizations.

When Keil initially published his work, he and other researchers explained this as a "characteristic to defining" shift which is part of lexical and conceptual development. On this picture, small children are phenomenalistic beasts, who slowly come to appreciate the proper definitions of words. This was mistaken, as the purely phenomenalistic picture failed to explain the stability of the younger children's categorization behavior with "uncle." For example, no child thought that uncles needed to wear glasses, even if every uncle they had been exposed to wore glasses. In addition, Keil and others ran up against the problem familiar to lexicographers stated above, in that very few words in any natural language have definitions which uniquely specify an intension. Thus the "characteristic" part of the "characteristic to defining shift" model was mistaken in its associationistic explanation of young children's concepts, and the "defining" part was mistaken in its use of the analytic-synthetic distinction to explain older children's concepts.

The first failure led to, and was part of, a great deal of research on how and why young children pick the features they do in their categorization behavior. This research has led to evidence for the claim that prelinguistic children of even six months
are sensitive to different appropriate forms of causation for different objects, such as that between humans and inanimate objects. A wide variety of data has shown that children in some sense bring to the acquisition process something very much like distinct theories of causation for physical objects (efficient causation), humans (mental causation), plants and animals (teleological causation), and artifacts (functional causation). Thus, in explaining why children are able to learn categorization behavior with word meanings as well as the very robust set of *a priori* beliefs involving word meanings, ineliminable reference must be made to domain specific causal principles.

Thus the child’s earlier use of the word “uncle” is best thought of as the child placing the word in an explanatory system which explains important social interactions at an earlier age. The “shift” comes from learning to think of people in biological/functional terms and learning a theory of kinship relations for people. Some such theories, such as that of kinship, provide words with plausible necessary and sufficient conditions, but according to Keil this is the exception rather than the norm. (As pointed out above, in mathematics “number” has no necessary and sufficient conditions; but in tightly specified theoretical contexts certain kinds of numbers do, of course).

The second reason the prototype theory failed is implicit in the first. It seems very implausible that the causal information needed to explain why children learn certain feature structures rather than others can itself be encoded as another entry in a list of weighted feature structures. Part of this is due to the kind of lexical ambiguity that arises from Keil’s work. The most obvious such forms are so-called “theta-role” ambiguities involved in such sentences as “John hit the building.” Is John here understood as an agent which is doing the hitting, or is he understood as a patient in sentences such as, “The explosion caused the baseball and John to hit the building”? More complicated cases concern the kinds of causal explanations young children will accept. Children will accept that plants are green for teleological
reasons, such as that it helps them to photosynthesize, but reject that plants are
green for reasons involving efficient causation, such as that little particles in the
plants' leaves reflect light a certain way. When we explain human behavior, the
ambiguity multiplies. For example, we can explain John's fear of snakes in virtue of
John's beliefs and desires, his adaptive behavior (as children understand plants), or
in terms of behavioristic causal conditioning (as biochemistry understands plants).
Such ambiguity should make us suspicious that all three causal explanations could be
encoded in one feature structure. At best, there would be different feature structures
relative to each causal explanation.

On the concepts-in-theories view, to grasp a word's meaning is to possess a theory
involving the word. For example, it is not enough merely to know that stereotypical
birds have wings. One also must know that stereotypical birds use those wings to
fly. These theories will vary in detail from the young child to the ornithologist,
and vary in detail among ornithologists. However, enough "overlap" is necessary for
communication to proceed.

Here we do well to return to Lakoff's hedge, *technically*, which seemed to offer
some evidence for a two-factor neo-classical view. I think that Keil's work shows that
*technically* should be thought of as contextually selecting a theoretical context with
constraints on identifying instances of a concept which are either more definitionally
critical and less prototypical, or perhaps simply criterial in a different way. For
example, architects have told me that "Technically window blinds are furniture,"
is true in some design contexts and false in others. Good so far. Now we need
to ask if this provides any evidence for a two-factor neo-classical view of concepts.
I don't think so; rather, it just shows that in some theoretical contexts criteria
for determining that something is in the extension of a context are relatively clear
("Technically, (an) x are/(is a) y" thus is only felicitous when there is some context
at hand where criteria for being a y are both more explicit and more important). It
may be the case that sometimes the contexts are so clear that genuinely necessary
and sufficient conditions are being appealed to for the term in question. Thus the architecture for a lexical semantics or theory of concepts ought to predict this in such cases; however, this is a far cry from using analytically true definitions in the “base” of such an architecture to explain word meaning. Rather, we now have that in some theoretical contexts some words are treated as definable. There is no solace here for the classical theorist though. There is no evidence that such cases are very extensive, and there is some evidence that they are pretty rare.

Moreover, it seems that the more criterial the theoretical context is, the more the word meanings (in that context) suffer the kind of underdetermination to which Mark Wilson calls attention. Can we honestly say that there is a robust set of analytical sentences for the word “electron”? —that one misunderstands the word “electron” if and only if one thinks that one of these sentences are false? This seems epistemologically bizarre.

In fact, it is not strange to hold that those who understand the word “electron” best are the ones most likely to be able to deny any sentences in which “electron” occurs, so long as a better physical theory involves such change. Are we constrained to say that the meaning of “electron” changes every time we discover something new about electrons? This is perverse. The analyticity theorist again would have to say that changing certain sentences and changing them alone forces change in meaning. But it beggars the imagination to think that a plausible set of criteria could be set down to explain our decisions concerning whether or not ontology or ideology is shifting as our theory of the world shifts, and that’s exactly what the analyticity theorist would have to do.

Thus, technically provides no solace for the classicist. To the extent that it selects a context where identification of instances of a concept proceeds more criterially than prototypically, the criteria seem to be the most subject to Wilsonian underdetermination, which is itself in considerable tension with the attempt to explain meaning-individuation and grasp of meaning with the analytic-synthetic distinction.
As far as I know, no-one has presented a compelling formal model which correctly predicts acquisition data of either the concepts-in-theories view, or Keil's more reasonable hybrid view, which is really the demand to keep what was right about the prototype view in any formalization of the concepts-in-theories view. This, I take it, is one of the most pressing problems in both lexical semantics and the theory of concepts. In a sense, this is, or is at least akin to, a Kuhnian "prerevolutionary period" in science. All of the evidence shows that the classical view is deeply mistaken as a view about concepts or word meanings, but no rigorous presentation of a theory has yet been offered to replace it.

§ 4.2.3 What This Tells Us About Dummett's Use of the Analytic-Synthetic Distinction

The point of this chapter is not to fire a volley in a Kuhnian revolution, but rather to show that Dummett's verificationist manifestationism is so wedded to the classical view of concepts, that the disconfirmation of the classical view of concepts is simultaneously a disconfirmation of manifestationism.

In section 4.1 I showed that, prima facie, Dummett needs to use robust sets of analytic sentences to render his verificationism plausible. To repeat, for Dummett a logical semantics should recursively correlate verification conditions with disambiguated natural language sentences; then grasp of those sentences is to be explained as a recognitional capacity with regard to the verification conditions generated. However, standard compositional Heyting semantics merely generates verification conditions for "x is an electron" of the form "a procedure c which allows the determination for any arbitrary object a whether or not a is an electron." Independently of whether or not recognition of such procedures is particularly relevant to the understanding of "electron," it is the case that the recognitional capacity in question requires grasp of a lot of background theory involving sentences longer than "x is an electron." Thus, it seems that, prima facie, Dummett's proposal is victim to a vicious circularity.
As was stated earlier, if understanding of complex sentences is a function of understanding the meanings of the words in those sentences and the syntactico-semantic principles of combination involved, and if understanding of words requires understanding of a lot of very complex sentences, then it seems Dummett's proposal is essentially that one understands a complex sentence if and only if one understands a lot of other complex sentences.

While I think such a view is right as far as it goes, it is anathema to Dummett. For it raises the spectre of a Quinean holism that undermines Dummett's demand for a verificationist theory of sense in the first place. If our grasp of a sentence is in part a function of our grasp of the role that sentence plays in a broader theory, then, as I will show, there is little reason to subscribe to Dummett's manifestation constraints. However, as shown above, if, as it was not irresponsible to believe at the time, "x is an electron" is a natural language surface structure which lexically decomposes by way of analytic sentences into an essentially Generative Semantics style deep structure, Dummett could more plausibly hold that the verificationist semantics applies to the deep structure logical form, and maintain his molecularism, which held that understanding a sentence only required understanding sentences with logically simpler deep structures.

Thus we begin to appreciate how Dummett needs to use the analytic-synthetic distinction. However, every consideration presented above from relatively diverse fields added to a building crescendo of disconfirmations of such a use of the distinction. We began by more clearly sketching the model of word meaning and conceptual content underlying the view that word meanings and the concepts they correlate with could usefully be specified by a set of analytic sentences. Then we considered problems from philosophy proper, psychology, linguistics, lexicography, and acquisition theory which showed both that word meaning and conceptual content are not individuated by a set of analytical sentences involving the words with the meanings in question or correlated with the concepts in question, as well as that our grasp
of word meanings and concepts is not attained by grasping such a set of analytical sentences. Independently of Generative Semantics' disconfirmation for purely syntactic reasons, not discussed above, we know from the above considerations that the architecture of a linguistic theory, and parallel theory of concepts, is not the kind of architecture needed to render Dummettian molecularism plausible. Without lexical decomposition licensed by analytically true sentences, it is just false to say that one could understand a sentence by understanding sentences of only "lesser logical complexity." As goes analyticity (and the Generative Semantics view of decomposition, for that matter) so goes molecularism.

§ 4.3 WHAT THIS TELLS US ABOUT MANIFESTATIONIST VERIFICATIONISM

What we have yet to establish, however, is that as goes molecularism so goes Manifestationism. For Dummett's verificationist the central theses of his Manifestationist view are the Recognition Thesis and the Canonicity Requirement. These have been given above in the following manner.

Recognition Thesis

\[ X \text{ understands } P \text{ if, and only if, were } X \text{ presented with a construction } c, \text{ then } X \text{ could recognize whether } c \text{ verifies } P \text{ and whether } c \text{ verifies } \neg P. \]

Verificationist Canonicity Requirement

The constructions quantified over in the Publicity Requirement and Recognition Thesis must be such that two sentences \( A \) and \( B \) have the same meaning if, and only if, for all constructions \( c \) (from the same domain of constructions quantified over in the Publicity Requirement and Recognition Thesis), \( c \) verifies \( A \) if, and only if, \( c \) verifies \( B \), and \( c \) verifies \( \neg A \) if, and only if, \( c \) verifies \( \neg B \).

We saw in section 4.1 that these two constraints lead to a straightforward version of the Extricability Thesis. Our reasoning in brief was that if verifiying constructions individuate the meanings of sentences (as stated in the Canonicity Requirement), and ability to recognize such verifying constructions explains understanding of sen-
sentences (as stated in the Recognition Thesis), then whether or not a speaker can recognize all of the canonical verifications and falsifications of a claim determines whether or not she attaches the correct meaning to the claim or a deviant meaning. If a different speaker operates with a different set of such canonical verifications, then the two speakers attach different meanings to the sentence in question. The Extricability Thesis was stated in this manner above.

The Extricability Thesis

1. For any utterance $P$, $P$'s truth or falsity is a function of the meaning ($M$) of the sentence and the way the world is ($W$), and

2. The $M$ component bifurcates from the $W$ component in such a way that, if $P$ is true given $M$ and $W$, and a person $S$ takes $P$ to be false, then there is some fact of the matter which determines whether or not $S$ attaches a deviant meaning $M'$ distinct from $M$ to $P$ and possibly has a correct belief about $W$, or attaches the right $M$ to $P$ and has an incorrect belief about $W$.

So, by Modus Tollens, if the Extricability Thesis is indefensible in light of the above considerations, then Dummett must either abandon the Verificationist Canonicity Requirement, or the Recognition Thesis.

If the Extricability Thesis straightforwardly entailed the use of the analytic-synthetic distinction to explain our grasp of word meaning, then the above criticism of the analytic-synthetic distinction would straightforwardly undermine the Extricability Thesis, and thus, by our Modus Tollens, Dummettian verificationist Manifestationism. While I don't think there is any straightforward logical proof that one cannot affirm the Extricability Thesis while eschewing the kind of use of the analytic-synthetic distinction debunked above, it is at least extremely difficult to see how one could be committed to one and not the other. For Dummett, we saw in section 4.1 that his commitment to a verificationist Manifestationism strongly motivated using the analytic-synthetic distinction. If word meanings are not individuated by sets of analytical sentences, and grasp of word meaning isn't explained by recognition of the truth of these sentences, then it is entirely unclear how Dummett can plausibly recursively generate verification conditions of natural language
sentences which simultaneously are involved (via the statement that a person understands a sentence if, and only if, she can recognize verifications and falsifications of that sentence) in stating the conditions for grasp of meaning of those sentences. In section 4.1 we were able to render such a position plausible only by assuming lexical meaning is secured by such analytically true sentences.

This leaves open the possibility that the inventive anti-realist might somehow defend the Extricability Thesis (as well as the Recognition Thesis and Verificationist Canonicity Requirement) without using the analytic-synthetic distinction in the manner proscribed above. If this were a plausible project, then our dialectic thus far could be charged with a massive failure of imagination, and perhaps nerve as well.

I don’t think that such a project is plausible, largely because the anti-analyticity dialectic above provides plenty of evidence that the Extricability Thesis is false. First, when considering lexicography we saw the Thesis undermined by the lexicographic problem concerning the distinction between (i) information about a word’s use relevant to grasp of the meaning of a word and (ii) grasp of collateral information. In a related discussion we saw that quasi-externalist appeals to “expert knowledge” do not at all help. The externalist thought was that perhaps word meanings as used by experts do have strict definitions. The need for precision and clarity in many theoretical contexts which experts work in seems to support this.

However, by reconsidering Wilson’s arguments for the underdetermination of word meaning, it became very clear that inextricability is, if anything, more pronounced in such theoretical contexts. It is a good-making feature of scientific practice that any sentence held true is subject to revision, if evidence and theoretical concerns such as simplicity, elegance, and relation to other theories give us good reason to revise the sentence. The analyticity theorist is then forced into the unenviable position of saying that each word carries with it a set of sentences determining the intension of that word such that if two experts argue about whether or not one of those sentences is true or false, then the two experts are either talking past one
another (since they mean different things by their words) or are engaged in a debate about the meaning of a word, when they seem to be debating about the way the world is. The analyticity theorist must also say that when one of these sentences is revised, the meaning of the word has changed. Thus, the implausibility of the Dummettian use of the analytic-synthetic distinction in these contexts itself shows the implausibility of the truth of the Extricability Thesis in these contexts.

The Putnamian externalist is likely to respond with some form of “a-ha!” and say that the reason the two experts are not talking past one another is that the word they are arguing about is a “natural kind term,” whose meaning is fixed, not by any set of analytical truths, but by a natural kind the word picks out. One virtue of the Keil-type hybrid view is that it shows the Putnamian response here to be particularly ill-motivated. In the context in which the experts are arguing, they surely agree on enough collateral theory to be able to take themselves to be arguing about the same kind of thing, even in the limiting case where that kind of thing doesn’t exist in the world (e.g. people arguing about the properties of phlogiston, witches, caloric, etcetera).

Our discussion of Keil’s views also applied pretty directly against the Extricability Thesis itself. If concepts and word meanings really are as Keil’s research suggests, then it is more plausible to think that the Extricability Thesis is false. If word meaning is a function of background theory then one would expect nearly any disagreement on the truth value of a sentence to be able to be cashed out as a disagreement concerning the way the world is rather than as a disagreement concerning the meanings of terms in the sentence, as long as enough background theory concerning the world is shared by the interlocutors. For example, say that Sam thinks that “Necessarily, all bachelors are unmarried” is true, and I hold that “It is possible that some bachelors are married” is true. I think that homosexual marriage will be legalized and, say given recent court decisions involving the use of the term “bachelor,” have good reason to think that it is possible that some young men mar-
ried to one another will continue to be called bachelors when homosexual marriage is legalized.\textsuperscript{31} Frank could not think that homosexual marriage will be legalized; he could think that the court cases in question will be overturned; he could think that homosexual marriage will be legalized but that men married to one another cannot be called bachelors... The point is, Frank and I could argue about this, and if we shared enough background beliefs and linguistic usage in some cases it would be very misleading to say that we disagreed about the meanings of the two sentences.

Independent of these concerns is the fact that, once the analytic-synthetic distinction is no longer used to explain individuation of word meanings and grasp of word meanings, there is no reason to subscribe to the Extricability Thesis. In light of our above discussion, the Extricability Thesis begins to look like an implausible consequence of using the analytic-synthetic distinction the way Dummett needs to. Once we eschew the analytic-synthetic distinction, and examine what seems to be the most promising post-classical approach to concepts and word meanings, we realize that the Extricability Thesis is both implausible and unnecessary. Moreover, given that we couldn’t discern a way for Dummett to affirm Manifestationism without commitment to the Extricability Thesis with analytic sentences doing the meaning-individuation mentioned therein, we have very good evidence supporting the claim that as the analytic-synthetic distinction loses any evidential warrant and explanatory power in the theory of content and lexical meaning, so does the Extricability Thesis.

We should turn to the broader question upon which Dummett’s dialectic hinges; can grasp of the meanings of sentences reasonably be identified with the ability to recognize verifications and falsifications? One entailment of this position, which does seem unacceptable in light of the above, is that for any two speakers who both disagree about the truth value of a sentence, it is the case that they either: (1) disagree

\textsuperscript{31} It might not be implausible to maintain that the majority of supreme court decisions involve cataloging various good and bad reasons for “going on” in various ways with word meanings. That is, legal interpretation plausibly provides very good examples of how we have to deal with Wilsonian underdetermination.

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about whether or not a verifying or falsifying construction for the sentence exists in
the actual world (a world disagreement), or (2) disagree about what sets of verifying
and falsifying constructions are attached to the sentence (a meaning disagreement).
Thus, it seems that Dummett’s position is committed to the Extricability Thesis.

However, this does not address whether or not one can characterize the verifications
and falsifications in such a manner that one can still uphold something like the
Recognition Thesis without extricability. To address this we need to examine the
picture of word meanings and concepts that emerged from our above discussion in
slightly more detail. Here, I will recapitulate some of the planks of the classical view
of concepts in light of the above discussion. I don’t take this to be a “theory” of
concepts or word meaning, but rather a description of some properties of concepts
and word meanings that a correct post-classical theory might have. Thus, I will
attempt to present the most classical-like view I take to be warranted in light of the
above discussion.

First I will present the properties which our discussion did not engage. For our
purposes it is good to assume that concepts and word meanings should still be
thought of as possessing these properties. These were properties i. (intensionality),
iii. (expressibility), v. (publicity), and vi. (bifurcation). I here renumber them so
that they are again presented in a numerical order.

(i') **Intensionality:** A concept possesses an intension, which, given the way
the world is, determines its extension.

(ii') **Expressibility:** Many, and perhaps all, concepts are expressible in lan-
guage. For many a word, its meaning can be identified with a concept.

(iii') **Publicity:** If two people grasp the same concept then it is possible for
someone to discover that they do, and if two people grasp different con-
cepts, then it is possible for someone to discover that they do.

(iv') **Bifurcation:** Concepts are either basic or nonbasic. Nonbasic concepts
are those whose grasp presupposes grasp of other concepts.

While the above are unscathed by the above discussion, certainly our understanding
of the words occurring in the statement of these properties have changed.
Instead of property iv. (determinacy) and property viii. (definability) we now have at best the following weaker properties.

(v.') *Semi-determinacy:* For any concept \( C \), there exists a fairly robust set of possible objects which are determinately paradigm instances of \( C \), paradigm instances of things not \( C \), or neither. There is also possibly a robust set of possible objects such that any object in this set could be considered a paradigm instance of \( C \), paradigm instance of not-\( C \), or neither.

(vi.') *Characterizability:* Nonbasic concepts can be characterized with a combination of weighted feature structures (where the features needn't be either individually necessary nor jointly sufficient), and certain features of the inferential and behavioral role of the concept relative to a set of domain-specific theories.

I will not attempt to discern post-classical analogues to properties vii. (summary representability), and ix. (inheritability of features), because those properties were so tied to the architecture of the classical theory. Any post-classical analogues would make specific reference to the architecture of a post-classical theory, and I don't think that enough successful formalization of post-classical views has taken place to confidently predict very much about such an architecture.

Property ii. (graspability) has also changed. In light of Keil's hybrid view, we should give it as.

(vii.') *Graspability:* Concepts can be grasped by human beings. For many concepts a human being who grasps them can, ceteris paribus, recognize of a healthy range of possible prototypical objects whether they fall under the concept. For all concepts, a human being who grasps them can make a healthy range of correct inferences involving those concepts and otherwise act appropriately for one who grasps them.

We see how the abilities a creature requires for concept possession need not enable that creature to recognize correctly whether or not an arbitrary object falls under the concept in question. While it must be admitted that there is probably a rough ordering along which lay concepts in theories determined by how much recognitional ability is needed to be correctly attributed possession of the concept or grasp of a word's meaning, it is mistaken to identify concept possession with such an ability.
For example, color concepts like RED might require such a recognitional capacity (though we don’t usually say that color blind people misunderstand the word ‘red’—so expressibility may fail here). At the other end of the ordering would be concepts like GROOVY, where grasp is almost entirely adjudicated relative to the behavioral and inferential commitments one undertakes by calling something “groovy.” Important concepts in scientific theories are likely somewhere in between (e.g. we don’t want to say of a great theorist who is also a terrible experimentalist that her faulty recognitional capacities mean she doesn’t understand the language in which she theorizes). This view of a continuum of concepts-in-theories strikes me as prima-facie much more plausible than the rooting of conceptual mastery in recognitional capacities. More importantly, it is what Keil’s view predicts. If part of conceptual content is determined by domain-specific theories in the manner current psychological research suggests, we should expect that many concepts and word meanings do not either have or need a socially agreed upon set of possible objects which must be recognized by those who possess the concept in question.

One might attempt to save the spirit of verificationism by using analytically true sentences to explain grasp of “groovy” type predicates. Perhaps sentences which do not have a clear set of verification and falsification conditions (e.g. “that’s groovy”) might none-the-less have a clear set of analytically true sentences which must be affirmed by anyone who can be attributed understanding of the sentence. However, in light of our earlier discussion of the analytic-synthetic distinction such a strategy seems particularly ill-motivated.

Thus, from Keil and others’ work we must conclude that a competent speaker could grasp enough of the inferential and behavioral role of a word (without possessing very robust recognitional capacities) to be correctly attributed grasp of the word in many theoretical contexts (e.g. the expert theoretician who is also a bungling experimentalist). This shows that we should not take such recognitional capacities to be necessary for grasp of word meaning.
On the other hand, one could have very good recognitional capacities vis-à-vis certain words without grasping the meaning of the word. Consider the theoretically ignorant technician who can recognize readings on photo-electric plates well enough to say “There’s a photon” in all and only the situations where a photon is measured. Consider a person who reliably recognizes birds but does not know that birds fly, and in fact has a very bizarre theory about what birds are and the functioning of birds’ wings. In many contexts it would be incorrect to say that such a person either grasped the meaning of “bird” or had the concept BIRD. This shows that we should not take recognitional capacities to be sufficient for grasp of word meaning.

§ 4.4 Conclusion

I have not criticized the idea of a non-verificationist manifestationist theory of sense. Thus one could still try to answer to Dummett’s challenge, identifying meaning with use in the manner enshrined by the manifestation constraints and the Canonicity Requirement. However, I think that, in light of the above discussion, skepticism should be our default assumption when considering the possibility of a non-verificationist theory of sense. Again, one could charge such a skeptic with failure of imagination or nerve in concluding this, but if we reconsider Manifestationism we can marshal some grounds for such skepticism.

The backbone of the manifestationist position consisted of the following two principles.

The Entailment of Identity of Speaker’s Meaning from Identity of Use
If two speakers agree in all possible correct uses of a sentence, then they agree about the meaning of the sentence.

The Equivalence of Correct Use and Grasp of Meaning
One can correctly use a sentence if, and only if, one correctly grasps its meaning.

In our discussion in Chapter 2 we also saw how the Dummettian anti-realist was
committed to the following third principle.

**Canonicity Requirement**

The dispositions to correctly use a sentence mentioned in the above two requirements must individuate the meanings of sentences.

Does the above discussion provide us with the material to explicate a notion of meaning consistent with these principles, or does it provide us with reasonable grounds for doubting their truth? Though it may be premature, given that no-one has formalized either the concepts-in-theories view, or Keil's hybrid view, we do have some evidence that the above principles are incorrect, or at least that the kind of "correct use" mentioned in the principles does not consist in sets of dispositions which can be recursively correlated with natural language sentences. If the kind of "correct use" relevant to grasping a word's meaning is determined by inferential, behavioral, and recognitional capacities determined by grasp of background theory, then we should expect that different people can have fairly divergent dispositions governing their inferential, behavioral, and recognitional capacities and merit in many contexts the attribution of grasp of the same word meaning. Given Wilsonian underdetermination, Quinean lexicographic versus encyclopedic inextricability, and the problem of sense disambiguation in lexicography, we can confidently state that words do not carry with them a core set of inferential, behavioral, and recognitional capacities making it correct to say that two people grasp the same word meaning if, and only if, they possess that core set of dispositions. But that is what the above three principles call for.

Finally, it is fairly clear that rather than considering understanding of word meaning to be an on-off phenomenon we should consider it as loosely ordered. In our earlier discussion of comparatives I showed that any natural language noun or verb could be used as a comparative. The same is true with "understanding." In many contexts it is correct to say that one person understands words better or worse than another. Whatever grounds the truth of such attributions, from the felicity of the
comparative it is clear that it is not thought of as a term with necessary and sufficient conditions for its use. True, if the principles underlying the comparative are clear enough, then it is simple enough to devise a metric with a cut-off point for having or lacking understanding in many contexts (e.g. “hot” and “cold”). But our question is whether or not “understanding” (when applied to words) is like “uncle”—where plausible necessary and sufficient conditions can be invoked in many contexts, or whether it is what we might call “irreducibly comparative.”

That is, can we only characterize understanding and lack thereof as the bottom and top segment of some scale? Moreover, can necessary and sufficient conditions be given for determining of any two speakers and a sentence, who understands the sentence better? While such a metric may be very fruitful experimentally and theoretically when restricted to the understanding of very specific domains, if Keil’s view is correct then we should not expect to be able to characterize such a function for all of language. It would involve discerning and then weighing specific inferences, behavioral expectations, and recognitional capacities involving words. This doesn’t seem very promising.

Given that we are in a theoretical context, the Dummettian has every right to throw up her hands in exasperation and say something to the effect of “Put your money where your mouth is.” That is, one can’t claim to have disconfirmed an empirical theory without presenting a better theory. Therefore, in the next chapter I shall sketch a Davidsonian view that will help us to make sense of explaining linguistic competence in part by attributing tacit knowledge of a compositional semantics.
In the first chapter of this monograph I presented Dummett’s main argument in
the following manner.

1. Knowing the meaning of a sentence requires knowing what it would be for
that sentence to be true,
2. All that is involved in knowing the meaning of a sentence is knowing how
to use that sentence correctly, and
3. Given 1. and 2., the assumption that all true sentences are verifiable al­

ows us to provide the best explanation of knowledge of the meaning of a
sentence.
4. The assumption that all true sentences are verifiable provides strong evi­
dence for the incorrectness of classical truth conditional semantics, as well
as evidence for the correctness of intuitionistic constructive semantics.

Throughout, we have understood the notion of “knowing what it would be for a
sentence to be true” (in the first premiss of the above argument) in terms of tacit
knowledge of a semantics that generates truth conditions for the sentence in ques­
tion. Much of Chapter 1 consisted in an elaboration of the relevant notion of tacit
knowledge in a manner maximally charitable to the first premiss of Dummett’s main
argument.

The other three premisses of Dummett’s main argument have been progressively
undermined as we have pursued our investigations. In-so-far as “verifiability” in the
second premiss is understood to be sufficient for truth (as Dummettians take it to
be), Chapter 2 disproved the second premiss by showing how the most plausible form
of verification conditions which speakers can be charged with recognizing are not
sufficient for truth. In Chapter 3 this fact proved to be the key to undermining the revisionary conclusion of Dummett's master argument. Finally, in the last chapter I undermined the claim that verificationism provides a good explanation of knowledge of the meaning of a sentence.

All of our discussion has not been negative though. In the first chapter I developed and defended an explication of tacit knowledge of a proposition, and in the previous chapter I used the evidence presented against a verificationist account of word meaning to characterize grasp of word meaning in a non-verificationist manner. Here I will use both of these positive discussions to defend a non-Dummettian view of linguistic understanding. By attending to some of Donald Davidson's views about the relationship between a grammar and evidence for that grammar, and incorporating our earlier positive proposals about tacit knowledge and word meanings, we shall be able to answer Dummett's challenge. The classical semanticist, following Davidson, can provide an account of meaning both constrained by use and such that it renders postulation of tacit knowledge of a grammar explanatory.

§ 5.1 Davidson's View in Outline

In Chapter 1 we contrasted the following position (originally attributed to Davidson by Dummett)

Modest View

(A) A speaker $S$ is competent in the language $L$ if, and only if, $S$ has tacit knowledge of a correct theory of meaning for $L$ (we call this theory $M_L$)

(B) $M_L$ is identical to a correct syntax and compositional semantics for $L$, and

(C) A speaker $S$ of $L$ has tacit knowledge of the theory of meaning $M_L$ for $L$ if, and only if, it is not possible to have explicit knowledge of $M_L$ and fail to be a competent speaker of $L$, with Dummett's own position. We saw how the dialectic of "What is a Theory of Meaning? (I)" could be reconstructed as a criticism of the Modest View and motivation for the Dummett's Manifestationist View, presented this position as the
following,

**Manifestationist View**

(A) A speaker $S$ is competent in the language $L$ if, and only if, $S$ has tacit knowledge of a correct theory of reference for $L$ (we call this theory $R_L$),

(B) The theory of meaning for $L$ (which we will call $M_L$) is identical to $R_L$ plus a theory of sense (which we will call $S_L$), which correlates sets of dispositions with truth conditions generated by $R_L$, and

(C) A person can correctly be attributed tacit knowledge of $R_L$ if, and only if, she possesses the dispositions correlated with the truth conditions of $R_L$ by $S_L$.

Though Dummett initially characterized the Modest View as a Davidsonian position, the appendix of “What is a Theory of Meaning? (I)” contains a retraction of the attribution.

To Dummett, Davidson’s key idea is that the evidence for the correctness of a syntax and semantics is a proper part of the characterization of a speaker’s mastery of language in terms of the syntax and semantics. He writes,

... but I think that the impression which not only I but, I believe, a number of Davidson’s supporters had, that a theory of meaning of his kind is to be interpreted as a modest one, is to be rejected. A large part of the reason for so interpreting it lies in the fact that Davidson has always represented the collection of data about the judgements actually made by speakers as to the truth and falsity of sentences as standing in a relation of evidence to the resulting theory of truth; where on the holistic conception of sense which I sketched above they do not provide external support to the theory, but are integral to it.

...what Davidson calls the ‘evidence’ for the theory of truth is actually internal to it. The theory is not something that we base upon the ‘evidence’, but which can be understood without knowing what the evidence for it may be: we cannot grasp or convey the content of the theory without explicit mention, in detail, of the sentences which jointly determine the references of our words; for without such mention, we cannot tell what references the theory of truth asserts those words to have.

(Dummett, (1976a, pp. 28–29))

Though Dummett goes on to criticize the position he attributes to Davidson as being too holistic, such criticism needn’t concern us.

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Where (following Quine’s famous *Gedankenexperiment* in Chapter II of *Word and Object*) a radical interpreter\(^{32}\) is a linguist immersed in an alien speech-community, Davidson’s view can be presented in the following manner.

(i) A speaker \(S\) is competent in the language \(L\) if, and only if, \(S\) has tacit knowledge of a correct theory of reference for \(L\) (we call this theory \(R_L\)),

(ii) \(R_L\) must be such that one who has explicit knowledge of \(R_L\) could, in virtue of this knowledge, interpret speakers of \(L\) correctly, and

(iii) A person can correctly be attributed tacit knowledge of \(R_L\) if, and only if, her behavior is such that a competent radical interpreter could discover that \(R_L\) is a correct theory of reference for the language she speaks.

If this is Davidson’s view, then what are we to make of Dummett’s claim that evidence for the theory of reference is a proper part of a Davidsonian meaning theory? For Davidson a theory of meaning will consist in a syntax and semantics for a natural language, while the theory of meaning concerns methodological and philosophical reflection about radical interpretation. This seems to contradict Dummett’s assertion. However, before solving this puzzle, we would do well to flesh out slightly some of the key components in Davidson’s view.

Like Dummett, Davidson stresses that it is knowledge of language that allows us to understand others’ utterances. Thus, our problem of “what we know when we know a language” is, in part, a problem of how we interpret others. For Davidson, a theory of meaning thus must be, at the very least, a theory of interpretation.

Also, with Dummett, Davidson realizes that our ability to understand a potential infinity of utterances requires a compositional account of how aspects of a sentence’s meaning are a function of aspects of the meaning of the parts of that sentence and

\(^{32}\)See (Davidson, 1973), (Davidson, 1974), (Davidson, 1975), and (Davidson, 1976) for the canonical discussion of radical interpretation. I hope that it will be clear that Davidson’s fascinating discussions of the incoherence of certain forms of skepticism, the relationship between language and thought, and the differences between the hermeneutic and physical sciences survive transplantation to, and perhaps flourish in, the view I defend in this chapter. All of these discussions are motivated by Davidson’s sustained meditation on the relation of evidence for a grammar to that grammar, the relation which Dummett realizes is one of the cornerstones of Davidson’s understanding of the psychological reality of a grammar. Following Davidson, this relation plays just as central a role in the non-Dummettian position I characterize here.
the way those parts are put together. He writes,

A satisfactory theory for interpreting the utterances of a language, our own included, will reveal significant semantic structure: the interpretation of utterances of complex sentences will systematically depend on the interpretation of utterances of simpler sentences, for example.

(Davidson, 1973, p. 130)

As we noted earlier while discussing Dummett, Davidson thought that Tarski-style truth definitions for simple formal languages like first-order logic would have particular salience for explaining compositional aspects of sentence meaning. Perhaps more perversely than with Dummett, I shall not emphasize this in my discussion of Davidson, but will instead presuppose the framework of contemporary semantic research described in Chapter I.

Davidson also holds with Dummett that a certain kind of psychological reality claim should be true for one's theory of interpretation. For example, he writes,

Theory of interpretation is the business jointly of the linguist, psychologist and philosopher. Its subject matter is the behaviour of a speaker or speakers, and it tells what certain of their utterances mean. Finally, the theory can be used to describe what every interpreter knows, namely a specifiable infinite subset of the truths of the theory.

(Davidson, 1974, pp. 141-142)

Thus, position (i),

(i) A speaker $S$ is competent in the language $L$ if and only if $S$ has tacit knowledge of a correct theory of reference for $L$ (we call this theory $R_L$),

can be attributed fairly to Davidson. Again, I take it to be a virtue of my discussion that we needn't saddle Davidson with the view that $R_L$ be a Tarski style truth definition.

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Purists may balk. In (Davidson, 1977) the author defends the view that semantic notions such as reference and satisfaction should be, in some sense, instrumentally construed. If one focused on this line one might argue that it is false to say that a speaker need have tacit knowledge of a meaning theory. Perhaps what is constitutive of a person's grasp of meaning is merely the know-how involved in acting so that a radical interpreter can interpret that person correctly. The semantics used in interpretation are then perhaps just an artifact, not something of which the speaker is understood as having tacit knowledge. I don't go into this strain of Davidson's thought because our main point is that the Davidsonian has the resources to answer Dummett's challenge as posed.

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The second claim in our initial sketch of Davidson's position was given in this manner,

(ii) $R_L$ must be such that one who has explicit knowledge of $R_L$ could, in virtue of this knowledge, interpret speakers of $L$ correctly.

This claim can also fairly be attributed to Davidson. He writes,

It is still not clear, of course what it is for a theory to yield an explicit interpretation of an utterance. The formulation of the problem seems to invite us to think of the theory as the specification of a function taking utterances as arguments and having interpretations as values. But then interpretations be no better than meanings and just as surely entities of some mysterious kind. So it seems wise to describe what is wanted of the theory without apparent reference to meanings or interpretations: someone who knows the theory can interpret the utterances to which the theory applies. (Davidson, 1973, p. 128)

One can begin to see why Dummett mistakenly thought that Davidson was committed to the Minimal View. If Davidson's theory was committed to (i) and (ii), then he would be committed to the Minimal View.

In "Radical Interpretation," however, Davidson makes very clear that his position consists in more than just (i) and (ii). For example, he writes,

The second general requirement on a theory of interpretation is that it can be supported or verified by evidence plausibly available to an interpreter. (Davidson, 1973, p. 128)

Moreover, the article itself is presented as an attempt to support affirmative answers to the following three questions.

1. Is it reasonable to think that a theory of truth of the sort described can be given for a natural language?

2. Would it be possible to tell that such a theory was correct on the basis of evidence plausibly available to an interpreter with no prior knowledge of the language to be interpreted?

3. If the theory were known to be true, would it be possible to interpret utterance of speakers of the language? (Davidson, 1973, p. 131)

Clearly, the second question shows that Davidson accepts the third plank in our
characterization of his view.

(iii) A person can correctly be attributed tacit knowledge of $R_L$ if and only if her behavior is such that a competent radical interpreter could discover that $R_L$ is a correct theory of reference for the language she speaks.

The above quotes show that Davidson accepts, as a success condition on a theory of interpretation, that that theory be discoverable by a radical interpreter.

But why? I think the thought moving Davidson is similar to that which Quine gives voice to in his well-known beginning to "Word and Object."

语言是社会的艺术。在获得它时，我们必须完全依赖于来自社会可观察到的刺激的相互主体性可用的线索来决定说什么和什么时候。因此，除非在以人们对外界刺激的直接反应的术语来解释如何理解说话者，否则没有理由赋予语言意义。

(Quine, 1960, p. ix)

Quine and Davidson's thought is that the evidence available to the radical interpreter is the same evidence available to us when we interpret one another. Or rather, that a theory which a radical interpreter can both discern from observation as well as use to understand speakers will explain how normal speakers discern what others mean from their behavior.

With Dummett, Davidson also feels that people's dispositions to recognize verifiers and falsifiers plays an important role in grasp of meaning. However, Davidson's position manages to capture this insight without commitment to the view that a speaker understands a sentence if, and only if, she can recognize verifications of it.
He writes,

Suppose, then, that the evidence available is just that speakers of the language to be interpreted hold various sentences to be true at certain times and under specified circumstances. How can this evidence be used to support a theory of truth? On the one hand, we have T-sentences, in the form:

(T) 'Es regnet' is true-in-German when spoken by $x$ at time $t$ if and only if it is raining near $x$ at $t$.

On the other hand, we have the evidence, in the form:

(E) Kurt belongs to the German speech community and Kurt holds true 'Es regnet' on Saturday at noon and it is raining near Kurt on Saturday at noon.

We should, I think consider (E) as evidence that (T) is true. Since (T) is a universally quantified conditional, the first step would be to gather more evidence to support the claim that:

(GE) $(x)(t)$(if $x$ belongs to the German speech community then $(x$ holds true 'Es regnet' at $t$ if and only if it is raining near $x$ at $t$)).

The appeal to a speech community cuts a corner but begs no question: speakers belong to the same speech community if the same theories of interpretation work for them. (Davidson, (1973, p. 135))

While this is phrased in terms of the machinery Davidson thought appropriate to semantic theory, the key insight transcends commitment to that machinery. That is, if one's theory generated sentence/conditions-for-truth-in-a-model pairs (using a set of indexes for the contextual information), and if one had a translation manual for the 'non-logical' words (and presumably a lexical semantics for German, stated in English, would translate such German expressions into English and explain general principles which license such translation), then the right hand side in an example such as the above would be a sentence of mathematical English asserting the conditions under which an utterance of 'es regnet' are true.34

34Friends who work on categorial grammars have told me that categorial grammars are incomparably more successful for the task of developing translation algorithms than transformational ones. This is interesting, given the tight fit between syntax and semantics in categorial grammars. It is
Thus, Kurt's grasp of "Es regnet" is explained by his having tacit knowledge of a theory that predicts, among other things, that when suitably prompted he will say "Es regnet" in conditions when it is raining nearby. This raises questions concerning how different this is from Dummett's verificationist explanation of grasp of meaning. Consider.

The obvious objection is that Kurt, or anyone else, may be wrong about whether it is raining near him. And this is of course a reason for not taking (E) as conclusive evidence for (GE) or for (T); and a reason not to expect generalizations like (GE) to be more than generally true. The method is rather one of getting a best fit. We want a theory that satisfies the formal constraints on a theory of truth, and that maximizes agreement, in the sense of making Kurt (and others) right, as far as we can tell, as often as possible. The concept of maximization cannot be taken literally here, since sentences are infinite in number, and anyway once the theory begins to take shape it makes sense to accept intelligible error and to make allowance for the relative likelihood of various kinds of mistake. (Davidson, (1973, p. 136))

Davidson goes on to show that charity, or the principle that inexplicable error is to be minimized while constructing a meaning theory is independently very plausible (by arguing that the notion of massive error is incoherent). These arguments needn't concern us however.

For our dialectical purposes, what is significant is that the holistic nature of the evidence for a syntax and semantics, given Davidson's insight, entails an holistic construal of linguistic competence. For a Davidsonian, a speaker is competent in the language $L$ to the extent that a radical interpreter could discern a correct meaning theory for $L$ by studying her behavior. When we study the kind of evidence available to the radical interpreter as she makes her attributions of sameness of meaning, we discover a lot of slack in how to construe the native's language. That is, there is very little evidence for the existence of non-vague conditions necessary and sufficient

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Also interesting that, as far as I know, no-one has used this fact to mount a sort of Davidsonian counteroffensive against blanket claims by Chomsky and his followers that categorial grammars are "not explanatory."
for the translation of words in the native's idiolect into the interpreter's. But then, given Davidsonian's insight there is very little evidence for the existence of non-vague conditions necessary and sufficient for understanding of a sentence.

A Davidsonian would say that a person understands a sentence $P$ in the language $L$ to the extent that her behavior with regard to the sentence is that predicted by a meaning theory for $L$. In what sense does a meaning theory predict behavior? A meaning theory for $L$ ($M_L$) predicts that speakers of $L$ will behave in such a way as to lead a radical interpreter to discover $M_L$ from observing the speakers of $L$ (and hence be able to use $M_L$ to interpret the speakers of $L$). But depending upon the constraints placed upon an acceptable theory of radical interpretation, this allows for a lot of reasonable slack.

For example, consider Goldbach's Conjecture, the claim "Every even number is the sum of two primes." The Dummettian requires a speaker to be able to recognize a proof of this sentence in order to understand it. A Davidsonian could require that their inferential and recognitional behavior with sentences of this "logical form" be correct, and that they recognize as true many sentences involving the words in the statement of Goldbach's Conjecture. These are the kinds of facts that would provide evidence for an interpreter's knowledge that a speaker means the same as she does by Goldbach's Conjecture. Moreover, a compositional and lexical semantics would predict many of these facts; if a speaker recognized them as true, other things being equal, this is sufficient for her understanding the claim. The compositional semantics will, via a definition of truth-in-a-model, predict compositional entailments that speakers grasp, and a lexical semantics will predict standard lexical entailments.

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35 We begin to see how Dummett's view can be presented in a Davidsonian framework. The Dummettian requires sameness of verification conditions for correct translation, and is very optimistic about the existence of such conditions.

36 I hope that a lexical semantics will contain more than the trivial seeming "$\nu(\text{dog}(e)) = 1$ if $e$ is a dog, and 0 otherwise" type clauses used by the compositional semantics. Quine and Davidson's gedankenexperiment suggests what else is involved. How do we determine that Kurt means "dog" by "Hund"? Whatever principles we use have a good candidate status for being relevant to our own grasp of "dog." How this should be modelled is somewhat problematic, given the thrust of Chapter IV.
(i.e. "a dog is an animal") that speakers grasp. This is the sense in which we say
the meaning theory predicts behavior with regard to a sentence $P$.

Satisfaction by a speaker of some perhaps weighted set of the capacities we have
mentioned (the ability to recognize compositional and lexical entailments as well as
recognition of verification conditions (e.g. the "es regnet" example)) can be taken to
be sufficient for grasp of meaning, all other things being equal. This position will be
clarified and rigorized somewhat in the next section. Here we do best to appreciate
it in outline, so that we can see how the Davidsonian differs from the Dummettian.
As Davidson notes, the fit between theory and practice will, of course, be somewhat
loose.

The Dummettian agrees that it is by assessing such evidence that we in fact
determine in practice that a speaker grasps Goldbach's Conjecture. But she is moved
to posit dispositions (constitutive of grasp of meaning) over and above those. The
Davidsonian is not.

While the Dummettian thinks that the ability to recognize verification condi­
tions is constitutive of grasp of meaning, to the Davidsonian what is constitutive
of a speaker's grasp of meaning is the behavior that allows others to interpret that
speaker. In practice interpretation is an holistic endeavor which involves weighing a
number of features. Only one of these is evidential prowess with a sentence. In the
last chapter I argued that such evidential prowess is neither necessary nor sufficient
for grasp of meaning. That this is what one would expect if Davidson's view is
correct is itself evidence for Davidson's view.

Again, in practice we determine that others attach the same meaning to a word
by examining whether they accept as true many of the sentences involving it which
we accept as true, examining whether their behavior "makes sense" in light of attri­
bution of these beliefs even if we don't share the beliefs (e.g. Sam and I can disagree
in all of our judgments about what is groovy), and whether they accept many of the
same inferences involving the word that we do. Since the Davidsonian characterizes
grasp of the meaning of a word in terms of acting in such a way as to license attribution of sameness of meaning, the Davidsonian characterizes grasp of the meaning of a word in terms of accepting as true sentences in which the word occurs, behaving in characteristic ways that “make sense” given such acceptance, and acceptance of inferences involving the word.

Now that we have some grasp of the Davidsonian perspective, we can return to Dummett’s claim that (pace some of Davidson’s own descriptions of his project) the evidence for a syntax and semantics are part of the theory of meaning for Davidson. We remarked that this is at odds with Davidson’s own usage, as Davidson describes a T-theory as being a theory of meaning. As we see how this is not a difference that makes a difference, it will become clear how Davidson and Dummett’s positions can be contrasted. That is, Davidson’s view can be presented as being committed to a kind of Manifestationism.

In Chapter I we understood Dummett’s verificationist proposal for a theory of meaning in this manner.

Verificationist Manifestationist View

(A) A speaker S is competent in the language L if, and only if, S has tacit knowledge of a correct constructive theory of reference for L (we call this theory $R_L$), which compositionally determines verification conditions of the sentences of L,

(B) The theory of meaning for L (which we will call $M_L$) is identical to $R_L$ plus a theory of sense, which is equal to the claim that, for all sentences $P$ in L, S understands $P$ if, and only if, were S presented with a construction c, then S could recognize whether c verifies $P$ and whether c falsifiess $P$, and

(C) A person can correctly be attributed tacit knowledge of $R_L$ if, and only if, for all sentences $P$ in L, were S presented with a construction c, then S could recognize whether c verifies $P$ and whether c falsifiess $P$.

Note that this merely resulted from filling in some of the lacunae in the statement of the Manifestationist View in the manner Dummett urges. One begins to wonder whether these lacunae can be filled in differently, in a manner that still equates meaning with use, but eschews the Dummett’s verificationism.
One quite interesting fact is that the Davidsonian View can likewise be used to fill in lacunae in the Manifestationist View. Consider:

Realist Manifestationist View

(A) A speaker $S$ is competent in the language $L$ if, and only if, $S$ has tacit knowledge of a correct truth conditional theory of reference for $L$ (we call this theory $R_L$), which compositionally determines truth conditions of the sentences of $L$.

(B) The theory of meaning for $L$ (which we will call $M_L$) is identical to $R_L$ plus a theory of sense, which is an explanation of what an $L$ speaker's behavior must be like for $R_L$ to be a correct theory of reference for $L$ via an explanation of how a radical interpreter could determine that $R_L$ is correct from studying speakers' behavior, and hence use $R_L$ to understand speakers of $L$.

(C) A person $S$ can correctly be attributed tacit knowledge of $R_L$ if, and only if, for all sentences $P$ in $L$, $S$'s behavior with regard to $P$ is such that a competent radical interpreter would postulate all of the relevant axioms concerning $P$ in the the theory $R_L$.

Thus we can begin to understand Dummett's claim that Davidson allows the evidence for the syntax and semantics to be part of the theory of meaning. Dummett demands of a theory of meaning that it include a theory of sense, which explains a speaker's grasp of the meanings of sentences in terms of how a speaker uses the sentences. Davidson does allow use to constrain an account of meaning in terms of how a radical interpreter could discover a semantics for native speakers. Thus a Davidsonian theory of radical interpretation does fulfill the role that Dummett would have a theory of sense fulfill. This is the reason that Davidson's view allows itself to be presented, like Dummett's, as a way to fulfill the lacunae in the statement of Manifestationist View.

§ 5.2 SOME REFINEMENTS

In this section I further develop and defend a Davidsonian alternative to Dummettian Manifestationism. One of our ongoing concerns has been the extent to which these "theory of meaning" debates presuppose a false view of grammar. While dis-
cussing Dummett and Davidson above I have gone out of my way to reconfigure the
dialectic in a manner that does not involve commitment by any of the participants
to an unworkable architecture of a linguistic theory. For example, we stated the first
clause of both the Modest and Manifestationist Views as,

(A) A speaker $S$ is competent in the language $L$ if and only if $S$ has tacit
knowledge of a correct theory of reference for $L$ (we call this theory $R_L$),
in both the Modest and Manifestationist Views (and added the word “constructive”
and “truth conditional” in the appropriate places, respectively, in the Verificationist
Manifestationist and Realist Manifestationist Views). Identifying a theory of refer­
ence merely with a syntax and semantics has served us well. Given the high level of
philosophical abstraction this discussion resides in, it is better if our view is invariant
over linguistic fault lines.

However, in another way, clause (A) as given has been wrongheaded from the
start. To the extent that we've focused on semantics in this context our focus has
been on compositional semantics. Thus, in section 1.2, as an example of the mech­
anisms of such a theory we gave the following two derivations.

**Syntax Derivation**
1. every $\in P_T/\mathcal{CN}$ lexicon
2. man $\in P_{\mathcal{CN}}$ lexicon
3. every man $\in P_T$ 1,2 S2
4. walk $\in P_{\mathcal{IV}}$ lexicon
5. every man walks $\in P_t$ S4

**Translation**
1'. $\lambda P[\lambda Q\forall x[P(x) \to Q(x)]]$ 1 translation manual
2'. man' 2 translation manual
3'. $\lambda P[\lambda Q\forall x[P(x) \to Q(x)]](\text{man'})$ 1,2,1',2' T2
4'. walk' 4 translation manual
5'. $\lambda P[\lambda Q\forall x[P(x) \to Q(x)]](\text{man'})(\text{walk'})$ 3,4,3',4' T4

Then, using lambda conversion we can simplify this into an equivalent formula that
is more tractable.

\[
\lambda Q \forall x [\text{man}'(x) \rightarrow Q(x)](\text{walk}')
\]

5'. lambda conversion

\[
\forall x [\text{man}'(x) \rightarrow \text{walk}'(x)]
\]

6'. lambda conversion

Finally, standard semantic clauses for first order logic deliver the following interpretation.

\[
\begin{align*}
\text{Interpretation} \\
\nu(\forall x [\text{man}'(x) \rightarrow \text{walk}'(x)]) &= T \\ 
\forall d \in D &\nu\overset{d/u}{(\text{man}'(u) \rightarrow \text{talk}(u))} = T \\ 
\forall d \in D &\nu\overset{d/u}{\text{(man}'(u))} = \text{F} \text{ or } \nu\overset{d/u}{\text{(talk}(u))} = T \\ 
\forall d \in D & (d \notin \nu(\text{man}') \text{ or } d \in \nu(\text{talk}')).
\end{align*}
\]

Thus, if we think of English as the "object language" and mathematical English as the meta-language, our semantics has delivered.

"Every man walks" is true in the model \((\nu, D)\)

\[
\iff \forall d \in D (d \notin \nu(\text{man}') \text{ or } d \in \nu(\text{talk}')).
\]

The model-theoretic translation predicts a set of inferences that people who understand "every man walks" can be charged with recognizing. For example, every model for the translation of "every man walks" and "John is a man" will also be a model for "John walks." The theory thus predicts that competent speakers can recognize that the third sentence must be true if the first two are true. Competent speakers inferential capacities are thus used to test the theory.

Now suppose that in the Davidsonian view as given above the theory of reference just is a compositional semantics of this sort, i.e. one which predicts valid compositional entailments. Then, by clause (B) of the Realist Manifestationist View,

(B) The theory of meaning for \(L\) (which we will call \(M_L\)) is identical to \(R_L\) plus a theory of sense, which is an explanation of what a speaker of \(L\)'s behavior must be like for \(R_L\) to be a correct theory of reference for \(L\) via an explanation of how a radical interpreter could determine that \(R_L\) is correct from studying speaker's behavior, and hence use \(R_L\) to understand speakers of \(L\),

a theory of sense must be added to explain how a radical interpreter could determine that the compositional semantics is correct (and hence be able to use that semantics
to communicate with speakers of the language that the semantics is a semantics for). Thus, a theory of what constitutes the evidence for the semantics being correct does becomes the theory of sense.

Part of this would be by determining that "every man walks" has the same compositional entailments as those predicted by the semantics. But this would clearly not be enough. What is lacking? Clearly the interpreter would need to know that "man" and "walk" in the object language translates correctly into "man'" and "walk'" in the metalanguage. Thus the Davidsonian theory needs to include a general theory of what licenses such translation.

What is it about Kurt's use of "Mann" that determines that it translates well into "man'"? What is it about Joe's use of "man" that determines that I do not err when I assume he means the same by "man" as I do? That an answer to these questions is all that is required by a Davidsonian meaning theory is a point that has confused philosophical gods, so we mortals would do well to ponder it. For example, a well-known criticism by Hilary Putnam of Davidson involves misunderstanding
this very fact. Putnam writes,

If we rule out such truth definitions (strictly speaking, clauses, but I shall continue using “truth definition” both for individual clauses and for the whole system of clauses, for simplicity) as

\[ \text{“X is water” is true if and only if X is water} \]

on the grounds that they don’t say anything about the meaning of the word “water,” and we rule out such truth definitions as

\[ \text{“X is water” is true if and only if X is H}_2\text{O} \]

on the grounds that what they say is wrong as a description of the meaning of the word “water,” then we shall be left with nothing.

The problem is that we want

\[ \text{“X is water” is true if and only if—} \]

to satisfy the conditions that (1) the clause be extensionally correct (where — is to be thought of as a condition containing “x,” e.g., “x is H}_2\text{O}”); (2) that — be a translation of W — on our theory, this would mean that the stereotype associated with W is approximately the same as the stereotype associated with —; (3) that — not contain W itself, or syntactic variants of W. If we take W to be, for example, the word “elm,” then there is absolutely no way to fulfill all three conditions simultaneously. Any condition of the above form that does not contain “elm,” and that is extensionally correct will contain a — that is absolutely terrible as a translation of “elm.”

Even where the language contains two exact synonyms, the situation is little better. Thus

\[ \text{“Heather” is true of x if and only if x is gorse} \]
is true, and so is

\[ \text{“Gorse” is true of x if and only if x is heather} \]
– this is a theory of the meaning of “gorse” and “heather”? (Putnam, (1975, pp. 189–190))

Given our explication of the Davidsonian position, it is very clear that Putnam has misjudged the success-conditions of a Davidsonian meaning theory. The Davidsonian needn’t produce, in the homophonie case, a translation equivalent to, for example, “man” in her meaning theory. Rather, she must explain why we are justified in
understanding another English speaker's use of 'man' to commit that speaker to meaning the same as we do. These conditions will be conditions that license translation of "Mann" into "man," but it is these conditions of use that interest the Davidsonian, not the fact of translation per se. Thus a Davidsonian theory of sense will include a description of how speakers must behave in order to understand a word.

That Davidson did not say very much about a word meaning has led to criticisms that Davidson's theory is of the following kind, here again stated by Putnam.

Relativized to such a theory (relativized to what we admittedly don't yet have), the theory comes down to this: we want a system of truth definitions which is simultaneously a system of translations (or approximate translations, if perfect translation is unobtainable). If we had a theory which specified what it is to be a good translation, then we could rule out the above truth definition for "water" [which translates "water" in the object language to "H$_2$O" in the metalanguage] as uninteresting on the grounds that $x$ is H$_2$O is not an acceptable translation or even near-translation of $x$ is water (in a prescientific community), even if water = H$_2$O happens to be true.

This comes perilously close to saying that a theory of meaning is a truth definition plus a theory of meaning.

(Putnam, (1975, pp. 188–189))

This is unfair. The Davidsonian perspective charges a lexical semantics with explaining what justifies our attribution of sameness of meaning of a word between interlocutors speaking the same language and what justifies attribution of good translation of a word between speakers of different languages.

For Dummett, two words (or the same word in different speaker's idiolect) have the same meaning if and only if they contribute to sentence's verification conditions in the same ways (that is, where $P[a/x]$ denotes the result of substituting occurrences of the word $a$ for all occurrences of the word $x$ in the sentence $P$, $a$ and $b$ have the same meaning if and only if for all sentences $P$, $P[a/x]$ and $P[b/x]$ have the same verification conditions). From this an interpreter could determine that two speakers mean the same by studying their recognitional behavior with regard to the sentences.
In the last chapter, while trying to dispel the Dummettian view that word meanings are individuated by verification conditions of the sentences they occur in, I presented some evidence for the conclusion that concepts (and the word meanings) can be broadly characterized by means of the following seven properties, the first four of which were held in common with the classical view of concepts.

(i.) **Intensionality:** A concept possesses an intension, which, given the way the world is, determines its extension.

(ii.) **Expressibility:** Many, and perhaps all, concepts are expressible in language. For many a word, its meaning can be identified with a concept.

(iii.) **Publicity:** If two people grasp the same concept then it is possible for someone to discover that they do, and if two people grasp different concepts, then it is possible for someone to discover that they do.

(iv.) **Bifurcation:** Concepts are either basic or nonbasic. Nonbasic concepts are those whose grasp presupposes grasp of other concepts.

Our discussion of faults with the classical view provided some evidence that the next three non-classical properties hold of concepts and word meanings.

(v.) **Semi-determinacy:** For any concept $C$, there exists a fairly robust set of possible objects which are determinately paradigm instances of $C$, paradigm instances of things not $C$, or neither. There is also possibly a robust set of possible objects such that any object in this set could be considered a paradigm instance of $C$, paradigm instance of not-$C$, or neither.

(vi.) **Characterizability:** Nonbasic concepts can be characterized with a combination of weighted feature structures (where the features needn't be either individually necessary nor jointly sufficient), and certain features of the inferential and behavioral role of the concept relative to a set of domain-specific theories.

(vii.) **Graspability:** Concepts can be grasped by human beings. For many concepts a human being who grasps them can, *ceteris paribus*, recognize of a healthy range of possible prototypical objects whether they fall under the concept. For all concepts, a human being who grasps them can make a healthy range of correct inferences involving those concepts and otherwise act appropriately for one who grasps them.

This characterization suggests a manner in which answering Davidson's question does not involve a Dummettian construal of word meaning.
If this picture of concepts (and, by expressibility, word meanings) is right, then a lexical semantics needs to provide a formal model involving such weighted feature sets and theory internal inferences. It also needs to be one that plausibly "fits" in some manner with the compositional semantics\(^{37}\) For our purposes what is important is that this picture of word meaning is what one would expect word meanings to be like if, with Davidson, we take meaning to be whatever allows translation, and then attend to the holistic nature of translation (as was described in the previous section).

This suggests a reformulation of our statement of the Davidsonian position. I have just shown that we can understand the theory of reference to be a compositional semantics and include the lexical semantics as part of the Davidsonian theory of sense. However, it is less misleading to have the lexical semantics be part of the theory of reference. Making this shift, as well as filling in slightly more lacunae, give us,

(A) A speaker \(S\) is competent in the language \(L\) if, and only if, \(S\) has tacit knowledge of a correct syntax and compositional and lexical semantics for \(L\) (we call these, taken together, \(R_L\)).

Clauses (B) and (C) can be given as above. We still require of the theory of reference that a radical interpreter could both arrive at it by studying native speakers and use it to communicate with them. We still characterize competence in \(L\) as consisting in dispositions to behave in such a way that a radical interpreter could discover a correct theory of reference for \(L\). However, we now see how these requirements place strong explanatory constraints on both lexical and compositional semantics.

While we're reformulating the statement of the Davidsonian view we should admit that the restriction to these three parts of a linguistic grammar are wholly unmotivated. Any linguistic grammar divides into phonetic, phonological, morphological, syntactic, semantic (lexical and compositional), and pragmatic components.

\(^{37}\)This is a big problem. Many of properties of word meanings in the most successful approaches to semantics are those characterized by the classical view of concepts described in Chapter 4. In (Cogburn & Cook, 1999) Cook and I attempt solutions to this and related problems.
While optimal architecture will not necessarily treat these different areas as autonomous modules, they do pick out different kinds of abilities used by speakers while communicating with language.

At the price of horrendously oversimplifying, we can say that a phonetics will predict the speech sounds that speakers articulate and perceive. A phonology predicts the patterning of sounds in the language that competent speakers can articulate and recognize as correct or incorrect. A morphology predicts those facts about the internal structure of words that competent speakers grasp. A compositional semantics predicts facts about meaning which occur systematically as a function of the distributional classes in which words occur and the syntactic operations on these words and phrases. A lexical semantics predicts facts about word meaning which do not so occur. A pragmatics predicts how context of use affects the interpretation of sentences. All of these "modules" of a grammar are tested by appeal to judgments of competent speakers.

Without detailed discussion of these areas it is clear that the first clause should reflect the different competencies explained by contemporary linguistic theory. Thus, we state the new position as,

Davidsonian View

(A) A speaker $S$ is competent in the language $L$ if, and only if, $S$ has tacit knowledge of a correct grammar, $G_L$, for $L$ (containing phonetic, phonological, morphological, syntactic, semantic (lexical and compositional), and pragmatic components),

(B) The theory of meaning for $L$ (which we will call $M_L$) is identical to $G_L$ plus a theory of sense, which is an explanation of what an $L$ speaker's behavior must be like for $G_L$ to be a correct grammar for $L$ via an explanation of how a radical interpreter could determine that $G_L$ is correct from studying speakers' behavior, and hence use $G_L$ to understand speakers of $L$.

(C) A person $S$ can correctly be attributed tacit knowledge of $G_L$ if, and only if, for all sentences $P$ in $L$, $S$'s behavior with regard to $P$ is such that a competent radical interpreter would postulate all of the relevant axioms concerning $P$ in the theory $G_L$.

Clause (A) of this view is so entrenched among the vast majority of linguists that
one can pick nearly any introductory textbook and find some formulation of it. This is at least some evidence for the position it occurs helps define. It is very nice that Davidson's insight (that one has tacit knowledge of a grammar to the extent that one's behavior would provide the evidence for the correctness of that grammar) does not decrease in plausibility by identifying the grammars in question with those that linguists construct.

However, a very strange thing has happened. After stating the Davidsonian View in this manner, one wonders whether a theory of interpretation is not exhausted by the linguist's practice of constructing grammars and the psychologist's practice of crafting a theory of concepts (given concepts' correlation with word meanings). In a sense, on this view, all that is left that is uniquely philosophical are issues concerning the relationship between evidence and linguistic and psychological theory. Philosophy of language, to the extent that it doesn't overlap with linguistics and psychology (which it surely does much of the time), becomes philosophy of science, where the sciences in question are linguistics and psychology.\(^\text{38}\)

While this vague metaphilosophical tangent will no doubt be pleasing to orthodox Quineans interested in naturalizing this and that area of philosophy, the position that prompts it may not be. As we discussed in Chapter 1, linguists often test grammars by asking informants about language. Indeed, this practice provided the key to the solution to our problems with trying to define tacit knowledge of a sentence in some plausible way. Given that people make claims about their language that are justified by linguistic theory (and thus used to test the correctness of that theory) we were

\[^\text{38}\text{I don't intend this to be any great revelation. Nearly all of Davidson's substantive philosophical claims (about, for example, the falsity of radical forms of skepticism, the difference between the interpretative and physical sciences, and the connection between thought and talk) spring out of sustained meditation by him on the relationship between evidence for a grammar and that grammar. The extent to which his conclusions rest upon his view of grammar is not an issue that can be addressed here. The main point is that Davidson has always conceived of a theory of meaning as an empirical theory. Given this, it is not misleading to characterize Davidson's own meditations about the relationship between the evidence for such a theory and the theory as philosophy of science about linguistics and psychology (see the quote presented above where the theory of interpretation is taken to be the business of the linguist, psychologist, and philosopher).}^\]
able to see the linguist’s insistence that people know a correct grammar for their 
language as a dogged internalist epistemology concerning the justifiers for claims 
they make about their language.

These considerations led us to define tacit knowledge of a grammar in this manner.

1. At the level of output, one who possesses the state of tacit knowledge that 
   $P$ is disposed to do and think some of the things which one who had the 
   ordinary belief that $P$ would be inclined to do and think (given the same 
   desires).

2. At the level of input, one who possesses the state of tacit knowledge that 
   $P$ will very probably have acquired that state as a result of exposure 
   to usage which supports or confirms (though far from conclusively) the 
   proposition that $P$, and hence in circumstances which might well induce 
   in a rational person the ordinary belief that $P$.

3. One who possesses the state of tacit knowledge that $P$ is such that, with 
   some finite (possibly null) extension of their cognitive capacities, she can 
   recognize that $P$ is true when presented with an explicit statement of $P$.

4. The idealization of cognitive capacities in 3. must be licensed by the tacit 
   knower’s behavior, in that $P$ must function as a justifier for normative as­
   sessments and knowledge claims that the tacit knower does, or is prepared 
   to, make.

Therefore, if our view really is to be contiguous with linguistics, we effectively allow 
the radical interpreter (grammarian) to query people about their language. While 
Davidson never explicitly denies that native’s claims about a language can be used 
as evidence for constructing a theory of reference, neither does he discuss this kind 
of evidence. Moreover, he does present the radical interpreter as the offspring of 
Quine’s radical translator, presented in Chapter II of *Word and Object*. In Quine’s 
discussion, all that is available to the radical translator are the native’s dispositions 
to assent and dissent to sentences in controlled environments.

The harsh finale of Putnam’s “Critique of Davidsonian Semantic Theory” seems 
to criticize Davidson for not allowing the translator access to this kind of evidence.
He writes,

The contention that the only evidence available to the linguist is speakers' dispositions with respect to whole sentences is, furthermore, vacuous on one interpretation, and plainly false on the interpretation on which it is not vacuous. If dispositions to say certain things when queried about individual words or morphemes or syntactic structures are included in the notion of dispositions to use sentences, then the restriction to dispositions to use sentences seems to rule out nothing whatsoever. On the nonvacuous interpretation, what Davidson is saying is that the linguist cannot have access to such data as what informants (including the linguist himself) say when asked the meaning of a word or morpheme or syntactic structure. No reason has ever been given why the linguist cannot have access to such data, and it is plain that actual linguists place heavy reliance on informants' testimony about such matters, in the case of an alien language, and upon their own intuitions as native speakers, when they are studying their native languages. In particular, when we are trying to translate a whole sentence, there is no reason why we should not be guided by our knowledge of the syntactic and semantic properties of the constituents of that sentence, including the deep structure. As we have seen, there are procedures for gaining information about individual constituents. It is noteworthy that the procedure that Quine and Davidson claim is the only possible one—going from whole sentences to individual words—is the opposite of the procedure upon which every success ever attained in the study of natural language has been based. (Putnam, (1975, p. 190))

While this may be a problem for the theory Putnam takes from Davidson, it is not a problem for our Davidsonian View.

On the contrary, the best account of tacit knowledge of a proposition \( P \) we could discern in Chapter 1 involves \( P \)'s role as a justifier for claims that speakers readily assent to. This is exactly the notion that is needed for the Davidsonian View. Consider again the clause concerning tacit knowledge in the Davidsonian View,

\[ (C) \text{A person } S \text{ can correctly be attributed tacit knowledge of } G_L \text{ (a correct grammar for } L) \text{ if, and only, if for all sentences } P \text{ in } L, S's \text{ behavior with regard to } P \text{ is such that a competent radical interpreter would postulate all of the relevant axioms concerning } P \text{ in the theory } G_L. \]

If the competent radical interpreter is allowed to ask people about their language to
test her grammar against speakers' intuitions about, for example, distributional and inferential data, then the sense in which the propositions of $G_L$ are tacitly known is exactly the sense in which we defined a proposition to be tacitly known in our earlier discussion of tacit knowledge. Also, Putnam's fear that the Davidsonian approach is in tension with successful approaches to linguistics no longer lurks, as the grammar $G_L$ is tested in exactly the way linguists test their theories.\(^{39}\)

Is anything left of Dummett's challenge? We have seen how one can accept classical semantics, reject the claim that grasp of meaning is equivalent to the ability to recognize verifications, and still equate grasp of meaning with the ability to correctly\(^{40}\) use a sentence. The manner in which I have explicated this position does commit us to an holistic (or perhaps "fuzzy molecularist") view of grasp of word meanings in which competence with a word is explained by satisfaction of sufficiently many of certain inferential and behavioral criteria. Another way to put this might be to say that we were forced to understand "understanding" as a Wittgensteinian family-resemblance term. However, in Chapter 4 I showed that this is not an implausible view, and that this view is more plausible than Dummett's.

I conclude that Dummett's challenge to classical truth-conditional semantics can be adequately met.

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\(^{39}\)One huge lacuna in this discussion is the extent to which facts about acquisition constrain a grammar. Considering this issue in light of our discussion will either involve adding substantial refinements and changes to the view, or criticizing some linguists' view of how facts about language acquisition plausibly constrain a grammar. I would like to pursue the latter tack, but doing so is a large undertaking. For a nice critique of Chomsky's current a priori arguments from the poverty of stimulus to the conclusion that his (current) framework is the only syntactic framework that can be claimed to be psychologically real, see (Johnson & Lappin, 1997).

\(^{40}\)One might argue that the notion of correctness has been abandoned to the Davidsonian's peril. Isn't the Davidsonian committed to a perfectly awful philosophy of logic which identifies logical laws with dispositions to infer, and thus can make no sense of the normativity involved in logic? In (Cogburn, 1998) I argue that this is not the case.
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