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TRUTH AND PROOF: THE PLATONISM OF
MATHEMATICS*

1.

What is the relation in mathematics between truth and proof?

An arithmetical proposition A , for example, is about a certain structure, the system of natural numbers. It refers to numbers and relations among them. If it is true, it is so in virtue of a certain fact about this structure. And this fact may obtain even if we do not or (for example, because of its relative complexity) cannot know that it does. This is a typical expression of what has come to be called the *Platonist* (or *platonist* or *realist*) point of view towards mathematics.

On the other hand, we learn mathematics by learning how to do things – for example, to count, compute, solve equations and, more generally, to prove. Moreover, we learn that the ultimate warrant for a mathematical proposition is a proof of it. In other words, we are justified in asserting A – and therefore, in any ordinary sense, the truth of A – precisely when we have a proof of it.

Thus, we seem to have two criteria for the truth of A : it is true if (indeed, if and only if) it holds in the system of numbers, and it is true if we can prove it. But what has what we have learned or agreed to count as a proof got to do with what obtains in the system of numbers? I shall call this the *Truth/Proof problem*. It underlies many contemporary attacks on Platonism.

The argument against Platonism begins with the observation that the first criterion, holding in the system of numbers, is inapplicable because we have no direct apprehension of this structure. Sometimes this argument is augmented by the thesis that ‘apprehension of’ would involve causal interaction with the elements of the structure and, since numbers are ‘abstract’ (i.e., not in spacetime), no such interaction is possible. In any case, the argument continues: it follows that a proof cannot be a warrant – or even incomplete evidence – for holding in the structure. For no kind of evidence is available that the canons of proof apply to the structure. Thus, if proof is a warrant for A , then A cannot be about the system of numbers. If, on the other hand, proof is

not a warrant, then we have no mathematical knowledge at all. An even stronger argument, to the effect that we cannot even meaningfully *refer* to numbers, is based on the thesis that reference also involves causal interaction.

Because of these considerations, many writers have felt that mathematics is in need of a foundation in the revisionist sense that we must so construe the meaning of mathematical propositions as to eliminate the apparent reference to mathematical objects and structures. Some of these writers see Platonism itself as a foundation, i.e., as a theory of what mathematics is about – but one which, no matter how naively plausible, is refuted by the Truth/Proof problem.

There are many interesting problems which might reasonably be called problems in the foundations of mathematics; but I shall argue here that among them is *not* the need for a foundation in this revisionist sense. The Truth/Proof problem, which seems to demand such a revision, will resolve itself once we are clear about what truth and proof in mathematics mean and what is involved in the notion of a proposition holding in a structure. These notions seem to me to be surrounded in the literature by a good deal of confusion which gets attached to Platonism. Free of this confusion, Platonism will appear, not as a substantive philosophy or foundation of mathematics, but as a *truism*.

2.

Many who reject Platonism on the grounds of the Truth/Proof problem take discourse about sensible objects to be, not only unproblematic, but a paradigm case of the apparent content of a proposition being its real content. Thus

- (1) There is a prime number greater than 10

is not really about the system of numbers, as we might naively read it, because our warrant for it is a proof – and what has that to do with the system of numbers? On the other hand,

- (2) There is a chair in the room

really is about the sensible world – about chairs and rooms – because we verify it by looking about the room and seeing a chair. Thus, Dummett (1967) begins

Platonism, as a philosophy of mathematics, is founded on a simile: the comparison between the apprehension of mathematical truth to [sic] the perception of physical objects, and thus of mathematical reality to the physical universe.

He then argues that there is no analogue to observation in the case of mathematics and so the simile is misconceived. And Benacerraf (1973) writes

One of its [i.e., the 'standard' Platonistic account's] primary advantages is that the truth definitions for individual mathematical theories thus construed will have the same recursion clauses as those employed for their less lofty empirical cousins. (p. 669)

The 'standard' account is that, for example (1) has the 'logico-grammatical form'

There is an F which bears the relation G to b

and he takes it as unproblematic that (2) has this form. But

For the typical "standard" account (at least in the case of number theory or set theory) will depict truth conditions in terms of conditions on objects whose nature, as normally conceived, places them beyond the reach of the better understood means of human cognition (e.g., sense perception and the like). (p. 667-8)

I shall have more to say bearing on these passages in the course of my paper; but for now I intend them only as instances of the view that, whereas the naive (Platonistic) construal of (1) is problematic, the naive reading of (2) is acceptable, indeed as a paradigm case.¹

3.

Why does the experience that I describe as "seeing a chair in the room" warrant the assertion of (2) any more than a proof warrants the assertion of (1)? I am not referring here to the possibility of perceptual error or illusion: the nearest analogue to that in the case of (1) would perhaps be error in proof or ambiguity of symbols. Rather, I am asking a traditional sceptical question: what have my experiences to do with physical objects and their relationships *at all*?

For, in the case of (2) *also*, I am applying the canons of verification that I have been trained to apply. Among other things, this training involved learning to say and react to sentences such as "I see a chair", "There is no chair in the room", etc., under suitable circumstances. It is true that, unlike the case of (1), these circumstances involve sensory experience. But (2) is about physical objects, not my sensations.

One may feel that the crucial difference between (1) and (2) is this: in the former case, proving is inextricably bound up with what I have been trained to do; whereas in the latter case, the role of training is confined to language learning and this consists simply in learning to put the right (conventional) names to things. And, after that, training plays no further role: I simply read the true proposition off the fact as I observe it.

This view of (2) is in essentials the so-called 'Augustinean' view of language which, in my opinion, is thoroughly undermined by Wittgenstein's *Investigations*, §§1–32. My learning to put names to things *consists* in my learning to use and respond, verbally and otherwise, to expressions involving these names. For example, how is it that I am naming the chair as opposed to naming its shape, color, surface, undetached chair part, temporal slice, etc.? The answer is that it is the way we *use* the word "chair" that determines this. And the point is not merely that the act of naming is ambiguous as to which among several categories it refers. Ambiguity itself presupposes language: to understand words like "shape", "color", etc., is to have a mastery of a language. My point – or rather, Wittgenstein's point – is that *nothing* is established by the act which we call an act of naming, without a background of language or, at least, without further training in how the name is to be used.² We do not read the grammatical structure of propositions about sensible objects off the sensible world nor do we read true propositions about sensibles off prelinguistic 'facts'. Rather, we master language and, *in* language, we apprehend the structure of the sensible world and facts. To apprehend the fact that *A* is simply to apprehend that *A*. And this apprehension presupposes language mastery.

So, if *A* is a proposition about the sensible world of rooms and chairs, then it is true if and only if it holds in that world. But we sometimes count what we experience as verification for *A*. And why should these two things, what holds in the world of rooms and chairs and what we experience, have anything to do with each other? Note that it is not sufficient to point out that verification is not conclusive in the way that the existence of a proof is, since the question is why verification should have *anything* to do with what holds in the world of rooms and chairs.

Thus, I see nothing special to mathematics about the Truth/Proof problem. We have described a Truth/Verification problem which is its

analogue in the case of the sensible world. Moreover, the latter is not really a new argument but, in essentials, has been a standard part of the sceptics armory. It is perhaps this analogy that Gödel has in mind when he wrote (1964, p. 470) that “the question of the objective existence of the objects of mathematical intuition... is the exact replica of the question of the objective existence of the outer world”. At any rate, I know of no argument against the existence of mathematical objects which does not have a replica in the case of sensible objects. For example, some writers argue against Platonism that, if there is a system of numbers, then why shouldn't there be more than one of them, all indistinguishable – how would we distinguish them? And why should our theorems refer to one such system rather than another? Answer: why shouldn't there be more than one physical world, all indistinguishable from one another and such that my 'seeing a chair' is a seeing a chair in all of them? Why should (2) be about one of these worlds rather than another? If you answer that it is about the world you inhabit, then I shall ask: which you?, etc. Sceptics about mathematical objects should be sceptics about physical objects too.

Of course, scepticism about either is misplaced, and both the Truth/Proof and the Truth/Verification problems are consequences of confusion and are not real problems.

Perhaps this becomes more evident when we note that, in both cases, the problem purports to challenge our canons of warrant (i.e., proof and verification, respectively); but carried to its logical conclusion, it also challenges our canons of meaningfulness. Why should the structure of reality be what is presupposed by the grammatical structure of our language as we have learned it? For example, the meaningfulness of a sentence involving “+” presupposes the truth of the sentence which expresses that “+” is well defined in the numbers. So scepticism about truth will already imply scepticism about meaning.

4.

For both Benacerraf and Dummett in the above cited papers, what is special about discourse about physical objects is the possibility of sense perception, and the difficulty that they raise for Platonism is based on the absence in the case of mathematical objects of any such “better understood means of human cognition.”

Some writers, for example, Gödel (1964), Parsons (1979–80) and Maddy (1980), attempt to meet this difficulty by arguing that there is perception or something like it in the case of mathematical objects. But, of course, from the point of view of the Truth/Proof problem, the issue is not whether we perceive mathematical objects, but whether our canons of proof obtain their meaning and validity from such perceptions. And the answer to this seems to me to be clearly no. We perceive sets, for example, only when we have mastered the concept ‘set,’ i.e., have learned how to use the word “set.” For example, it does not seem reasonable to suppose of people, before the concept of set was distinguished, that when they perceived a heap of pebbles, they also perceived a set – a different object – and simply spoke ambiguously. And, in whatever sense we may perceive numbers, it is hard to see how that can provide a foundation for the use of induction to define numerical functions, for example. The canons of proof are like canons of grammar; they are norms in our language governing the use of words like “set,” “number,” etc. What we call ‘mathematical intuition,’ it seems to me, is not a *criterion* for correct usage. Rather, having mastered that usage, we develop feelings, schematic pictures, etc., which guide us. Of course, such intuitions may play a causal role in leading us to correct arguments and even to new mathematical ideas; but that is a different matter. In any case, the appropriate response to the anti-Platonists is not to argue that there is something like perception in the case of mathematics. Rather it is to point out that, even in the supposedly paradigm case of sensible objects, perception does not play the role that they claim for it. That this is so is manifest from the Truth/Verification problem.³

5.

Platonism is often identified with a certain “account” of truth in mathematics, namely Tarski’s. That this is so for Benacerraf (1973) is clear from the first of the above quotes from that paper and

I take it that we have only one such account [of truth], Tarski’s, and that its essential feature is to define truth in terms of reference (or satisfaction) on the basis of a particular kind of syntactico-semantic analysis of the language, and thus that any putative analysis of mathematical truth must be an analysis of a concept which is a truth concept at least in Tarski’s sense. (p. 667)

It is difficult to understand how Tarski's 'account' of truth can have any significant bearing on any issue in the philosophy of mathematics. For it consists of a definition *in* mathematics of the concept of truth for a model in a formal language L , where both the concept of a formal language and of its models are mathematical notions. For example, \exists in L is interpreted in terms of the mathematical 'there exists.' But Benacerraf is concerned with mathematical truth, not with truth of a formal sentence in a model. How can Tarski's account apply here? What is the locus of the definition, i.e., what is the metalanguage? Not the language of mathematics, of course, since that is the language whose meaning they wish to explain.

Benacerraf's remark that truth is defined "in terms of reference (or satisfaction)" is at first sight puzzling, since truth is a special case of satisfaction. But by "satisfaction" he undoubtedly means *valuation*, i.e., assigning values to variables. But it is misleading to speak here of reference. The model assigns values to the constants of L ; but this, like the notion of valuation, is expressed in terms of the notion of function, and the concept of reference does not enter in. It is the more misleading when Benacerraf goes on to advocate a causal theory of reference.

An enlightening way to look at Tarski's truth definition is in terms of the notion of an interpretation: with each formula ϕ of L , we define by induction on ϕ a formula $I(\phi)$ (in the same variables) of the metalanguage, i.e., of some part of the ordinary language of mathematics in which we defined the model. The truth definition now is just the 'material condition for truth': a sentence ϕ of L is true iff $I(\phi)$.

Dummett 1973 writes

On a platonistic interpretation of a mathematical theory, the central notion is that of truth: a grasp of the meaning of a sentence belonging to the language of the theory consists of a knowledge of what it is for that sentence to be true. (p. 223)

But when he speaks of what it is for ϕ to be true or "of what the condition is which has to obtain for [ϕ] to be true" (p. 224), to what condition can he be referring here other than the condition that $I(\phi)$? But now, in what consists a grasp of the meaning of $I(\phi)$? (Wittgenstein 1953, §198: "... every interpretation, together with what is being interpreted, hangs in the air. . . .") Dummett is aware of this infinite regress, but he uses it as an argument against classical reasoning in mathematics which he identifies with Platonism (pp. 216–17).

But of course the infinite regress disappears when we note that Platonism does not consist in an interpretation of mathematical theories. We do indeed interpret theories in mathematics, as when we construct inner models of geometries or set theory or when we construct examples of groups, etc., with certain properties. But we do this in the language of mathematics, and our 'grasp' of this consists in our ability to use it. Dummett agrees with this (p. 217); but because he takes Platonism to be an interpretation, he believes that this conclusion is an argument against Platonism.

Benacerraf and Dummett seem to me to be typical of those who adopt a particular picture of Platonism. The picture seems to be that mathematical practice takes place in an object language. But this practice needs to be explained. In other words, the object language has to be interpreted. The Platonist's way to interpret it is by Tarski's truth definition which interprets it as being about a model – a Model-in-the-Sky – which somehow exists independently of our mathematical practice and serves to adjudicate its correctness. So there are two layers of mathematics: the layer of ordinary mathematical practice in which we prove propositions such as (1) and the layer of the Model at which (1) asserts the 'real existence' of a number.

This is the picture that seems to lay behind the distinction in Chihara (1973, pp. 61–75) between the 'mythological' Platonist and the 'ontological' Platonist. The former simply does mathematics while refraining from commitment to the interpretation. The latter accepts the interpretation and so is committed to the 'real' existence of a prime number greater than 10 and to the 'real' existence of 10.

But one cannot explain what this interpretation is supposed to be. An interpretation in the ordinary sense is a translation. Into what language are we supposed to be translating the language of ordinary mathematics?

The Platonist, on this picture, is the Realist that Wittgenstein (1953) criticizes at §402, along with the Idealist and the Solipsist – and he might have added the Nominalist in the contemporary sense – , when he says that the latter "attack the normal form of expression as if they were attacking a statement" and that the Realist defends it as though he "were stating facts recognized by every reasonable human being." Needless to say, it is not this version of Platonism that I am defending or that I even understand. Thus, I should not be understood to be taking part in any realism/antirealism dispute, since I do not under-

stand the ground on which such disputes take place. As a mathematical statement, the assertion that numbers exist is a triviality. What does it mean to regard it as a statement outside of mathematics?⁴

6.

It is ironic that Dummett should think that Platonism is founded on a comparison between mathematical reality and the physical universe and that Benacerraf should think that it is motivated by the desire to have the same account of truth for mathematics as for its less lofty empirical cousins. Plato, who was, as far as we know, the first Platonist, was entirely motivated by his recognition of the fact that the exact empirical sciences of his day – geometry, arithmetic, astronomy and music theory, for example – were not literally true of the sensible world in the semantical sense and, indeed, did not literally apply to it. He did not have our distinction between mathematics and empirical science nor the idea of mathematical objects. Thus, in no sphere did he think that scientific truth was truth in the semantical sense. And Tarski's truth definition, while it concerns the semantical notion of truth, is a piece of mathematics, concerning the mathematical notion of a model of a formal language.

The fact is that *we can* regard numerical propositions, say, as being about a well-defined structure – the system of natural numbers. This would be misleading only if it led us to think that our propositional knowledge of this structure derives from some sort of non-propositional cognition of it or of its elements. In the case of sensibles, on the other hand, there is no such well-defined structure. For example, if my desk remains the same object after I scratch it as before – and clearly we must agree to this for a sufficiently light scratch – then transitivity of identity fails for sensible objects, since a finite number of such scratches will reduce the desk to a splinter which we would not identify with it. Nor can we avoid this conclusion by such resources as speaking of the desk-at-an-instant; for this is no longer a sensible object. Moreover, the predicates we apply to sensibles – for example, of shape, color or size – are inherently vague. Thus, the canons of exact reasoning, as embodied say in some system of deductive reasoning, do not apply to the domain of sensible objects.

And when we idealize the domain of sensibles so that it takes on the character of a well-defined structure and logic applies, then the other

part of the picture of empirical knowledge painted by Dummett and Benacerraf becomes manifestly problematic. For example, if reference to my desk is replaced by reference to a spacetime region and reference to colors, shapes and sizes by reference to magnitudes, then the relevance of sense perception becomes less direct. It can no longer be understood on the model of observation to observation sentence and, at least judging by the literature on the subject of theory confirmation, it is *not* one of the “better understood means of human cognition.” The relevant perceptions are of measurement; and the measuring devices and measurements perceived are not elements of the idealized domain, but are sensible objects like my desk. The role of sense perception in confirming or applying mathematical models of the phenomena is very complex. Yet it is only when we are thinking of such a model, and not of the sensible world itself, that the picture of the universe as a well-defined structure applies. Thus, when Benacerraf ignores the vagueness of the terms “large” and “older than”, he is not merely setting aside a complication (p. 663). He is raising the ‘less lofty empirical cousins’ to an altogether loftier state where they too may suffer their share of the attacks on Platonism.⁵

7.

Benacerraf seems to believe that the “better understood means of human cognition” all involve causal interaction between the knower and the known. He writes (p. 671)

I favor a causal account of knowledge in which for X to know that S is true requires some causal relation to obtain between X and the referent of the names, predicates and quantifiers of S . I believe in addition in a causal theory of *reference*, thus making the link to my saying knowingly that S *doubly* causal.

His problem then is that on the Platonist view we would not be able to refer to mathematical objects, much less know anything about them, since we do not causally interact with them. (Of course, one may feel that the same problem arises for the referents of the predicates “large” and “older than”, to take Benacerraf’s examples.) His argument for “some such view” is that we would argue that X does not know that S by arguing that he lacks the necessary causal interactions with the grounds of truth of S – for example: he wasn’t there. Of course, this argument is plausible only if S is an empirical proposition (and

Benacerraf's example is empirical) and so it would seem to be a complete *non sequitur* in the case of mathematical knowledge. In the latter case, we might rather argue that X does not know that S by arguing that he hasn't the competence to produce a proof of S .

However, Benacerraf thinks that whatever account we give of mathematical knowledge, it should be extendable to embrace empirical knowledge as well (p. 262). And if that is so, then indeed the correct account of knowledge of sensibles had better be extendable to mathematics; and so there is no *non sequitur*. But his argument that our account of mathematical knowledge should be extendable to empirical knowledge is that to "think otherwise would be, among other things, to ignore the interdependence of our knowledge in different areas." But this seems to me to be a very weak argument. Consider a case of interdependence: a mathematical prediction of the motion of a physical object. First, we read the appropriate equations off the data – i.e., we chose the appropriate idealization of the phenomenon. Second, we solve the equations. Third, we interpret the solution empirically. When Benacerraf speaks of mathematical knowledge in his paper, the relevant kind of knowledge is knowledge that S , where S is a mathematical proposition. But *that* kind of knowledge is involved only at the second step, and it involves nothing empirical.⁶ The first and third steps involve only knowing how to apply mathematics to the phenomena. But I don't see why an account of this kind of knowing requires that, if empirical knowledge involves causal interaction, then so does mathematical knowledge. The fact is that we do know how to apply mathematics and we do not causally interact with mathematical objects. Why doesn't this fact simply refute a theory of knowing how that implies otherwise?

We may wish to explain why it is that idealization of the phenomena works. We may also wish to explain why language and inductive inference work. But these seem to me to be scientific 'why's', to be answered by an account of how we process information and of how this means of processing information (and so, creatures like us) evolved.

Although it is unnecessary for the purpose at hand, let me comment briefly (and certainly insufficiently) on the double causal interaction that Benacerraf thinks must be involved in knowledge about objects. It seems to me that when we speak of mathematical knowledge in the ordinary way, we are referring to the ability to state definitions and

theorems, to compute, to prove propositions, etc.: in general it is a matter of knowing *how*, of competence. Anyway, it is this kind of knowledge that we test students for. The ideas of propositional knowledge (knowledge *that*) and knowledge in the sense of acquaintance with (knowledge *of*) also seem to me ultimately to reduce to the idea of knowledge *how*; and this is so, not only in the (relatively simple) domain of mathematics, but in general. This is of course very different from the Cartesian notion of knowledge, since knowledge in this sense presupposes a communal practice against which competence is to be measured and so cannot serve as an external foundation for a critique of that practice. Critique must come from within, measuring our practice against the purpose of that practice. Also, knowledge in this sense is not a matter of all or nothing: we recognize degrees of knowing. (For example, when is giving a proof *really* giving a proof – with understanding? Compare this with Wittgenstein's discussion of reading.) We may indeed obtain a causal account of knowledge in this sense; but it does not seem at all plausible that such an account, even in the case of knowledge about physical objects, will involve causal interaction with those objects. (The appearance of plausibility here arises from the possibility that I might unwarrantedly believe a true proposition. But one may reasonably doubt that there is a sense in which my belief is unwarranted that would not show up in what I am disposed to *do*.) As for the view that reference involves causal interaction, the motive for this seems to me to confuse the question of how we come to use a word in the way we do with the question of how it is in fact used. (Cf. Wittgenstein 1953, §10.)

8.

Platonism is taken to be an account of mathematics which says, for example, that number theory is about a certain model. And then it is challenged to tell us what that model is. One asks: how do we get to know this model? Or: how do we know when we speak together that we are speaking about the same model?, etc. It is as if we have a formal system and are told that there is an intended model for it. But no one can tell us what this model is and so we do not even know why the formal system is grammatically correct, much less valid.

Thus, Dummett (1967, p. 210) writes

To say that we cannot communicate our intuition of the natural numbers unequivocally by means of a formal system would be tolerable only if we had some other means to communicate it We cannot know that other people understand the notion of all properties (of some set of individuals) as we do, and hence have the same model of the natural numbers as we do.

and (pp. 210–11)

. . . we arrive at the dilemma that we are unable to be certain whether what someone else refers to as the standard model is really isomorphic to the standard model we have in mind.

What is my intuition *of* the numbers? I can only be said to have intuitions *about* them – and then, only when I have some minimum understanding of number theory. And this understanding is not an ‘intuition’ (although there may be accompanying feelings and pictures); it is a competence. What does it mean to ‘have a model’ or to ‘have one in mind’? And what does it mean for us to have the same one? This can only mean that we do the same number theory – the one which is part of our common language.⁷ And I can ask: how do we know that you have the same physical universe that I have? Dummett seems to believe that we must *explain* our ability to communicate mathematics and that Platonism is inadequate because it fails to do this. But no explanation is necessary, unless one is calling for a general empirical account of human communication. Mathematics *presupposes* the fact of communication – the fact of our common disposition to use and react to symbols in specific ways. If we lacked such common dispositions we could not be said to have mathematics any more than, if we lacked legs, could we be said to walk.

Every reasonably schooled child understands the language of arithmetic. It is the schizophrenic parent of the child who, motivated by an inappropriate picture of meaning and knowledge, develops ‘ontological qualms.’ The picture is read into Platonism and then, because it is inappropriate, Platonism, i.e., our ordinary conception of mathematics, is rejected. The fact that the picture is *generally* inappropriate is simply ignored. We owe to Chihara a clear illustration of the schizophrenia, namely in his mythological Platonist.

9.

Strangely, Dummett understands that the notion of a model is a mathematical notion and that we construct or describe models *in* mathematics (1967, pp. 213–14 and 1963). He is ascribing to Platonism an idea that he must find incomprehensible. Why? Part of the answer at least may be found in the terms in which he argues in the 1967 paper that Frege's context principle undermines realism:

When we scrutinize the doctrines of the arch-Platonist Frege, the substance of the existential affirmation finally appears to dissolve. For him mathematical objects are as genuine objects as the sun and moon: but when we ask what these objects are, we are told that they are the references of mathematical terms, and 'only in the context of a sentence does a name have a reference.' In other words, if an expression functions as a singular term in sentences for which we have provided a clear sense, i.e. for which we have legitimately stipulated determinate truth conditions, then that expression *is* a term (proper name) and accordingly has a reference; and to know those truth conditions *is* to know what its reference is, since 'we must not ask after the reference of a name in isolation.' So, then, to assert that there are, e.g., natural numbers turns out to be to assert no more than that we have correctly supplied the sentences of number theory with determinate truth conditions; and now the bold thesis that there are abstract objects as good as concrete ones appears to evaluate to a tame assertion that few would want to dispute.

I, for one, would dispute the 'tame assertion' that we have "correctly supplied the sentences of number theory with determinate truth conditions," unless, of course, we are speaking about some formal system of number theory and we have explained their meaning in ordinary mathematical terms. We interpret formal systems; but in what language do we interpret ordinary mathematics to give it 'determinate truth conditions'?

There are many difficulties and complexities in connection with Frege's context principle; he applies it in Frege (1884) to justify his definition of the numbers and he applies it in Frege (1893) to justify the introduction of course-of-values in terms of which the numbers are defined. One complication is that he is proposing an extension of the ordinary mathematical discourse of his time – a new norm for mathematics – and another is that his extension is inconsistent. Also, he formulates his argument (1884, §60) that numbers are objects against the background of his object/function ontology. Also, because he was concerned with mathematics, he did not concern himself with the problem of terms such as "Homer" which function grammatically like

terms but may not denote. Moreover, and possibly for the same reason, he was concerned only with the role of names in the context of declarative sentences and not in other kinds of linguistic expressions. Finally, his formulation of the principle leaves open the question of the meaning of sentences. But one nowhere finds him saying that mathematical objects are the references of mathematical terms in answer to the question of what they are. Rather, he is giving a criterion for the meaningfulness of terms and he suggests in §60 that the criterion extends beyond mathematics. And he then says that there is nothing more to the question of the self-subsistence of numbers than the role that number words play in propositions. I take this to mean that to say that a term refers is to say that it *is* a meaningful term. There is certainly no implication here that every mathematical object is the reference of a term.

One should note that, anyway, Wittgenstein's reformulation of the context principle, replacing the context of a proposition by the context of a language (cf. 1953, §10 and the discussion in footnote 2), must, for Dummett, also tame the same bold thesis. But, if so, Wittgenstein's argument also tames the bold thesis that there are physical objects. The issue between the Realist and the Idealist of §402 is a non-issue too. So if abstract objects are not 'as good as' Dummett conceives concrete objects to be, then neither are concrete objects. Dummett (1973) wishes to accept Wittgenstein's critique of language to the extent of accepting the formula that meaning is determined by use as a rough guide to the analysis of mathematical language. But I think that the above passage shows that he does not accept the full consequences of the critique. However, that is already shown by the fact, noted by Lear (1982), that he adopts the above formula to argue for a revisionist view of mathematics contrary to Wittgenstein (1953, §124).

10.

On the basis of the preceding discussion, I think that we can now begin to resolve the Truth/Proof problem. This problem arises because there seem to be two, possibly conflicting, criteria for the truth of a mathematical proposition: that it hold in the relevant structure and that we have a proof of it.

The first step of the resolution is to see that the first criterion is not a criterion at all. The appearance that it is arises from the myth of the

Model-in-the-Sky, of which we must – but do not seem to – have some sort of nonpropositional grasp, with reference to which our mathematical propositions derive their meaning and to which we appeal to determine their truth. The fact is that there are no such Models; there are only models, i.e., structures that we construct *in* mathematics. Our grasp of such a model *presupposes* that we understand the relevant mathematical propositions and can determine the truth of at least some of them – e.g., those whose truth is presupposed in the very definition of the model. Thus, rather than saying that holding in the model is a criterion for truth, we would better put it the other way around: being true is a criterion for holding in the model.

The myth of the Model tends to get attached to Platonism (or at least to ‘epistemological’ Platonism in the sense of Steiner [1973]) because the view that mathematics is about things like the system of numbers is compared with the view that propositions about sensible things are about the physical world; and here there is a tendency to believe that there *is* such a nonpropositional grasp, namely sense perception, which does endow meaning on what we say and to which we appeal to determine truth. But I hope that, if not what I have said, then Wittgenstein’s critique of this view of discourse about sensibles will convince the reader that it is inadequate.

11.

However, the first step of the resolution of the Truth/Proof problem may appear to have thrown out the baby with the bath water so far as Platonism is concerned; and both Benacerraf and Dummett think that this is so. For, if we reject the myth of the Model, then how *are* we to understand the notion of truth in mathematics? There might seem to be no alternative here to identifying it with the notion of provability. But then the independence of truth from the question of what we know or can know, which is the essence of Platonism, would be lost.

Benacerraf takes the less dogmatic line, not that this is the *only* alternative, but that it is the only one that has been substantially considered. But he takes the notion of proof here to be that of deducibility in some formal system, and he argues for the obviously correct conclusion that this yields an inadequate notion of truth. Dummett (1973) takes the view that, in giving up the myth of the model, we are giving up the notion of truth and, with it, classical

mathematics. He holds that the only viable alternative is to replace the notion “ A is true” by “ p is a proof of A ”, where the notion of proof here is the intuitionistic one.

Although I have argued in an earlier paper (Tait, 1983) that Dummett is wrong here and, indeed, that the intuitionistic conception is not entirely coherent, I nevertheless think that his response is, in a sense, in the right direction. Namely, I think that the intuitionists’ view that a mathematical proposition A may be regarded as a type of object and that proving A amounts to constructing such an object is right. Of course, to say that we may regard A as a type of object does not mean that we normally regard theorem proving as a matter of constructing objects. Indeed, when we are interested in constructing an object, say a real number, characteristically we are concerned with constructing one with a particular property. As a proposition, ‘Real Number’ is trivial. In the case of propositions, we are generally concerned with finding *some* proof and only rarely are we concerned with its properties. My point is rather that, independently of what we would say we are doing when we are theorem proving, what we are actually doing may be faithfully understood as constructing an object. The basic mathematical principles of proof that we use, e.g., the laws of logic, mathematical induction, etc., are naturally understood as principles of construction.

12.

However, the intuitionists also hold that the objects that a proposition A stands for, the objects of type A , *are* its proofs; and that I think is wrong. A proof of A is a *presentation* or *construction* of such an object: A is *true* when there is an object of type A and we prove A by constructing such an object.

Here then is the answer to one of our questions: why is proof the ultimate warrant for truth? The answer is of course that the only way to show that there is an object of type A is to present one. (To prove that there is an object of type A will mean nothing more than to prove A , and that means to exhibit an object of type A .)

Consider the equation $s = t$ between closed terms of elementary number theory. What does this equation mean? We may say that it expresses something about the system of numbers. That is certainly so,

but it is also not to the point until we say what that something is, without simply repeating the equation in the same or other terms.

The intuitionists seem to me very convincing when they say that what the equation expresses is that there is a certain kind of computation, namely, one which reduces s and t to the same term. For not only do we initially learn the meaning of such terms and equations by learning how to compute, but we take the existence of such computations as the ultimate warrant for the equation. Thus, it seems entirely natural to construe the equation as standing for the existence of such a computation and to take the equation to be true precisely when there is one.

Dummett (1973) accepts this analysis of such equations, but Dummett (1967) feels that, in accepting it, one is rejecting the Platonist point of view. His argument is that once we have accepted it there is no reason to invoke the notion of truth in the sense of 'holding in the system of numbers' to account for the meaning of the equation. But, of course, we are not accounting for its meaning in this way and, indeed, could not do so without circularity. *That* it holds in the system of numbers – in other words, the *fact* about this system which it expresses – is that there is such a computation. And we *prove* the equation by producing one.

At least part of the reason why Dummett believes that the above analysis of equations amounts to a rejection of Platonism is that he, along with the intuitionists, identifies the proofs of the equations, i.e., the *presentations* of the computations, with the computations themselves; and when we do that we can no longer account for the possibility of true but unprovable equations (Dummett 1967, p. 203). One might object that the Platonist need not account for this possibility providing he can account for there being *some* true but unprovable propositions. But the identification of computation with proof is a special case of the intuitionistic identification of the object with its construction in general. I do not believe that this identification is ultimately intelligible; but one sees that, in accepting it, there is in general no possibility of true but unprovable propositions.

However, it seems to me that, even in the case of the above sort of equations, the intuitionists are wrong and that one should not identify computations with proofs. For example, we easily prove $10^{10} = (10^5)^2$ as an instance of a more general theorem; but in the canonical notation $0, S0, SS0, \dots$ for numbers, I shall be unable to explicitly

compute 10^{10} and, even for terms with much shorter computations, the chance of my computing accurately is very small. Dummett (1977) makes the distinction here between ‘canonical proofs’, which in the present context are the explicitly presented computations, and the sort of proof one obtains from proofs of more general propositions, which are shorthand descriptions of canonical proofs. But when we know that the computation is longer than human beings, individually or collectively, are able to perform, we must ask the question: canonical proof for whom? To answer this by reference to an ‘ideal computer’ seems highly unsatisfactory. In the first place, proof is a *human* activity – and this would seem especially important to an intuitionist. But secondly, I am unable to see a significant difference between referring to an ideal computer who can compute $f(n)$ for *each* n and one who can compute it for *all* n and hence can decide whether $f(n)=0$ for all n or not. I don’t mean that there isn’t a *formal* difference, but rather that it is hard to see why the one idealization is legitimate and the other not. Yet the intuitionists reject the latter one, which would lead to the law of excluded middle for arithmetic propositions.

Computations are mathematical objects, forming a mathematical system like the system of numbers. One may object to the use of the term “computation” here, because of its association with computing as a human activity. But the term is also used in my sense, for example in the mathematical theory of computability. The ease with which one can confuse the two senses may contribute to the apparent plausibility of the intuitionistic identification of the computation with its presentation.

13.

When we extend the conception of mathematical propositions as types of objects to propositions other than equations, the distinction between object of type A and proof of A becomes even more evident. For example, let ϕ be a function which associates with each object a of type A , expressed by $a : A$, a type ϕa . Then

$$\forall x : A . \phi x$$

is the type of all functions f defined on A such that $fx : \phi x$ for all $x : A$,

and

$$\exists x : A . \phi x$$

is the type of all pairs (x, y) such that $x : A$ and $y : \phi x$. These definitions of the quantifiers are essentially forced on us by the propositions as types conception.⁸ The remaining logical constants are definable from the quantifiers, the null type 0 and the two-element type 2, whose objects we denote by \top and \perp . Thus, if we identify the type B with the constant function $\phi \equiv B$, then implication and negation are defined by

$$A \rightarrow B = \forall x : A . B \quad \neg A = A \rightarrow 0$$

and, if $\psi \top = A$ and $\psi \perp = B$, then

$$A \wedge B = \forall x : 2 . \psi x \quad A \vee B = \exists x : 2 . \psi x$$

Again, these definitions are essentially forced on us.⁹

In this way, the logical operations appear as operations for constructing types and the laws of logic as principles for constructing objects of given types. In this respect, there is no essential difference between constructing a number or set of numbers and proving a proposition. As Brouwer insisted should be the case, the logic of mathematics becomes part of mathematics and not a postulate about some transcendent model. However, Brouwer's view that the objects of mathematics be mental objects does not seem to me coherent. And the intuitionists' view that, for example, when we construct a number, we should be able to determine its place in the sequence $0, 1, 2, \dots$ ignores the difficulty that we cannot in any case do this for sufficiently complex constructions. Anyway, it is a restriction on ordinary mathematical practice that is inessential to the conception of propositions as types. The law of excluded middle amounts to admitting objects of types $A \vee \neg A$ which we may not otherwise be able to construct; and this does indeed lead to the construction of numbers whose positions in the above sequence are not computable. But it is not essential to our conception that they should be.

An object of an \forall -type is a function and I have argued elsewhere (cf. Tait, 1983) that, even in the case of constructive mathematics, one must distinguish between a function and a presentation of it, by a rule of computation or otherwise. I shall assume that, in the case of nonconstructive mathematics, no argument \exists is needed for this and,

therefore, that the distinction between objects of type A and proofs of A is clear.

14.

Of course, we have not really specified the types 0 and 2 nor the operations \forall and \exists until we have specified the principles of construction or proof associated with them. A brief discussion of this occurs in Tait (1983) (though the treatment of equations there is inadequate) and a fuller treatment is in preparation. These principles underlie mathematical practice in the sense that arguments that cannot be reduced to them are as a matter of fact regarded as invalid.

Questions about the legitimacy of principles of construction or proof are not, in my opinion, questions of fact. For mathematics presupposes a common mathematical practice and it is this that such principles codify. Without agreement about these principles and their application, there are no mathematical 'facts' (cf. note 8). Of course, many factors, including the requirement of logical consistency, would be involved in explaining why our mathematics takes the form it does; but the view that there is some underlying reality which is independent of our practice and which adjudicates its correctness seems to me ultimately unintelligible.¹⁰

In this respect, the controversy between constructivists and non-constructivists is similar to controversies about what is good or just between people of different moral or political outlook. In the latter case, one may ask what precisely *is* the issue. Why not simply use the terms 'just₁' and 'just₂'? It seems to me that the answer is that there is agreement about what I shall call the *normative* content of the term 'just' (or 'good'). Namely, to hold an action X to be just is to be disposed to act in certain ways. And I am not referring here entirely to linguistic acts such as affirming that one ought to do X . Rather, I have in mind Aristotle's practical syllogism: to hold that X is just is to be disposed to *do* X . If there were no agreement about this normative content of the term 'just', then there would be no point in disputing its *material* content, i.e., the question of what acts are to count as just. But the latter sort of dispute seems to me not necessarily to involve matters of fact, in as much as there may not be a sufficient basis of ethical agreement to decide the issue.

In the same way, there is a normative content of the term 'valid'. To

hold an inference to be valid is to be disposed to make the inference. *Because* we agree about this normative content, it is significant to argue about its material content, about what inferences are to count as valid. But, here too, there may be no matter of fact, only a matter of persuasion and adjustment of mathematical 'intuitions'. It is no accident that the dispute over the law of excluded middle often takes a moralistic tone. There are no noncircular arguments for this law and, in spite of all efforts to show otherwise, there are no arguments against it which are not essentially to the effect that it leads to noncomputable objects.

Constructivists do not deny any instance of the law of excluded middle, of course: that would lead to inconsistency. Rather, they refrain from its application. Thus, in principle, constructive mathematics may be viewed as a restriction within ordinary mathematics on the methods of proof or construction.¹¹ Aside from this, it is a striking fact that there simply is no disagreement concerning the valid principles of mathematical reasoning. Of course, I have not mentioned all of the type-forming operations involved in mathematics; nor is it clear that one could do so. For example, set theory involves the types obtained by 'iterating' the operation of passing from a type A to $PA = A \rightarrow 2$ into the transfinite. This involves the idea of creating new types by inductive definitions. However, although there might be disagreement about what inductive definitions one ought to admit, there is none about the principles of proof to be associated with such a definition when it is admitted.¹²

15.

The answer to the initial question of this paper, concerning the relation between truth and proof in mathematics, is that a proposition A is true when there is an object of type A and that a proof of it is the construction of such an object. *That* there is an object of type A is the 'fact' about, say, the system of numbers that A expresses. It is clear from this why proof is the ultimate warrant for truth.

The Platonist view that truth is independent of what we know or can know is entirely correct on this view. In the first place, there may be propositions which we can in principle prove on the basis of existing mathematics, but whose proofs are too complex for us to process. Secondly, there may be propositions which are not provable on the

basis of what we now accept, but are provable by means that we *would* accept. When I speak here of new means of proof, I do not of course mean the acceptance of new logical principles concerning 0, 2, \forall , \exists , inductive definitions, etc., but rather the introduction of further types to which we can apply these principles. For example, by the introduction of new types we may construct numerical functions, i.e. 'proofs' of $N \rightarrow N$, which we cannot otherwise construct.

It is, incidently, this open-endedness of mathematics with respect to the introduction of new types of objects that refutes the formalistic conception of mathematics, even if we leave aside the fact that mathematical concepts such as the number concept have a wider meaning than that given by their role in mathematics itself. The formalists seem to me right – in any case, we have not one example to refute them – that the above type-forming operations are completely determined in mathematics by the principles of inference we as a matter of fact associate with them. The incompleteness of formal systems such as elementary number theory is best seen as an incompleteness with respect to what can be expressed in the system rather than with respect to the rules of inference. For example, Gödel's undecidable proposition for elementary arithmetic can indeed be proved by induction; but the induction must be applied to a property not expressed in the system itself.

NOTES

* Earlier versions of this paper were presented to the Philosophy Department of the University of Wisconsin at Madison in the Winter of 1984, at the Tarski Memorial Conference at Ohio State in the spring of 1984 and at the Pacific Division meeting of the APA in the spring of 1985. I received many valuable comments on all of these occasions and, in particular, from Paul Benacerraf and Clifton McIntosh, who commented on my paper at the APA meeting. I should also like to thank Michael Friedman for his comments on an earlier version and for our many discussions of its subject matter.

¹ Many other contemporary authors could of course have been cited for essentially the same point. I shall focus primarily on Benacerraf (1973) and Dummett (1967, 1973) in citing the literature because these seem to me to represent most clearly and fully the two most important formulations of difficulties with Platonism. Benacerraf's paper is frequently cited as grounds for revisionist foundations of mathematics – e.g., in Field (1980, 1981), Kitcher (1978, 1983) and Steiner (1975). It consists in arguing that, in the context of mathematics, there is an apparent conflict between our best theory of truth, which is Tarski's, and our best theory of knowledge, which is causal, because we do not

causally interact with mathematical objects. As I understand him, Benacerraf himself, unlike many who cite him, is not calling for a revision of our conception of mathematics but only for a resolution of the apparent conflict. Dummett's critique of Platonism rests on a conception of meaning which he argues is incompatible with Platonism and, indeed, leads to the intuitionistic conception of mathematics; and so it is revisionist. My purpose, however, is not to review these papers. I cite them only because I wish to undermine conceptions which I myself cannot coherently formulate. On the other hand, I shall, I believe, resolve the difficulties that they find with Platonism in the course of this paper.

² The issue here is not 'inscrutability of reference.' That idea makes sense in connection with translating one language into another. But in what sense is *our* reference to the chair or to the number two inscrutable? When Wittgenstein (1953) writes "What is supposed to show what [the words] signify, if not the kind of use they have" (§10), his point is not that there is a well defined universe of things (perhaps described in the language of God) and that a word succeeds in referring to one of these things rather than another because of the kind of use it has. Rather, it is that we call a word 'referring' because of the kind of use that it has. And we ask the question "To what does the word 'X' refer?" in language, and it can only be answered there, by pointing perhaps or by saying "X refers to Y", where "Y" is "X" or some other term.

³ Gödel 1964 wrote:

But, despite their remoteness from sense experience, we do have something like 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. (pp. 483–84)

Many authors regard Gödel as an archetypal Platonist and this passage as a bold statement of what every Platonist must hold if he is to account for mathematical knowledge. In the words of Benacerraf (1973) (who, incidently, inadvertently left out the words "something like" in quoting the above passage):

[Gödel] 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. . . . he postulates a special faculty through which we "interact with these objects. (p. 675)

But I don't think that this is a fair reading of Gödel's remark. To understand what he means by "something like perception", one should look at his argument for it: "the axioms force themselves upon us as being true." One should also look at the paragraph immediately following the quoted one:

It should be noted that mathematical intuition need not be conceived of 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 combinations 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 the 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.

If anything is being ‘postulated’ here it is this other kind of relationship, not a faculty. This relationship is to account for the objective validity, not only of the ‘something like a perception’ of mathematical objects, but also of our ideas referring to physical objects. For it concerns the ‘given’ underlying mathematics, which are closely related to the abstract elements contained in our empirical ideas – e.g., the elements giving rise to our idea of an object (cf. *Theaetetus* 184d–186). That Gödel intends this relationship to be necessary for the objective validity of empirical as well as mathematical knowledge is indicated by the first sentence of the next paragraph, which I have already partially quoted, indicating that the question of the objective existence of mathematical objects is the exact replica of that concerning the objective existence of the outer world.

But he writes that the former question “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 truth or falsity of propositions like Cantor’s continuum hypothesis.” The point seems clear: the ‘something like a perception’, namely, mathematical intuition, is not what bestows objective validity on our theorems, any more than the perceptions of the Brain-in-the-Vat bestow objective validity on its assertions about the physical world. Yet, the Brain-in-the-Vat will have grounds for asserting (2); and, in the same way, mathematical intuition yields grounds for asserting (1). Thus, the ‘something like a perception’ is not the ‘another kind of relationship between ourselves and reality’ to which Gödel refers.

I do not entirely agree with Gödel here. What is objective about the existence of mathematical or empirical objects is that we speak in a common language about them – and this includes our agreement about what counts as warrant for what we say. And this view guides my estimation, stated above, of the role of mathematical intuition vis-à-vis grounds for asserting mathematical propositions. I cannot make the distinction Gödel seems to want to make between subjective validity, founded on our intuition, and objective validity. But it is worthwhile to point out that Gödel’s ‘something like a perception’ is not a ‘special faculty through which we interact with [mathematical] objects.’ Indeed, he was far less naive about the role of ordinary sense perception in empirical knowledge than the many writers who have focused on the passage in question as the Achilles heel of Platonism.

⁴ The ‘external question’ of the existence of numbers would seem to presuppose a univocal and nonquestion-begging notion of existence against which to measure mathematical existence. But what is it? Quine (1953, fn. 1), indeed attempts an argument to the effect that the desire to distinguish mathematical from spacetime existence on the grounds that the latter, but not the former, involves empirical investigation is unfounded. His argument is that showing that there is no ratio between the number of centaurs and the number of unicorns involves empirical investigation.

But the mathematical fact here is that 0 has no reciprocal; and that needs no empirical investigation.

I think that Carnap (1956) is right that 'external questions' of existence have no *prima facie* sense. But his attempt to make an absolute distinction between theoretically meaningful questions and those without theoretical meaning on the basis of his notion of a linguistic framework fails. For example, his framework for number theory is a formal system. But correct and sufficiently expressive formal systems for number theory are incomplete and, moreover, do not express all the properties of numbers. In later writings, Carnap attempted to solve the problem of incompleteness by allowing the system to contain the infinitary ω -rule. But now the internal question "Does there exist a number n such that $\phi(n)$ " can only mean "Does there exist an infinitary deduction of $\exists x\phi(x)$?" But this is an external question and may be mathematically nontrivial. But, anyway, linguistic frameworks are constructed in our everyday language; and it is hard to see how, lacking a precise notion of theoretical meaningfulness for it, we can convincingly determine when we have a 'good' framework and when we do not.

⁵ In the nominalism of Field (1980, 1981), the mathematical model is identified with the physical world. Thus, spacetime regions become nominalistically acceptable objects and mathematics is involved only insofar as such objects as numbers, sets and function are. Regions are real because we causally interact with them or at least can do so with some of them. This idea is developed in the 1980 book to show how to free Newton's theory of gravitation of mathematics, to make it a nominalistic theory. Of course, there is a difficulty in that, for a wide range of phenomena, Newton's theory is inadequate and, if we replace it by Einstein's theory, for example, the 'nominalization' has yet to be demonstrated. Moreover, Einstein's theory does not account for other ranges of phenomena and it is open whether it is compatible with an account of them. Finally, even if we had a reasonable universal physics, i.e., an account of all known forces, we should still have to ask (at least if we took Field's position) whether it was true. Well, let us suppose that we have such a 'true' universal physics, which is a spacetime physics. Won't causation be a relation between spacetime points or regions? But, unless some Supreme Court decisions – made with greater precision than, not only is it accustomed to, but than it is *in principle* capable of – are begged, I am not a spacetime region and so do not causally interact with such things. The world of chairs and rooms and us is different from the world of mathematical physics. The latter is called an *idealization* of the former; and this only means that we can use the mathematical theory in a certain way.

⁶ Putnam (1979) also seems confused on this point. He writes that Wittgenstein may have had in mind the following 'move':

One might hold that it is a presupposition of, say, " $2 + 2 = 4$," that we shall never *meet* a situation that we would *count* as a counterexample (this is an empirical fact); and one might claim that the appearance of a "factual" element in the statement " $2 + 2 = 4$ " arises from *confusing* the mathematical assertion (which has *no* factual content, it is claimed) with the empirical assertion first mentioned.

The 'empirical fact' and 'empirical assertion first mentioned', I assume, is that we shall never meet a situation that we would count as a counterexample. But this is, for Wittgenstein and in fact, no more an empirical fact than that we shall never meet, in a

game of chess, a situation which we would count as one in which the king is captured. Of course, neither of these assertions is a prediction about our future behavior or an assertion about our past behavior; they are each part of a description of a certain game. It is indeed an empirical fact that we play the game – that we do mathematics and play chess – but that is another matter. Putnam goes on:

This move, however, depends heavily on overlooking or denying the circumstance that an empirical fact can have a partly mathematical explanation. Thus, let T be an actual (physically instantiated) Turing machine so programmed that if it is started scanning the input “111,” it never halts. Suppose that we start T scanning the input “111,” let T run for two weeks, and then turn it off. In the course of the two-week run, T did not halt. Is it not the case that the explanation of the fact that T did not halt is simply the mathematical fact that a Turing machine with that program never halts on the input, together with the empirical fact that T instantiates that program (and continued to do so throughout the two weeks)?

The answer is simply: yes. But what has this to do with the fact that the mathematical proposition “ $2 + 2 = 4$ ” or “Turing machine t with input ‘111’ never halts” is not the sort of proposition for which the idea of empirical counterexamples makes sense? This example is no different from our explanation of the motion of a physical object. We model the behavior of T with t . If it is a good model (and this idea defies precise analysis), then the fact that t doesn’t halt (in the mathematical sense) should lead us to believe that T doesn’t (in the physical sense) halt. But what has this to do with the conceivability of an empirical counterexample to the statement that t doesn’t (in the mathematical sense) halt? The sense in which it is claimed that “ $2 + 2 = 4$ ” has no ‘factual content’ is not intended to imply that it has no empirical applications.

⁷ Consider systems $\mathcal{A} = \langle A, a, f \rangle$, where A is a type of object, a is an object of type A ($a: A$) and f is a function from A to A ($f: A \rightarrow A$). Dedekind (1887) characterized the system $\mathcal{N} = \langle N, 0, S \rangle$ of numbers as such a system in which $0 \neq S^n$ for all $n: N$, $S^m = S^n \rightarrow m = n$ and, if X is any set of numbers containing 0 and closed under S , then it is the set of all numbers. There is no question of *identifying* the system of numbers: it is, as Dedekind puts it (§73), a ‘free creation of the human mind.’ We have created it in the sense that we have specified once and for all its grammar and logic. Moreover, given any other system \mathcal{A} satisfying this characterization, the proof that \mathcal{A} is isomorphic to \mathcal{N} is a trivium and we shall not disagree about that. We might indeed disagree about the principles used to construct some set P of numbers or some system \mathcal{A} ; but that is a different matter and, anyway, if we leave aside those who wish to use only constructive principles, then as a matter of fact, there is no such disagreement (cf. §15). Moreover, the possibility of this kind of disagreement exists even in constructive mathematics, which Dummett (1973) is advocating. In that case, Dedekind’s characterization should be replaced by the classically equivalent one essentially given by Lawvere (1964), namely that \mathcal{N} has the property of *unique iteration*: given any system \mathcal{A} , the equations $g0 = a$ and $gS = fg$ define a unique function $g: N \rightarrow A$. But we may still disagree about when a system \mathcal{A} has been legitimately introduced.

⁸ Suppose that we already have that, for any $x: A$, $\phi(x)$ is already identified with a type of object. Then $\exists x: A \phi(x)$ means that, for some $x: A$, $\phi(x)$, and so that, for some $x: A$,

there is a $y: \phi(x)$, and so that there is a pair (x, y) of the required type. $\forall x: A\phi(x)$ means that $\phi(x)$ for all $x: A$, and so that, for each $x: A$, there is a $y: \phi(x)$. So we have 'reduced' the meaning of \forall to 'for all x , there exists a y .' We avoid an infinite regress here only by taking the latter to mean that we have a function f which gives us $y = fx$ for each x . This is, as a matter of fact, the way in which we do reason. The appearance that it isn't arises from the fact that we often are thinking of the reasoning as taking place in a model in which no such f occurs. So $\forall x: A\phi(x)$ may be true in the model without there being the required f in the model. But that of course is different from saying that there is no such f .

Our analysis of the quantifiers yields the Axiom of Choice in the form

$$\forall x: A \exists y: B\psi(x, y) \rightarrow \exists z: A \rightarrow B \forall x: A\psi(x, zx).$$

For let f be of the antecedent type. Then for each $x: A$, fx is of the form (y, u) , where u is of type $\psi(x, y)$. Let $z: A \rightarrow B$ be defined by $zx = y$ and let $v: \forall x: A\psi(x, zx)$ be defined by $vx = u$. Then (z, v) is of the type of the conclusion. The argument above for our analysis of the universal quantifier looks itself like an application of the Axiom of Choice:

$$\forall x: A \exists y(y: \phi(x)) \rightarrow \exists f \forall x: A(fx: \phi(x)).$$

But there are two respects in which it is different. First, it contains two variables, y and f , whose type is unspecified and, secondly, it involves 'propositions' of the form ' $u: C$ '. Concerning the first point, the notion of a mathematical object in general seems a problematic notion and certainly is no part of the Platonistic conception that I am discussing. Concerning the second, ' $u: C$ ' is not a mathematical proposition in the sense that I am discussing. Otherwise, we would have to know what are the objects v of type $u: C$, the objects of type $v: (u: C)$, etc., leading to an infinite regress. The fact is that we have a type C only when we have agreement as to what counts as an object of type C . Thus, statements such as ' $u: C$ ' are grammatical statements. It is the wrong picture to think that there is a universe of 'mathematical objects' and then we must determine for one of them, u , what type it has. (This seems to me to be the view behind Jubien (1977), where its absurdity is well illustrated.) 0 is a mathematical object because it is a number. If I have introduced an object u of type C , then either $u: C$ or else I have been indulging in nonsense.

⁹ Cf. Tait (1983). In the case of negation, note that $A \wedge B \rightarrow 0$ should be a requirement for a negation B of A . But when this holds, $B \rightarrow \neg A$; and so $\neg A$ is the weakest candidate for negation. One may feel, nonetheless, that negation presents a counterexample to the view of propositions as types; since, if ' A is true' is to mean that there is an object of type A , then ' A is false' ought to mean that there is no such object, and this is not an existence statement. But if there is no object of type A , then there is an object of type $A \rightarrow 0$, namely the null function. But, in any case, there is something deceptive about the discussion. What does 'not' mean when we say that it is not the case that there is an object of type A ? For this too is a mathematical proposition and, indeed, simply means $\neg A$. We should not think that there is a meaning of 'not' that somehow transcends mathematical practice.

¹⁰ The question of the truth of mathematics, as opposed to truth *in* mathematics has historically been the concern of many philosophers. In some cases, e.g., Plato and

Leibniz, this question has been distinguished from that of why mathematics applies to the phenomena and in others, such as Aristotle and Kant, it has not. This latter question, of why mathematization of the phenomena works, has itself been a source of anti-Platonism. But, as I have indicated in §7, the only kind of answer to that question would be in terms of cognitive science and an account of why it is that we have evolved.

¹¹ Of course, if one is interested only in constructive mathematics, one may diverge from the classical development of, say, analysis, by choosing concepts more amenable to constructive treatment than the classical analogues. My point is only that the principles of construction and reasoning used in the development remain classically valid. Apparent counterexamples such as Brouwer's proof that every real-valued function on the continuum is continuous are a result of ambiguity, not of using classically invalid principles.

¹² There is another method of obtaining new types which derives from Dedekind (1888) and which we may refer to as 'Dedekind abstraction.' For example, in set theory we construct the system $\langle \omega, \phi, \sigma \rangle$ of finite von Neuman ordinals, where $\sigma x = xU\{x\}$. We may now abstract from the particular nature of these ordinals to obtain the system \mathcal{N} of natural numbers. In other words, we introduce \mathcal{N} together with an isomorphism between the two systems. In the same way we can introduce the continuum, for example, by Dedekind abstraction from the system of Dedekind cuts. In this way, the arbitrariness of this or that particular 'construction' of the numbers or the continuum, noted in connection with the numbers in Benacerraf (1965), is eliminated. It is incidentally remarkable that some authors such as Kitcher (1983) have taken Benacerraf's observation to be an argument against identifying the natural numbers with sets, but have been content to identify the real numbers with sets, although there are again various ways to do that. Kitcher (1978) contains an amazing argument based on Benacerraf's observation, to the effect that Platonism is false: on grounds of economy, all 'abstract' objects should be sets. Numbers are abstract. But there is no canonical representation of the numbers as sets. Therefore, the view that there are such things as numbers is false. (A person makes up a budget and, on grounds of economy, fails to budget in for food. But we need to eat. So the notion of a budget is incoherent.)

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