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Dummett’s Case for Intuitionism

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Dummett’s case against platonism rests on arguments concerning the acquisition and manifestation of knowledge of meaning. Dummett’s arguments are here criticized from a viewpoint less Davidsonian than Chomskian. Dummett’s case against formalism is obscure because in its prescriptive considerations are not clearly separated from descriptive. Dummett’s implicit value judgments are here made explicit and questioned.

‘Combat Revisionism!’
Chairman Mao

1. Texts

Some philosophers approach mathematics saying, ‘Here is a great and established branch of knowledge, encompassing even now a wonderfully large domain, and promising an unlimited extension in the future. How is mathematics, pure and applied, possible? From its answer to this question the worth of a philosophy may be judged’. Other philosophers approach mathematics in a quite different spirit. They say, ‘Here is a body, already large and still being extended, of what purports to be knowledge. Is it knowledge, or is it delusion? Only philosophy and theology, from their standpoint prior and superior to that of mathematics and science, are worthy to judge’. While this inquisitorial conception of the relation between philosophy and science is less widely held today than it was in Cardinal Bellarmine’s time, it continues to have many distinguished advocates.

Prominent among these is Michael Dummett, who has repeatedly advanced arguments for the claim that much of current mathematical theory is delusory and much of current mathematical practice in need of revision – arguments for the repudiation, within mathematical reasoning, of the canons of classical logic in favour of those of intuitionistic logic. While nearly everything Dummett has written is pertinent in one way or another to his case for intuitionism, there are two texts specially devoted to starting that case: His much-anthologized article 1973a on the philosophical basis of intuitionistic logic; and the concluding philosophical chapter of his guidebook (1977) to the elements of intuitionism. The present paper offers a critical examination of these two texts.

1. For more on the contrast between the two approaches to philosophy of mathematics, see the opening section of the editorial introduction to Benacerraf and Putnam 1964.
2. Dummett’s case against platonism

Dummett has remarked of his case for intuitionism that ‘...it is virtually independent of any considerations relating specifically to the mathematical character of the statements under discussion. The argument involves only certain considerations within the theory of meaning of a high degree of generality, and could, therefore, just as well have been applied to any statements whatever, in whatever area of language’ (1973a, 226). Hence it is best to begin an examination of his case by considering some of his general views on meaning.

Especially important for Dummett are what I will call neutrally theories of language of the first type. On a theory of this type, the meaning of a sentence is identified with the conditions for the correctness of the sentence (or pedantically: of an assertion made by uttering the sentence) as a representation of reality. A speaker’s ability to use the sentence is explained by reference to his grasp of these correctness-conditions. Correctness may be conceived of in more than one way, and hence more than one subtype within the first type of theory of language is possible.

Especially important for Dummett is the distinction between those conceptions on which correctness is, and those on which it is not, something always at least in principle potentially recognizable by human beings. The best-known theory operating with a conception of correctness as always recognizable is the intuitionist proof-conditional theory of meaning for the mathematical part of language. (The provability of a mathematical conjecture is recognizable by discovering a proof.) The generalization of this theory to the whole of language would be a verification-conditional or verist theory.

The best-known correctness-conception on which correctness is sometimes recognizable, sometimes unrecognizable, is the usual conception of truth. (On the usual conception, the truth of a mathematical conjecture need not imply the existence of a proof of the conjecture or of any other means of recognizing the conjecture as true.) To avoid ambiguities, I will call truth-as-usually-conceived verity. Many distinguished philosophers have advocated a verity-conditional or verist theory of meaning. Dummett calls the specialization of such a theory to the mathematical part of language a platonist theory (though I imagine that he would be hard pressed to locate a passage in the Republic or the Timaeus where such a theory is taught).

How can graspable correctness-conditions be assigned to each of the indefinitely many sentences of a language? The only answer that immediately suggests itself is: inductively. The theories of language of the first type considered by Dummett take the sentences of the language to fall into a hierarchy of degrees of complexity, with the correctness-conditions for those of higher degree being determined inductively from the correctness-conditions for those of lower degree. For example, such theories include an induction clause indicating how the correctness-conditions of a disjunction are determined from those of its disjuncts. On an intuitionist or verification theory this clause takes the form:

1. A proof (or verification) of a disjunction consists in the specification of one of its disjuncts together with a proof (or verification) of that disjunct.

On a platonist or verist theory this clause takes the form:

2. A disjunction is true if and only if at least one of its disjuncts is true.
On any theory of language of the first type there is an external standard against which rules of implication are to be judged. A rule is acceptable if and only if it preserves correctness, leading in all instances where the premises are correct to a conclusion that is correct. It is by appeal to such induction clauses as (1) and (2) above that one can seek to demonstrate that certain rules are correctness-preserving or sound.

Given the platonist theory of meaning, the usual soundness proof for classical logic establishes that all the rules of that logic are acceptable. Given the intuitionist theory of meaning, the usual soundness proof for intuitionistic logic establishes acceptability for all intuitionistic rules, but not for all classical rules: the acceptability of rules depending on the laws of double negation or excluded middle is doubtful when such rules are applied to sentences for which an effective decision procedure is lacking, such as those involving unbounded quantification over an infinite domain. Dummett's strategy is to argue for the repudiation of classical logic in favour of intuitionistic logic by arguing for the repudiation of platonist (or more generally: verist) theories of meaning in favour of intuitionist (for more generally: verificationist) theories.

On a verist theory, sentences whose correctness need not be recognizable may be said to represent transcendent features of reality, while sentences whose correctness must be recognizable may be said to represent immanent features of reality. Dummett, like Brouwer, denies that any sentence of any possible language can represent transcendent features of reality. But where Brouwer sees this denial as expressing a limitation on reality, Dummett sees it as expressing a limitation on language.

Dummett's case against verism rests on principles summed up in the slogan that meaning is use. He offers various formulations of these principles, some in terms of meaning, others in terms of understanding, some telling us what these consist in, others through what they are exhaustively manifested. The following are typical (1973a, 216, 217):

"The meaning of... a statement cannot be, or contain as an ingredient, anything which is not manifest in the use made of it, lying solely in the mind of the individual who apprehends that meaning: if two individuals agree completely about the use to be made of the statement, then they agree about its meaning. The reason is that the meaning of a statement consists solely in its role as an instrument of communication between individuals. An individual cannot communicate what he cannot be observed to communicate..."

"[T]here must be an observable difference between the behaviour or capacities of someone who is said to have... knowledge of the meaning of an expression] and someone who is said to lack it. Hence it follows... that a grasp of the meaning of a... 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."

Shared by all such formulations is an association of meaning with public and observable use of language as a vehicle of communication, and a dissociation of meaning from private and hidden use of language as a vehicle of thought. Likewise,
any association of meaning or understanding with something in the conscious or unconscious mind, or in the structure or functioning of the brain, is rejected. Though he himself avoids the label, Dummett may be called a behaviorist in his approach to meaning, provided this label is understood in a broad enough sense to cover not only the stimulus-response behaviorism of Skinner, but also the logical behaviorism of Ryle.

A thorough-going behaviorist will require that any apparatus posited by a semantic theory must be identified or directly correlated with some isolable feature of publicly observable verbal behavior. As Dummett formulates it, the behaviorist demand is that there must be a ‘one-one correspondence between the details’ of the apparatus posited by a semantic theory and ‘observable features of the phenomenon’ (1977, 377). Dummett describes the rejection of this behaviorist demand as one form that the rejection of the principle that meaning is use might take.

The great majority of contemporary linguists reject this behaviorist demand as likely to lead only to sterility and stagnation in semantics. The great majority of contemporary linguists posit in their semantic theories an apparatus neither identified nor directly correlated with any set of isolable features of publicly observable verbal behavior. The original version of Chomsky’s semantic theory, for example, posited an apparatus of deep structures. Chomsky and the great majority of contemporary linguists claim that the apparatus posited in their semantic theories is psychologically real, represented in ways as yet undiscovered in the mind or brain. But they do not claim the apparatus to be directly represented in behavior.

Thus the principle that meaning is use, on which Dummett bases his case for a revision of current mathematics, itself already amounts to a demand for revision of current linguistics. For this reason Dummett’s arguments for the principle are of interest apart from their role in his case for intuitionism. These arguments have often been criticized by Davidsonians. Here they will be criticized from a viewpoint closer to that of the Chomskians.

Two arguments for the principle that meaning is use are to be found in the texts under examination. They are versions of what have come to be called the acquisition and manifestation arguments. The first (1973a, 217) begins:

[O]ur proficiency in making the correct use of the statements and expressions of the language is all that others have from which to judge whether or not we have acquired a grasp of their meanings. Hence it can only be in the capacity to make a correct use of the statements of the language that a grasp of their meaning, and of those of the symbols and expressions which they contain, can consist.

The rather familiar line of this opening alerts us that the argument is going to turn on

3. A minority of linguists adopt the position advocated in Soames (to appear) regarding such claims of psychological reality as at best premature, but nonetheless insisting on the legitimacy of introducing an apparatus of deep structures or the like in semantic theory, despite behaviorist objections.

4. Charles Chihara has pointed out in conversation the parallelism between the argument of Dummett just quoted and the notorious argument of Norman Malcolm against the conception of dreams as mental or neural activity taking place at specific times during sleep. Malcolm’s argument may be paraphrased: our telling stories when we wake up is all that others have from which to judge whether we have dreamt. Hence it can only be in the disposition to tell stories when we wake up that having dreamt can consist.
how an observer Y, say a teacher, can judge that a speaker X, say a learner, attaches the standard meaning to an expression E. Perhaps before proceeding further it would be well to review schematically the accounts of such judgments offered by behaviorists, on the one hand, and by those anti-behaviorists who associate meaning or understanding with a state of the mind or brain, on the other hand.

On the behaviorist account, for X to attach the standard meaning to E is for X to be able to use E standardly. On this account, Y’s judgment that X attaches the standard meaning to E is a simple inductive inference from the premise that X has been able to use E standardly in all observed instances to the conclusion that X will be able to use E standardly in all instances. The anti-behaviorist account is much more complex.

The first step towards an anti-behaviorist position is acceptance of the general psychological principle that different people similar in their outward behavior are normally similar also in the inward mental or neural states that causally underlie behavior. The second step is acceptance, as a special linguistic instance, of the hypothesis that there exists a mental or neural state S(E) normally causally and underlying the ability to use E standardly. Thus far a behaviorist may or may not go along. Where the behaviorist must refuse to follow is at the anti-behaviorist’s third step, the identification of attaching the standard meaning to E with being in the state S(E) rather than directly with being able to use E standardly.

To appreciate the rather subtle distinction here, imagine an abnormal case where a native English-speaker X is able to communicate with a native Chinese-speaker W only because X has implanted inside his skull a mini-super-computer programmed to translate back-and-forth between English and Chinese sentences. There may be no ‘difference in the behavior or capacities’ of X and W that is observable to those of us lacking telepathic powers and X-ray vision. X and W may ‘agree completely about the use to be made’ of various Chinese words and phrases. Yet on account of the absence of ‘an ingredient … lying solely in the mind’ or brain, the anti-behaviorist will deny that X attaches the standard meanings, or any meanings at all, to those words and phrases.

The status of such science-fiction examples is in itself a matter of slight importance, but there are more important further differences between behaviorists and anti-behaviorists. One who identifies attaching the standard meaning to E with being in the hypothetical state S(E) will presumably be willing to entertain hypotheses about the composition and components of S(E) and to permit a theory of the standard meaning of E to posit an apparatus correlated with these hypothetical components of S(E). But while S(E) itself is normally correlated with the ability to use E standardly, there is no reason to suppose its components to be directly correlated with any ‘isolable, though interconnected, practical abilities’ (1977, 377). Hence the anti-behaviorist requirement that the apparatus posited in a theory of the standard meaning of E must be directly correlated with isolable features of publicly observable verbal behavior, which we have already seen to be the issue dividing Dummettians and Chomskians.

On the anti-behaviorist account, for X to attach the standard meaning to E is for X to be in a mental or neural state S(E) posited to underlie, in normal cases, the ability to use E standardly. On this account, Y’s judgment that X attaches the standard meaning to E rests on: (a) the evidence for the presupposition that there exists a
mental or neural state underlying, in normal cases, the ability to use E standardly; (b) the evidence that X's case is a normal one; and (c) the evidence that X has been able to use E standardly in all observed instances. The evidence (c) is the only evidence cited in the behaviorist account. The evidence (b) may consist in no more than the absence of evidence that X's case is an abnormal one. The evidence (a) may consist in no more than the evidence for the general psychological principle that different people who are similar in their outward behavior are normally similar also in their inward mental and neural states.

A behaviorist, of course, may question the strength of the evidence for this general psychological principle. Quineans, for example, have claimed that different people identical in their outward behavior may be 'like different bushes trimmed to resemble identical elephants'. Dummett's objections to anti-behaviorism, however, do not take this form, being a priori and philosophical rather than a posteriori and psychological. Returning now to the argument whose opening was quoted above, it continues (1975a, 217):

To suppose that there is an ingredient of meaning which transcends the use that is made of which carries the meaning is to suppose that someone might have learned . . . [a] way of behaving in every way like someone who understands that language, and yet not actually understand it, or understand it only incorrectly. 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 . . . 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.

In the earlier parts of this passage, Dummett claims that if there is anything more to X's attaching the standard meaning to E than X's being able to use E standardly, then Y can never know or have a guarantee that X attaches the standard meaning to E. Two comments are called for. First, on both the anti-behaviorist and the behaviorist accounts, Y's judgment that X attaches the standard meaning to E is an inductive inference. On the behaviorist account, it is an inference from a limited number of observed instances of use to an unlimited number of possible future instances of use. No inductive inference can provide certain knowledge or an indubitable guarantee. But if the possibility of skeptical doubt and uncertainty somehow undermines a theory of meaning, it must undermine not just the anti-behaviorist theory, on which understanding is something mental or neural transcending behavior, but also the behaviorist theory, on which understanding is an open-ended behavioral ability or capacity, transcending any finite number of its manifestations. This point has been mentioned in passing by Susan Haack (1974, 107–108) and developed at length by Crispin Wright (1980, 171–129).

Second, it is far from obvious that the impossibility of skepticism-proof guaranteed
knowledge in any way undermines a theory of meaning. In the later parts of the passage under examination, Dummett seems to try to draw out damaging consequences from the absence of such guaranteed knowledge. He seems to claim that its absence somehow makes communication between mathematicians impossible, and hence makes mathematics as an activity involving communication impossible. Surely such a claim would be mistaken. For whether mathematicians X and Y succeed in communicating through their use of the expression E surely depends only on whether X and Y do in actual fact attach the same meaning to E, and not on whether they possess skepticism-proof guaranteed knowledge that they do so. One hesitates to accuse a distinguished authority on modal logic of arguing from $\Box \neg p \to \neg \Box p$, but Dummett does almost seem to wish to move from the (epistemic) possibility that X and Y do not succeed in communicating to the (metaphysical) impossibility of X and Y succeeding in communicating.

To avoid fallacies in the 'acquisition' argument we must distinguish the claim that a language-learner cannot come to attach the standard meaning to an expression from the claim that no one can have guaranteed knowledge that the language-learner has attached the standard meaning to the expression. Dummett fails to show that the former follows from an anti-behaviorist approach to meaning; and while the latter may follow, it is not obviously unacceptable. This last point has been noted by Dag Prawitz, who writes (1977, 10) of Dummett's argument that

One could contest it by arguing that when we learn a language by seeing how its sentences are used, we only get some hints about their meaning. The samples of use with which we are presented never completely determine the meaning but only enable us to form some theories or hypotheses about the meaning. (The fact that we nevertheless agree rather well about meaning could perhaps be explained by reference to a genetic disposition to see certain kinds of patterns and hence to form certain kinds of theories upon seeing a few examples.) Such a view would entail that we could never be sure that we knew the meaning of a sentence; a new unexpected use of it could show us that we had misunderstood the meaning and would force us to revise our theory. And to some extent this may be a correct picture of our situation.

Dummett's other argument calls for less comment. It runs as follows (1977, 217):

Now knowledge of meaning...is frequently verbalisable knowledge, that is, knowledge which consists in the ability to state the rules in accordance with which the expression or symbol is used... But to suppose that, in general, a knowledge of meaning consisted in verbalisable knowledge would involve an infinite regress: if a grasp of the meaning of an expression consisted, in general, in the ability to state its meaning, then it would be impossible for anyone to learn a language who was not already equipped with a fairly extensive language. Hence that knowledge which, in general, constitutes the understanding of the language...must be implicit knowledge. Implicit knowledge cannot, however, meaningfully be ascribed to someone unless it is possible to say in what the manifestation of that
knowledge consists: there must be an observable difference between the behavior 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 ... 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.

In the first part of this passage, Dummett invokes infinite-regress considerations to establish that knowledge of meaning is not 'in general' verbalizable, and even that it is 'in general' unverbalizable. If one tries to restate the argument without the use of the puzzling phrase 'in general', then one finds that all the infinite-regress considerations seem to establish is that for the part of language learned first, the most elementary part, knowledge of meaning is unverbalizable. It would then seem that any behavioristic conclusions drawn from the argument as a whole ought to be restricted to this part of language; nothing follows about the part of language learned later, the more advanced part.

In the second part of the passage under examination, Dummett invokes a premise about unverbalizable knowledge to reach a conclusion about knowledge of meaning. To avoid equivocation, we must distinguish four claims here, some stronger, some weaker, some more general, some more specific:

(a) Ascriptions of knowledge of meaning must be supported by appeal to observable evidence.
(b) Knowledge of the meaning of an expression consists in no more than the ability to use it in a certain way.
(c) Ascriptions of implicit knowledge must be supported by appeal to observable evidence.
(d) Implicit knowledge consists in no more than the ability to behave in a certain way.

The anti-behaviorist rejects (b) but accepts (a). (The anti-behaviorist account of the observable evidence supporting an ascription of knowledge of meaning has been reviewed schematically above.) Since the conclusion Dummett desires is (b) and not (a), the premise he requires is (d) and not (c), even if his own formulations are less than unequivocal (1973a, 217; 1977, 376):

Implicit knowledge cannot, however, meaningfully be ascribed unless it is possible to say in what the manifestation of that knowledge consists ... [An] ascription of implicit knowledge must always be explainable in terms of what counts as a manifestation of that knowledge, namely the possession of some practical ability.

The anti-behaviorist will argue that if—as the implanted-computer example suggests—(b) is false, then this implies that (d) is false. Dummett, however, invokes the controversial premise (d) without offering supporting considerations, as if it were

5. Paul Benacerraf suggested to me in general terms that Dummett's arguments might have force for one part of language but not another.
self-evident. For this reason anti-behaviorists may with some justice reject the 'manifestation' argument as manifestly question-begging.

Circularities in Dummett's arguments for behaviorism do not, however, deprive his case against verism of all its force. Dummett's complaint against verism comes down to this, that verists have returned no answer, formulated in behavioral terms, to the following question: In what can a grasp of the correctness-conditions for a sentence consist if the correctness of that sentence need not even in principle be potentially recognizable by human beings? So long as one insists that verists must return an answer, formulated in psychological terms, to the foregoing question, one will have to sympathize with Dummett's complaint against verism, even if one does not sympathize with his behaviorism, and adopts the approach of introspective or of physiological rather than of behavioral psychology. For the best-known advocates of verism either return no answer at all to Dummett's question; or worse, they answer that to grasp the truth-conditions of a sentence is to associate with that sentence the set of possible worlds where it is true, but do not explain how a mind or brain confined to the actual world can effect such an association.

One might also sympathize with Dummett's rejection of verism for any of a number of reasons quite unlike Dummett's own, for example, on account of the apparent conflict between truth-conditional theories of meaning and theories of truth in the style of Tarski or Kripke. This route to a rejection of verism is worth mentioning here because Dummett himself sometimes touches on it tangentially in his writings. In one paper, 1959, he notes that there appears to be a conflict between the view that a biconditional like (2) above constitutes an account of the meaning of 'or' and the view that it constitutes part of an account of the meaning of 'true', a point that Tarski has also discussed in one of his papers (1944) as an anonymous objection against his theory. Truth-conditional theories of meaning appear to regard truth as a primitive concept, possession of which is a prerequisite for any language-learning, while a theory like Kripke's appears to regard the concept of truth as one acquired fairly late in the process of language-learning, when the learner has acquired a fairly extensive ability to talk of persons, places and things, and is beginning to learn to talk of talk.6

For any of a number of reasons, good or bad, like or unlike Dummett's, many philosophers of language now reject verism. In inveighing against verism, Dummett is to a large extent preaching to the converted. Dummett himself recognizes that Wittgenstein, for one, and Quine, for another, have rejected verism. Yet somewhat surprisingly he can be found writing: 'The idea that a grasp of meaning consists in a

6. A not unrelated reason for rejecting truth-conditional theories of meaning is advanced in Harman 1982:

Davidson, Lewis, and others have argued that an account of the truth conditions of sentences of a language can serve as an account of the meanings of those sentences. But this seems wrong. Of course, if you know the meaning in your language of the sentence S, and you know what the word 'true' means, then you will also know something of the form 'S is true if and only if...'; for example, 'Snow is white' is true if and only if snow is white or 'I am sick' is true if and only if the speaker is sick at the time of utterance. But this is a trivial point about the meaning of 'true', not a deep point about meaning.

For more on the philosophical significance (or lack of it) of the concept of truth, see Soames (to appear).
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grasp of truth-conditions was [in 1959], and still is [in 1978], part of the received wisdom among philosophers..." (1978, xxi). A poll of my own department convinces me that Dummett is wrong here. How could a theory rejected by a constellation of such luminaries as Wittgenstein, Tarski, Quine, Kripke and Harman, not to mention Dummett himself, be considered 'received wisdom among philosophers'? For many philosophers what is most puzzling about Dummett's case for intuitionism will not be the question arising in his case against platonism:

(a) Why are we supposed to reject verist semantics?

but rather the question arising in his case against formalism:

(b) How is the rejection of verist semantics supposed to lead to the rejection of classical mathematics?

This latter question will now be taken up.

3. Dummett's case against formalism

Criticism of a theory of language may take any of three forms. The ordinary descriptive critic advances evidence that we do not actually speak a language of the sort the theory depicts, whether or not we ought to. The radical descriptive critic advances evidence that we could not possibly speak a language of that sort, so that the question whether we ought to does not arise. The prescriptive critic (or advocate) advances motives why we ideally ought not (or ought) to speak such a language, whether or not we currently do.

Dummett is a philosopher not primarily renowned for the clarity of his prose, and the interpretation of his works will always be a matter of controversy, not least because he declines to distinguish explicitly factual or descriptive from normative or prescriptive considerations. As I interpret it in section 2, Dummett's criticism of platonist or verist theories of meaning was descriptive and radical, claiming that we could not possibly possess (because we could not possibly acquire or manifest) a grasp of correctness-conditions that are transcendent rather than immanent. As I interpret it, Dummett's advocacy of intuitionist or verificationist theories of meaning is prescriptive. For surely he cannot claim that such theories describe and explain the actual, current patterns of use of any but a tiny minority of (Dutch) mathematicians. As I interpret it, Dummett's position is that no theory of language of the first type, verist or verificationist, provides a description and explanation of the actual, current patterns of use of the overwhelming majority of mathematicians.

Looking beyond theories of the first type, especially important for Dummett are what I will call neutrally theories of language of the second type or dualist theories. Such theories depict language as containing two different kinds of sentences: primary sentences, possessing decidable correctness-conditions, and secondary sentences, lacking correctness-conditions.

The best-known theories of this type are the theory of mathematical language associated with the name of Hilbert 1925, and the theory of scientific language associated with the name of Quine 1951. On the former theory, primary sentences,

7. Saul Kripke has suggested that Dummett's statement may be accurate as an account of local conditions at Oxford. But surely it would bespeak a certain parochialism to confuse 'fashionable among Oxford philosophers' with 'received among philosophers generally'.

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called *inhaltlich*, consist of simple arithmetical sentences decidable by computation; secondary sentences, called *ideal*, may contain non-computational mathematical vocabulary (e.g. that of set theory). On the latter theory, primary sentences, said to 'lie on the periphery', consists of simple empirical sentences decidable by observation; secondary sentences, said to 'lie in the interior', may contain non-observational scientific vocabulary (e.g. that of quantum theory). Hilbert's overall position in philosophy of mathematics is usually called *formalism*. Quine's overall position in philosophy of science is often called *holism*. Both labels have been used in the literature with so many different connotations that they are perhaps best avoided. Dummett repeatedly stresses the affinities between Hilbert and Quine (1973a, 219; 1977, 397).\(^8\)

On either theory, primary sentences are distinguished from secondary sentences by their restricted vocabulary. Not only is their non-logical, mathematical or scientific, vocabulary restricted to be computational or observational, but also their logical vocabulary is restricted. According to the precise version of the theory considered, primary sentences are restricted either to be *atomic*, containing no logical particles at all, or else to be *quantifier free*, containing only connectives.

On either theory, secondary sentences serve merely as intra-linguistic instruments for deducing primary sentences, and not as representations of any extra-linguistic reality. Computational and observational facts are represented by primary sentences. It is claimed that the scope, accuracy and efficiency of the representation of computational and observational facts is enhanced by the presence in the language of sentences that do not themselves represent such facts but that can be used to deduce sentences that do.

On either theory, the question arises how the ability to use secondary sentences can be learned. For example, how is the ability to use disjunctions in deductions acquired? On a verist or verificationist theory the answer is: by grasping the correctness-conditions (1) or (2) above. This answer is not available on a dualist theory, and no explicit alternative answer is offered in Hilbert 1925 or Quine 1951. There is, however, an answer that immediately suggests itself, namely, that the ability is acquired by directly grasping such rules of implication as the following:

(3) A disjunction is implied by each of its disjuncts.
A disjunction implies whatever is implied by each of its disjuncts.

In other words, the 'meaning of the logical constants'—if what determines their use may be called their 'meaning'—consists 'directly in the validity or invalidity of possible forms of inference' (1977, 363). It seems to be this answer that Dummett associates with dualism. It is worth mentioning that quite apart from any general dualist views, the specific view that an account of the 'meaning' of the logical particles is best given in terms of such implication-conditions as (3) rather than such truth-conditions as (2) has had many distinguished advocates, including (according to Prior 1960) several of Dummett's Oxford colleagues.

\(^8\) It is something of an oversimplification to describe Quine as a dualist, inasmuch as he often indicates that he regards the distinction between the observational periphery and the theoretical interior as a matter of degree rather than of kind. But for Dummet the similarities between Quine's position and that of the prototypical dualist Hilbert are more important than such differences.
Dualist theories may be called semi-verificationist. They are not verificationist in the strict sense, since some sentences are not assigned correctness-conditions. They are verificationist in a loose sense, since all sentences that are assigned correctness-conditions are assigned decidable, recognizable, verifiable correctness-conditions.

May dualist theories of language be called theories of meaning? Dummett sometimes takes '(theory of) meaning' in a broad sense, and insists on an affirmative answer (1973c, 378):

A model of language may also be called a model of meaning, and the importance of the conception of language sketched at the end of ‘Two Dogmas’ was that it gave in succinct form the outline of a new model of meaning. It is well known that some disciples of Quine have heralded his work as allowing us to dispense with the notion of meaning. But even the most radical of such disciples can hardly propose that we may dispense with the notion of knowing, or having mastery of, a language; and there is nothing more that we can require of a theory of meaning than that it give an account of what someone knows when he knows a language . . . [W]hatsoever warrant there may be for asserting that Quine has destroyed the concept of meaning does not appear from the ‘Two Dogmas’ model of language taken by itself. That has merely the shape of one theory or model of meaning among other possible ones.

However, Dummett sometimes takes ‘(theory of) meaning’ in a narrow sense, and insists on a negative answer (1973b, 309):

The theory of meaning, which lies at the foundation of the whole of philosophy, attempts to explain the way in which we contrive to represent reality by means of language. It does so by giving a model for the content of a sentence, its representative power. Holism is not, in this sense, a theory of meaning: it is the denial that a theory of meaning is possible.

When in the narrow, negative mood (as throughout 1977) Dummett is prepared to join Brouwer and Heyting in declaring that many of the sentences of classical mathematics are ‘incoherent’ and ‘unintelligible’. This sounds odd. For Dummett can hardly deny that the sentences of classical mathematics possess a definite usage within pure mathematics and a definite utility through applied mathematics. How can he, as a professed adherent of the slogan that meaning is use, then deny that those sentences have a meaning? Taken literally, the slogan implies that a sentence having a use thereby has a meaning. The answer, the explanation of the oddity, is, of course, that Dummett, as we have already seen, adheres to the slogan that meaning is use only in a non-literal, almost idiosyncratic, sense.

The narrow, negative terminology need not be misleading provided the following point is never forgotten: When Dummett says that many sentences of classical mathematics lack meaning-in-the-narrow-sense, he is only saying (in highly emotive terms) that the theory of meaning-as-conditions-for-correctness-as-a-representation-of-reality is inapplicable to those sentences. This factual claim about how language is cannot by itself imply any normative claim about how mathematics ought to be.
Some extra, tacit premise of a normative or prescriptive character is needed. As I interpret it, Dummett's criticism of dualism is prescriptive, and rests on an extra, tacit premise of anti-instrumentalism or representationalism, according to which every sentence of a language ideally ought to play a representational rather than merely an instrumental role. Thus when Dummett writes: 'A sentence is a representation of some facet of reality' (1973a, 309), according to my interpretation he has not quite accurately reflected his own view; 'ought to be' ought to be where 'is' is in the quoted formulation.

The requirement of representationality is, of course, accepted by platonists, who hold, in opposition to intuitionists and formalists, that this requirement is already met by our current language. Representationalism unites platonists and intuitionists in opposition to formalists, much as behaviorism unites Quineans and Dummettians in opposition to Davidsonians and Chomskians. (There are, however, important differences between the Harvard behaviorism of Skinner or Quine and the Oxford behaviorism of Ryle or Dummett. Moreover, it is not obvious that a verificationist or dualist must be a behaviorist.)

Consider the situation of a philosopher initially sympathetic, for behavioristic or other reasons, to a naive descriptive verificationism like that of the early positivists, who comes to appreciate that such a theory is inadequate as an account of the actual, current patterns of use in our language. One response would be to revise the theory to fit the facts of language, perhaps falling back to a semi-verificationist, dualist position. Another response would be to require a revision of language, to fit the norms of the theory. Quine and Dummett exemplify these two responses. Dummett's case against Quine stands or falls with the success or failure of his attempts to motivate the requirement of representationality.

In both texts under examination Dummett discusses, by way of offering such motivation, the following worry about languages of the sort depicted by dualist theories: In such a language, there is a threat of deducing incorrect primary sentences by means of secondary sentences. In one text, the worry seems to be that incorrect primary sentences might be deduced from (theories composed of) secondary sentences (1973a, 220):

> With what right do we feel an assurance that the observation statements deduced with the help of complex theories, mathematical, scientific and otherwise, embedded in the interior of the total linguistic structure, are true, when these observation statements are interpreted in terms of their stimulus meanings? To this the holist attempts no answer, save a generalised appeal to induction: these theories have 'worked' in the past, in the sense of having for the most part yield true observation statements, and so we have confidence that they will continue to work in the future.

This worry, or rather, the demand for a guarantee against it, is easily dismissed. Of course there is a threat that a scientific theory about, say, black holes or quarks may have incorrect observational consequences. We have seen such threats realized many times in the history of science, and we have known since the time of Hume that...
there can be no guarantee against them. And of course there is a threat that a mathematical theory about, say, $p$-adic cohomology or $\omega$-complete ultrafilters may have incorrect and even inconsistent computational consequences. We have seen such threats realized a few times in the history of mathematics (in connection with infinitesimal calculus and naive set theory), and we have known since the work of Gödel that there can be no guarantee against them. If and when such threats are again real-
ized, we will, as we always have in the past on such occasions, revise our theories. But why even then, let alone now, revise our logic? It is a delusion to imagine that a pre-
emptive change of logic could provide a guarantee against such threats, unless, indeed, the new logic were so restrictive as to make the formulation of any non-trivial theories impossible.

In the other text, the worry seems to be that incorrect primary sentences might be deduced from correct primary sentences by way of secondary sentences. On a sequen-
tial formulation of logic, this is the worry that a sentence

$$A_1, \ldots, A_n \rightarrow B,$$

with the $A_i$ primary and correct and $B$ primary and incorrect, might be deducible by pure classical logic, if secondary sentences are allowed to appear in the deduction. If only primary sentences are allowed to appear in the deduction, there is nothing to worry about, since primary sentences are decidable, and not even intuitionists doubt the trustworthiness of classical logic as applied to decidable sentences. As Dummett says, it would be a 'severe defect' in the classical rules of implication if by means of them 'we can construct a deductive chain leading from correct premises to an incorrect conclusion' (1977, 364). He reminds us that even on a dualist theory there is an external standard against which the acceptability of rules of implication is to be judged. A rule is acceptable only if it is sound, only if it preserves correctness in all instances where the notion of correctness is applicable, and in all instances where the premises and conclusion are all primary sentences.

Thus even if the 'meaning' of the logical particles is given by implication-conditions, not just any old particles and conditions will do. This point has been illustrated by Prior 1960. Prior's 'tonk' logic does not claim that classical logic is, in this sense, demonstrably unsound (as is Prior's 'tonk' logic). What worries Dummett is that classical logic seems to be not demonstrably sound. He desires a guarantee of soundness, or what would, as we have seen above, be sufficient for this, a guarantee of conservativeness, a guarantee that the addition of the secondary sentences to the language does not permit the deduction of any sequences (*) involving only primary sentences that were not deducible already (1977, 363–364). Dummett seems to hold that such a guarantee could only be provided by a semantic soundness or conservativeness proof, and that such a proof or 'justification' will be available only if we revise the language and extend the assignment-of-correctness-conditions or 'interpretation' to 'all the statements or formulas with which we are concerned' (1975, 229). Dummett seems here to overlook the possibility of a purely syntactic proof of soundness or conservativeness. As Richard Grandy has pointed out in a perceptive review 1982, just such a guarantee as Dummett seems to desire is...
provided by the famous Cut-elimination Theorem of Gentzen, according to which any sequence (*) that has a deduction at all has a deduction in which no symbols occur that do not occur in (*) already. Moreover, though Gentzen's theorem is about classical logic, Gentzen's proof is given in intuitionistic metamathematics. Thus the threat that worries Dummett seems elusive, to say the least.

In any case, the guarantee he desires would be intangible, on his own admission. For it is precisely the theme of his paper 1973b, that no 'justification of deduction' or soundness proof can be 'suasive', that can persuade anyone sincerely in doubt as to the soundness of logic. For any such proof, being a proof, would itself use logic.

In opposition to Kreisel, Dummett is concerned to argue for intuitionism not as one legitimate form of mathematics among others, but as the sole legitimate form (1977, 360). Dummett is concerned to argue for a revision amounting not to a reform, but to a revolution, in mathematics. Any revolution involves cost; that the benefit of an intangible guarantee against an elusive threat of unsoundness seems insufficient to outweigh. It seems that its desirability as a means towards the end of guaranteeing soundness is not a consideration sufficient to motivate the requirement of representationality.

Dummett might, of course, rest his case against formalism on the desirability of representationality as an end in itself. That he indeed values representationality highly for its own sake is suggested by his applying the unchallenged-for emotive term 'unintelligible' to sentences that he knows perfectly well how to use, but that happen to lack meaning-in-the-narrow-sense-of-conditions-for-correctness-as-a-representation-of-reality. Dummett's value-judgement might, however, be questioned by many mathematicians.

It hardly needs saying that the requirement of representationality will be rejected by the many pure mathematicians who value mathematics as an art. From their point of view, there is no sole legitimate form of mathematics. A mathematician may work now in intuitionistic, now in classical mathematics, just as a painter may work now in a representational, now in an abstract style. Personal taste will dictate how much time is devoted to each, though it may be said that the overwhelming majority of mathematicians find more beauty in the classical than in the intuitionistic style.

What does perhaps need saying is that the requirement of representationality may also be questioned by the many applied mathematicians who value mathematics for its contribution, through science, to the theoretical prediction and practical control of experience. From their point of view, it is essential that language should contain some sentences to serve as records or predictions of experience, as representations of empirical reality. But beyond this it seems wise to accept the advice of Carnap 1950 and be 'tolerant in permitting linguistic forms'. It is questionable whether the scope, accuracy and efficiency of applications to the empirical world would be enhanced by imposing the restriction that all sentences must play a representational rather than a merely instrumental role in language. Many physicists, mathematicians, logicians and philosophers have suggested precisely the contrary, that intuitionistic restrictions on mathematics would be detrimental to applications. Such views as the following are often voiced (Martin 1977, 172–173):
[C]onstructivism is in no sense ‘another mathematics’. It is, rather, a sophisticated subsystem of classical mathematics, which rejects the extremes in classical mathematics and carefully nourishes its effective computational apparatus. Unfortunately, it seems that it is these ‘extremes’—bold extrapolations, abstractions which are infinite and do not lend themselves to a constructivist interpretation—which make classical mathematics effective. One should try to imagine how much help mathematics could have provided twentieth century quantum physics if for the past hundred years it had developed using only abstractions from ‘constructive objects’.

I do not pretend to be an expert in such matters, but there are several studies in the literature that seem to me to indicate that such complaints are not entirely without foundation. As one example, there is an important series of papers by Pour-El and Richards (1979, 1981, 1983, 1984) establishing that much of the machinery of functional analysis deployed in quantum physics cannot be developed in its usual form within recursive analysis. And experience shows that what can or cannot be done recursively is a usually reliable (though by no means infallible) guide to what can or cannot be done intuitionistically.

As another example, there is a paper of Douglas Bridges, 1981, examining quantum physics from an intuitionistic viewpoint. Bridges is, to be sure, a follower of Bishop’s intuitionism-without-choice-sequences rather than of Brouwer’s intuitionism-with choice-sequences, but Bishop’s school has thus far been able to go further than Brouwer’s school in reconstructing applicable portions of functional analysis. Bridges is obliged to concede that ‘a constructive examination of the mathematical foundations of quantum physics does reveal substantial problems’. It is also worth mentioning that even if the indications cited are misleading and it turns out to be possible in principle to get by with intuitionistic functional analysis in applications to quantum physics, getting by in this way is very likely to be infeasible in practice.

Whether applications to the empirical world are of value is a question on which philosophers’ judgements vary over a wide spectrum. On the far right stands Plato, who regarded such applications as evil. On the far left stand the Gang of Four, who regarded the development of mathematics for any purpose but such applications as evil. Among intuitionists, Brouwer was, in this respect, a thorough-going Platonist, with an attitude not of passive indifference, but of active hostility towards applications (see van Stigt 1979). Weyl, however, seems to have been uneasy over his inability to reconcile his philosophical attraction towards intuitionism with his scientific interest in applications.

One need not be a Maoist to sympathize with this unease, and to be disturbed by an argument for the claim that intuitionism is the sole legitimate form of mathematics in which any consideration of widely-held doubts as to the adequacy of intuitionism for applications is omitted. (The omission is the more surprising in an argument directed against Quine, since doubts as to adequacy for application have been central among Quine’s objections against other revisionist proposals such as those of the nominalists.) The omission suggests a tacit system of values so unworldly as to be irresponsible.
Dummett’s Case for Intuitionism

Dummett may perhaps be absolved personally from charges of irresponsibility and inquisitorial interference with science. For though he advances an argument for intuitionistic revisionism, he is cautious enough to distance himself personally somewhat from that argument. He is not so bold as to claim that its conclusion ought to be accepted and put into practice. What he claims is that it is an argument ‘of considerable power’ (1973a, 226). In view of the gaps and weakness in the argument that I have tried to point out, even this more cautious claim might well be challenged.

4. Summary

The following table summarizes my interpretation of and commentary on Dummett's case for intuitionism.

<table>
<thead>
<tr>
<th>Philosophies of mathematics</th>
<th>FORMALISM (Hilbert)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Associated theories of meaning</td>
<td>HOLISM (Quine)</td>
</tr>
<tr>
<td>Character of objection to theory of meaning</td>
<td>PRESCRIPTIVE</td>
</tr>
<tr>
<td>RADICAL DESCRIPTIVE</td>
<td>We do not and could not speak a language of the sort described by the theory</td>
</tr>
<tr>
<td>BEHAVIORISM</td>
<td>REPRESENTATIONALISM</td>
</tr>
<tr>
<td>There can be nothing more to knowing the meaning of a sentence than being able to use it</td>
<td></td>
</tr>
<tr>
<td>Principle upon which objection is based</td>
<td>Every sentence ought to serve as a representation of extra-linguistic reality, not a mere intra-linguistic instrument for deducing other sentences</td>
</tr>
<tr>
<td>Argument for principle</td>
<td>Representationality is desirable</td>
</tr>
<tr>
<td>If there were anything more to such knowledge, the extra ingredient could be neither acquired nor manifested</td>
<td>(a) as a means towards guaranteeing soundness</td>
</tr>
<tr>
<td>(b) as an end in itself</td>
<td></td>
</tr>
<tr>
<td>Comment</td>
<td>(a) Soundness can be guaranteed without representationality</td>
</tr>
<tr>
<td>The acquisition and manifestation arguments involve fallacies and circularities</td>
<td>(b) Representationality may conflict with the desirable end of applicability</td>
</tr>
</tbody>
</table>
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"Who is a theory of truth?", to appear.

