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PLATONISM IN MATHEMATICS*,[1]

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Abstract: In this paper I examine arguments by Benacerraf and by Chihara against Gödel's platonistic philosophy of mathematics.

Key-words: Benacerraf. Gödel. Chihara. Platonism. Mathematics.

PLATONISMO NA MATEMÁTICA

Resumo: Neste artigo examino argumentos de Benacerraf e de Chihara contra a filosofia platônica da matemática de Gödel.

Palavras-chave: Benacerraf. Gödel. Chihara. Platonismo. Matemática.

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[1] This paper derives from my response to Benacerraf (1973) in the American Philosophical Association symposium Mathematical Truth. I briefly commented on Benacerraf's paper at the symposium, and then developed my response in more detail, presenting versions of it at Cornell, Princeton, and Rockefeller in 1974-75, and later at other places as well. It was an important part of the background motivation for my book *Logical Forms*, where a more sustained examination of the issues raised by Benacerraf begins to be articulated. I am very pleased to dedicate it to Itala D'Ottaviano on this occasion.

1. THE BACKGROUND OF BENACERRAF'S ARGUMENT

According to Benacerraf, the chief virtue of realist accounts of mathematics is that the treatment of mathematical truth is continuous with what he takes to be the right account of truth for non-mathematical discourse. This account is Tarski's, and its main feature is the referential analysis of truth in terms of naming, predication, satisfaction and quantification.

Reference is what connects language and reality, and Kripke (1972) gives an illuminating picture of that connection. This picture is most fully developed for proper names and certain common names (of natural kinds), but is suggestive for other cases as well. The basic idea is that what a speaker refers to, if anything, by his use of a name in a given occasion, is determined by an ancestral chain, beginning with the occasion of the speaker's use of the name and linking backwards, from one speaker's use to another's. Although Benacerraf raises the question whether a realist account of mathematical truth really fits in with Kripke's picture, the main emphasis of his discussion of realism is on mathematical knowledge.

Benacerraf holds that a satisfactory account of knowledge, mathematical or not, must connect *what* we know with *how* we know it. For knowledge that involves justified true belief, this condition requires that for X to know that a proposition p is true, there must be some connection between the grounds of X's belief that p and the grounds of p's truth. That is to say, the features of the world in virtue of which X believes that p must be related in a suitable way to the features of the world in virtue of which p is true. In support of this one can argue that if the grounds of X's belief that p are not related to the grounds of p's truth, then it would seem that whatever justification there might be for X's belief, there is no justification *on those grounds*, and, hence, since those are X's grounds, that X does not know that p. Of course, we may also build the condition into what we allow to count as justification and say that in those cases where there is no connection, although there appears to be

justification, there really is none. Either way, it is reasonable to conclude from this that the grounds of *X*'s belief that *p* cannot be "independent" of the grounds of *p*'s truth; i.e., that it cannot be a question of mere concurrence of unrelated features of the world. It is natural to consider, therefore, what is involved in these connections.

Benacerraf thinks something essentially causal is involved. His reasons are basically two. The first depends on the observation (p. 413) that if

> (*a*) it is claimed that *X* knows that *p*,

but

> (*b*) we think that *X* could not know that *p*,

and

> (*c*) we are satisfied that *X* has normal inferential powers, that *p* is indeed true, etc.,

then

> (*d*) we are often thrown back on arguing that *X* could not have come in possession of the relevant evidence or reasons: that *X*'s four-dimensional space-time worm does not make the necessary (causal) contact with the grounds of the truth of the proposition for *X* to be in possession of evidence adequate to support the inference (if an inference was relevant).

That we argue thus, and sometimes justifiably, I quite agree. But whether any inferences about knowledge in general can be drawn from this will have to be considered with some care.

Benacerraf's second reason derives from his acceptance of an empiricist epistemology. He thinks that all knowledge should be accountable as boiling down, by inference, to knowledge in the present of ordinary objects close at hand. These cases are analyzed as justified true belief with causal condition. His sample case is the following (pp. 412-413):

> (*e*) For Hermione to know that the black object she is holding is a truffle is for her (or at least requires her) to be in a certain (perhaps psychological) state. It also requires the cooperation of the rest of the world, at least to the extent of permitting the object she is holding to be a truffle. Further—and this is the part I would emphasize—in the normal case, that the black object she is holding is a truffle must figure in a suitable way in a causal explanation of her belief that the black object she is holding is a truffle.

He acknowledges that he cannot state what the causal condition really amounts to, but he suggests that (*a*)-(*d*) shows that "some such view must be correct and underlies our conception of knowledge" (p. 413). The extension to the general case is stated as follows (p. 413):

> (*f*) Other cases of knowledge can be explained as being based on inferences based on cases such as these, although there must evidently be interdependencies. This is meant to include our knowledge of general laws and theories, and, through them, our knowledge of the future and much of the past. This account follows closely the lines that have been proposed by empiricists, but with the crucial modification introduced by the explicitly causal condition mentioned above—but often left out of modern accounts, largely because of attempts to draw a careful distinction between "discovery" and "justification."

It is essentially for these reasons that Benacerraf considers the connections to be causal. In fact, since he views both the Kripkean account of reference and the account of knowledge just outlined as causal accounts, he concludes that *X* saying knowingly that *S* involves *X* in two causal relations to the referents of the names, predicates, and quantifiers of *S*; via *X*'s use of these names, predicates, and quantifiers, and via the grounds of *X*'s belief that *S* (p. 412).

Although calling these views "causal" is neither unreasonable nor unmotivated, it is not at all clear, in either case, what is the appropriate interpretation of "causal". At any rate, without a great deal of further development, it would seem to me unreasonable to argue thus:

(I) A realist account of mathematical truth analyzes the truth conditions of mathematical propositions in terms of mathematical objects standing in certain relations to one another.

(II) All knowledge that certain objects are thus and so involves some causal connection between the knower and the objects known to be thus and so.

(III) As viewed by the realist, it is in the nature of mathematical objects that they do not enter into causal relations.

(IV) Therefore, on the realist account mathematical truths must be unknowable.

(Appealing to the causal account of reference, one may infer further that in the realist view not only can we not know any mathematical truths, but we cannot even talk about mathematical objects.)

Even if these premises turn out to be true, (I)-(IV) is not a convincing *argument* against realism. For, in order to make the premises plausible, one would have to give an alternative account of mathematical truth and mathematical knowledge. In other words, if one does not have an alternative account, and one believes that mathematical knowledge *is* knowledge, to appeal to empiricist views to support (II) begs the question.

Yet, such an interpretation is encouraged by Benacerraf[2] by using "causal" somewhat carelessly, and by concluding his presentation of the causal account of knowledge with the remark (p. 414):

> (*g*) It will come as no surprise that this has been a preamble to pointing out that combining *this* view of knowledge with the "standard" view of mathematical truth makes it difficult to see how mathematical knowledge is possible. If, for example, numbers are the kinds of entities they are normally taken to be, then the connection between the truth conditions

[2] And it is attributed to him by Steiner (1973, p. 58), and he does not protest (p. 414 note 8).

> for the statements of number theory and any relevant events connected with the people who are supposed to have mathematical knowledge cannot be made out.

Benacerraf does not seem to mean this to be interpreted as (I)-(IV), however, and there are many indications in his paper that the structure of the argument is more cautious. Namely:

(1) A philosophically satisfactory account of truth, reference, meaning, and knowledge must embrace them all, and must be adequate for all the propositions to which these concepts apply; in particular, it must include mathematical as well as non-mathematical discourse (p. 404). Moreover, unless there are good reasons to the contrary, the account should apply more or less uniformly to both.

(2) An account of truth, for any kind of proposition (or sentence), should somehow connect what is said to be true with some feature of the world, so that one can see *why* the proposition or sentence is true given that the world is thus and so.[3]

(3) An account of knowledge, for any kind of proposition, should establish some relation (and say something about what it is) between what is known and *how* it is known.

(4) At least for the empirical case, an account of truth *à la* Tarski, in terms of the referential apparatus of naming, predication, satisfaction, and quantification, combined with a Kripkean account of reference itself, seems to be on the right track.

(5) At least for the empirical case—but also for mathematical propositions that are known by proof—a causal account of knowledge as justified true belief, requiring some kind of causal connection to obtain

[3] This is somewhat vague but it is hard to state. I am not sure that Benacerraf would agree with my statement, but his first adequacy condition (p. 408, and *cf.* also pp. 418-419) seems to be getting at something like this. (His appeal to a theory of truth theories is something like this.)

between what is known and how we know it, seems also to be on the right track, and in the empirical case to mesh nicely with the Tarskian account of truth.

(6) Realist accounts of mathematical truth treat it uniformly with non-mathematical truth by analyzing the truth conditions of mathematical propositions in terms of reference to mathematical objects bearing certain relations to one another.

(7) A realist account of mathematical knowledge (Gödel's), that fits in nicely with a realist account of mathematical truth, and that is a natural extension of the causal account of knowledge to the mathematical case, does not, in fact, establish a believable connection between what we know in mathematics and how we know it.

(8) There are no other realist accounts of mathematical knowledge that are even *prima facie* plausible candidates (given the various conditions previously stated).

(9) Therefore, at least as far as one can judge at this point, although mathematical realism *seems* to have many virtues on account of its analysis of mathematical truth, it has not yet provided a plausible view of mathematical knowledge—and, hence, of mathematics as a whole.

Whereas Benacerraf may not agree with this exactly as stated, it is the message I get from his paper. Since (1) seems, as Benacerraf says, obvious, and I have already motivated (2)-(4) to some extent, in the remaining sections of the paper I will discuss what is involved in (5)-(8), and then try to evaluate whether one can conclude (9), and what force it has.[4]

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[4] It is important to emphasize that Benacerraf draws the same conclusions, *mutatis mutandis*, about the other views of mathematics, and that he says (pp. 416, 403) that these conclusions throw some doubt on the adequacy of the accounts for the empirical case as well.

2. BENACERRAF'S VIEW OF KNOWLEDGE

It is not clear that Benacerraf is committed to the view, even for the empirical case, that the connection between the grounds of *X*'s belief that *p* and the grounds of *p*'s truth is a causal connection in any but a rather loose sense of "causal." In fact, in his repeated attempts to get across what he means, he seems to be emphasizing more that there *is* a connection of some kind than its causal nature; e.g. (p. 414):

> The connection between what must be the case if *p* is true and the causes of *X*'s belief can vary widely. But there is always *some* connection, and the connection relates the grounds of *X*'s belief to the subject matter of *p*.

As is also illustrated in this statement, by comparison with some others that I quoted earlier, Benacerraf's use of "causal" varies a bit from context to context. Obviously he believes something causal to be going on, but exactly what it is, and how it can be formulated in a general way, he does not know—and says so. It is clear, however, that he sees much of the content of the causal view as given by (*a*)-(*d*), and I want to discuss this argument more fully.

As I said earlier, I agree that we argue as in (*d*)—at least we often argue in a similar way to conclude that someone does not know something. The most striking cases are the Gettier type examples, of course, but we use this form of argument all the time. I believe, for example, that the colleague who left my office a half-hour ago is now on campus. The grounds of my belief are, essentially, that he told me that he was going to lecture. Given the facts, I do not know (now) that he is now on campus. And the reason is as given in (*d*); namely, that the grounds of my belief (his *telling me* that he was going to lecture) do not connect in the appropriate way (causally, let us say) with his *being* on campus.

But Benacerraf's argument involves a modality, which I am not sure he means seriously—and, if so, what depends on it. One way the modality may be meant is: *X* could not know on such and such grounds (some specific ones) that *p*. This emphasizes, without adding much

further content, that X does not know on those grounds that p, and is what I had in mind as a non-serious (although perfectly correct) use of the modality. This interpretation may be indicated by Benacerraf's statement (p. 414, 1st paragraph) that if "X's ... evidence is [not] drawn from the range determined by p ... then X could not know that p." On the other hand, the statement of (*a*)-(*d*) suggests a stronger notion, for which there is only one reasonable interpretation; namely, that X does not, up to the time of the claim to knowledge, connect appropriately with *anything* that, on the one hand, would be grounds for X's believing that p, and, on the other hand, connects appropriately with the grounds of the truth of p. In other words, the modality is a real modality, but is relative to the facts obtaining up to the time of the knowledge claim. (It is clear that the modality cannot mean that even had circumstances been different, X would not know that p.)

On this stronger interpretation (*a*)-(*d*) is quite reasonable, although I am unsure just how often we can argue this way. In connection with my earlier example, I should think that I could not know (now) that my colleague is now on campus. This would require arguing not only that his telling me so a half hour ago does not connect in the appropriate way with his being on campus now, but also that my space-time worm does not make (causal) contact with anything appropriately connected with his being now on campus, which I could use as grounds to believe that he is now on campus. And this is not all that easy, partly because there are interdependencies—as Benacerraf points out. (Of course, in this particular case I am a bit handicapped to give the argument, because I do not know my colleague's thereabouts, and hence I do not fit (*c*). Others may be able to give the argument, however.)

In any case, one could try using (*a*)-(*d*) to support a reasonably strong interpretation of "causal" applying to all cases of empirical knowledge, by suggesting that for any such p there is an X such that X and p fit (*a*)-(*d*). But this is not very plausible, because for a proposition such as

(*i*) Liquids seek their own level,

there is little chance of finding an X that will fit (*a*)-(*c*) for which we can argue as in (*d*)—including the "etc." in (*c*), which, among other things, presumably rules out X's not understanding p and not having any "justification" for believing that p. Anyone's experience of the world would seem to be broad enough to include causal contact with the grounds of the truth of (*i*). Perhaps one may argue that one can build up a case of an X which would fit (*a*)-(*d*). Perhaps so, but consider now the propositions

(*ii*) There are many things,

(*iii*) Objects interact with each other,

(*iv*) Some things are bigger than others.

For these cases I cannot see how one can even *conceive* of a normal human being whose space-time worm does not make the necessary causal contacts. These are things, I suppose, that we do come to know in due course, but for which we simply could not argue as in (*a*)-(*d*).

Of course, these are not counterexamples to the causal connection *per se*; on the contrary, the reason that we cannot argue causally is because there is causal contact *all over*. So, at this point, Benacerraf can appeal to his second reason, and say that our knowledge of (*i*)-(*iv*) obviously fits in with his empiricist schema. So let us consider that now.

Some of what gives bite to the causal condition for the basic cases of knowledge, as that of Hermione, is that such knowledge is obtained by direct sensory experience—but, of course, as Gettier's examples show, it has to be the appropriately connected sensory experience. (In his reply to Gödel (p. 415), Benacerraf seems to hint that part of the account, for physical objects, of "the link between our cognitive faculties and the objects known" depends on the causal theory of perception, supported

by physics, biology, etc.) Of course, not all knowledge of ordinary objects close at hand is obtained in this way; for example, my present knowledge that the person sitting across the room has a liver, is not. Some may deny that I can know this without closer examination, but I am assuming that Benacerraf does not, and I take it that it is precisely such cases that he means to cover by his "interdependencies", by viewing them—correctly, in my opinion—as depending on inferences from knowledge of general principles and various particular facts.

The point of the empiricist assumption is, then, that for these cases, as well as for all other non-basic cases, *X*'s knowledge that *p* can be analyzed as depending on inferences that eventually come to rest on *X*'s knowledge of basic p_1, p_2, ..., p_n, involving direct causal contact with various objects. It may be difficult in practice to determine this for specific cases, but the general picture is clear enough. One can then argue, I suppose, that if *X* is justified in inferring *p* from p_1, p_2, ..., p_n, then there must be some connection between what is the case by *p*'s truth and what is the case by the truth of p_1, p_2, ..., p_n. Hence, since *X*'s knowledge of p_1, p_2, ..., p_n involves a causal connection between the grounds of *X*'s beliefs and what is the case by the truth of these propositions, we get some connection, involving various causal links, between the grounds of *X*'s belief that *p* and the grounds of *p*'s truth. Of course, unless the connection between the grounds of *p*'s truth and the grounds of the truth of p_1, p_2, ..., p_n is itself essentially causal, at least in some respects, it does not follow that there is a causal connection between the grounds of *X*'s belief and the grounds of *p*'s truth. But this does not matter much, for, as long as there is some connection as described above, one would expect to be able to appeal to (*a*)-(*d*) to show that *X*'s causal contact with the world can make a difference for this kind of case. In any case, my examples (*ii*)-(*iv*) are not the sort of thing that raises problems for this side of the argument.

I do not know whether Benacerraf would agree with any of this, but, as I see it, it fits in with what he says. I shall discuss later to what

extent these considerations support his premise (5). I want to consider now whether anything follows directly about mathematics.

Benacerraf emphasizes that the realist cannot avoid his criticism in (*g*) by simply appealing to proofs (p. 414):

> (*h*) One obvious answer—that some of these propositions are true if and only if they are derivable from certain axioms via certain rules—will not help here. For, to be sure, we can ascertain that *those* conditions obtain. But in such a case, what we lack is the link between truth and proof, when truth is directly defined in the standard way.

The realist *can* give an account of the connection between proof and truth, but in order for this to have any force in connection with knowledge it would have to depend on a direct account of our knowledge of what is not proved. Of course, anything can be proved from something or other, but there is a clear distinction, at least in some cases, between what is proved to gain knowledge that it is the case, and what is proved to gain knowledge about other things. That $1 < 2$ is a case in point; if we prove this in a systematic development of first order arithmetic, we do it to show something about the system itself, not to gain conviction that $1 < 2$.

One way in which this ties up with the preceding discussion, is that Benacerraf's argument in (*g*) suggests that the realist cannot say how anybody's experiences can be relevantly connected with his mathematical knowledge. I would reformulate (at least part of) his point as follows.

In the empirical case we can show, at least in some cases, that certain events connected with a person X are relevant to his knowledge, or lack of knowledge, of one or another proposition p. Sometimes we may be able to do this more or less directly by showing how these events tie up with the grounds of p's truth. In some other cases we can appeal to the type of argument in (*d*). Why can't the realist, given his view of mathematics, do the same for at least some cases? Not in terms of proof, of course, but for some of the propositions that are not proved. For,

unless the only differences in our mathematical knowledge lie in our knowledge of *proved* propositions, it would seem that if X knows a basic mathematical proposition that Y does not, there must be some relevant facts about X and Y that account for X's knowledge and Y's lack of knowledge. On the face of it, it seems likely that there are such cases; but if there are not, or if all of them involve either abnormality or lack of understanding on Y's part, then what sorts of facts about *all* of us account for *that*?

These are relevant questions, I agree. I do not know whether there are any mathematical cases not involving proof that will fit something like (*a*)-(*d*), for example, but I shall have something to say about this later. Since Gödel speaks to this point, Benacerraf considers his views.

3. BENACERRAF'S CRITICISMS OF GÖDEL'S VIEWS

> (*i*) ... the objects of transfinite set theory ... clearly do not belong to the physical world and even their indirect connection with physical experience is very loose ...
>
> But, despite their remoteness from sense experience, we do have a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception, which induces us to build up physical theories and to expect that future sense perceptions will agree with them and, moreover, to believe that a question not decidable now has meaning and may be decided in the future. The set-theoretical paradoxes are hardly any more troublesome for mathematics than deceptions of the senses are for physics.

After quoting these remarks, except for the last sentence, from Gödel (1947, pp. 483-484), Benacerraf expresses mixed feelings about them. On the one hand, he finds Gödel's analogy between mathematics and physics encouraging, because it fits in with his conception of the general features that an account of knowledge should have: tie up what is known with the knower, and apply in a reasonably uniform way to both

mathematical and non-mathematical knowledge. On the other hand, however, Benacerraf does not think that Gödel has even begun to make a case for his views (pp. 415-416):

> (*j*) ... without an account of *how* the axioms "force themselves upon us as being true," the analogy with sense perception and physical science is without much content. For what is missing is *precisely* what my second principle demands: an account of the link between our cognitive faculties and the objects known. In physical science we have at least a start on such an account, and it is causal. We accept as knowledge only those beliefs which we can appropriately relate to our cognitive faculties. Quite appropriately, our conception of knowledge goes hand in hand with our conception of ourselves as knowers. To be sure, there is a *superficial* analogy. For, as Gödel points out, we "verify" axioms by deducing consequences from them concerning areas in which we seem to have more direct "perception" (clearer intuitions). But we are never told how we know even these, clearer, propositions. For example, the "verifiable" consequences of axioms of higher infinity are (otherwise undecidable) number-theoretical propositions which themselves are "verifiable" by computation up to any given integer. But the story, to be helpful anywhere, must tell us how we know statements of computational arithmetic—*if they mean what the standard account would have them mean.* And *that* we are not told. So the analogy is at best superficial.

A bit further down, while indicating what he finds encouraging about Gödel's attitude, he sums up the matter thus (p. 416):

> (*k*) He sees, I think, that something must be said to bridge the chasm, created by his realistic and platonistic interpretation of mathematical propositions, between the entities that form the subject matter of mathematics and the human knower. Instead of tinkering with the logical form of mathematical propositions or with the nature of the objects known, he postulates a special faculty through which we "interact" with these objects. We seem to agree on the analysis of the fundamental problem, but clearly disagree about the epistemological issue—about what avenues are open to us through which we may come to know things.
>
> If our account of empirical knowledge is acceptable, it must be in part because it tries to make the connection evident in the case of our theoretical knowledge, where it is not *prima facie* clear how the causal account is to be filled in. Thus, when we come to mathematics, the

> absence of a coherent account of how our mathematical intuition is connected with the truth of mathematical propositions renders the overall account unsatisfactory.

These are Benacerraf's criticisms of Gödel. No doubt they are not meant as an *argument* that Gödel's views are not right, but merely as raising some basic questions for which he does not see an answer, and which, as long as they are not answered, robs the account of much of its credibility.

My presentation so far supports the view I expressed earlier that Benacerraf's argument is something like (1)-(9).[5] His last remark, in particular, seems to show that the strictly causal argument is quite off the mark. I do not take this to mean, however, that Benacerraf does not *believe* that ultimately all our knowledge, including our mathematical knowledge, cannot be accounted for by something like a causal theory of knowledge. I should think that he does. His dilemma is that, as long as we cannot account for our mathematical knowledge in this way we cannot trust the causal view even for the cases for which it seems to work, because, since there are clear interdependencies between our empirical and our mathematical knowledge, the reason we cannot see how to extend it to cover the mathematical case might be that it is simply wrong for the empirical case as well (see pp. 403-404).

Now that the background is set, let me turn to a more systematic discussion of some issues.

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[5] The only premise that I have not motivated is (8). Benacerraf, in fact, does not explicitly say anything like this—although after his criticisms of Gödel he hints, by means of a reference to Plato's theory of recollection, that the realist may have to end up appealing to innate knowledge—but it seems to me that (8) is implicit in Benacerraf's paper. (I will discuss the point about recollection later.)

4. CHIHARA'S MYTHOLOGICAL PLATONISM

It is sometimes suggested, at least in verbal discussions, that mathematical intuition, as Gödel sees it, is not something really had by the run of the mill mortal. Only mathematicians of his caliber have it, and although it might allow them to have contact with mathematical reality, and perhaps to "see" such things as that the Continuum Hypothesis is false, the rest of us have to follow blindly in their steps and take it on faith.

An alternative suggestion is that mathematical intuition, for all that Gödel says, may best be viewed as the kind of insight that we can develop just about anything—say, a work of fiction—only about mathematics. Chihara (1973, Chapter 2) argues for this and concludes that all that Gödel has offered support for is what he calls "mythological platonism."

I think that both of these suggestions are quite off the mark. To take the second first, it is undoubtedly true that we can develop intuitions, in the ordinary sense of the word, about all sorts of things, including fictional works and characters. Sherlock Holmes buffs will tell you at great length what he would or would not do in certain novel circumstances. Moreover, there is often considerable agreement among them, even when there is no neatly circumscribed part of the novels from which these conclusions follow in a more or less straightforward way. Since it seems fairly obvious that whatever may be going on here does not warrant realism about Holmes, Watson, and the rest, are we to conclude then by analogy, that what Gödel's views support is at best some form of conceptualism, a mythological platonism?

It is plain that if one wants to support one's views about something by appealing to an analogy with something else, one should be prepared to argue for similarity of certain central features, and to show that these features and similarities thereof are enough to support one's claim that the two situations are parallel, and that certain conclusions, if warranted for one, are therefore also warranted for the other. It is equally

plain that if one wants to reject the proposed conclusions by appealing, in the manner suggested above, to a further analogy, one should argue in the same way that one's example is parallel to the view under criticism and, the other way around, that it is not parallel to the allegedly supporting view—although such examples are usually set up so that this last is supposed to be clear. Chihara's strategy is based on this.[6]

He tells a story about a world, Myopia, whose inhabitants have a long story which is passed on from generation to generation, and is continually extended, and which supposedly is in some important respects analogous to our mathematical theories—there is general agreement about new extensions, extendibility appears to be open ended, Myopians who cannot grasp a certain amount of the story are considered abnormal, and the story is of great importance to their social life. The story is about a god, Myo, and, as it happens, at some point a certain question (the "Axiom of Choice") is shown to be undecidable from as much of the story as has been developed so far. The question is whether individuals are free to choose to use artificial contraceptives—and this depends on whether Myo himself used artificial contraceptives or not. An ontological platonist argues, somewhat like Gödel, that the axiom must be objectively true or false, and a mythological platonist argues that the whole thing is a myth, and that all that follows from the continued extendibility of the story is that some day it might be settled whether the axiom is true (or false) to the story (pp. 67-69).[7]

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[6] Chihara also argues directly (pp. 75-83) that Gödel has not supported his analogy, raising a number of basic questions which he claims that Gödel has not answered—some of these points are similar to some of Benacerraf's points mentioned above, Like Benacerraf, Chihara does not want to conclude that Gödel is wrong, but only that he has not offered good arguments for his views, and that it seems rather unlikely that any such arguments are to be found. Although I will discuss some of Chihara's other points later, at this point I am only concerned with his argument by analogy.

[7] Later (pp. 78-79) Chihara offers another, more "real", analogy in terms of our own views concerning the existence of God.

Chihara does not think that mathematics is a story, but he suggests (pp. 70ff) that we can also talk about something being true to a concept without requiring a realist view of concepts, and that what Gödel's arguments may be reasonable arguments for, is this mythological platonism—he thinks, for example, that what impressed Gödel as the axioms of set theory forcing themselves upon us as being true, really boils down to their being true *to a concept* (p. 79). Chihara suggests that Gödel himself may have had mythological platonism in mind in the following remark (1947, pp. 484-5):

> ... the question of the objective existence of the objects of mathematical intuition (which, incidentally, is an exact replica of the question of the objective existence of the outer world) is not decisive for the problem under discussion here. The mere psychological fact of the existence of an intuition which is sufficiently clear to produce the axioms of set theory and an open series of extensions of them suffices to give meaning to the question of the truth or falsity of propositions like Cantor's continuum hypothesis.

For, if we do not assume that there are sets, then in what sense but something like true to a concept could the Continuum Hypothesis be true or false? (*Op. Cit.*, p. 70) Although there is something right to this point of Chihara's, in the context of his discussion his remarks are somewhat misleading. For Gödel is not only remarking upon certain features of the concept of *set*, but also upon certain features of the concept of *truth*. He continues the above passage with:

> What, however, perhaps more than anything else, justifies the acceptance of this criterion of truth in set theory is the fact that continued appeals to mathematical intuition are necessary not only for obtaining unambiguous answers to the questions of transfinite set theory, but also for the solution of the problems of finitary number theory (of the type of Goldbach's conjecture), where the meaningfulness and unambiguity of the concepts entering into them can hardly be doubted. This follows from the fact that for every axiomatic system there are infinitely many undecidable propositions of this type.

Although Chihara sees no problem in the distinction between objectively true and true to a concept, my understanding of Gödel's remarks is that what gives bite to our concept of truth is not just, or even primarily, the assumption of some objects "out there", but, rather, our having criteria which make it plausible that every question concerning a certain domain (physics, set theory, etc.) has a unique and non-arbitrary solution. Since he thinks that there are such criteria for set theory, Gödel claims that it makes sense to talk about the Continuum Hypothesis being true or false, even if we have not yet settled it one way or another, and cannot settle it on the basis of our present axioms. Chihara's suggestion is that we *have* a concept of set—and we may in the future figure out that the Continuum Hypothesis is true (or false) to this concept—but that a mythological platonist need not be committed to this, for he can simply say that the Continuum Hypothesis is neither true nor false to our concept of set (pp. 71-72). This is *precisely* what Gödel denies, however, *independently* of the assumption that sets are objectively existing entities.

In my view, Gödel is not talking about truth to our concept of set in Chihara's sense, but about our concept of set *and* our concept of truth. To put the point another way, in the case of the material world, what gives bite to our talk of truth is not the assumption of the objective existence of the objects, but the possibility of uniquely and non-arbitrarily settling various kinds of questions. Since, if we assume neither the objective existence of material objects, or of mathematical objects, the situation is parallel, it makes sense to talk about truth and falsity in both cases. Moreover, since it is precisely this uniqueness and non-arbitrariness that leads us to assume the objective existence of the material world, and feel justified in doing so, it is for the same reason that we can feel justified in our assumption of the objective existence of mathematical reality.

So, with respect to Benacerraf's discussion of truth, I think that he misinterprets Gödel's views by suggesting that the motivation for his realism is to make sense of our talk of truth. In fact, both Benacerraf and

Chihara suppose that the motivation for realism has to be *semantic* in order to make sense of mathematical truth. Gödel's motivation seems to me primarily epistemological and metaphysical, not semantic.

Benacerraf's criticisms of the assumption of proof as a basis for truth are well taken, but the reason proof will not do all by itself is not because it lacks the referential features of Tarskian truth, but, rather, because it cannot be used to justify the axioms, and, hence, in cases of undecidability, it cannot by itself provide non-arbitrary unique answers.

5. ANALOGY: GÖDEL, CHIHARA

Gödel's analogy between mathematics and physical science is based on the following considerations:

(1) Both are extremely rich parts of our conceptual view of the world.

(2) Both have been systematically developed in the form of various theories about their respective domains.

(3) In both cases our theories have been constantly extended to cover undecided or problematic cases in a non-arbitrary way. (The oft cited example of geometry is discussed by Gödel and argued not to be similar to the case of set theory (1947, pp. 482-3).

(4) In both cases there are criteria for choosing between various alternatives which *themselves* suggest that unique non-arbitrary answers can be arrived at. What gives force to these criteria is *not* primarily the assumption that one or the other kind of entities objectively exist. (If anything, the fact that we can reach such non-arbitrary answers and that we can envisage obtaining them, by such criteria, for as yet undecided cases, is *part* of what gives force to the claim of objective existence.)

(5) In both cases we cannot satisfactorily account for the views we have merely on the basis of our data. In the case of physics the data are

our sensations (or perceptions) and it is not possible to interpret the propositions we want to assert about material objects as propositions about our sensations. Similarly in the mathematical case, although we have clear intuitions, it is not possible to interpret classical mathematics as being *about* these. (Much of Gödel's examination of Russell's views is to this point.)

A striking feature of Chihara's examples, both the Myopian example and the real example concerning philosophical discussions about God's attributes, is that they bear very little resemblance to mathematics. In the Myopian example what we have to go on are the few claims concerning general understanding and agreement, extendibility, importance, and an undecided question. These claims are not even built up to the point where they begin to hang together. In fact, in order to envisage the situation as Chihara wants us to, we have to appeal to the situation in mathematics. Yet, on the basis of these few claims having no intrinsic content, Chihara wants us to decide which of the various Myopian views concerning the "Axiom of Choice" is more plausible. And not only that, but since the example is purposefully designed so that any but the myth view is made to look ridiculous, we are put in the position that any other choice is out of the question. The real example, on the other hand, does not depend so much on the implausibility of the alternatives to the myth choice. But the only feature of this case that is claimed to have any resemblance to what Gödel says about mathematics, is that an imaginary axiomatizer of his conception of God may consider that certain axioms force themselves upon him as being true. And Chihara argues that this could be better described as the axiom in question forcing itself upon him *as true to his concept of God.* From which he concludes (pp. 78-79):

> ... the mere fact that, by going through some such process of analysis this philosopher should be able to arrive at axioms about which there is, among Christian philosophers, general agreement, would not imply that

> we humans are able to "perceive" God or that we have "theological intuitions." So far as I can see, the phenomenon that Gödel describes as that of axioms of set theory forcing themselves upon us as being true is really more like the above case of an axiom's forcing itself upon us as being true to a concept, than a case of perceiving objects "external to us" as Gödel suggests.

But isn't it obvious that this differs from the situation we have in mathematics? Where is the wealth of experience? The systematic development? The agreement? The extendibility? The unambiguity of some central concepts leading to a rich theory?

Even if these examples are not convincing, *could* Chihara come up with a reasonable example? I think not. For consider the difficulties of producing an example that would serve his purposes. On the face of it, there does not seem to be any real example of the kind. In order to build it up from scratch as a science-fiction story, one would have to attempt to impart some feeling for, even if minimally, an extremely rich and essentially ever-present kind of experience or intuition, *and* an extremely rich and systematic kind of theory connected with it. And if this were done—as opposed to merely *claimed*—one would still have to argue that a Gödelian view of the subject in question is implausible. So, it looks to me as if this method of argument does not have much future.

6. SOME REMARKS ON MATHEMATICAL INTUITION

Gödel says (1947, p. 484):

> It should be noted that mathematical intuition need not be conceived as a faculty giving an *immediate* knowledge of the objects concerned. Rather it seems that, as in the case of physical experience, we *form* our ideas also of those objects on the basis of something else which *is* immediately given. Only this something else here is *not*, or not primarily, the sensations. That something besides the sensations actually is immediately given follows (independently of mathematics) from the fact that even our ideas referring to physical objects contain constituents qualitatively different from sensations or mere combination of sensations, e.g., the idea of object itself, whereas, on the other hand, by our thinking we cannot

> create any qualitatively new elements, but only reproduce and combine those that are given. Evidently the "given" underlying mathematics is closely related to the abstract elements contained in our empirical ideas. It by no means follows, however, that the data of this second kind, because they cannot be associated with actions of certain things upon our sense organs, are something purely subjective, as Kant asserted. Rather they, too, may represent an aspect of objective reality, but, as opposed to the sensations, their presence in us may be due to another kind of relationship between ourselves and reality.

It is natural to ask, in connection with this passage, for an account of what is immediately given, of how we get it and from what, and of how we form our ideas from it. These are some of the questions that Benacerraf claims we can begin to answer for physics but not for mathematics (as viewed by Gödel). I think that Benacerraf is mistaken in his view that there is a real asymmetry here; not because we can give really satisfactory answers for the mathematical case, but because he overestimates the answers we have for the empirical case.

In the empirical case we have theories (physics, biology, etc.) which tell us something about the material world, including our bodies, and about the relation between it and our conscious experiences of it. Since we have equally good mathematical theories, that the realist alleges to be about mathematical reality, it is the account of the relation between us and reality that Benacerraf emphasizes. I suppose that the beginnings of a causal explanation, which he believes we have in the empirical case, has to do with such things as light being reflected from objects, striking our retinas, neurons firing, etc. Unfortunately, however, we have no account whatsoever of the connection between all this and our conscious experiences. Consciousness remains completely unexplained in physical and biological terms. (Gödel even believes that it is unlikely that it will be explained solely on those terms.[8]) What I am emphasizing here is not

[8] Wang reports (1974, pp. 324-326) that Gödel considers the assumption that there is no mind separate from body "a prejudice of our time, which will be disproved scientifically (perhaps by the fact that there aren't enough nerve cells to perform the observable operations of the mind" (p. 326)).

dualism, however, but the fact that we have no account of our conscious states. When he gives the Hermione example, Benacerraf adds the following note (p. 412 to "state"):

> If possible, I would like to avoid taking any stand on the cluster of issues in the philosophy of mind or psychology concerning the nature of psychological states. Any view on which Hermione can learn that the cat is on the mat by looking at a real cat on a real mat will do for my purposes. If looking at a cat on a mat puts Hermione into a state and you wish to call that state a physical, or psychological, state, I will not object so long as it is understood that such a state, if it is her state of knowledge, is causally related in an appropriate way to the cat's having been on the mat when she looked. If there is no such state, then so much the worse for my view.

My previous point is that he cannot appeal to science to explain the connection between the cat being on the mat and Hermione's conscious state of knowledge, because science tells us nothing, so far, about how conscious states arise or what they are. But there are at least two other problems: it is not at all clear what the connection between seeing and knowing is (and Benacerraf offers no explanation), and it is not at all clear what in fact is given in perception and how we form our ideas from it.[9]

Empiricist philosophers often assume that our basic experiences of the physical world are essentially our sensations of color, shape, solidity, etc., and that we form our ideas on the basis of this kind of data. It is quite obvious though that even our simplest perceptions do not seem to be analyzable in these terms. The most natural way to describe our perceptions involves both sensations and relations of various sorts. Some psychologists talk about our perception of abstract features of reality, say, ratios of brightness, not as something we cook up in a certain way but as a direct perception. In fact, James J. Gibson has presented a

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[9] Maybe we can put the point diagramatically as follows:

Knowledge (conscious state) —[???]— Perception (conscious state)—[???]— Brain —[some]— Sensory organs —[quite a bit]— Reality.

view of perception that seems to fit in quite well with some of Gödel's remarks. I quote one rather general passage (1966, pp. 266-267):

> Up to the present time, theories of sense perception have taken for granted that perception depends wholly on sensations that are specific to receptors. I have called these theories of sensation-based perception. The present theory asserts the possibility of perceptual experience without underlying sensory qualities that are specific to receptors, and I have called this a theory of information-based perception. ...
>
> ...
>
> The evidence of these chapters shows that the available stimulation surrounding an organism has structure, both simultaneous and successive, and that this structure depends on sources in the outer environment. If the invariants of this structure can be registered by a perceptual system, the constants of neural input will correspond to the constants of stimulus energy, although the one will not copy the other. But then meaningful information can be said to exist inside the nervous system as well as outside. The brain is relieved of the necessity of constructing such information by *any* process—innate rational powers (theoretical nativism), the storehouse of memory (empiricism), or form-fields (Gestalt theory). The brain can be treated as the highest of several centers of the nervous system governing the perceptual systems. Instead of postulating that the brain constructs information from the input of a sensory nerve, we can suppose that the centers of the nervous system, including the brain, resonate to information.

I do not mean to suggest that Gibson would agree with Gödel's views on mathematics, but his position is compatible with (and supportive of) much that Gödel says in the quotation at the beginning of this section. I do not mean to suggest either that most psychologists agree with Gibson's views, but the point is that these are controversial matters among scientists, and that Benacerraf cannot dismiss this aspect of Gödel's views by a blank appeal to science.

One may argue that as much of a connection as one has between the world and our experience of it already involves us in a relation with both concrete and abstract features of it. In connection with this, it should be noted that Benacerraf's acceptance of Kripke's views on reference puts him in the position of either having to accept reference to

abstract properties for non-mathematical cases, or to declare even his own simple example as unaccounted for. His example was

(*v*) There are at least three large cities older than New York,

and his analysis of it (p. 405) involves, among other things, the referent of "older than". It is not at all clear, however, that on a Kripkean view of reference one could say that "older than" refers to a material thing. In fact, Kripke is committed to the view that some predicative and relational terms, though not all, refer to (abstract) properties and relations. I do not see, for example, how Kripke's discussion of the "meter" example (1972, pp. 54-56) could make sense if "meter" were to refer to a concrete thing, or to a mereological sum of such things.

Of course, one might try not to talk about the referents of predicative and relational terms, and talk about satisfaction instead—Benacerraf does it for "city" in connection with (*v*). It is well known, however, that this approach is quite problematic. Part of the problem is that in order to account for our learning to apply predicates and relations to concrete things, a whole hearted empiricist like Quine has to resort to the assumption of a large number of pre-linguistic innate quality spaces (1960, pp. 83-85)—which assumption, as long as it carries some content beyond saying that since we learn certain things there must be something that allows us to do so, seems to me rather less plausible than Gibson's position.

But, returning to mathematics, one could still ask what is the relation between all this and our perception of mathematical objects. In particular, how does this help in answering Benacerraf's criticism as I formulated it at the end of section 2.

Part of what makes mathematical cases for which one could argue as in (*a*)-(*d*) hard to find, is that the grounds of our mathematical beliefs lie in experiences that are universal in the same way that the experiences connected with (*ii*)-(*iv*) are universal; the world, as it were, wears its

mathematical properties all over, and it is hard to conceive of a human being that fails to experience them. This is not to say, of course, that one can have more or less extensive mathematical knowledge before one forms one's ideas of these mathematical properties, but in this respect the situation seems no different for mathematical and for empirical knowledge. I would, however, be tempted to say that to the extent that animals may be said to have knowledge at all, they have some mathematical knowledge as well. But however this may be, I think it plausible, quite independently of realism, that our mathematical experiences have this kind of universal character. The question for the realist is whether our mathematical ideas are formed by experiencing mathematical entities of some sort, and, if so, what does this experience consist in.

I think that our basic mathematical experiences (or perceptions) could be described as being of such (abstract) properties as: Unity, Plurality (not necessarily involving finiteness), Nullity, Duality, Likeness (or Similarity), Unlikeness, Succession, Continuity, etc. This list, of course, is mostly Plato's—and I could just as well have called these properties "forms"—but this seems closely related to what Gödel has in mind. I believe that the development of mathematics makes it quite plausible that perception of the sorts of properties I listed is basic to the development (formation) of our mathematical ideas. But, as Gödel says, even apart from mathematics, it seems reasonable to suppose that our perception of reality involves concrete and abstract components, both of which correspond to real features of the world. For, as I have already suggested, even our simplest perceptions of so-called physical reality involve these abstract features in an essential way, and they—i.e., experiences of similarity, or of plurality, or of continuity, etc.—do not appear to be a matter of inference or association from simple sensations of color, shape, etc.

There are a number of problems here, however. One is whether what I am saying is not really much weaker than what Gödel says. For, on the one hand, Gödel's remarks at the beginning of the passage

originally quoted by Benacerraf seem to be at odds with a natural interpretation of mine, and, on the other hand, in the passage I quoted at the beginning of this section Gödel suggests a greater separation between our experiences of concrete and abstract features of reality ("another kind of relationship").

Among the questions that arise are: Is our perception of abstract features of reality (say, properties) dependent upon (or necessarily concurrent with) our perception of material objects that "exhibit" (in some sense) the abstract features in question, or can we somehow perceive at least some of these abstract features independently of there being any physical realizations of them (directly)? And, in either case, but particularly in the second, is there some "special faculty" we have, distinct from our senses (though possibly a part of our brain) that allows for these abstract perceptions? I do not know exactly how to answer these questions, but let me begin by discussing Gödel's views.

On the basis of what he says in (1944) and (1947) it does not seem that Gödel is committed to the view that there is some special physical organ—say, a differentiated part of our brain—that accounts for our abstract perceptions. Nor does he seem to be committed to some form of dualism that makes it a specific function of the mind (as distinct from the functions of our brain) to experience the abstract features of reality. In his book, however, Wang reports as follows (1974, p. 85):

> Gödel conjectures that some physical organ is necessary to make the handling of abstract impressions (as opposed to sense impressions) possible, because we have some weakness in the handling of abstract impressions which is remedied by viewing them in comparison with or on the occasion of sense impressions. Such a sensory organ must be closely related to the neural center for language. But we simply do not know enough now, and the primitive theory on such questions at the present stage is likely to be comparable to the atomic theory as formulated by Democritus. Philosophy as an exact theory should do to metaphysics as much as Newton did to physics. Gödel thinks it is perfectly possible that the development of such a philosophical theory will take place within the next hundred years or even sooner.

Although I do not see quite clearly the implications of this passage, the "because" in the statement of Gödel's conjecture seems to indicate that the handling of abstract impressions is a function of mind, which, however, due to certain weaknesses, must be aided by a physical organ. Since Gödel is reported by Wang to be a dualist, this is the most natural interpretation. Alternatively, we could understand the conjecture as saying, on the one hand, that we have a physical organ that handles abstract impressions, and, on the other hand, that due to some weakness in *its* handling of them, it functions in close association with our sensory organs. Although the "because" makes this reading of the statement unnatural, it is not so in itself, and, in fact, it is how I first interpreted the passage. But, however this may be, Gödel is not suggesting that we have any clear understanding of these questions. It is important to consider these suggestions as deriving from Gödel's in depth analysis of the situation in mathematics, not as claims intended to support his realist views. In other words, Benacerraf's suggestion that Gödel is building his epistemological views to make sense of his realism seems to me mistaken. I believe, on the contrary, that Gödel's views arise primarily from epistemological considerations, which, in conjunction with metaphysical ones, lead to his realism, and which, just as in the case of any of our views about the world and our knowledge of it, lead to hypotheses, conjectures and suggestions as to what goes on. I do not think that Gödel expects anyone to take these things on faith, but, rather, that the development of science and philosophy may lead to better formulations and confirmation of his views.

From Benacerraf's point of view, however, the question is whether there is reason to suppose that any of this may be right. My point is that his disagreeing with Gödel on "what avenues are open to us through which we may come to know things" does not, without further argument, cut much ice—for, as I have already suggested, what Benacerraf says about the causal account of knowledge is not clear enough or substantial enough to give him an argument without begging the question.

7. ASIDE ON *ANAMNESIS*

At the end of his discussion of Gödel, Benacerraf hints, by means of a reference to Plato's views on *anamnesis*, that one must eventually resort to this in order to save realism—he does not *say* this, but it is implied. I will comment on this point appealing to my earlier suggestion concerning the universality of our mathematical experience.

Plato, of course, had various reasons for believing in the immortality of the soul, and I will not dispute Benacerraf's suggestion (p. 416) that "Plato had recourse to the concept of *anamnesis* at least in part to explain how, given the nature of the forms as he depicted them, one could ever have knowledge of them." But apart from Plato's motives, what is interesting about the example in the *Meno* (82-87) is that Socrates draws out from the slave boy an answer to the question "what is the ratio between the sides of two squares given that one has twice the area of the other?" in essentially the same way in which, say, a good police investigator may get someone to remember the specific circumstances of certain events he witnessed some time past, without bringing any information he may have into the questioning. This kind of questioning will work in those cases in which all the relevant experiences have already been had, and what is wanted are recollections of such experiences, or conclusions from inferences based upon them. Thus, if in fact, at least at a certain level, our mathematical experience is universal, then Socrates' method will work for mathematical knowledge because no specifically new experiences will be required. By the same token, however, one should say that the method will work to draw out knowledge about certain general features of the physical world. Certain kinds of experimental checking of general hypotheses may be seen as playing an analogous role to Socrates' pointing out of mistakes in the slave's initial answers (e.g., that if the side were twice as big, then the area would be four times larger). What the method cannot accomplish is to obtain answers to questions requiring specifically new experiences (e.g., in certain circumstances, is there a person in the next room right now?).

Although these points about physics may sound a bit odd, they would seem to fit with the following remarks of Gibson (1966, p. 280):

> A kitten perceives the course of a rolling ball, an outfielder perceives the trajectory of a batted ball ... [T]he kitten and the ball player *expect* the ball to continue on a predictable path and that is why they can both start out on a dead run to intercept it. This foreseeing is much like ordinary seeing, and not much like Tolman's expectancies, for it depends on a continuous flow of stimulation. But the two kinds of situation do have something in common. The unbroken continuation of the optical motion is a consequence of the invariant laws of inertia and gravity in physics. The ball continues in a straight line, or a trajectory, because of Newton's Laws. The invariant is implicit in the motion. Both the kitten and the ballplayer may have to practice and learn in order to detect it accurately, but in a certain sense what they are learning is to perceive the laws of motion.

The distinction between a ball player and Newton can then be seen, at least in part, on the latter's explicit insight and in his ability to formulate it in a specific way.

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