ON THE METAPHYSICS OF NUMBERS

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ABSTRACT

We reason that if there are numbers, they are abstract and therefore acausal. The question thus arises as to how we have knowledge of these objects. My thesis consists of three parts, each of which explores a response to this question.

Charles Parsons has argued that we know about the numbers because we can apprehend (or "intuit") some abstract objects such as expression-types which form a model for arithmetic and which therefore stand as representations of the numbers. Although I think we can intuit some abstract objects in the way Parsons describes, I show that our knowledge that arithmetic has a model cannot be based on our intuition of these objects. I conclude that intuition does not offer a route to the numbers.

Constructivists such as the traditional intuitionists offer a different sort of answer. They argue that numbers are constructions which describe the mental processes one goes through in doing mathematics, albeit at a certain level of abstraction. Our knowledge of these constructions is thought to be straightforward since, after all, they are objects of our own experience. I show that this account of the numbers is circular, and that the only way to avoid this circularity commits the intuitionist to a version of mathematical finitism. I conclude that numbers are not mental constructions.

Mathematical antirealists such as Michael Dummett argue that the truth of a mathematical statement cannot intelligibly transcend evidence for the truth of that statement, and that an account of mathematical truth which recognizes this fact commits us to an intuitionistic logic and mathematics. I show that Dummett's arguments for intuitionism involve the crucial assumption that one's understanding of a language is constituted by one's linguistic behavior. I argue that Dummett's reasons for making this assumption are not well-grounded and, therefore, that his arguments for intuitionism fail.

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1. INTRODUCTION: BENACERRAF'S DILEMMA

1. My thesis is concerned with questions about the natural numbers: Are there such numbers? If there are these numbers, what kind of entities are they? How do we have knowledge of them? In discussing these questions I find it useful to consider the dilemma presented by Paul Benacerraf in his article "Mathematical Truth".[1] As Benacerraf observes, we talk as though there are numbers. We claim, for example, that there is a number which succeeds the number 4, and that there is an infinity of prime numbers. This common way of talking is reflected in the most widely accepted theories of truth which treat expressions such as "the number 4" as genuine referring expressions. Yet we reason that if there are numbers, they are abstract hence acausal.[2] Benacerraf claims, however, that the best theories of knowledge are causal. If he is correct, then there is an obvious dissonance between what we take to be the best theory of truth for mathematics and what some, at least, take to be the best epistemology. We then face a dilemma ("Benacerraf's dilemma") in deciding which of these

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[1] Benacerraf [2].

[2] It is notoriously difficult to give a satisfactory account of what makes an object an abstract object. For present purposes I will assume that an object is abstract if it lacks spatiotemporal location and if it is acausal. I recognize, however, that this characterization may not be a completely happy one. Abstract objects such as types may enter the causal swim through their concrete tokens. And it might take an extreme realist to hold that, for example, English words and sentences have always existed.

theories we should revise or abandon.

Benacerraf's dilemma is sometimes stated more forcefully. If, as Benacerraf claims, there is an incompatibility between the most widely accepted theories of mathematical truth and knowledge, then one might claim that either our mathematical statements are not true or mathematical truths are unknowable. Yet I do not think this more dramatic way of stating the dilemma gets to the real issue. There are mathematical truths; the statement "2+3=5", for example, is true. Moreover, we have some mathematical knowledge. We know that 2+3=5 if we know anything. It seems to me, therefore, that Benacerraf's dilemma is best thought of as a challenge: How can we construct a coherent account of mathematical truth and knowledge? I believe that a satisfactory response to this challenge will involve a satisfactory response to the questions with which I began this essay.

In the following chapters I examine three responses to this challenge. Before I turn to those responses, however, I will first consider one strategy for resolving Benacerraf's dilemma which I do not find attractive but which brings to the fore an important assumption that I wish to make. I will also discuss why I do not think Benacerraf's claim that the best theories of knowledge are causal threatens either the view that there are mathematical truths, or the claim that we have mathematical knowledge.

2. One extreme strategy for resolving Benacerraf's dilemma is to give up the view that mathematical singular terms refer. (One might argue, for example, that expressions such as "the number 4" function syntactically

but not semantically as singular terms.) I do not find this strategy attractive. I agree with Benacerraf that a satisfactory analysis of singular terms should be applied uniformly across the language, and I see no alternative to treating expressions such as "Rudolf Carnap" (as that expression features in "Rudolf Carnap published the Aufbau in 1928"), or "Germany" (as that expression features in "Germany seeks reunification") as referring expressions. My reason for thinking that an analysis of singular terms should be applied uniformly across the language is the usual one. I know of no satisfactory grounds for treating "Germany" but not "the number 4" as a referring expression. For the purposes of this paper, therefore, I will assume that mathematical singular terms refer.

By assuming that numerical singular terms refer, I have assumed that the correct answer to the ontological question "Are there numbers?" is "There are".[3] Yet this assumption leaves unanswered what I think of as the primary metaphysical question concerning numbers; namely: What kind of entities are they? One might argue, for example, that the numbers are some sort of mental construction. (I will argue in chapter three that they are not.) One might also argue that the numbers are "platonic" objects which exist independently of mathematicians. Now, although I do not make any assumptions about the nature of the numbers (my goal, after all, is to discover what their nature is), it does seem to me that a satisfactory

[3] In his [1], Benacerraf has argued that there is no unique collection of objects to which number expressions refer. On this view, for example, "the number 4" is a kind of dummy name referring to the fifth element in any progression which satisfies the Dedekind-Peano axioms. If Benacerraf is correct, then my assumption that number expressions refer to a unique set of objects is incorrect. I shall not, however, take up Benacerraf's point here.

account of what the numbers are will show that they are abstract. If the numbers were concrete, they would have spatiotemporal location. But, as I see it, the very question of where the numbers might be located (in France?) betrays a misunderstanding of the concept of number.

3. I now review why Benacerraf thinks the best theories of knowledge (and, as we shall see presently, of reference) are causal. I present this review in order to show that the reasons Benacerraf gives for adopting causal theories are not so compelling that we must abandon either the view that number expressions refer, or the claim that we can have mathematical knowledge.

Benacerraf writes:

> I favor a causal theory of knowledge on which for X to know that [a statement] S is true requires some causal relation to obtain between X and the references 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 (Benacerraf [2] p.412).

If Benacerraf is correct, then some causal theory of reference best explains how the name/bearer relation is established for number expressions, and some causal theory of knowledge best explains how we know our arithmetical statements are true.[4] Benacerraf does not discuss

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[4] Note that a satisfactory mathematical epistemology will need to explain how we acquire our mathematical knowledge only if mathematical knowledge is something acquired. It may not need to explain how we acquire our mathematical knowledge if our mathematical knowledge is innate or otherwise _apriori_. Although Benacerraf acknowledges this last point, he does not pursue it. (See Benacerraf [2] p.414.) For the moment I shall not pursue it either, although I shall return to it briefly in chapter 5.

which theories he thinks accomplish these tasks because he wants to make certain general points not tied to the success or failure of any particular causal theory. What I will consider here is what he thinks these general points are.

Consider reference first. As with any discussion of reference, it is a good idea to begin with Frege for whom the sense of a proper name provides a condition (often expressed in terms of definite descriptions) such that whatever satisfies that condition is the referent of that name. Following common practice, I will call this type of theory a _description theory_ of reference. So, for example, one's sense of "Rudolph Carnap" may be given by the descriptions "Frege's most famous student", "the author of the _Aufbau_", and so forth. What is important to note is that the description theory is entirely compatible with reference to abstract objects. One's sense of "4", for example, may be given by the descriptions "the square root of 16", "the number of major points on the compass", and so on.

Some find a description theory inadequate not just for mathematics but for epistemologically more straightforward cases as well. Recall, for example, Kripke's story of a community which associates the name "a" with a set of conditions which they take to be satisfied by individual _A_ but which in fact is satisfied only by another individual _B_ who is causally isolated from that community.[5] If the description theory is correct, then, when a community member uses "a" with _A_ in mind he is nevertheless referring to _B_. But how can the community members refer to someone from

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[5] See Kripke [1] lectures I and II. Here I depart from Benacerraf's exposition.

whom they are causally isolated? If the only acceptable response to this last question is that they cannot, then it appears the description theory is inadequate. An alternative theory sensitive to the problem of causal isolation may seek to link the name "a" with an object $\underline{A}$ just in case there is an unbroken, reference-preserving causal chain linking one's use of "a" to refer to $\underline{A}$ back to an original ostensive definition of $\underline{A}$ as "a".[6] But if we accept the claim that a satisfactory theory of reference must contain this causal component, then Benacerraf's problem with reference to abstract objects is clear: Abstract objects are acausal; consequently, number expressions cannot refer to abstract objects.

In response to this last point, it is not clear to me that we need direct causal contact with abstract objects in the way Kripke's example suggests. If Quine is correct, deferred ostension will do.[7] We may, to use his example, explain the abstract singular term "alpha" by pointing to a suitable Greek inscription. Moreover, it may be that pointing to this inscription involves enough causal contact with the type of which the inscription is a token to satisfy any reasonable causal constraint on reference. (We shall see something like this in Parsons's account of mathematical intuition.) It seems to me, therefore, that the argument that we cannot refer to the numbers because of their causal isolation is far from conclusive. And, if I am right about this last point, I do not see the description theory seriously imperiled by questions of causal contact. Finally, if a causal theory of reference is truly incompatible with

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6 The suggestion has been made by Kripke, among others.

7 See Quine [2] pp.39-41.

reference to numbers, it seems to me that the correct conclusion to draw from this incompatibility is not that there are no numbers, but rather that a causal theory of reference is simply not the appropriate theory to explain how we refer to the numbers.

The basic idea behind a causal theory of knowledge is even more straightforward. How do I know, for example, that "There is a red book on my desk" is true? According to Benacerraf, for me to know "There is a red book on my desk" is true: There must be a red book on my desk; I must be in the relevant state of belief that there is red book on my desk; and that there is a red book on my desk "figures in a suitable way" (to be spelled out by the particular causal theory) in the explanation of how I come to believe there is a red book on my desk.[8] Benacerraf claims this explanation typically establishes a causal connection between those objects which make the statement true and my belief that the statement is true. One way this connection might be established in the present case is by giving an account of how unfiltered sunlight reflects off the book and strikes my retinas, setting off a further causal chain in my nervous system and brain, inducing in me the belief that there is a red book on my desk. But, it is argued, claims to mathematical knowledge cannot be explained in this way. If numbers are acausal, there can be no causal relation obtaining between those states of affairs by virtue of which a number statement is true and one's belief that the statement is true.

It is not clear to me that Benacerraf's sketch tells us enough about how we have knowledge of ordinary physical objects to be very useful. That

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[8] Benacerraf [2] p.412.

complaint aside, however, my main point is the same as that offered with respect to the causal theory of reference. If adopting a causal theory of knowledge for mathematics leads to the conclusion that we have no mathematical knowledge, then the obvious lesson to draw is that mathematical knowledge is not causal knowledge. (Note, however, that this conclusion is not meant to rule out the possibility that a satisfactory epistemology for mathematics might have some causal component.) Therefore, even if Benacerraf is correct that some causal theory best explains how we know some statements are true, it is another matter entirely whether a causal theory can best explain how we know our mathematical statements are true.

4. In the following chapters I examine three responses to the challenge of constructing a satisfactory account of mathematical truth and knowledge. Charles Parsons has argued that we know about the numbers because we can apprehend (or "intuit") some abstract objects such as expression-types which form a model for arithmetic and which therefore stand as representations of the numbers. Although I think we can intuit some abstract objects in the way Parsons describes, I show that our knowledge that arithmetic has a model cannot be based on our intuition of these objects. I conclude, therefore, that intuition does not provide a route to the numbers. In two appendices I argue that accounts of mathematical knowledge offered by Resnik and Berkeley fail for relevantly similar reasons.

Constructivists such as the traditional intuitionists (Brouwer, Heyting, *et al.*) offer a different sort of answer. They argue that numbers are constructions which describe the mental processes one goes through in doing mathematics, albeit at a certain level of abstraction. Our knowledge of these objects is thought to be straightforward because, after all, they are objects of our own experience. I show, however, that the intuitionists' account of the numbers is circular, and that the only way I see of avoiding this circularity commits the intuitionist to a version of mathematical finitism. I conclude that numbers cannot be mental constructions.

Contemporary antirealists such as Michael Dummett argue that the truth of a mathematical statement cannot intelligibly transcend evidence for the truth of that statement, and that an account of mathematical truth which recognizes this fact commits us to an intuitionistic logic and mathematics. I show that Dummett's arguments for antirealism involve the crucial assumption that one's understanding of a language is constituted by one's linguistic behavior. I argue that Dummett's reasons for making this assumption are not well-grounded and, therefore, that his arguments for mathematical antirealism fail. I conclude with some brief comments about how I plan to continue my investigation, focusing on the view that mathematical knowledge is not causal knowledge.

2. PARSONS'S THEORY OF MATHEMATICAL INTUITION

1. Introduction.

In a number of recent articles, Charles Parsons has argued that we know about the numbers because we can apprehend, or "intuit", certain kinds of abstract objects such as expression-types which form a model for arithmetic, and which therefore stand as representations of the numbers.[1] Although I think we can intuit some abstract objects in the way Parsons describes, I argue that our knowledge that arithmetic has a model cannot be based on our intuition of these objects. I conclude that intuition does not offer a route to the numbers.

Following this introduction, I divide this chapter into two main sections and two appendices. In section 2 I outline how Parsons thinks intuition works, how he thinks we intuit mathematical objects, and why he thinks our knowledge that arithmetic has a model involves our intuition of these objects. I present my objections in section 3. In the first appendix I discuss Michael Resnik's structuralist account of numbers which I think fails for the same sort of reason that Parsons's account fails. In the second appendix I argue that Berkeley's account of geometric knowledge involves an appeal to something very much like intuition, and that it fails as well.

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[1] Parsons [2], [3], [5] and [7].

2. Parsons's account of mathematical intuition.

(2i). How Parsons thinks we intuit some abstract objects.

According to Parsons, we apprehend, or "intuit" a type whenever we perceive or imagine an object as a token of that type.[2] Consider, for example, the following underlined array of ink: Theaetetus. By perceiving this array of ink as a token of the word-type "Theaetetus", Parsons claims the reader intuits the word-type "Theaetetus".[3] Consider another example. The reader is invited to imagine an inscription of "Theaetetus". In thinking of that image as a token of the word-type "Theaetetus", Parsons claims the reader intuits that word-type. He concludes:

> At least one kind of essentially mathematical intuition, of symbol- and expression-types, is perfectly ordinary and recognized as such by ordinary language (Parsons [3] p.155).

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2 It is unclear whether Parsons thinks we can intuit objects other than types. He lists the following as examples of intuitable objects: symbol-types and expression-types, geometric figures "as traditionally conceived", and "perhaps" sets or sequences of concrete objects (Parsons [8] p.2, [2] p.43, [3] pp.153-54). He does not, however, say whether he thinks this list is exhaustive.

A terminological point: Parsons distinguishes two uses of "intuition" common to the philosophic literature (Parsons [3] pp.146-147). The first is what he calls the "propositional attitude" use, an example of which is "I intuit that statement A is true", where what is meant is that the truth of A is in some sense evident or self-evident. Parsons suggests that the Cartesian clear and distinct perception may be an intuition of this kind. The second use is what Parsons calls "object-relational", an example of which is "I intuit (object) a", where "intuit" expresses a relation between the intuitor and object a. It is obviously this second use of "intuition" with which we are primarily concerned here.

3 Obviously, one might lack the conceptual resources to see the underlined array of ink as a word-token. In that case, Parsons concludes that one would perceive the array of ink but would not intuit the word-type. See Parsons [3] pp.154,162.

I agree that what Parsons describes as the intuition of symbol and expression types is perfectly ordinary. I shall now explain why he thinks it is "essentially mathematical".[4]

(2ii). How Parsons thinks we intuit mathematical objects.

In order to see how Parsons thinks we intuit mathematical objects, we first need to see which objects Parsons thinks are mathematical. According to Parsons, what is "essential" to mathematical objects "is the relations constituting the structures to which they belong".[5] Therefore, following Parsons, we may say that what is essential to an arithmetical, or number-theoretic object is that it occupies a place in some structure which has the form

$$x(0), x(1), x(2), \ldots, x(n), x(n+1), \ldots$$

in which, paraphrasing Russell, there is an initial object, a successor to each object, no repetition of objects, and every object in the series can be reached from the initial object in a finite number of steps.[6] Following

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4 Here the reader may expect an account of how Parsons thinks intuition works; what psychological mechanisms are responsible for it, and so forth. I do not think, however, that this would be a reasonable expectation. No one, to my knowledge, has any idea how the phenomenon which Parsons describes as intuition works. Yet with the exception of some extreme nominalists I do not think anyone really doubts that we do intuit some symbol and expression types in the manner just described.

5 Parsons [3] p.151. See also Parsons [8] p.2.

6 Russell [1] p.8. These are, of course, the conditions codified by the Dedekind-Peano axioms.

common practice, I will call any sequence of objects satisfying these conditions an "ω-sequence".[7]

In order to see how Parsons thinks we intuit these arithmetical objects, it will be useful to introduce a simple language L whose only basic symbol is the "/", or stroke.[8] The well-formed expressions in L are finite linear strings of strokes (////, for example). Following Parsons, we may say that we intuit the stroke symbol-type whenever we perceive or imagine an object as a stroke, and that we intuit a stroke string expression-type whenever we perceive or imagine an array of strokes as a token of that type. So far, so good. Now consider the sequence

L*: /, //, ///, ////, ...

If we interpret the initial single stroke as the series' initial object, the operation of concatenating one stroke to the right of any stroke string as successor, and if the series continues in such a way that it satisfies each of Russell's conditions, then the stroke strings which make up L* form an ω-sequence.

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[7] Two comments may be in order here: First, in order to recognize an object as a number-theoretic object, it may be sufficient that we recognize that object as an element in some initial segment of an ω-sequence. After all, the child who can perform some elementary arithmetical operations may know about the number 4, but may not know that the number series is infinite. Second, if all that is essential for an object to be a number-theoretic object is that it occupies a place in some ω-sequence, then it appears to be the case that any object is, or can be, a number-theoretic object. Suppose, for example, that we think of the moon as an element in some ω-sequence. So conceived, it follows on Parsons's view that the moon is an arithmetical object.

[8] See Parsons [2] p.43-46, [3] pp.153-154.

We now have all the information we need to see why Parsons thinks we can intuit number-theoretic objects. Parsons thinks that whatever objects form an ω-sequence are number-theoretic objects. Thus, presumably, the stroke strings which make up L^* are number-theoretic objects. And since we can intuit at least some of these objects, it follows that we can intuit at least some number-theoretic objects.[9]

(2iii). Why Parsons thinks our knowledge that arithmetic has a model is intuitive.

We now know what Parsons thinks intuition is and how he thinks we intuit some mathematical objects. In this section I consider why he thinks our knowledge that arithmetic has a model is intuitive. I'll begin by outlining when Parsons thinks knowledge is intuitive.

Parsons writes:

> I shall not call knowledge intuitive unless it rests on the intuition of objects (Parsons [7] p.215).

I interpret this claim in what I take to be the most straightforward way: Our knowledge that a statement S is true is intuitive just in case it involves our intuition of those objects which make S true. Now, Parsons is unclear about how much propositional knowledge he thinks can be founded

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[9] I add the qualification that we can intuit <u>some</u> number theoretic objects because while it is clear to me that we can intuit very short stroke string expressions such as ////, it remains to be seen in what sense we can intuit a string of 10^{10} strokes. Of course, much depends on how we interpret the modality in the claim that one <u>can</u> intuit a stroke string of such-and-such a length. I'll return to this issue in section 3.

on the intuition of objects. It may be, for example, that our knowledge that "// is a stroke string" is true involves only our intuition of the relevant stroke string. It is less clear, to me at least, to what degree Parsons thinks our knowledge that "/// succeeds //" is true is intuitive since he claims our knowledge of operations such as successor is not intuitive.[10]

If my interpretation of Parsons's claim is correct, we may conclude that Parsons thinks our knowledge that arithmetic has a model is intuitive because he thinks it involves our intuition of those objects which make the Dedekind-Peano axioms true. The Dedekind-Peano axioms are:

> (PA1) Zero is a natural number. ($N(0)$).
>
> (PA2) Zero is not the successor of any natural number. ($\forall x(Nx \rightarrow (x' \neq 0))$)
>
> (PA3) Every natural number has a successor which is also a natural number. ($\forall x(Nx \rightarrow \exists y(y=x' \& Ny))$).
>
> (PA4) Different natural numbers have different successors. ($\forall x \forall y(Nx \& Ny \& x \neq y \rightarrow x' \neq y')$).
>
> (PA5) If a property F holds of zero, and holds also of the successor of every natural number of which it holds, then F holds of every natural number. ($\forall F[F(0) \& \forall x \forall y(Fx \& y=x' \rightarrow Fy) \rightarrow \forall z(Nz \rightarrow Fz)]$).

At first glance, therefore, it appears Parsons thinks our knowledge that the Dedekind-Peano axioms have a model is intuitive because he thinks we can intuit the natural numbers. The problem with this conclusion, however,

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[10] Parsons [7] p.215. Compare: In order to see that one object is to the left of another object, it is not enough that we see the two objects; we must also see that they stand in a particular spatial relation to one another. See also Parsons [7] p.225 where he contrasts operations with objects and states explicitly that the iteration operation is not given in intuition.

is that even though Parsons thinks our knowledge that the Dedekind-Peano axioms have a model is intuitive, he does not think we can intuit the natural numbers. More precisely, he does not think we can intuit the natural numbers directly. I'll now explain this qualification.

In order to see why Parsons does not think we can intuit the natural numbers directly, it will be useful to introduce the distinction he draws between what he calls "pure" abstract objects and what he calls "quasi concrete" abstract objects.[11] Intuitable objects such as expression-types are quasi concrete; they are abstract, but have concrete instances.[12] Pure objects, on the other hand, are not intuitable because they have no concrete instances. One way to see how Parsons thinks of pure objects is to see why he thinks the natural numbers are pure. According to Parsons, we can intuit various quasi concrete objects which, taken together, form various quasi concrete ω-sequences. (L^* is thought to be one such sequence.) Parsons claims, however, that there is no principled way to identify any particular quasi concrete ω-sequence as <u>the</u> natural numbers. He writes:

> This thesis [that ω-sequences are given in intuition] does not imply that numbers are given in intuition. One could draw that conclusion only by an arbitrary and questionable construal of numbers as elements of the intuitively given structure [...] (Parsons [5]).

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11 Parsons [6] p.25, [8] pp.2-3.

12 Note that Parsons does not identify concrete objects with material objects. He thinks the intuition of a type can be founded on the perception or imagining of a token of that type. Thus Parsons classifies an imagined visual image as a concrete object.

Rather, Parsons thinks of the natural numbers as those pure, wholly abstract objects which make up an ω-sequence independently of any way it might be presented quasi concretely.[13] He writes:

> [...] we look in vain for anything else to identify [pure objects] beyond the basic relations of the structures to which they belong: for the natural numbers 0, S (successor), and perhaps arithmetic operations [...] (Parsons [8] p.2)

Therefore, as I understand it, Parsons thinks we indirectly intuit the pure natural numbers through our direct intuition of their various quasi concrete representations.[14] As an example of how Parsons thinks we do

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[13] It may be useful to think of the pure natural numbers in the way Resnik suggests; that is, as structureless points in the wholly abstract ω-sequence. See appendix A.

[14] Parsons sometimes draws the direct/indirect distinction in terms of weak versus strong intuitability. He writes: "An object is strongly intuitable if it can be intuited, i.e., if it can itself be an object of intuition. An object is weakly intuitable if it can be represented in intuition without itself being intuitable" (Parsons [4] p.496).

Parsons is not at all clear about what he thinks constitutes the representation relation he sees obtaining between pure and quasi concrete objects. One way to think of what Parsons may have in mind is to consider what I take to be the analogous relation Kant saw obtaining between what he characterized as pure and empirical intuitions. (See, e.g., Kant [1] A19ff.) For example, Parsons writes: "I do think that the objects considered in arithmetic and predicative set theory can be construed as <u>forms</u> of spatiotemporal objects" (Parsons [1] p.135). I understand Parsons to be claiming that the pure numbers give the form 1,2,3,... any quasi concrete series of objects must take in order to represent the pure number series. I suggest, therefore, that we think of what Parsons sees as the weak or indirect intuiting of a pure object though the direct intuition of a quasi concrete representation of that pure object as analogous to a Kantian pure intuition. I believe this reading is supported by Parsons writing of Kant's theory that: "An empirical intuition functions, we may say, as a pure intuition if it is taken as a representative of an abstract structure" (Parsons [1] p.136).

We are now in position to see how Parsons's theory of intuition, if correct, might lead to a solution of Benacerraf's dilemma. Following this, I'll now consider why he thinks our knowledge that L* forms a model for arithmetic is intuitive.

Because we are now concerned with the question of why Parsons thinks our knowledge that L* forms a model for arithmetic is intuitive, it will be useful to recast the Dedekind-Peano axioms in the following way:[15]

> (PA1') / is a stroke string.
>
> (PA2') / is not the successor of any stroke string.
>
> (PA3') Every stroke string has a successor which is also a stroke string.
>
> (PA4') Different stroke strings have different successors.
>
> (PA5') If a property F holds of /, and holds also of the successor of every stroke string of which it holds, then F holds of every stroke string.

The most detailed account Parsons gives as to why he thinks our knowledge that L* forms a model for the Dedekind-Peano axioms is the following.[16]

> Suppose our numerals are to be /, //, ///, ////, and so forth, and suppose (as fits the notation better), our theory is to be

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Parsons, we may say that number expressions refer to the numbers. We know about the numbers through our intuition of their representations. Moreover, because intuition involves the causal processes of perception or imagining, intuition may even be compatible with a reasonable causal constraint on knowledge or reference.

[15] The formalization remains the same as that given for PA1-5. Substitute "/" for "0" and read "N" as "is a stroke string".

[16] I have changed the logical notation to fit the available print. Parsons's "<u>a</u>/" is his notation for the result of concatenating a single additional stroke to a stroke string <u>a</u>.

of <u>positive integers</u>.

> We can say that the number 1 exists because we can construct an inscription equivalent to "/". We can say that every number has a successor, because given an inscription of the form /.../ we can add another / to it. It is clear, moreover, that if two such inscriptions are of the same type, then so are their successors, so we have $x=y \rightarrow Sx=Sy$.
>
> If we have two inscriptions a and b such that $a/$ and $b/$ are of the same type, it is clear that a and b are also. Hence we have $Sx=Sy \rightarrow x=y$.
>
> This covers all the Dedekind-Peano axioms except induction (Parsons [2] p.46).

It will be useful to go over this passage in some detail. According to Parsons, our knowledge that the initial stroke string exists is intuitive because we can intuit the initial string /. Indeed, by conceiving of the "/" which ends the previous sentence as a token of that stroke string type, the reader has just done so. Thus, Parsons concludes, our knowledge that PA1' is true is directly intuitive. Furthermore, because Parsons thinks that L* represents the pure number series, he concludes that PA1 is true and that our knowledge that it is true is therefore indirectly intuitive. Note, however, that something extra is needed in order to see that PA1' (and therefore (PA1)) is true; namely, that the "/" is intuited not just as a stroke string, but also as the initial element in an ω-sequence.

Parsons does not say why he thinks PA2' is true or why he thinks our knowledge that it is true is intuitive. One reason he may think PA2' is true is that concatenation (successor) can only extend the length of a stroke string. Therefore, one stroke string can succeed another stroke string only if it consists of at least two strokes. It is unclear, however, that this conclusion is intuitive since, as I have observed, our knowledge that one stroke string succeeds another depends in part on our understanding of successor, and Parsons thinks that our understanding of successor is not intuitive. I conclude, therefore, that Parsons owes an explicit account of why he thinks our knowledge that PA2' is true is intuitive.

Parsons thinks our knowledge that every stroke string has a successor is intuitive because he thinks that if we are given an inscription of any stroke string, we see that we can add another stroke to it. Of course, we must also see that the extended string succeeds the original string. Therefore, when Parsons claims our knowledge that one stroke string succeeds another is intuitive, I take it that what he means is that we can intuit those objects which we otherwise know stand in the successor relation to one another.

It is easy to see why Parsons thinks that if we know we can add a new stroke to any given stroke string inscription we may conclude that every stroke string type has a successor. Two stroke string inscriptions a, b are inscriptions of the same type iff there is a 1-1 correspondence between the strokes which compose a and those which compose b.[17] Therefore, if we extend a stroke string inscription of length n to one of length n+1, the inscription changes from being a token of the stroke string type of length n to being a token of the stroke string type of length n+1. Therefore, if any stroke string inscription can be extended, it follows that every stroke string type has a successor. Therefore, if every stroke string inscription can be extended, it follows that PA3' (and thus PA3) is true. It is, of

[17] Parsons [2] p.45. More generally, two inscriptions are of the same type just in case they spell the same.

course, another matter whether we can always add a new stroke to any given stroke string inscription and, if we can, whether our knowledge that we can is intuitive. Parsons thinks we can always add a new stroke and that our knowledge that we can is intuitive. It is these claims which I find most controversial and to which I return in section 3.

Finally, Parsons claims that if we are given two stroke string inscriptions a, b which we know are tokens of the same type, we may conclude that their successors a/, b/ are also tokens of the same type. Similarly, Parsons thinks that if two inscriptions a/, b/ are tokens of the same type, we see that removing one stroke from each inscription leaves us with two inscriptions a, b which are also of the same type. It is, however, unclear to me why Parsons thinks our knowledge that 1 added to or taken away from equals yields equals is intuitive. To claim it is intuitive makes it appear to be some form of empirical knowledge, yet Parsons is explicit that his theory is not empiricist.[18] I conclude, therefore, that Parsons owes a more fully developed account of why he thinks our knowledge that no two stroke strings have the same predecessors or successors is intuitive.

As Parsons concludes, this covers all the Dedekind-Peano axioms except the induction axiom. Parsons thinks our knowledge that PA5' is true is not intuitive. He writes:

> Induction as a general principle has an essentially higher-order character, and for that reason it seems evident that it cannot be intuitively known (Parsons [7] p.227).

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[18] For a brief but critical appraisal of empiricism see Parsons [3] pp.151-152.

If I understand him correctly, Parsons thinks our knowledge that PA5' is true cannot be intuitive because he thinks the values of the second-order quantifier (whatever they turn out to be) are not intuitable.[19] Therefore, to conclude, when Parsons claims our knowledge that L* forms a model for arithmetic is intuitive, I take him to mean that our knowledge that L* forms a model for arithmetic involves our intuition of those stroke strings which make PA1'–4' true. As yet, however, I think it is unclear whether we should accept this conclusion for any of the axioms except, possibly, PA1'.

3. Why our knowledge that arithmetic has a model is not intuitive.

(3i). In this section I argue that our knowledge that L* forms a model for the Dedekind-Peano axioms is not intuitive. In particular, I argue that our knowledge that every stroke string has a successor cannot be intuitive.

Recall that Parsons thinks we know every stroke string has a successor

> [...] because given an inscription of the form /.../ we [see that we] can add another / to it (Parsons [2] p.46).

What I will consider is whether Parsons's reason for thinking every stroke string has a successor is a good one, and to what degree, if any, it involves intuition.

As I understand it, Parsons's argument for thinking any stroke string can be extended is a straightforward case of reasoning by universal generalization (UG).[20] Recall that UG arguments proceed in the following

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[19] See also Parsons [3] p.46, [4] p.164.

[20] See Parsons [3] section v. One might suppose we could show that any

way. In order to establish the truth of a general statement of the form "∀xEx" from a set of assumptions Q, we first derive "Ea" from Q. Then, if Q doesn't involve a, the inference from "Ea" to "∀xEx" is logically permissible, since what could be established about a on the basis of Q could be established about any object in the relevant domain. Parsons's UG argument goes as follows. We first show that some exemplar stroke string inscription can be extended. We then show that there is nothing particular to this inscription which figures in our seeing that it can be extended. We

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stroke string inscription can be extended using mathematical induction. Parsons, however, warns us that such a proof would be circular (Parsons [3] pp.157-158). There is, I think, another problem with using mathematical induction here. In order to show that any stroke string inscription can be extended using mathematical induction, two things must be demonstrated:

> (i) An initial stroke string inscription of the form "/" can be extended by concatenating one additional stroke to it.
> (ii) If a stroke string inscription of length n can be extended by concatenating one additional stroke to it, then the extended stroke string of length n+1 can be extended by concatenating one additional stroke to it.

Demonstrating (i) is trivial: Construct an inscription of a single stroke and show that it can be extended by adding another stroke to it. Thus // establishes (i). The difficulty comes with (ii). A demonstration of (ii) proceeds by showing that if the antecedent condition obtains, the consequent condition follows. But the antecedent condition for (ii) obtains just in case there is an infinity of stroke string inscriptions. Therefore, a demonstration of (ii) involves the question-begging assumption that there is an infinity of stroke string inscriptions. (I include this argument because I will argue that Parsons's UG strategy suffers from the same sort of difficulty.)

Finally, it might also be thought that the fact we can physically extend inscriptions of strings of 1-n strokes (for some finite n) supports the enumerative inductive conclusion that any stroke string inscription can be extended. But what underlies this suggestion is a form of Millian empiricism and, as we have seen, Parsons's theory is not empiricist.

generalize, concluding that every stroke string inscription can be extended and, therefore, that every stroke string type has a successor.

Parsons's UG argument depends on our knowing that there is nothing about the exemplar stroke string inscription which figures in how we know that inscription can be extended. All that individuates stroke strings is their number of strokes. Therefore, Parsons's UG argument depends on our knowing that the number of strokes which make up the exemplar inscription does not figure in how we know that inscription can be extended. Indeed, the two ways Parsons suggests we might think of the exemplar inscription assumes that the number of strokes which make up the inscription is irrelevant to our seeing that it can be extended. He writes:

> There seems to be a choice between imagining [a stroke string inscription] _vaguely_, that is imagining a string of strokes without imagining its internal structure clearly enough so that one is imagining a string of _n_ strokes for some particular _n_, or taking as paradigm a string (which now might be perceived rather than imagined) of a particular number of strokes, in which case one must be able to see the irrelevance of this internal structure, so that in fact it plays the same role as the vague imagining (Parsons [3] pp.156-57).

I will show that neither way of thinking of the exemplar inscription gets Parsons the results he requires.

Parsons's first suggestion is this. To imagine a stroke string inscription "vaguely" is to imagine an inscription consisting of no definite number of strokes. Since the inscription consists of no definite number of strokes, and since a stroke string is individuated solely by the number of its strokes, the imagined inscription could be an inscription of any stroke string. Therefore, by showing that this imagined inscription can be extended, we may conclude that any stroke string inscription can be extended and, therefore, that every stroke string type has a successor.

I find two problems with Parsons's suggestion. First, we cannot know that the result of adding one stroke to a vaguely imagined stroke string inscription is a stroke string inscription one stroke longer than the string was before the new stroke was added. It may be the case, for example, that in adding the new stroke some other stroke dropped out. The only way we can be sure that something like this did not occur is by knowing that the initial array consisted of a string of $\underline{n}$ strokes (for some particular $\underline{n}$), and that the result of adding the new stroke is a string of $\underline{n+1}$ strokes. But this is exactly what we cannot know about a vaguely imagined stroke string inscription. Therefore, we cannot conclude that by adding a new stroke to a vaguely imagined stroke string inscription that the inscription has been extended.

The second problem is this. Elsewhere Parsons writes:

> I shall assume that we do not have <u>objects</u> unless we can meaningfully apply the identity predicate. I hardly know how to begin arguing for this. [...] It is characteristic of objects that they can be represented in different ways, from different perspectives. But this statement hardly makes sense unless it means that the <u>same</u> object is thus represented (Parsons [4] p.497).

For example, if $\underline{a}$ and $\underline{b}$ are both inscriptions of stroke string types, I take it that Parsons thinks we must be able to tell, at least in principle, whether they are inscriptions of the same stroke string type. Yet again, two stroke string inscriptions $\underline{a}$ and $\underline{b}$ are inscriptions of the same stroke string type iff there is a 1-1 correspondence between the strokes which make up _a_ and those which make up _b_. But if _a_ and _b_ consist of no definite number of strokes, it is uncertain what a 1-1 correspondence between the strokes which make up _a_ and those which make up _b_ could come to. Therefore, following Parsons's own criterion for stroke string identity, I conclude that vaguely imagined stroke string inscriptions cannot be inscriptions of stroke string types, and so whatever knowledge we acquire through these vague imaginings cannot be intuitive.

In response to this second objection, it may be argued that a vaguely imagined stroke string inscription is a token of the general stroke string type whose tokens are stroke string inscriptions of any length. The problem I find with this response, however, is it involves the assumption that this general stroke string type is a stroke string of any length. (If this was not assumed, we could not carry out the generalization step after showing that some token of this general type can be extended.) But if we assume that the general stroke string type is a stroke string of any length, then showing that some token of it can be extended tells us nothing we have not already assumed. If, for example, we suppose that a vaguely imagined stroke string inscription is an inscription of _n_ strokes, showing that it can be extended to a string of _n+1_ strokes shows us nothing new since it is part of our assumption that the original inscription could be a stroke string of length _n+1_. Therefore, as I see it, the assumption that there is this general stroke string type begs the question of how we know any stroke string inscription can be extended.

Parsons's second suggestion as to how we might see that the number of strokes which make up an exemplar stroke string inscription is irrelevant to the question of whether that inscription can be extended is this: Consider a stroke string <u>a</u> of definite finite length <u>n</u>. Any inscription of <u>a</u>, whether perceived or imagined, will occur against a spatial ground. Because there is always space in this ground for an additional stroke, the number of strokes which compose the inscription is irrelevant to the question of whether that inscription can be extended. Therefore, since the number of strokes which compose <u>a</u> is irrelevant to the question of whether an inscription of <u>a</u> can be extended, <u>a</u> could represent any stroke string. Therefore, by showing that an inscription of <u>a</u> can be extended, it follows that every stroke string has a successor.

The problem I find with this second line of argument is with the claim that there is always space in the ground against which an inscription of <u>a</u> occurs for an additional stroke. One way to see the problem with this claim is to consider the kind of space under consideration. Because it is the space in which perceived or imagined inscriptions occur, it can only be physical space or imaginable space. (Roughly speaking, I think of imaginable space as the space in one's "mind's-eye" against which imagined inscriptions occur.) Yet, as I will argue, to think of this space as either physical or imaginable space begs the question of how we know any stroke string inscription can be extended.

Suppose we think of the space against which a stroke string inscription occurs as actual physical space. Since it is assumed that there is always room in this space for an additional stroke, it must also be assumed, or otherwise known, that actual physical space is either infinitely divisible or infinitely extendible. But if we assume that actual physical space is infinite in either direction, we have then begged the question of how we know any stroke string inscription can be extended.[21] And if we have independent grounds for believing that actual physical space is infinite in either direction, then those grounds would show us that actual physical space can form a model for arithmetic, and Parsons's appeal to intuition would be superfluous.

The situation is the same if we think of this space as actual imaginable space. Here our knowing that any stroke string inscription can be extended depends on our knowing that our actual imaginative capacities are infinite. But if we assume that our actual imaginative capacities are infinite, we have begged the question of how we know any stroke string inscription can be extended. Worse, to assume that our actual imaginative capacities are infinite is to assume something which is surely false. We cannot, for example, imagine a string of 10^{10} strokes. (At the very least, we cannot imagine a string of 10^{10} strokes as a string of 10^{10} strokes.[22])

In addition to thinking we can see that any stroke string inscription can be extended by considering a spatial array of strokes, Parsons also thinks we can see that any stroke string inscription can be extended by

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21 And, of course, our assumption may simply be wrong; there may be only a finite number of discrete spatial regions.

22 As I see it, the claim that one can intuit a string of 10^{10} strokes can only be understood metaphorically. It is comparable, perhaps, to the claim that one can "see" some highly theoretical entity such as a microphysical particle when the only reason we have for believing that the entity exists is that it is required for the truth of some otherwise acceptable physical theory.

considering a temporally constructed sequence of strokes. He writes:

> Alternatively, we can think of the string as constructed step by step, so that the essential element is now succession in <u>time</u>, and what is then evident is that at any stage one can take another step (Parsons [3] p.156).

This suggestion fails, however, for the same reason it fails for a spatial array of strokes. If we take a stroke constructed at some moment as representing the series' initial element, and the construction of an additional stroke with the passing of each moment as representing successor, then, at first glance, the resulting sequence certainly appears to form a model for arithmetic. But the only way it is "evident", as Parsons claims, that every moment has a successor is to assume that time has no end, and this assumption begs the question of how we know any stroke string inscription can be temporally extended. I conclude, therefore, that Parsons's second line of argument fails if we take the space referred to in that argument as actual physical space or as actual imaginable space, or if we reinterpret the argument so as to be about time.

There is, of course, another way to think of the space in Parsons's argument, and that is to think of it as possible physical space or as possible imaginable space. Parsons's argument might then go as follows: Consider a stroke string inscription <u>a</u> of definite length <u>n</u>. Any inscription of <u>a</u>, whether perceived or imagined, will occur against a spatial ground. Because it is always possible that there is space for an additional stroke in this ground, the number of strokes which make up the inscription is irrelevant to the question of whether that inscription can be extended. Now, the obvious question to ask here is: How do we know it is always possible that there is space for an additional stroke? One response to this question is that it may be the case that actual physical space consists of only a finite number of discrete spatial regions. Yet if we understand what it is for the world to consist of n spatial regions (for some finite n), surely we could work out what would have to be the case for it to consist of n+1 spatial regions. In this way we should be able to work out what conditions would have to obtain in order for any stroke string inscription to be extended. Similarly, it may be the case that our actual imaginative capacities are finite. But if we understand what is involved in our imagining inscriptions of length n, then surely we could work out what would have to be the case for us to imagine inscriptions of length n+1 and, therefore, what conditions would have to obtain for any imagined inscription to be extended.

The problem with the foregoing argument is that it too begs the question of how we know any stroke string inscription can be extended. One way to see how it does this is to think of the possible situations in which stroke string inscriptions can be extended in terms of accessibility to possible worlds. Suppose, for example, that we have worked out what conditions would have to obtain for a stroke string inscription of any given finite length to be extended. We could then argue that for any world in which a stroke string inscription of length n occurs there is an accessible possible world in which a stroke string inscription of length n+1 occurs. We may then derive the infinity of the number series in the following way. Let

1. $\Box\forall x(Sx \rightarrow \Diamond\exists z(Sz \& Ezx))$

formalize the claim that necessarily: for every stroke string inscription x, there is a possible stroke string inscription z which extends x. Each of the possible worlds in which these inscriptions occur must be accessible from this world. (After all, our concern is with how we know there is an infinity of stroke string types in this world.) Therefore, we need the transitivity of accessibility rule R: "if possibly possibly p then possibly p" familiar from the modal system S4. Finally, we observe that a stroke string inscription of length 1, call it "S1", occurs in this world. We may then derive an infinite sequence of possible stroke string inscriptions in the following way:

1. $\Box\forall x(Sx \rightarrow \Diamond\exists z(Sz \& Ezx))$
2. $S(1)$
3. $\Diamond\exists z(Sz \& Ez1)$
4. $\Diamond S(2)$
5. $\Diamond\Diamond\exists z(Sz \& Ez2)$
6. $\Diamond\Diamond S(3)$
7. $\Diamond\Diamond S(3) \rightarrow \Diamond S(3)$ R
8. $\Diamond S3$
9. $\Diamond\Diamond\exists z(Sz \& Ez3)$
10. $\Diamond\Diamond S(4)$

... .

Noting that the existence of a type depends only on the possibility of a token of that type,[23] one might thus conclude that there is an infinity of stroke string types.[24]

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23 Parsons [3] p.160.

24 One may wonder why I have given this example in terms of accessibility to an infinite series of possible worlds rather than in terms of accessibility to a single possible world in which there is an infinite number of stroke string inscriptions. As I see it, nothing important depends on setting the example one way rather than another.

The problem with the foregoing argument is the obvious one. It involves the question-begging assumption (given as (1)) that any stroke string inscription can be extended. Furthermore, intuition plays no role in it other than the trivial one of demonstrating that a stroke string of length 1 occurs in this world. I hardly think, therefore, that such an argument can be called intuitive. I conclude, therefore, that Parsons's argument fails if we take the space referred to in that argument as possible physical space or as possible imaginable space.

Finally, it may be thought that we know any stroke string inscription can be extended because we cannot perceive or imagine an inscription without a surrounding ground, and so cannot conceive of not being able to extend that inscription to its surrounding ground. It is unclear, however, whether this simple fact about our conceptual abilities is sufficient to admit the conclusion that any inscription can be extended. But even if it is sufficient, I do not see how this knowledge could be intuitive. As we have seen, Parsons thinks intuition is only of objects, and there is no stroke string the intuition of which yields the conclusion that any stroke string inscription can be extended in a non-question begging way.

To conclude this section: The intuition of a type always involves the perception or imagining of a token of that type, both of which are physical processes. Therefore, if our knowledge that every stroke string has a successor is intuitive, I do not see how the modality in Parsons's claim that any stroke string inscription can be extended could be other than some sort of physical possibility. Yet, as I have argued, interpreting Parsons's claim in terms of physical possibility fails. I do not see, therefore, what the grounds could be for thinking that any stroke string inscription can be extended could be intuitive. I now turn to the question of how Parsons does understand the modality in his claim that any stroke string inscription can be extended. I argue that even if we understand this modality in the way Parsons suggests, our knowledge that any stroke string inscription can be extended cannot be intuitive.

(3ii). Parsons understands the modality in his claim that any stroke string inscription can be extended as mathematical possibility.[25] He explains his conception of mathematical possibility in the following extended passage.

> A tempting way of understanding the possibility in principle with which we are concerned is as a capacity of the <u>mind</u>. The same limitations of actual human capacity, therefore, force us to interpret such a capability as possessed by the mind in abstraction from its embodiment in the human organism. [...] Even a materialistic cognitive psychology might take this line, if the mind is construed as something like a Turing machine, and on the 'functional' level the capabilities of the mind are those of the abstract machine rather than those of its actual physical embodiment. The last version, however, makes explicit a difficulty that was already present in the more traditional ones: the potential infinity that we attribute to the mind's capabilities, by virtue of the indefinite iterability of certain operations, is really being conceived by means of mathematics, rather than its being the case that some independent insight into the mind's capabilities is telling us what is possible by way of mathematical intuition, construction, computation, or proof. The Turing-machine model makes explicit use of a mathematical model involving the concept of a computable function in order to say what the mind can do. There is thus a kind of circularity.
>
> The first lesson to be drawn from this state of affairs is

25 Parsons [3] p.158. See also Parsons [1] p.140, [2] pp.47-49.

> that the notion of possibility in terms of which it is true, for example, that for a particular computable function a value can be computed for any argument is an essentially mathematical one (Parsons [7] pp.223-24).

Following Parsons, let us say it is mathematically possible to carry out an arithmetical procedure just in case a suitable Turing machine could carry out that procedure. For example, suppose we have a Turing machine M for "constructing" L*.[26] We are then to understand the claim that every stroke string has a successor as the claim that given a string of any length n, M can construct a string of length n+1. Now to the important question: Is our knowledge of what M can construct intuitive?

To see why Parsons's response to this last question is, or should be, no, consider the distinction he draws in the following passage between what he calls "purely mathematical uses" of mathematical possibility and those uses which he thinks contain an intuitive component.

> [...] it is important to distinguish purely mathematical uses of this notion [of mathematical possibility] from those in which it is combined with epistemic or other non-mathematical concepts. The distinction is illustrated by an ambiguity in the above statement about a computable function. We may suppose the function to be given to us by a certain Turing machine programme. To say that a computation can be constructed may mean little more than that there can be a computation from this programme, where a computation is itself a mathematical object. The word "constructed" may be taken to be metaphorical, signaling the fact that there is an order of priority among computations, since longer ones involve or contain shorter ones. It is another matter to say that a value can be computed if this is to mean that some

[26] The claim that a Turing machine can "construct" a stroke string is purely metaphorical. Turing machines (as opposed to their various physical realizations) cannot construct anything. Parsons makes the same point in the passage I quote below.

mathematician can arrive at insight as to what this value is. In this case, what is said to be possible is something epistemic or psychological, someone's knowing what the value is, perhaps by intuiting the constructed computation and extracting from it the intuition of the value (Parsons [7] pp.224-25).

Here Parsons claims that our understanding of what M can construct is based on our understanding of what he calls the "purely mathematical use" of mathematical possibility. But as we learn in reading the above passage, Parsons thinks this purely mathematical use of mathematical possibility does not involve intuition. It seems to me, therefore, that Parsons is committed to the view that our understanding of what M can construct is not intuitive. But Parsons thinks our knowledge that any stroke string inscription can be extended is based on our understanding of what M can construct. Therefore, I do not see how he can fail to conclude that our knowledge that it is mathematically possible to extend any stroke string inscription is not intuitive. Furthermore, by interpreting the modality in question as mathematical possibility, it seems to me that Parsons has begged the question of how we know any stroke string inscription can be extended. If we assume that there is a Turing machine M which can construct a stroke string of any finite length, then we have, in effect, assumed that arithmetic has a model, one consisting of an ω-sequence of stroke strings which can be constructed, as it were, by M.[27]

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[27] It therefore seems to me that the circularity to which Parsons alludes in the passage with which I began (3ii) is more serious than he thinks. Parsons writes:

> The question of circularity raised above does seem to me a serious matter. But it does not, it should be clear, tell directly against the intelligibility of notions of 'in principle' possibility of the kind we have been considering, unless the

(3iii). One way to review the problems I find in Parsons's arguments is to look at his summary of how he thinks we know any stroke string can be extended. He writes:

> I think the matter is thus: we have a structure of perception, a 'form of intuition' if you will, which has the essential feature of Brouwer's two-one-ness, that however the idea of 'adding one more' is interpreted, we still have an instance of the same structure. But to see the possibility of adding one more, it is only the general structure that we use, and not the specific fact that what we have before us was obtained by iterated additions of one more. This is shown by the fact that in the same sense in which a new stroke string can be added to any string of strokes, it can be added to any bounded geometric configuration (Parsons [3] p.158).

How are we to unpack this? According to Parsons, we have a structure of perception, a "form of intuition" if you will, which has the essential feature that however "adding one more" is interpreted, we still have an instance of the same structure.[28] I take it that one example of a

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> theory of the mind that is being used is supposed to be part of a 'first philosophy', prior to mathematics and science (Parsons [7] p.225).

Insofar as I understand it, I find this response unsatisfying. Parsons claims the circularity in his account would be objectionable, and his conception of mathematical possibility unintelligible, only if we first tried to form a theory of the mind's capabilities independently of, or prior to, mathematics. I doubt that we could formulate a useful theory of the mind which did not somehow presuppose mathematics. But I do not think the circularity threatens the intelligibility of Parsons's conception of mathematical possibility in the way he thinks. Rather, the circularity shows that in trying to explain how we know every stroke string has a successor, an appeal to intuition has no explanatory power.

[28] I set aside the reference to Brouwer. Although I briefly discuss Brouwer's conception of two-one-ness in my chapter on traditional intuitionism, I find it to be an opaque and unhelpful notion.

structured perception is our seeing the underlined array of ink Theaetetus as a word token. (I also take it that what Parsons thinks structures the reader's perception in this way is the reader's possession of the relevant concept(s).[29]) Earlier, I suggested that Parsons thinks pure objects can be construed as forms which quasi concrete objects must take in order to represent those pure objects. Following this suggestion, I read Parsons as claiming that our concept of, say, the pure number series yields a form of intuition which structures our perceptions in such a way that we are able to perceive or intuit objects as representations of the pure numbers. Thus, when Parsons claims that this form of intuition has the essential feature that however we interpret "adding one more", we still have an instance of the same structure, I take him to mean that, for example, we possess those concepts which enables us to see /// and //// as instances of the same structure.

Parsons continues: "to see the possibility of adding one more, it is only the general structure that we use". Parsons writes in reference to this passage that: "We can call the possibility in question mathematical possibility".[30] Therefore, if our understanding of mathematical possibility does come from our having the form of intuition of the pure number series, it seems to me that we know any stroke string has a successor only because we know prior to any intuition that the number series is infinite.[31] To continue with the present example, the general structure of

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29 See Parsons [3] pp.154,162.

30 Parsons [3] p.158.

31 It may be that like Kant, Parsons thinks we can acquire insight into our

a stroke string is that of a stroke string of any length. Following Parsons, we may represent this stroke string as "/.../". I therefore read Parsons as claiming that to see the mathematical possibility of adding an additional stroke to any stroke string inscription, it is only the general structure /.../ that we use. But, no surprise, this is the claim with which we began our investigation at the beginning of section 3. (Recall that Parsons writes: "We can say that every number has a successor because given an inscription of the form /.../ we can add another / to it".) Yet if the arguments I presented in sections (3i) and (3ii) are correct, Parsons argument that we know any inscription can be extended involves either question-begging assumptions about how we know any inscription can be extended, or else does not involve intuition.

To sum up: Parsons cannot show that the number of strokes which make up the exemplar inscription is irrelevant to the question of whether that inscription can be extended without making some question-begging assumption, or without making the argument non-intuitive, or both. I conclude, therefore, that his UG argument for showing that every stroke string has a successor fails. Finally, nothing in the arguments I use to reach this conclusion is unique to stroke strings; the situation would be the same for any other series of quasi concrete objects. I conclude, therefore, that our knowledge that there is a model for arithmetic is not

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forms of intuition by considering the content of our empirical intuitions. (See, e.g., Kant [1] A20/B34ff.) It may be the case that we learn various truths about the numbers by considering representations of them. But if the arguments I present here are correct, our knowledge that there is an infinite number of numbers cannot be intuitive. I suspect a similar problem faces Kant's view.

intuitive. Intuition does not provide a route to the numbers.

Appendix A: Resnik's Structuralism.

Recently, Michael Resnik has offered an account of the natural numbers which in certain respects is quite similar to Parsons's and which I think runs into some of the same sorts of difficulties. Resnik writes:

> In mathematics, I claim, we do not have objects with an "internal" composition arranged in structures, we have only structures. The objects of mathematics, that is, the entities which our mathematical constants and quantifiers denote, are structureless points or positions in structures (Resnik [1] p.530).

For example, Resnik thinks "4" refers to the fourth position in the structure of the positive whole numbers. He thinks we know about these structures, or "patterns", as he sometimes calls them, through our perception or imagining, or, I suppose, intuition, of objects which form structures isomorphic to (perhaps only some segment of) the wholly abstract structure which they instantiate. So, for example, Resnik claims that 10 puppies in a litter instantiates the first 10 positions in the structure of the positive whole numbers.[32]

The obvious question to ask here is similar to that asked of Parsons: How does Resnik think we know that the structure of the number series is infinite? According to Resnik:

> Infinite patterns are first thought of, I would suggest, by thinking of finite patterns as indefinitely extended (Resnik

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[32] Resnik [1] pp.532-33. Note the similarity between Resnik's wholly abstract structureless points and Parsons's pure numbers. Note also that they both believe that we know about these wholly abstract objects through our contact with their instantiations.

[1] p.531).

As an explanation of how we know a finite series of objects can always be extended, Resnik suggests an analogy with how we think of music. He writes:

> [...] knowing that there is no greatest natural number may be like knowing that given a song in which a measure is repeated, say, twice, there is (or could be) another in which it is repeated three times (Resnik [1] p.531).

Here I think Resnik runs into trouble. How are we to understand the claim that any musical phrase <u>can</u> be indefinitely iterated? Resnik agrees that the modality in this claim cannot be any kind of physical possibility. He writes:

> [...] the epistemology of mathematics is no more (or not much more) mysterious than the epistemologies of linguistics and music. Like mathematics, they begin with experience, abstract from it and arrive at the unexperienced (and, perhaps, like mathematics, even the unexperientable) (Resnik [1] p.531).

When Resnik writes that this reasoning takes us into the "unexperientable", I take it he is committed to the view that this reasoning takes us beyond what is physically possible, even in principle. But at this point the analogy with music breaks down. A musical phrase considered independently of how it might be acoustically realized is simply a set of logical or mathematical relations. I do not see, therefore, how thinking of the iteration of a musical phrase is supposed to aid in our thinking of how the number series can be indefinitely extended, since to think of music in this way is to think mathematically. Therefore, I do not see how we could interpret the modality in Resnik's claim that a musical phrase can be indefinitely iterated as other than mathematical possibility. But, as we have seen, we must know what is mathematically possible in order to conclude that every number has a successor or, in this case, that any musical phrase can be indefinitely iterated. I conclude that our knowledge that the structure of the natural number series is infinite cannot be based on our perception or imagining (or intuition) of objects instantiating some finite segment of that structure in the way Resnik thinks. There is nothing in experience, after all, which tells us we can always go on. I conclude that Resnik's account of mathematical epistemology fares no better than Parsons's.

Appendix B: Berkeley and intuition.

As I have noted, Parsons's argument for thinking any stroke string inscription can be extended is an instance of reasoning by universal generalization. As is well-known, the UG method of proof is common in mathematics, and especially so in geometry. Moreover, an appeal to something like intuition can be found in several philosophers' accounts of how we establish the truth of a mathematical statement using UG. In this section I examine how something very much like intuition features in Berkeley's account of geometric knowledge.

Consider Berkeley's strategy for showing that the sum of the interior angles of any triangle equals the sum of two right angles. Berkeley writes:

> [...] though the idea I have in view whilst I make the demonstration be, for instance that of an isosceles rectilinear triangle whose sides are of a determinate length, I may nevertheless be certain it extends to all other rectilinear triangles, of what sort or bigness soever. And that because neither the right angle, nor the equality, nor determinate length of the sides are at all concerned in the demonstration. It is true the diagram I have in view includes all these particulars; but then there is not the least mention made of them in the proof of the proposition (Berkeley [1] p.54).

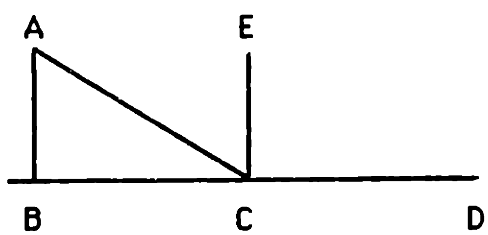
As Berkeley observes, one way to show that every triangle has the aforementioned property would be to examine a "demonstration" (i.e., a physically-realized instance) of every particular triangle. But since we cannot examine an infinite number of triangles, Berkeley suggests we prove that all triangles have the property that the sum of their interior angles equals two right angles by first proving that a particular triangle

ABC has that property. Then, by showing that ABC's individuating properties - the length of its sides and magnitude of its angles - play no essential role in the proof, we may conclude that ABC can represent any triangle. It follows by UG that all triangles have the property that the sum of their interior angles equals the sum of two right angles.

The proof that the sum of the interior angles of a Euclidian triangle equals two right angles is given by Euclid in the following passage:

> Let ABC be a triangle, and let one side of it BC be produced to D; I say that the exterior angle ACD is equal to the two interior and opposite angles CAB, ABC, and the three interior angles of the triangle are equal to two right angles. For let CE be drawn through the point C parallel to the straight line AB. [I.31] Then, since AB is parallel to CE, and AC has fallen upon them, the alternate angles BAC, ACE are equal to one another. [I.29] Again, since AB is parallel to CE, and the straight line BD has fallen upon them, the exterior angle ECD is equal to the interior and opposite angle ABC. But the angle ACE was also proved equal to the angle BAC; therefore the whole angle ACD is equal to the two interior and opposite angles BAC, ABC. Let the angle ACB be added to each; therefore the angles ACD, ACB are equal to the three angles ABC, BCA, CAB. But the angles ABC, BCA, CAB are also equal to two right angles. Therefore, etc. Q.E.D. (Euclid [1] I.32).

By way of illustration, the following inscription may serve as a verifying instance of Euclid's proof:



Following Parsons, it seems best to think of the above inscription of ABC as a token of a triangle-type. Thus, in perceiving the above inscription in this way, it may be claimed that we intuit triangle-type ABC. (Nothing hangs on the terminological issue of what we call this phenomena.) So again, the relavant UG argument goes as follows. We see that Euclid's proof involves no essential reference to the length of triangle ABC's sides or to the magnitude of its angles. We conclude, therefore, that the above inscription of triangle ABC could represent any stroke string. We generalize, concluding that all triangles have the property that the sum of their interior angles equals two right angles. It might thus be claimed that our knowledge that every triangle has the property that the sum of its interior angles equals the sum of two right angles is intuitive because it involves our intuition of triangle ABC in the way just outlined.

As I see it, the problem with this argument is a simple one. An examination of Euclid's proof shows that the intuition of triangle ABC plays no essential role in that proof. The drawing of triangle ABC and the intuition of that triangle type may play a useful heuristic role in our coming to believe that the proof holds, but that is all it does. I conclude, therefore, that our knowledge that proposition I.32 holds for all triangles is not intuitive. If Euclidian geometry is truly axiomatic, then our knowledge that proposition I.32 holds for all triangles follows from the definitions, axioms and postulates of Euclid's system, and does not involve the perception, imagining, or intuition of any triangle.

3. TRADITIONAL INTUITIONISM

_1i_. In this chapter I consider whether traditional intuitionism might offer a solution to Benacerraf's dilemma.[1] Recently, Scott Weinstein has sketched one way such a solution might go:

> [...] the intuitionists identify the truth of a mathematical statement, _A_, with our possession of a construction, _c_, which is a proof of the statement _A_. This latter statement, that the construction _c_ is a proof of _A_, involves no logical operations and is moreover the application [of] a decidable property to a given mathematical construction. Hence, this statement does not itself require a non-standard semantical interpretation and, it is hoped, can be understood along the lines of statements like "The liberty bell is made out of brass", or perhaps, "The sensation in my right toe is a pain." The idea is just that the intended intuitionistic interpretation of a mathematical language reduces the truth of any sentence of that language to the truth of an atomic sentence which is the application of a decidable predicate to a term and this latter sentence can be understood as having an ordinary referential interpretation. In addition, the intuitionist view makes it clear how we come to know the truth of statements of mathematics. We come to know a statement by constructing a proof of it and we know that a construction is a proof of a statement since the property of being such a proof is decidable (Weinstein [1] pp.268-69).

I divide this chapter into two sections and an appendix. In the first section I develop Weinstein's sketch, showing why the intuitionists identify the

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[1] I consider Brouwer, Heyting and Troelstra to be representative intuitionists. I have added the qualification "traditional" because I do not wish to consider intuitionists such as Dummett's antirealist at this time. As we shall see in chapter 4, the philosophical foundations of antirealism differ significantly from the philosophical foundations of traditional intuitionism.

truth of a mathematical statement with our ability to undergo a mental mathematical process. In section 2 I show that this account of mathematical truth is circular, and I argue that the only apparent way of avoiding this circularity commits the intuitionists to a version of mathematical finitism. I conclude with a brief appendix on Brouwer's conception of the natural numbers.

iii. In brief, the solution Weinstein outlines is this: Any mathematical statement A is intuitionistically true iff a corresponding statement of the form "Construction c is a proof of A" is true. The statement "Construction c is a proof of A" is to be understood in the standard referential way; that is, it is true iff the mathematical construction c is a proof of A. We know about these constructions because, as we shall see, they are objects of our own experience. We know whether a construction c proves A because it is assumed that "is a proof of" is, in principle, a decidable relation.

There is a difficulty with Weinstein's formulation of when "Construction c is a proof of statement A" is intuitionistically true which it will be useful to address before proceeding. Weinstein writes that "Construction c is a proof of A" is intuitionistically true just in case we possess a construction c which proves A. Yet this requirement that A is true just in case we possess a proof of A surely is too strong. If A is true just in case we possess a proof of A, then unproven mathematical statements are not true.[2] Suppose, for example, that no one has ever

[2] Which is not to say that the intuitionist thinks that the statement is false. Recall that, according to the intuitionists, a statement A is false just in case we have a proof that A is not provable.

proven that 31+6=37. (Since mankind has carried out only a finite number of proofs, many such examples exist, although they no doubt involve much larger numbers.) If "31+6=37" is true just in case we possess a proof that 31+6=37, it follows that "31+6=37" is not true. Moreover, suppose someone proves that 31+6=37, but then loses their proof or forgets it. If "31+6=37" is true just in case we possess a proof, we can only conclude that "31+6=37" was once true, but no longer is.

The claim that a mathematical statement *A* is true just in case we possess a proof of *A* commits the intuitionist not only to the view that *A* is true only during that time when we possess a proof of *A*, but also to the view that *A* is true only for the possessor of that proof. Suppose a Robinson Crusoe proves that 31+6=37. If "31+6=37" is true just in case someone proves that 31+6=37, then, unbeknownst to the rest of us, "31+6=37" becomes true. If Crusoe dies before he has an opportunity to communicate his proof to anyone else, "31+6=37" becomes not true again, all without our knowledge. Therefore, if we identify the truth of a mathematical statement with possession of a proof of that statement, what it seems we should say is that "31+6=37" is true for Crusoe but not for anyone else. Yet it now appears that someone who holds that "31+6=37" is true just in case we possess a proof that 31+6=37 should hold that "31+6=37" is true for an individual X just in case X possesses a proof that 31+6=37.

I assume that even the intuitionists should find these relativistic consequences unacceptable. I take it, therefore, that what the intuitionists should want to say is: A mathematical statement *A* is true iff a corresponding statement of the form "Construction c is a proof of A" is true, and "Construction c is a proof of A" is true iff we can carry out an arithmetical procedure which results in our possessing a construction c which proves A.[3] So, for example, since we have a proof procedure for addition, the intuitionist may conclude that "31+6=37" is true independently of whether anyone actually proves that 31+6=37.

1iii. I now turn to the issue of what kind of objects the intuitionists think mathematical constructions are. According to the intuitionists, mathematical constructions are those mental processes one goes through in doing mathematics.[4] A mathematical assertion is both a report that one has undergone a mental mathematical process and a description of that mental process, albeit one which is presented at a certain level of abstraction. So, for example, Heyting writes:

> Intuitionistic mathematics consists [...] in mental constructions; a mathematical theorem expresses a purely empirical fact, namely the success of a certain construction. "2+2=3+1" must be read as an abbreviation of the statement "I have effected the mental constructions indicated by "2+2" and by "3+1" and I have found that they lead to the same result" (Heyting [1] p.8).

I will present a detailed account of how the intuitionists think we establish the truth of "2+2=3+1" in the next section. First, however, there

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3 Nevertheless, as we shall see, this way of stating the intuitionists' claim does not resolve all the problems just noted. Also, much depends on how we interpret the modality in this claim that we can carry out a procedure which results in our possessing a proof of A. I discuss this issue in section (1vi).

4 See, e.g., Brouwer [1] pp.509-510, Heyting [1] pp.4,8, Troelstra [1] p.4.

are four points I wish to note about the intuitionists' account of mathematical constructions. The first point is that the intuitionists identify mathematical existence with our ability to effect a construction. Heyting writes:

> In the study of mental mathematical constructions 'to exist' must be synonymous with 'to be constructed'" (Heyting [1] p.2).

So, for example, the intuitionists think the number 4 exists just in case it can be constructed. (Again, I will show how the intuitionists think this is done in the next section.) Second, the intuitionists are thereby committed to the view that mathematical truths are both empirical and contingent. They are empirical in the sense noted by Heyting; they report the success of a mental construction. They are contingent insofar as it is a contingent matter that there are any beings who possess the mental processes required for doing mathematics.[5]

Third, although the intuitionists think our mathematical terms refer ultimately to our mental processes, it had better be the case that they think our mathematical terms refer to the structure, and not the content of these mental processes. As Frege pointed out at the very beginning of the Grundlagen, if our mathematical terms referred to the content of our mental mathematical processes, then, because the content of our individual experiences differ, it would follow that our mathematical expressions would not have common content, and mathematical

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[5] Thus one way the intuitionists' account of mathematical truth remains relativistic is that the existence of mathematical truths depends on the existence of mathematicians.

communication would be impossible.[6] The situation, I think, would be even worse. If our mathematical terms refer to the content of our mental mathematical processes, then what I mean when I mentally add oranges would be different then what I mean when I mentally add apples. In this case mathematics loses its generality; I would need one mathematics for adding apples, another for adding oranges, a third for adding apples and oranges, and so forth.

The fourth point involves a clarification in Heyting's claim about when a mathematical statement is true similar to the clarification I made in section (1ii). If a mathematical statement A is true just in case someone has undergone a mental mathematical process, then unproven mathematical statements are not true, etc. Therefore, it had better be the case that the intuitionists think a mathematical statement A is true iff our mental mathematical processes are such that someone could undergo that mental process c which constitutes a proof of A.[7]

1iv. Time now for some examples. In this section I show how the intuitionists think we construct the natural numbers and how they think we establish the truth of statements such as "2+2=3+1".

The intuitionists think of the natural numbers themselves as constructions. One of the more straightforward synopses of how the

[6] Frege [1] p.I. See also appendix A.

[7] Again, I discuss how we are to understand the modality in this claim in section (1vi).

intuitionists think the numbers are constructed is given by Troelstra:[8]

> Natural numbers are conceived as constructions of a very simple kind, obtained by juxtaposing units. The basis of this concept is the observation that we can conceive a unit, then another unit, look upon this two-ity (pair) as a new entity, and repeat this process as often as we like. In a picture
>
> I II III IIII, ...
>
> These very simple constructions are so to speak their own proof: for the concept of a certain natural number is given by the number itself, because its mode of generation is at the same time "proof" that it has been obtained by this process of generation of natural numbers (Troelstra [1] p.12).

Here we see that the intuitionists think of a number *n* as the mental process one goes through in constructing the numbers from 1 to *n*. So, for example, consider how the intuitionists think I construct the number 3. Following Troelstra, I first conceive of a single unit which I represent as /. I then conceive of a new unit which I juxtapose with the original unit; hence //. I now look at // as a single unit which I represent as (//). I repeat the process of juxtaposing a new unit with the single unit so that I now have (//)/. Disregarding the various groupings, I now have a representation /// of the mental process I went through in constructing the number 3. According to the intuitionists, I now have a proof that the number 3 exists.[9]

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[8] This method of thinking of how the numbers are constructed originates with Brouwer.
See appendix A.

[9] Note the strong metaphysical claim being made here. According to the intuitionists, nothing is a natural number unless it is constructed in the way just outlined. Moreover, each number contains within itself sufficient structure to identify it as a number and to identify its place in the number series. (By way of contrast, Parsons and Resnik think of the numbers as

In order to prove that 2+2=3+1, I first construct two instances of the number 2, one of the number 3, and one of the number 1 in similar fashion. Thus:

// // , /// /.

I show that 2+2=3+1 by establishing a 1-1 correspondence between the strokes on the left hand side of the comma with those on the right hand side.[10] In establishing this correspondence, I complete an intuitionistically acceptable proof that 2+2=3+1.

We are now in better position to see why the intuitionists think "31+6=37" is true even though it is assumed no one has proven that 31+6=37. The intuitionists believe that "31+6=37" is true because our mental mathematical processes include a constructive procedure for addition (the one just outlined for 2+2=3+1), and because they think someone could carry out that procedure for 31+6=37. That is, they believe that someone could construct a series of 31 strokes (or dots, or whatever),

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possessing no internal structure, but existing only as elements in an w-sequence.)

I take it that the intuitionists think of sets in a similar way; namely, as the mental processes one goes through in conceiving of individuals as collections of individuals. For example, I take it the intuitionists think of {a,b} as the mental process one goes through in conceiving of _a_ and _b_ as a single collection, and that nothing will count as a set for the intuitionist unless it is constructible in this way.

[10] To say with the intuitionists that there is a 1-1 correspondence between the strokes on the left hand side of the comma and the strokes on the right hand side is to say that a 1-1 correspondence _can_ be effected between them. Again, I consider the question of how we are to interpret this modality in the next section.

another series of 6 strokes, and a third of 37 strokes, and then establish a 1-1 correspondence between the first two series taken as a single grouping and the third.[11]

iv. I now turn to the question of whether the intuitionists think our mathematical language captures the entire mathematical content of those mental processes which they believe constitute our mathematics. I raise this question only because a review of their writings suggests that they think something essential to mathematics cannot be represented linguistically. Brouwer, for example, writes:

> [...] the exactness of mathematics [...] cannot be secured by linguistic means (Brouwer [2] p.443).

Similarly, Heyting writes:[12]

> [...] we can never be mathematically sure that the formal system expresses correctly our mathematical thoughts (Heyting [1] p.4).

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11 According to the intuitionists, therefore, we may think of our everyday method of showing that 31+6=37; e.g.,

$$\begin{array}{r} 31 \\ \underline{+\ 6} \\ 37 \end{array}$$

as an abbreviation of the intuitionistically complete proof given in the manner just outlined. The intuitionists regard this abbreviation as legitimate because we know what is involved in carrying out an intuitionistically complete proof, and we know that we could carry out the complete proof if we so desired.

12 It may be argued that in this passage Heyting is simply voicing the intuitionists' traditional mistrust of formal systems. Yet it seems to me that the reason the intuitionists mistrust formal systems is that they think there is something formal systems cannot represent.

What we would like to know is what it is about mathematics that the intuitionists think cannot be represented linguistically. It must be something they regard as essential, since otherwise it could be simply ignored. Unfortunately, of course, they cannot say what they think our language cannot represent.[13]

The view that our language cannot capture the entire mathematical content of our mental mathematical processes commits the intuitionist to a peculiar and, I think, untenable conception of language. Consider, for example, the claim Troelstra makes in the following passage:

> The language of mathematics is an attempt (necessarily nearly always inadequate) to describe [...] mental constructions. Talking about intuitionistic mathematics is therefore a matter of suggesting analogous mental constructions to other people. Similarity between the thought processes of various human individuals makes such communication possible (Troelstra [1] p.4).

Here Troelstra claims that a mathematical assertion (e.g., "2+3=5") is an attempt to describe a particular mathematical construction. Yet because he also thinks our mathematical language does not capture the entire mathematical content of our mental mathematical processes, the most a mathematical assertion can do is to serve as a kind of report to aid other

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13 According to Brouwer, what is left out has something to do with our knowledge of the "exactness" of mathematics. Yet it is an interesting question how Brouwer could now know that his mathematical experiences are exact if he cannot somehow represent that exactness to himself. At this point one may wonder whether the intuitionists think of mathematics as some sort of activity that goes on independently of thought as well as of language. If they do, it is an interesting question what they think that activity is. If they do not, however, they are committed to there being a division between what we can think and what we can say.

others in going through the relevant mental mathematical processes themselves.

It seems to me that Troelstra's conception of language leads to a serious difficulty. Suppose, for example, that I have a proof of a theorem T that I wish to tell you about. If I cannot represent the entire mathematical content of my proof, however, it follows that I cannot communicate the entire mathematical content of my proof to you. The best I can do is suggest to you how the proof might go. But if something essential to my proof has been left out of my report, there is no way for you to be sure what it is I am suggesting you prove, or how that proof might go. Nor is there any way for me to know whether you have carried out the proof I suggested to you. In this situation it seems to me that genuine mathematical communication becomes problematic, if not impossible. Yet the situation is even worse. If our language does not capture the entire mathematical content of our mental mathematical processes, then we as individuals cannot have coherent mathematical experiences across time. Suppose I have committed my proof of T to paper. If I was not sure that this written report captured the entire mathematical content of my proof, then I could not now be sure that I had in fact proven T. I am, in effect, left in the position of not knowing whether T is true at any time other than the time I am going through the mental process which the intuitionists think constitutes my proof of T. It seems to me, therefore, that the intuitionist who believes that language does not capture the full mathematical content of our mental mathematical experiences is committed to a kind of ultra-solipsism about mathematical knowledge. He can claim to know only that mathematics which is immediately and introspectively present to him in the form of a mental mathematical experience he his having at a particular time.[14]

This and related problems have led most intuitionists to accept the view that language does capture the entire mathematical content of our mental mathematical experiences. Heyting, for example, writes:

> If a natural number were nothing but the result of a mental construction, it would not subsist after the act of its construction and it would be impossible to compare it with another natural number, constructed at another time and place. It is clear that we cannot solve this problem if we cling to the idea that mathematics is purely mental. In reality we fix a natural number, x say, by means of a material representation; to every entity in the construction of x we associate, e.g. a dot on paper. This enables us to compare by simple inspection natural numbers which were constructed at different times (Heyting [1] p.15).

For the purposes of this paper, I will assume that the intuitionist position is as Heyting outlines it here. That is, I will assume that the intuitionists believe our mathematical terms refer ultimately to extra-linguistic mental processes, but that they also believe our mathematical language captures the complete mathematical content of these processes.[15]

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14 Brouwer seems to have accepted this conclusion.

15 Note, however, that there now appears to be a tension in Heyting's account of language. In the passage just quoted, Heyting accepts the view that language captures the complete mathematical content of our mental mathematical experiences. Yet in the passage quoted previously (which is from the same work), Heyting claims that language does not convey genuine mathematical information. A similar tension exists in Troelstra's work. If Brouwer avoids this problem, it is only because he accepts some of the more troublesome consequences discussed above.

lvi. I now turn to the question of how the intuitionists understand the modality in their claim that a mathematical statement A is true just in case we can undergo that mental process which constitutes a proof of A. It seems to me that the philosophical appeal of traditional intuitionism (such as it is) depends on its promise of a naturalized epistemology for mathematics. But such an epistemology is possible only if we identify our mental mathematical processes with our physically realized psychological processes. Therefore, it seems to me that the intuitionists are committed to interpreting the modality in their claim that A is true just in case we can go through that mental process which constitutes a proof of A as some kind of physical possibility.[16]

The intuitionists do not, of course, believe it is possible to go through every mental mathematical process in practice. They do not, for example, believe it is physically possible to construct any finite number in practice. Yet they recognize that they must admit numbers which they cannot construct in practice into their calculations or find themselves committed to some version of mathematical finitism. They claim, therefore, that it is possible to go through any mental mathematical process in principle. For example, although they recognize that we cannot construct numbers greater than, say, n in practice, they claim that given sufficient time and memory, our mental mathematical powers are such that we can construct them in principle.[17]

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[16] It is not clear to me that Brouwer would accept this conclusion. Here, however, I think we have to distinguish between Brouwer's mysticism and the other intuitionists' more scientifically responsible psychologism. I discuss this issue further in the appendix.

[17] The intuitionists sometimes explain what mathematics they think we

To summarize thusfar: According to the intuitionists, a mathematical statement $\underline{A}$ is true iff a statement of the form "Construction $\underline{c}$ is a proof of $\underline{A}$" is true. "Construction $\underline{c}$ is a proof of $\underline{A}$" is true iff we can, in principle, undergo that (physically realizable) mental process $\underline{c}$ which constitutes a proof of $\underline{A}$. We know about these constructions because they describe our (possible) mental processes, albeit at a certain level of abstraction.

21. I now present my objections to the view just summarized. As I see it, the intuitionists' account of mathematical truth is circular. One simple way of seeing this circularity is the following. Suppose a mathematical statement $\underline{A}$ is true iff one can, in principle, undergo a mental process. Suppose, moreover, that two individuals X and Y each go through a mental mathematical process but get $\underline{A}$ and $\underline{\text{not-A}}$ respectively. The problem is this: If a mathematical statement is true just in case one undergoes a mental process, it follows that X's and Y's results are each true, even though they are mutually inconsistent. X's results are true relative to the

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can do in principle using the device of the ideal mathematician; that is, the mathematician unencumbered by limitations of time and memory. (See, e.g., Brouwer [2] p.443, Troelstra [1] pp.4,95ff.) It is important to note that if the ideal mathematician is to be used to explain what mathematics we can do in principle, its powers must be linked to what we can do in practice. I think it is therefore questionable whether the intuitionists can include increased memory among the ideal mathematician's powers. It may be the case that an increased memory capacity would involve a change in our psychological structures such that the structure of our mental mathematical powers would also change. But an ideal mathematician with different mental mathematical processes cannot be used to explain what mathematics we can do with our mental mathematical processes, even in principle.

mental process he went through in carrying out his proof, and Y's results are true relative to the mental process he went through in carrying out his proof. The situation is, in fact, worse. Suppose X goes through a mental process at time t(1) and another mental process at time t(2), but gets A at t(1) and not-A at t(2). Again, if a mathematical statement is true just in case one can go through a mental process, then both A and not-A are true for X. Thus, an account of mathematical truth which holds simply that a mathematical statement is true iff we can go through a mental process is clearly inadequate. What the account lacks is any notion of mathematical normativity. Therefore, what the intuitionist had better say is that a mathematical statement A is true iff one can, in principle, undergo that mental process which constitutes a proof of A. But now the intuitionist owes an account of what constitutes the right mental process, and it is surely a necessary (if not the sole) criterion for a mental mathematical process being the right mental mathematical process that it yields the right mathematics. (It yields 2+3=5 and not 2+3=6, for example.) It seems to me, therefore, that the intuitionists' account of what makes A true cannot rule out the unacceptable possibility that inconsistent mathematical statements are each true unless they build into their account of when A is true a definition of mental process which they can't explain without recourse to mathematics. Therefore, I conclude, the intuitionists' account of mathematical truth is circular.[18]

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[18] I recognize that there would be no circularity if the intuitionist had a workable conception of proof which did not have a notion of truth built into it. I claim, however, that they have no such conception. The obvious appeal to proof in a formal system is not open to the intuitionists because, as we have seen, they believe mathematics cannot be adequately represented in a formal system. The intuitionists could avoid the inconsistency of finding both A and not-A true by claiming that the

2ii. I now consider three likely responses to the argument just presented. The first is this. Let us assume that our mathematical statements are true by virtue of our (possible) mental mathematical processes. Moreover, following Troelstra, let us assume that as humans we share common mental processes.[19] It follows that a check on whether one has undergone the right mental process in carrying out a proof is that one's mathematical results are in accordance with the results of the community of mathematicians.[20]

Even if we accept the claim that what makes our mathematical statements true is our mental mathematical processes, there are (at

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meaning of negation is other than the classical one. Yet the resulting mathematics would surely be too trivial to take seriously. (I owe this last point to Bob Stalnaker.)

The influence of Frege's anti-empiricist arguments should be apparent here. Recall that the intuitionists think our mathematical theorems express purely empirical facts. (See section (1iii).) But Frege points out that our evaluation of empirical facts often presupposes mathematics. For example, Frege notes that it is because we know that 5+2=7 that we know that when we add 2 units of liquid to 5 other units of another but do not get the expected total that we can assume that some other process such as a chemical reaction is at work (Frege [1] #9). So, just as Frege made it clear that mathematics is not about pebbles, I contend that it is not about bits of mental "stuff" either.

19 By claiming that X and Y share common mental processes, I assume that Troelstra means that they share a common type of mental process.

20 Note that this way of establishing how we know our mathematical statements are true conflicts with the intuitionists' claim that language can only imperfectly represent the content of our mental mathematical experiences. As I have noted, if we cannot adequately represent our our mental mathematical experiences linguistically, we cannot check to see if others have had the same experience.

least) two rather obvious difficulties with the claim that we know we have the right mathematical results when our results accord with the results of the community of mathematicians. First, mathematical truth cannot be a matter of consensus. Suppose, for example, that part of the population ingests a drug which alters their mental processes in such a way that their mathematics differs from those who do not ingest the drug. Not only would we then have two populations with a mutually inconsistent mathematics (which is bad enough), but we would also have no way of knowing which population we should check our results against. (We can't, of course, choose the population whose mathematics yields the "right" results without falling into a circle.)

The second difficulty is somewhat more involved. In order for the intuitionists to claim that two individuals X and Y share common mental mathematical processes, they must know that there is an isomorphism obtaining between X's and Y's mental mathematical processes. But in order to know that there is this isomorphism, there must be a construction, a mental process, which establishes this isomorphism. Yet this mental process must go on in someone's mind (Where else?), and now the intuitionist is caught in a regress. Suppose, for example, that X and Y agree that "2+3=5" is true. What they need to know is whether they mean the same thing by "2+3=5". As intuitionists, they agree that they mean the same thing if they share common mental processes, and so they agree that they mean the same thing if there is an isomorphism obtaining between, say, the mental process c which X went through in proving 2+3=5 and the mental process d which Y went through in proving 2+3=5. For purposes of illustration, let us suppose that X proved that 2+3=5 by establishing a 1-1 correspondence between

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in the way outlined in section (1iv), and that Y proved that 2+3=5 by establishing a 1-1 correspondence between

** ***, *****

in the same way. Now, X establishes this isomorphism between c and d by going through a new mental process c' which establishes a 1-1 correspondence between the "/" symbols and the "*" symbols. Similarly, Y establishes the isomorphism by going through a new mental process d' which also establishes a 1-1 correspondence between the "/" and "*" symbols. They conclude that there is an isomorphism obtaining between c and d. But no ground has been gained. What they need to know now is whether they mean the same thing by "There is an isomorphism obtaining between c and d". Clearly, X and Y will enter into a regress by attempting to answer this question by going through new mental processes c'' and d'' which purport to establish an isomorphism between c' and d'.

The same problem arises if we consider the individual checking his own computations. In order for X to know that the mental process c he goes through in carrying out a proof at one time is the same mental process as the mental process d he goes through in carrying out the proof at a later time, X must establish an isomorphism between c and d. But what establishes this isomorphism cannot be a mental process. Suppose it is thought to be a mental process e. What then establishes the isomorphism between c and e on the one hand and d and e on the other? If the answer is another mental process, then the same regress has set in.[21]

I conclude that the intuitionists' attempt to identify mathematics with mental processes leaves them with no intuitionistically acceptable grounds for thinking there is an isomorphism obtaining between X's and Y's mental mathematical processes, and therefore it leaves them with no intuitionistically acceptable grounds for claiming that we know we

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[21] The reader may notice a similarity between the type of argument I present here and the one used by Kripke in his account of Wittgenstein's rule following considerations. (See Kripke [1] ch.1). As Kripke points out, the skeptic who argues there is no fact of the matter as to whether by "plus" I mean what he means by "quus" will not be satisfied by my producing a rule which I claim to follow and which is incompatible with his use of "quus". Suppose, to use Kripke's example, I claim that my use of "plus" to refer to the addition function is governed by a rule that determines how addition is to be continued in novel cases and which is incompatible with his use of "quus" (Kripke [1] pp.15-16). A crude rule might be: To add _x_ to _y_: count out _x_ many marbles in one heap, _y_ many in another; join the two heaps; count the number in the new common heap. Yet, as Kripke notes, if the skeptic has his wits about him he will now question whether by "count" I refer to the act of counting or the act of quonting.

The reader may also notice a similarity between the argument I present here and that used by Quine to show that the truths of logic are not conventional (Quine [1] pp.103-104). (I do not, of course, mean to claim that Quine's and Kripke's arguments are at all similar.) Quine showed that if we set up certain logical conventions in the form of general statements, we need logic to derive the truths of particular cases from the general case. But, on pain of regress, the logic used to carry out these derivations cannot be a matter of convention. If it was, then the convention governing it would have to be expressed in the form of general statements for which we would need logic to derive particular cases. But then the logic used to carry out _this_ derivation cannot be a matter of convention. If it was a matter of convention, then... . This regress can only be stopped by a logic which is not a matter of convention. Quine's point is: Logic cannot be a matter of convention. My point is: Mathematics cannot be a matter of mental processes. (George Boolos has pointed out to me that this argument has a much older pedigree. It can be found, for example, in Lewis Carroll's "What the Tortoise said to Achilles".)

undergo the same mental processes in carrying out our proofs.[22] The fact that they typically do make this claim leads me to suspect that they have smuggled intuitionistically unacceptable assumptions into their arguments.

The second response to the objection I present in (2i) is that the only essential mental process involved in constructing the numbers is that of adding new units to previously constructed units.[23] Therefore, it might be argued that anyone able to go through the mental process of adding 1 correctly will have an objective standard against which he can check his arithmetic proofs; and, furthermore, that he will have at least the same basic mathematics as anyone else able to add 1 correctly.

It may be the case that anyone able to add 1 correctly will have the same basic mathematics as anyone else able to add 1 correctly (at least if it is assumed in both cases that one can always <u>go on</u> adding 1 correctly). Yet how do we know when we have added one <u>correctly</u>?[24] If the intuitionists' criterion for adding 1 correctly is that one gets 1,2,3,... then

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[22] I do not wish to question the view that as humans we share common psychological structures. What my arguments are meant to show is that mathematics cannot be explained in terms of psychology, not that psychology cannot admit of mathematics. Indeed, I do not see how one could formulate a useful psychological theory which did not somehow presuppose mathematics.

[23] See section (1iv).

[24] This, of course, is the problem which troubles Wittgenstein. How do we know, he asks, that we don't continue the series 1,2,3,4,... by 1002,1004,1006...? See the rule following considerations in Wittgenstein [1] **185-219.

it seems to me the intuitionist is caught in the same circle noted in section (2i). For example, the intuitionist who claims that "2+1=3" is true just in case we can go through that mental mathematical process c which yields 3 when 1 is added to 2 knows that c is the right mental process only because he knows that "2+1=3" is true.[25]

The third response is this. Suppose we assume that what makes our mathematical statements true is our mental mathematical processes. Furthermore, suppose we assume that our mathematical language captures the complete mathematical content of our mental mathematical experiences.[26] Then, because we share a common language, it follows that we share common mental processes. The problem with this response, however, is it seeks to explain the conclusion that we share common mental processes by assuming that we share a common language. Therefore, someone taking this line of argument cannot explain our sharing a common language in terms of our sharing common mental processes.

2iii. In this section I present one reason why I think the traditional intuitionist is committed to some form of mathematical finitism. Again, let us suppose that what makes our mathematical statements true is the structure of our mental mathematical processes. So, for example, let us assume that "2+3=5" reports a brute fact about how these processes work. Furthermore, let us suppose we know that "2+3=5" is true because, given

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[25] As an example of how this problem is passed over by the intuitionists, recall that Troelstra's account of how we construct the numbers simply assumes that we know how to add one correctly.

[26] See section (1iii).

the structure of these mental processes, we cannot conceive of 3+2 not equalling 5.[27] I will call this account of mathematical truth and knowledge the "brute" theory. I claim that the intuitionist who wishes to avoid the circle noted in section (2i) is a brute theorist.

One problem with the brute theory is that the only mathematical statements the brute theorist knows to be true are those statements the mental mathematical processes for which we have actually undergone. The theory leaves the brute theorist with no intuitionistically acceptable grounds for saying what may be the case in counterfactual situations such as those involved in attempting to prove that 31+6=37. (If the brute theorist claims we will find that "31+6=37" is true, then he has fallen into the circle noted in (2i).) But since we have undergone only finitely many mental mathematical processes, the brute theorist cannot claim to know that there are infinitely many mathematical truths. In effect, the brute theorist is committed to the view that a mathematical statement $\underline{A}$ is true just in case we can, in practice, go through that mental process $\underline{c}$ which constitutes a proof of $\underline{A}$.

The reader who agrees with me thusfar may see what is coming. If the argument I presented in sections (2i) and (2ii) are correct, then the intuitionist has no intuitionistically acceptable grounds for thinking we share common mental mathematical processes, and so has no intuitionistically acceptable grounds for claiming that others know the

[27] Thus, although the intuitionists believe it is a contingent matter that there are any mathematical truths because it is a contingent matter that there are any mathematicians, it is consistent with their view to argue that what mathematical truths there are there are necessarily.

mathematics he knows. Moreover, if the argument I present in this section is correct, the intuitionist has no intuitionistically acceptable grounds for thinking there are any mathematical truths other than those he has actually proven. It seems to me, therefore, that the intuitionist is committed not only to an extreme form of mathematical subjectivism, but he is committed to an extreme form of mathematical finitism as well.

2iv. As I noted in section (2i), the intuitionists' account of mathematical truth lacks a viable conception of mathematical normativity. One reason I think it does is that it is unclear what the intuitionists' notion of mathematical objectivity is. I recognize that it is difficult to give an adequate account of objectivity. Yet I think it will be useful to consider what the intuitionists' account might be.

Perhaps the least controversial account of objectivity comes from the natural sciences where, roughly, what is objective is that which is independent of us, and so what is scientifically true is not a matter or human judgment. If we adopt this account of objectivity for mathematics, it appears we should claim that what is objective about mathematics is independent of us, and therefore what is mathematically true is not a matter of human judgment. I think this account of mathematical objectivity is the right one. Nevertheless, it has two consequences which may be thought objectionable. First, it pushes one naturally in the direction of platonism with its attendant difficulties. Second, it appears to beg the question against the intuitionist by defining what is objective in terms of what is independent of us.

I'll set platonism aside for another time.[28] I do not think, however, that platonism has been shown to be any more implausible than intuitionism. In response to the second point, I do not see how an alternative account of mathematical objectivity might go. I do not think we can equate mathematical objectivity with what the individual knows, or even with intersubjective agreement. The individual is often wrong, and "2+3=6" is surely false, even if a massive drug ingestion leads us all to believe otherwise. Neither does it seem right to claim that what is objective is what we know to be the case under epistemically ideal conditions. I do not know what more ideal conditions there could be under which we could determine whether "2+3=5" is <u>really</u> true, or that "2+3=6" is <u>really</u> false. Furthermore, even if we accept the view that what makes our mathematical statements true is the structure of our mental mathematical processes, we could not identify epistemically ideal conditions with the proper functioning of those processes. Change those mental processes by ingesting a drug and what counts as proper functioning changes as well. (And again, we cannot characterize proper functioning of a mental process in terms of that process giving us the right mathematical results without falling into a circle.) I conclude that the intuitionist owes an account of mathematical objectivity, and I predict he will not be able to provide an intuitionistically acceptable one.

<u>2v</u>. To conclude: I hope to have shown two things in this chapter. First, that traditional intuitionism does not offer a satisfactory philosophy of mathematics, and certainly does not offer a satisfactory solution to

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[28] I do discuss platonism in chapter 1 and chapter 4, appendix B.

Benacerraf's dilemma. Second, it seems to me that traditional intuitionism represents an unsatisfactory halfway point between classical mathematics and finitism. Perhaps it has enjoyed what success it has had because there are those who find the foundations of classical mathematics unsatisfactory and yet recognize finitism to be at least as unattractive.[29] Yet this halfway point is unsatisfactory for the reason most halfway points are unsatisfactory; it picks up the worst rather than the best of both worlds. Intuitionism is mathematically unattractive because it leads to an unacceptable truncation of mathematics. (So would finitism.) I hope the arguments I have presented here demonstrate its philosophical unacceptability.

Finally, one may question whether there is a version of intuitionism which is compatible with the central philosophical tenets of traditional intuitionism and which avoids the difficulties I have presented here. I do not think there is. The defining characteristic of traditional intuitionism is its mentalism. Moreover, it is this mentalism which is initially attractive because it promises an account of mathematical knowledge consonant with a naturalized epistemology. Abandon this mentalism and the intuitionist is left without an account of what mathematical objects are, or how we could have knowledge of them. I conclude that the traditional intuitionist cannot abandon his mentalism and remain a traditional intuitionist.[30]

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29 There is a story to be told about why intuitionism has often appeared attractive. Perhaps, however, this is a story better suited to the history or sociology of ideas.

30 As I discuss in the next chapter, intuitionism without the mentalism leaves us with something very much like Dummett's antirealism.

Appendix A: Brouwer on the natural numbers.

According to Brouwer, we derive the structure of the natural number series from our observation of how time passes. The most accessible characterization Brouwer gives of how he thinks this is done is given in the following passage:

> [...] intuitionist mathematics is an essentially languageless activity of the mind having its origin in the perception of a move of time, i.e. of the falling apart of a life moment into two distinct things, one of which gives way to the other, but is retained by memory. If the two-ity thus born is divested of all quality, there remains the empty form of the common substratum of all two-ities. It is this common substratum, this empty form, which is the basic intuition of mathematics (Brouwer [1] p.510.).

As I understand the view presented in this passage, Brouwer thinks that, for example, I am aware of <u>this</u>(1) singular present moment. I am subsequently aware of <u>this</u>(2) singular present moment which is distinct from, and succeeds, moment 1, the memory of which I retain. Here what Brouwer calls a "life moment" (moment 1) has "fallen" into two distinct parts: moments 1 and 2. Now, while the content of these two moments as I have experienced them is subjective, I am supposed to note the irrelevance of these subjective qualities, disregard them, and thereby extract the pure, or "empty" form of the move from 1 to 2. Brouwer calls the awareness of this move from 1 to 2 the "basic intuition" of mathematics. The basic intuition is clearly meant to be that of successor. Thus we see that Brouwer thinks our understanding of successor comes from our understanding of how time passes.

Let us assume, for the moment, that Brouwer is correct that what he calls the "basic intuition" gives us successor. How, then, are we supposed to derive the potential infinity of the number series? One idea which does not work is the following: Having observed _this_(1) moment, and _this_(2) moment, I then become aware of _this_(3) moment, and observe that moment 3 is distinct from moments 1 and 2 which are now seen as a singularity from which 3 has "fallen away". This process continues through moments 4, 5 and so on, and at some point I conclude that this process can always go on; that for any moment _n_ there will be a moment _n+1_ which is distinct from, and succeeds, moment _n_. A moment's reflection, however, shows that this conclusion is a bad one. There is nothing in my observation of moments 1-_n_ which allows me to conclude that there is, or will be, a moment _n+1_. Therefore, our knowledge that there is a potential infinity of numbers cannot be based on our apprehension of how time passes.

Brouwer thinks we derive the infinity of the number series in another way. As I understand it, Brouwer thinks our understanding of the basic intuition (the move from 1 to 2) is derived from our observation of how time passes. The potential infinity of the number series, however, is based on what he calls the "unlimited self-unfolding" of this basic intuition.[31] What Brouwer means by this is anything but clear. What I take him to mean is that the type of move which the basic intuition represents (i.e., successor) _can_ be repeated an unlimited number of times, thereby

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[31] Brouwer [2] p.443.

generating the series 1,2,3,...,<u>n</u>,<u>n+1</u>,... . What we want to know, of course, is why he thinks this process of adding one can be indefinitely iterated. It seems to me that the answer to this question lies in how we are to interpret the modality in his claim that the basic intuition <u>can</u> be indefinitely iterated.

I noted in section (1vi) that the best, and perhaps only way to interpret the intuitionists' modality is as in principle physical possibility. Yet if the arguments I presented in section 2 are correct, the intuitionist has no way of knowing what mathematics is physically possible in principle without reintroducing mathematics. Moreover, the intuitionist cannot equate what is mathematically true with what is physically possible in practice without ending up a finitist. Therefore, as I see it, interpreting the intuitionists' modality as physical possibility fails. But the view that our mental mathematical powers do not involve our physical powers leaves the intuitionist without any way of tying their account of mathematical knowledge to a naturalized epistemology.

Equally severe consequences follow if we try to interpret the modality in any other way. The intuitionists' modality cannot be logical possibility because the intuitionists reject the view that mathematics can be founded on logic.[32] Neither can it be some kind of mathematical possibility.[33] What the intuitionists think of as mathematically possible is simply what mathematics we can do. Therefore, the claim that a mathematical

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[32] See, e.g., Brouwer [1] p.510.

[33] See chapter 2 section (3ii) for a discussion of mathematical possibility.

statement $\underline{A}$ is true just in case it is mathematically possible to construct a proof of $\underline{A}$ is vacuous. I conclude that there is no way of interpreting the modality in Brouwer's claim which is intuitionistically acceptable, non-circular, and non-question begging.

4. DUMMETT'S MATHEMATICAL ANTIREALISM

1. Introduction.

In this chapter I examine Michael Dummett's arguments for mathematical antirealism, his thesis that the correct account of how we understand our mathematical language leads to the adoption of a proof-conditional semantics for that language, and that the adoption of this proof-conditional semantics leads to the rejection of classical mathematics in favor of intuitionism.

I divide the chapter into five sections and two appendices. Before I outline the content of these sections, however, I wish to make two introductory comments. First, when Dummett concludes that we should reject classical mathematics in favor of intuitionism, what he means is that intuitionistic logic, not classical logic, represents the correct mode of mathematical reasoning. As I have indicated, Dummett bases this conclusion on issues having to do with how we understand our mathematical language. Thus the arguments Dummett offers in support of intuitionistic logic are of a very different sort from those offered by the traditional intuitionists. (Recall from chapter 3 that the traditional intuitionists argue against classical mathematics primarily on grounds having to do with what they take to be the nature of mathematical experience.) Second, mathematical antirealism is the doctrine which results from the application of certain general arguments about understanding to the mathematical fragment of our language. Because my criticisms of mathematical antirealism will be directed at these general arguments, they will most likely apply to other areas of discourse where antirealism threatens as well (antirealism about the past, for example). I will not, however, discuss these other varieties of antirealism here.

Now to the content of the various sections. In section 2 I present what I take to be the crucial assumption underlying Dummett's general argument for antirealism. This assumption is that one's understanding of an expression is constituted by one's linguistic behavior. I outline why Dummett makes this assumption, and I show that the arguments he offers in support of this assumption fail, in fact, to support it. I conclude that his assumption that behavior constitutes understanding is unsecured. In section 3 I show how the assumption that behavior constitutes understanding figures in Dummett's argument for mathematical antirealism. I argue that because this assumption is unsecured, his argument for mathematical antirealism is likewise unsecured. (A note of caution: I find Dummett's arguments somewhat obscure. Therefore, the account I present in sections 2 and 3 might best be characterized as a rational reconstruction.)

In section 4 I argue that understanding should not be identified with behavior. I contend that Dummett's assumption that behavior constitutes understanding rests on a failure to appreciate the crucial distinction between the question of how we know whether someone understands an expression and the question of whether they do, in fact, understand that expression. I argue that Dummett fails to appreciate the significance of this distinction because of his uncritical acceptance of Wittgenstein's private language considerations. In section 5 I briefly discuss some relevant aspects of the private language considerations, and I argue that at least as they are commonly understood, they fail to provide a secure foundation for antirealism. In the first appendix I discuss Dummett's controversial use of "conclusive verification", and in the second appendix I offer some comments on platonism and the question of mathematical objectivity.

2. Dummett's key antirealist assumption.

2.1. As I have indicated, Dummett's arguments for antirealism center on questions about how we understand our language. It would be useful, therefore, to know exactly what Dummett thinks a language is. Unfortunately, Dummett tends to discuss how he conceives of a language in only the most vague and general terms. One important clue is provided by his characterization of a theory of meaning for a language as a theory which yields the meaning of every sentence of that language.[1] Therefore, I do not think that I can go far wrong in claiming that Dummett thinks of languages extensionally; that he thinks of English, for example, as the set of English sentences or, perhaps more accurately, as the set of English sentence/meaning pairs.[2]

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[1] See, for example, Dummett [1] p.99. I should note that Dummett is admittedly lax about maintaining a sentence/statement distinction. (See his apology in Dummett [2] p.67.)

[2] Throughout I will assume that we can discuss questions about natural languages by discussing questions about English without loss of generality. Dummett sometimes writes of language as a practice. (See, for example, Dummett [7] p.473). I assume that what Dummett means when he claims that language is a practice is that English, for example, may be thought of

Now to the question of what Dummett thinks it is to understand a language or, more precisely, what he thinks it is to understand a statement of that language. According to Dummett:

> The meaning of a statement cannot be, or contain as an ingredient, anything which is not manifest in the use made of it, lying solely in the mind of the individual who apprehends that meaning [...] (Dummett [5] p.216).

If we assume that what one means by an expression is what one understands by that expression, then I find two claims in this passage. These are:

> (1) What a speaker X understands by an expression E must be publicly manifestable.
>
> (2) We are to identify X's understanding of E with X's behavior.

(1) is stated quite explicitly, and restates the well-known conclusion of Wittgenstein's private language considerations.[3] Unfortunately,

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as the practice of using English sentences. It seems to me, however, that the notion of English as a practice is in some sense dependent on the notion of English as a set of sentences (or sentence/meaning pairs). If English were simply whatever expressions English speakers used, then expressions such as "zeitgeist" or "glasnost" would qualify as English expressions. Finally, I should note that most cognitive linguists reject an extensional account of language as unhelpful in the study of what it is to understand a language. This rejection points to a significant disagreement between many philosophers and linguists. Much, I think, rests on which model of language proves more fruitful.

[3] See Wittgenstein [1] **201-202, 253ff. An example of the type of private understanding which (1) is meant to rule out is X's identification of the meaning of E with some sensation or mental image. It is not meant to rule out the possibility of a language which, as a contingent matter, is known only to a single individual. If, for example, every English speaker but X died of some plague, X would still understand English. (See Dummett

Dummett gives no explanation of how he understands the private language considerations, and little about why he thinks they are correct. (He says only that he finds them "incontrovertible".[4]) This will turn out to be an important omission.

It is perhaps less obvious that Dummett is committed to (2). I find a commitment to (2) in his claim that: "The meaning of an expression cannot be [...] anything not manifest in the use made of it". I recognize, however, that what Dummett means here is less than clear. Later in this section, therefore, I will show how his acceptance of (1) leads him to (2).[5]

There are two terminological points which it will be useful to introduce here. First, I will call the behavior which I claim Dummett thinks constitutes X's understanding of E X's linguistic behavior. It is important to note, however, that Dummett thinks X's linguistic behavior includes not just verbal behavior such as responding "five" to the question "What is three plus two?", but also includes some non-verbal behavior such as closing a door in response to a request. The reason Dummett includes

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[8] p.208 for a discussion of this last point.)

4 Dummett [6] p.xxxi.

5 It may be a more accurate characterization of Dummett's position to say that he identifies understanding with the ability to behave in a certain way. Yet this changes nothing essential to my point. Let us assume that X has an ability just in case X can, in principle, exercise that ability. Therefore, if we think that X understands E just in case X has the ability to publicly manifest his understanding of E, and if X has an ability just in case he can exercise that ability, we make the same point by claiming that X understands E just in case X can, in principle, publicly manifest his understanding of E.

this sort of non-verbal behavior as linguistic behavior is not simply that closing a door in response to a request is evidence that X understands that request (although it is certainly that), but because, as we shall see, Dummett also thinks that X's purely verbal account of what he understands by E is not sufficient to show that X really does understand E.

Second, the claim that behavior constitutes understanding certainly appears to mark Dummett as some sort of behaviorist. Yet we must be careful here. Dummett is no Skinnerian behaviorist; he nowhere denies the existence of mental entities. Rather, as we shall see, what Dummett questions is the role mental entities can play in an account of what it is to understand a language. Neither is Dummett a Rylian behaviorist; he does not claim that psychological predicates are logical constructions developed from behavior. Rather, Dummett is what I will call a methodological behaviorist. I will explain what I mean by this term in subsection (2iii).

2ii. As I have indicated, (2) depends on (1). Dummett offers two arguments in support of (1). These are known in the literature as the manifestation and acquisition arguments.[6] In this subsection I show how these arguments go, and why I think they fail to support (1).

Before I outline how Dummett thinks the manifestation and acquisition arguments go, it will be useful to introduce the distinction he draws

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[6] Dummett does not use the "manifestation" and "acquisition" labels. I adopt these terms from other commentators, though I make no claim that I understand the designated arguments in the way those commentators understand them.

between one's explicit understanding of an expression and one's implicit understanding of an expression.[7] According to Dummett, X's understanding of E is explicit if X can state the rules governing his use of E, or if he can state how E can be replaced by an equivalent expression E*. X's understanding of E is implicit if X cannot presently give an explicit account of how he understands E, but could, in principle, recognize an explicit account of how he understands E if presented with one.[8]

Now to the manifestation argument. Remember that Dummett is attempting to show that what X understands by E must be publicly manifestable. As we have just seen, X's understanding of E may be either explicit or implicit. X's explicit understanding of E is publicly manifestable by definition. But what if X's understanding of E is implicit? Dummett writes:

> Implicit knowledge cannot [...] meaningfully be ascribed to someone unless it is possible to say in what the manifestation of that knowledge consists: there must be an observable difference between the behavior or capacities of someone who is said to have that knowledge and someone who is said to lack it (Dummett [5] p.217).

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[7] See, e.g., Dummett [5] p.217.

[8] I take it that the existence of implicit knowledge is uncontroversial. We do not, after all, have an explicit understanding of most of the expressions we use. Furthermore, as Dummett notes, the existence of implicit knowledge is easily seen by considering the child learning his first language. If a child understands an expression just in case he can give an explicit account of how he understands that expression, he could never acquire a first language since he would lack the linguistic resources to state how he understands the expressions he is trying to learn (Dummett [5] p.217).

There is, I think, no argument here. Dummett claims that we cannot know what X understands by E unless X can manifest that understanding in his linguistic behavior. In one respect this claim is correct; we should not ascribe an understanding of E to X without evidence.[9] But the observation that we should not ascribe an understanding of E to X without evidence does not license the conclusion that X does not understand E unless he can publicly manifest that understanding. The fact that one individual Y might not be able to discover what another individual X understands by E is not sufficient to show that X does not understand E. It shows only that Y does not know if X understands E. One reaches the stronger conclusion that X does not understand E only by adding the premise that X understands E only if he can publicly manifest that understanding. But that is (1). Therefore, as I see it, Dummett's manifestation argument does not support (1), but depends on (1) to make its point.[10]

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[9] It is, of course, another matter why this evidence should be restricted to X's linguistic behavior. Dummett does not say why he imposes this restriction, although I think we can see one reason why he might. (It is, moreover, a reason to which I think his views commit him.) The evaluation of any evidence about understanding will be couched in language. Therefore, Dummett's point may be that what we understand by the expressions of that language must be in place before this evaluation can be carried out. It would follow, therefore, that no non-behavioral evidence is relevant to the question of how we understand a language. But this argument assumes that the only evidence which will settle what X means by E is behavioral evidence.

[10] Note the tension that now exists in Dummett's position. Dummett is committed to the view that all implicit knowledge can, in principle, be made explicit. Yet Dummett is also committed to the view that the child learning its first language will have an implicit understanding of some expressions which it cannot manifest (at least in any way we could recognize).

The claim that X understands E just in case X can publicly manifest that understanding also runs up against what I take to be the straightforward

Dummett's acquisition argument is this:[11] When someone learns, let us say, the language of mathematics, he cannot be taught what cannot be communicated to him. Therefore, it must be the case that what is essential to our understanding of our mathematical language is communicable, hence manifestable.

It no doubt strikes the reader that the acquisition argument may well fail in cases where we have an innate grasp of concepts associated with the expressions in question.[12] Suppose, for example, that we have an innate grasp of a concept <u>c</u>. Similar training leads each of us to associate E with that concept, with the result that we all understand E in the same way. On this view, therefore, what is essential to one's understanding of E is the association of E with <u>c</u>. And because the association of E with <u>c</u> does not involve communication, what we understand by E need not be manifestable.

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counterexamples of aphasia cases. We are at a loss to explain how aphasia victims regain their use of their language without benefit of retraining if they do not understand their language independently of their ability to manifest that understanding.

[11] See Dummett [5] p.217.

[12] What it means to "grasp" a concept has always mystified me. I suppose it means something like to "have possession of", although this hardly clarifies the matter since I find it almost as unclear what it means to "possess" a concept. Furthermore, that we have an innate understanding of some mathematical concepts seems especially plausible, but the situation may not be limited to mathematics. Suppose, for example, that there is something innate in us which allows us to pick out a dog as a dog. It seems to me that in that case Dummett's acquisition argument will fail for talk about dogs as well.

As far as I know, Dummett offers only two responses to this objection. The first is this.[13] Suppose X has a grasp of certain concepts antecedent to his associating any expressions with those concepts. (We may suppose X has an innate grasp of these concepts.) According to Dummett, X's antecedent grasp of these concepts can play no role in explaining what it is for X to understand the expressions he associates with these concepts. To see why this is so, Dummett asks us to consider the following analogy. Suppose X is a native speaker of English who learns Chinese. Dummett claims that a satisfactory account of X's understanding of Chinese will involve two components: an account of X's ability to associate Chinese expressions with English expressions, and an account of X's understanding of those English expressions. But now consider an account of X's understanding of English which is given in terms of X's antecedent grasp of concepts associated with those expressions. If this account is analogous to the account of what it is for X to understand Chinese (and Dummett seems to think it must be), then it also involves two components: an account of X's ability to associate English expressions with certain concepts, and an account of X's understanding, or grasp, of those concepts. But, Dummett argues, the analogy breaks down at the first stage. He claims there is no way to establish an association of English expressions with concepts without falling into a vicious regress. For example, in order to establish an association of an expression E with a concept c, we must linguistically represent c in some way. Suppose we represent it using expression E'. But now, in order to establish the association of E' with c, we must again represent c in some way, perhaps using expression E'', and

[13] See Dummett [4] pp.5-7.

so on into regress. Dummett concludes that an account of what it is to understand an expression which is given in terms of associating that expression with a concept must associate that expression with something which constitutes a grasp of that concept in a way which does not invite this regress. He claims that this account can only be given in terms of associating expressions with behavior.

This last claim may be made more clear if we consider a more specific example. According to Dummett, we cannot explain X's understanding of the word "square" simply by claiming that X understands the word "square" just in case he possesses the concept _square_ (and, of course, just in case he associates "square" with that concept). This because he thinks we are at a loss to explain what possession of the concept _square_ amounts to independently of the ways X might manifest that understanding. Dummett explains:

> At the very least, [to grasp the concept _square_] is to be able to discriminate between things that are square and those that are not. Such an ability can be ascribed only to one who will, on occasion, treat square things differently from things that are not square; one way, among many other possible ways, of doing this is to apply the word "square" to square things and not to others. And it can only be by reference to some such use of the word "square", or at least of some knowledge about the word "square" which would warrant such a use of it, that we can explain what it is to associate the concept _square_ with that word (Dummett [4] p.7).

Thus, Dummett concludes, even if X has a prior grasp of the concept _square_, that grasp plays no role in an account of what it is for X to understand the word "square".

The difficulty I find with Dummett's response is it does not address the question at hand. The question I posed is: If X has an innate grasp of a concept, why must X's understanding of the expression X associates with that concept be publicly manifestable? Dummett does not respond to this question. Rather, he simply assumes that in order for X to understand E what X understands by E must be publicly manifestable. Now, it may be the case that in order for someone other than X to know whether X understands the word "square", X must manifest his understanding of "square" in appropriate ways. But to assume that X does not understand "square" unless he can publicly manifest that understanding is simply to assume that X's understanding of "square" must be publicly manifestable. Therefore, as I see it, Dummett's first response fails.[14]

Now to Dummett's second response. Dummett claims that an account of understanding which explains X's understanding of E in terms of X's grasp of antecedently held concepts fails because it leaves X's ability to understand novel statements unexplained.[15] He thinks that if X's understanding of a novel statement is to be explained in terms of X's antecedent grasp of the concepts associated with that statement, it follows that X must already grasp those concepts. Indeed, in order to

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[14] I think, therefore, that we have some reason to doubt the usefulness of Dummett's original analogy. According to Dummett, what is wrong with an account of X's understanding of Chinese which simply associates Chinese expressions with antecedently held concepts is that we are at a loss to explain what it is for X to possess the relevant concepts independently of the way X might manifest his "grasp" of those concepts. But, as I have argued, this response simply ignores the possibility that X's understanding of Chinese may be the result of X's associating the Chinese expressions with the relevant concepts.

[15] Dummett [8] pp.198-99.

understand English, it would appear that X would have to come equipped with all the concepts which can be expressed in English.[16]

I find this response unpersuasive. I fail to see why it is required of someone defending the view that understanding may involve the association of expressions with antecedently held concepts that they defend the view that understanding involves only the association of expressions with these concepts. There may be cases where understanding is the result of associating expressions with antecedently held concepts, and there may be other cases where a different account is called for. Moreover, I don't see why it should be the case that, for example, the child must come equipped with all the concepts which can be expressed in his language. It may be the case that just as novel statements are understood compositionally, the concepts associated with these novel statements can be understood as constructed from more basic concepts.

I conclude that Dummett's defence of the acquisition argument fails. I claim that the acquisition argument does not support (1), but relies on (1) to make its point. I conclude, therefore, that both the manifestation and acquisition arguments are largely embroidery on a theme about the necessity of the public manifestation of understanding which Dummett simply takes for granted.

2iii. Setting aside the question of whether Dummett has any good arguments for (1), we are now in position to see how Dummett passes from (1) to (2). We know that Dummett thinks that what X understands by E must

[16] Some, I understand, have argued for this view.

be publicly manifestable. Furthermore, we have seen that Dummett thinks that nothing counts as evidence for X's understanding of E other than what X can publicly manifest. Therefore, as I understand it, Dummett's reason for thinking that (2) is correct is methodological. (It is for this reason that I characterized Dummett as a methodological behaviorist.) That is, Dummett claims that an account of X's understanding of E goes from an account of X's linguistic behavior to an ascription of the relevant implicit knowledge. Since Dummett thinks nothing counts as X's having this implicit knowledge other than X's linguistic behavior, I take Dummett as concluding that reference to this implicit knowledge can be dropped from the account of what it is for X to understand E. This leaves an account drawn in purely behavioral terms, hence (2).[17] If, however, the objections I presented in the previous subsection are correct, Dummett has not established (1). And since (2) depends on (1), I conclude that he has not established (2) either.[18]

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[17] Dummett's general point seems to be that the inner cannot explain the behavioral because nothing counts as being in a mental state which does not first satisfy the behavioral criteria for understanding. But this strikes me as a form of (now discredited) operationalism applied to the study of language.

[18] It might occur to the reader that Dummett is making a weaker and perhaps more plausible claim when he argues that understanding is constituted by behavior. The weaker claim is this: If we assume that language is an instrument of communication (which it certainly is), then what one can communicate to others must, in principle, be publicly manifestable. Stated this way, however, Dummett's claim reduces to the tautology that one can communicate to others only what one can communicate to others. Moreover, I do not think this weaker reading reflects Dummett's actual position. Dummett claims not just that language is an instrument of communication, but that it is solely an instrument of communication (Dummett [5] p.216). This qualification is crucial. Without it, we may admit that one cannot communicate what one cannot make public but still hold that one may understand an expression in some private

21v. Finally, a brief digression. We are also in position to see why Dummett thinks that an account of X's understanding of his language will not include any reference to X's psychological mechanisms. Dummett is conspicuously silent on this issue. To my knowledge, he offers only one explicit and one implicit argument in support of this view.[19]

Dummett's explicit argument is this.[20] Imagine a Martian who behaves in all the ways appropriate to a speaker of English (whatever these ways may be). Dummett claims that we would attribute an implicit understanding of English to the Martian even though its psychological workings are, it is assumed, different from ours. He concludes, therefore, that an account of psychological workings are irrelevant to the question of what it is to understand a language.

The Martian example does, I think, show that those who place too much emphasis on a psychological account of understanding often downplay the important point that language serves a communicative function. Yet the example does not show that what the Martian understands by an English expression must be publicly manifestable. It shows only that what we

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way. Only if it is assumed that language is solely an instrument of communication is the possibility that X understands E in some private way ruled out. I conclude, therefore, that the weaker reading is incompatible with Dummett's actual position.

19 It may be that Dummett thinks that Frege's and Wittgenstein's antipsychologistic arguments are decisive in this matter, and that he can safely build on their conclusions. Even if that is the case, however, he should at least signal that that is what he is doing.

20 Dummett [2] p.70.

know of what the Martian understands by an English expression must be publicly manifestable. As I see it, therefore, the Martian example fails to show that an account of what the Martian means by an English expression can ignore the Martian's psychological mechanisms. (The example does, of course, show that an account of understanding might not be psychology-specific.)

I find Dummett's implicit argument in his claim that one's antecedent grasp of a concept will play no role in an account of what it is to understand the expressions associated with that concept. Consider, for example, the following passage:

> [...] we have no idea what structure and character knowledge conceived as an internal state, may have, apart from the structure of what is known [...] we could not identify [inner structures] as cognitive ones save by the connections with their manifestations (Dummett [7] p.6).

Here again, Dummett claims that an account which seeks to explain our understanding of expressions in terms of inner, or psychological states will be unable to fix the association of expressions and these inner states with anything other than the behavioral manifestations of those inner states. He concludes, insofar as I understand him, that reference to whatever psychological mechanisms constitute these inner states can be dropped from the account of what it is to understand these expressions.[21] Yet as I argued in the previous subsection, this conclusion depends on the

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[21] Dummett's conclusion, even if correct, would be so only at the global level. For example, the singular attribution of psychological states to explain behavior (e.g., "He winced because he was in pain") is generally taken to constitute a legitimate explanation of behavior. (I owe this point to Jim Higginbotham.)

unsecured assumption that understanding must be publicly manifestable. End of digression.

3. Dummett's argument for mathematical antirealism.

In this section I present Dummett's argument for mathematical antirealism. I'll begin by outlining Dummett's overall strategy. He writes:

> An existing practice in the use of a certain fragment of language is capable of being subjected to criticism if it is impossible to systematize it, that is, to frame a model whereby each sentence carries a determinate content which can, in turn, be explained in terms of the use of that sentence (Dummett [5] p.220).

Some explanation is in order before I state what I think the main claim of this passage is. According to Dummett, we "systematize" a language, or some fragment of it, by constructing a theory of meaning for that language, or fragment of that language.[22] Dummett places three constraints on the form this theory should take which are relevant to our concerns. First, the theory should be molecular, not holistic.[23] It is difficult to find an article where Dummett does not inveigh against holism. He writes, for example, that:

> [...] I am asserting that the acceptance of holism should lead to the conclusion that a systematic theory of meaning is impossible, [...] my own preference is, therefore, to assume as a methodological principle that holism is false (Dummett [1]

[22] Dummett recognizes that the construction of a complete theory of meaning for a language is not a practical project. What we want, he claims, is an understanding of those principles which would make the construction of a complete theory possible in principle (Dummett [1] p.97).

[23] See, for example, Dummett [3], [4], [5], [7].

p.121).

Second, when Dummett writes that each sentence has a "determinate" content, I take him to mean that the content assigned to each statement by the theory is neither vague nor ambiguous.[24] Third, when Dummett claims that a theory of meaning should explain the content of each statement in terms of the use made of that statement, I take him to mean that the theory should explain what the content of each statement is in terms of the linguistic behavior which, as we have seen, he thinks constitutes one's understanding of that statement. So, as I read the above passage, Dummett's claim is that an existing practice is subject to criticism if it proves impossible to construct a molecular theory of meaning for the statements used in that practice which explains the content of each of these statements in terms of one's linguistic behavior. (Hereafter, I will omit the qualification that Dummett thinks a theory of meaning should be molecular as understood.)

Let us apply this claim to the case of mathematics. I now read Dummett as claiming that our mathematical practices are subject to criticism if it proves impossible to construct a theory of meaning for our mathematical statements which explains the content of each mathematical statement in terms of our linguistic behavior. As we shall see, Dummett does not think we can construct a theory of meaning which meets this constraint and which is consonant with classical mathematical practices. He does,

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[24] There may be an implicit rejection of holism in Dummett's claim that each statement of the language should have "determinate" content. According to the holist, the content of each statement is in some way dependent on the content of each of the other statements of the language, and so may not be individually "determinate".

however, think that we can construct a theory which meets this constraint and which is consonant with intuitionistic mathematics. He concludes, therefore, that we have "systematically misunderstood our own language",[25] and that we should abandon classical mathematics in favor of intuitionism.

One final note before turning to the details of Dummett's argument. Dummett claims that intuitionism is philosophically interesting only insofar as it represents the sole legitimate method of mathematical reasoning. He claims that if we could construct a theory of meaning consonant with our classical mathematical practices, the study of intuitionism would become "a waste of time".[26] My strategy will be to show that Dummett's reasons for thinking we cannot construct this theory fail.

I divide the presentation of Dummett's argument against classical mathematics into several subsections. I first outline (i) what Dummett thinks the key principle of classical mathematical reasoning is. Following Dummett, the question then becomes whether we can frame a theory of meaning for our mathematical statements which explains the content of each statement in terms of our linguistic behavior and which is consonant with this defining principle. Next, I review (ii) how Dummett conceives of

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[25] Dummett [2] p.101.

[26] Dummett [3] p.viii. See also Dummett [5] p.215. Note that even if Dummett is correct that we cannot construct this theory, we might well conclude that this demonstrates the inadequacy of his conception of a theory of meaning.

a theory of meaning and, in particular, I review two requirements he places on any satisfactory theory of meaning. I then explain (iii) why Dummett thinks that a theory of meaning which is consonant with our classical mathematical practices cannot meet these requirements. Finally, (iv) I outline what kind of theory Dummett thinks does account for our understanding of our mathematical statements, and why he thinks adoption of this theory leads to intuitionism.

(i). Classical mathematical reasoning is simply mathematical reasoning using classical logic. We may say, therefore, that the defining principle of classical mathematical reasoning is whatever the defining principle of classical logic is. According to Dummett, the defining principle of classical logic is the principle of bivalence.[27] The principle of bivalence is the principle that any (declarative, sufficiently non-vague) statement to which it applies is either true or false. Therefore, following Dummett, we may say that the defining principle of classical mathematical reasoning is the principle that every mathematical statement is either true or false. The question, as Dummett sees it, is whether we can frame a theory of meaning for our mathematical statements which explains the content of each statement in terms of our linguistic behavior and which is consonant

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[27] See, for example, Dummett [6] p.xxix. Note that our classical practices are not characterized by acceptance of excluded middle. 'Av-A' is merely a logical schema acceptable, under different interpretations, to both classical and intuitionistic logicians.

Dummett recognizes that classical logic can have a different, perhaps multi-valued, semantics as long as the truth values for this semantics form a boolean algebra. He claims, however, that this changes nothing essential to his argument, since each statement can have only one unchanging truth value from this range of truth values. For a discussion of this point see Dummett [2] p.103.

with the principle that every mathematical statement is true or false. If we can, then Dummett thinks the choice of classical logic for mathematics is the correct one. If we cannot, however, then he thinks the choice of classical logic for mathematics stands unjustified.[28] As we shall see, Dummett thinks that the theory of meaning which supports bivalence is a truth conditional theory. It will be useful, therefore, to review more about how Dummett conceives of theories of meaning in general, and a truth conditional theory of meaning in particular.

(ii). As Dummett summarizes it, the basic purpose of any theory of meaning is to provide a

> detailed specification of the meanings of all the words and sentence-forming operations of the language, yielding a specification of the meaning of every expression and sentence of the language (Dummett [1] p.99).

Although the purpose of a theory of meaning is to give an account of the meaning of all the statements of the language in question, I will assume that a theory of meaning is <u>complete</u> just in case it accounts for the meaning of all the declarative, sufficiently non-vague statements of the language. Furthermore, I will assume that a theory of meaning is satisfactory just in case it is complete.

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[28] Dummett [2] pp.103-104. Although Dummett thinks that the choice of logic for a particular domain of discourse as justified by the choice of an appropriate semantics for the statements of that domain, it is not clear to me that this is right. If we think of a logical system pragmatically; that is, simply as a device for organizing arguments, then one's justification for adopting some particular logic may be only that it meets some antecedently accepted criteria for what constitutes a good argument. I shall not, however, pursue this issue further.

Now, according to Dummett, a theory of meaning

> must give an explicit account, not only of what anyone must know in order to know the meaning of any given expression, but of what constitutes having such knowledge (Dummett [1] p.123).

We may divide this claim into two parts. That is, according to Dummett, a satisfactory theory of meaning must:

> (A) Give an account of what a speaker must know in order to understand any expression of the language.

and

> (B) Give an account of what constitutes having this knowledge.

Consider how Dummett thinks a truth conditional theory of meaning satisfies (A).[29] If a statement S is true, Dummett thinks there must be something - some state of affairs, let us say - by virtue of which S is true. Call this state of affairs the truth conditions for S. A speaker X therefore understands S as that understanding is described truth

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[29] I will assume that the basic structure of a truth conditional theory of meaning is familiar to the reader. To review briefly: Dummett thinks of a fully developed truth conditional theory of meaning as consisting of a theory of reference, a theory of sense, and a theory of force (Dummett [2] p.74). The theory of reference contains axioms which assigns references of the appropriate kinds to individual words, axioms which govern how sentences may be formed, and theorems which inductively specify the conditions under which statements of the language are true. The theory of sense explains what the speaker's understanding of the relevant parts of the theory of reference consists in by matching the speaker's understanding of statements formed in accordance with the theory of reference with particular abilities. The theory of force gives an account of the various kinds of linguistic acts (e.g., making a request, giving a command) which may be performed by making a statement.

conditionally just in case he understands, or "grasps", the truth conditions for S. For example, X understands "There is a red book on the desk" truth conditionally just in case he understands what state of affairs must obtain (that of there being a red book on the desk) in order for S to be true. So far, perhaps, so good. The problem, as we shall see in the next subsection, comes with (B).

We are now in position to see why Dummett thinks acceptance of the principle of bivalence for our mathematical statements is justified only by the acceptance of a truth conditional theory of meaning for our mathematical statements. If bivalence applies unrestrictedly to our mathematical statements, then it must be the case that for every mathematical statement S, there is a state of affairs (the truth conditions for S) which either does or does not obtain.[30] If I understand him correctly, Dummett thinks that we understand our mathematical statements in a way which licenses this conclusion only if the theory of meaning explains how we understand our mathematical statements is in terms of our "grasp" of these truth conditions. Therefore, as I understand him, Dummett concludes that bivalence applies unrestrictedly to our mathematical statements only if we understand our mathematical statements truth conditionally.[31]

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[30] Dummett [2] p.89. Thus acceptance of bivalence for our mathematical statements pushes one naturally in the direction of platonism, at least as that doctrine is usually understood. I have several comments about platonism which are too lengthy to include as a footnote. See appendix B.

[31] Some commentators have noted that acceptance of a truth-conditional theory of meaning need not commit one to bivalence. For example, one could hold that the meaning of a sentence is given by its truth conditions, but also hold that a declarative sentence such as "The present King of

(iii). Thusfar we have encountered several requirements which Dummett places on what he thinks constitutes a satisfactory theory of meaning for a given domain of discourse. Foremost among these requirements is that a satisfactory theory of meaning must be complete. This requirement is given in Dummett's requirement (A) that a satisfactory theory of meaning must give an account of what a speaker must know in order to understand any statement in the domain. We have also seen that Dummett thinks a satisfactory theory of meaning must explain the content of each statement in terms of our linguistic behavior. Therefore, when we consider Dummett's requirement (B) that a satisfactory theory of meaning must explain what constitutes having an understanding of any expression of the language, we see that Dummett thinks this explanation must be given in terms of the speaker's linguistic behavior.[32] Now, in this subsection I present Dummett's reasons for thinking a truth conditional theory of meaning is not satisfactory. As we shall see, he thinks it is not

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France is bald" is neither true nor false. Perhaps, following Strawson, one might argue that "The present king of France is bald" is meaningful but at the present time does not make a statement. One could then argue that bivalence applies only to statements, and thus does not apply to "The present king of France is bald". I take it, however, that Dummett thinks the commitment goes the other way around; that is, that one should not accept bivalence without accepting a truth-conditional theory of meaning.

[32] There is an interesting distinction to be drawn here between Dummett's project and its Fregean antecedent. Whereas for Frege the grasping of the sense of an expression is in some sense primitive, Dummett regards an account of how sense is grasped as essential to an account of understanding. (I owe this point to Jim Higginbotham.) Then, because Dummett thinks that one's understanding of an expression must be manifestable, it follows that he would think a satisfactory theory of sense must make explicit the connection between one's understanding of an expression and the actions which manifest that understanding.

satisfactory because he thinks it cannot explain the content of each statement in terms of our linguistic behavior and still be complete.

Some additional set-up is required before I present Dummett's argument that a truth conditional theory is not complete. When we analyze our mathematical statements truth conditionally, some mathematical statements are what Dummett calls "effectively decidable", and the others are what he calls "non-effectively decidable" (where, to avoid any ambiguity, what is meant is "not: effectively decidable").[33] A statement is decidable if there is a decision procedure for discovering what the truth value of that statement is. It is effectively decidable if it is possible to carry out that procedure in a finite number of steps. So, for example, "17 is prime" is effectively decidable. We have a decision procedure (Eratosthenes's Sieve) for discovering whether a number is prime and, by any standards, this procedure is effective for 17. A statement is non-effectively decidable if there is no procedure for discovering that statement's truth value, or if there is a procedure but it is not one which is effective.[34]

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[33] Dummett [2] p.81, [5] p.217. I am not sure that Dummett's use of "effectively decidable" is a happy one in this context. It appears to make little sense to say that statements are effectively decidable in the sense that, say, set membership is effectively decidable.

[34] There may be some difficulties with the notion of non-effective decidability as I have characterized it here. Consider, for example, the question of whether "$10^{10}+13$ is prime" is effectively decidable. If we follow the definition given above, the answer is clearly yes; we can decide whether $10^{10}+13$ is prime in a finite number of steps. Yet much depends on the question of how we interpret the modality in the claim that we can decide whether $10^{10}+13$ is prime. I think, for example, that it is questionable whether we have any clear conception of what it would be to physically carry out that enormous a computation. Here we arrive at the

Because Dummett does not provide an example of a mathematical statement which he thinks is non-effectively decidable, and because he is unclear about what he thinks it is for a statement to be decidable in principle, I find it difficult to be sure which mathematical statements he thinks are non-effectively decidable. He does write that one linguistic device which enables us to frame non-effectively decidable mathematical statements is quantification over infinite totalities.[35] I take it, therefore, that an example of a statement which Dummett would classify as non-effectively decidable when that statement is analyzed truth conditionally is Goldbach's Conjecture: "All even numbers are the sum of two primes". The reason I assume Dummett would classify Goldbach's Conjecture as non-effectively decidable is that when the quantifier is interpreted truth conditionally, it is understood to range over an infinite totality of numbers. Thus, even though it may be assumed that we can discover whether any particular finite number is the sum of two primes, we have no effective procedure for discovering whether all even numbers are the sum of two primes.[36]

We are now in position to see why Dummett thinks a truth conditional

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important question of how Dummett understands this modality, and whether his understanding of it commits him to some version of mathematical finitism. I suspect it does, but that is a matter for another paper.

[35] Dummett [2] p.81, [5] p.231.

[36] Of course, Goldbach's Conjecture would become decidable if either a proof or counterexample was discovered. Another possible example of a non-effectively decidable sentence in a formal system might be a Godel sentence for that formal system.

theory of meaning for our mathematical statements cannot be complete. We know Dummett thinks that a truth conditional theory must explain the content of each mathematical statement in terms of our linguistic behavior. We also know that Dummett thinks that X understands S as that understanding is described truth conditionally just in case X grasps the truth conditions for S; that is, just in case X understands what state of affairs would have to obtain for S to be true. Therefore, following Dummett, if X's understanding of S is as that understanding is described truth conditionally, there must be some linguistic behavior which constitutes X's grasp of the truth conditions for S. Now, according to Dummett, a necessary component of this behavior involves X's ability to recognize, at least in principle, the truth conditions for S as obtaining should they obtain (and, of course, to recognize these conditions as the truth conditions for S.)[37] The problem, as Dummett sees it, is that in cases where S is non-effectively decidable, there is no way for X to recognize the truth conditions for S as obtaining should they obtain. (X cannot, for example, survey an infinite totality of numbers to see if each even number is the sum of two primes.) Dummett concludes that there is no linguistic behavior which constitutes X's understanding of S as that understanding is described truth conditionally. Therefore, assuming that S is meaningful, Dummett concludes that our understanding of S cannot be as that understanding is described truth conditionally. Therefore, if he is right, a truth-conditional theory of meaning for our mathematical

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[37] See, e.g., Dummett [5] pp.224-225.

mathematical statements cannot be complete.[38]

There is the obvious question to raise here: Why must X be able to recognize, at least in principle, the truth conditions for S as obtaining should they obtain in order to understand S truth conditionally? As I understand it, Dummett's response to this question is that to understand a statement S truth conditionally is to "grasp" the truth conditions for S, and to "grasp" the truth conditions for S is to be able to recognize the truth conditions for S as obtaining should they obtain. I believe my reading of Dummett's position is supported by his claim (noted at the beginning of section 2) that an account of X's understanding of S must include some non-verbal behavior, since he thinks a purely verbal account is insufficient to show that X understands S. The reason Dummett thinks that a purely verbal account of how X understands S is insufficient to demonstrate an understanding of S is, I think, quite straightforward. If the schema for the truth conditional model is simple disquotation, then X can state the meaning of S without any understanding of S. (For example, X may know that "The pi-meson is a quark-antiquark pair" is true iff the pi-meson is a quark-antiquark pair even if X has no understanding of "The pi-meson is a quark-antiquark pair".) Furthermore, even if we insist that X explain the meaning of S in other terms, Dummett claims that X is still trapped in a circle. Suppose, for example, that when asked what S means, X replies that S means S* (where S* is synonymous with S). If we continue this process

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[38] In contrast to the positivist, the antirealist does not claim that certain apparently meaningful statements are really meaningless. Rather, the antirealist thinks our understanding of these statements cannot be as that understanding is described truth-conditionally.

by asking what S* means, and if X continues to respond by claiming that S* means S**, S** means S***, and so on, then at some point X will be forced into a circle.[39] Dummett concludes:

> An ability to state the condition for the truth of a sentence is, in effect, no more than an ability to express the content of the sentence in other words. We accept such a capacity as evidence of the meaning of the original sentence on the presumption that the speaker understands the words in which he is stating its truth-condition; but at some point it must be possible to break out of the circle: even if it were possible to always find a synonymous expression (Dummett [5] p.224).

The way Dummett thinks that X breaks out of this circle is through X's exhibiting the appropriate kind of non-verbal behavior. He claims the behavior which exhibits a truth conditional understanding of S involves X's behavior when placed in circumstances through which X can discover whether the truth conditions for S obtain.

To return to our original example, Dummett thinks that because X cannot survey an actual infinite domain of numbers in order to see if each even number is the sum of two primes, X's understanding of Goldbach's

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[39] One might argue that X will not be forced into a circle if his linguistic resources are in some sense infinite, although in that case X will be forced into a regress. A moment's speculation: Here Dummett may be following Wittgenstein, who in Investigations **256-57 argues that, in order for me to follow a rule there must be a criterion other than what I say by which another could judge whether I follow that rule correctly. In these sections Wittgenstein is concerned directly with avowals (e.g., "I am in pain") but the point may be generalized to include all claims to understanding. (See also Kripke [2] p.102n83.)

A related reason one might claim that a purely explicit account of understanding will not do is that it appears that a well-programed computer could give an explicit account of the meaning of S, S* etc., without any understanding of those expressions.

Conjecture cannot be truth conditional.[40] One may respond to this conclusion by noting that we appear to understand many statements truth conditionally even though we are never in position to observe the relevant truth conditions. It is certainly the case, for example, that we appear to understand statements about the past truth conditionally even though we cannot observe past times.[41] Much about this response, however, depends on how we think of our being "in position" to observe the relevant truth conditions. Dummett thinks that we may legitimately conceive of ourselves with extended observational powers as long as those extended powers are linked to our actual observational powers. So, if I understand him correctly, Dummett thinks we may conceive of ourselves checking a very large even number to see if that number is the sum of two primes even though the computation involved is far too great to carry out in practice. But, Dummett claims, we cannot legitimately conceive of ourselves checking an infinite totality of even numbers.[42] Dummett thinks the only way we might explain our ability to make such an observation (by assuming some godlike perspective, by completing an infinite number of tasks in a finite amount of time, etc.) has no explanatory power because the powers involved are not extensions of our actual powers. Thus, he

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[40] X can, of course, say many things about Goldbach's Conjecture: X can say what would count as a proof of it, what follows from it if is it is true, if it is false, etc. But, according to Dummett, this fact only supports his claim that the meaning of a mathematical statement is to be given in terms of the proof-conditions for that statement, not in terms of its truth-conditions. More on this presently.

[41] Although I think we do understand statements about the past truth conditionally, I will not consider antirealism about the past here.

[42] See Dummett [2] pp.99-100.

claims, there is no way of connecting the observation of an infinite totality with how we understand Goldbach's Conjecture.[43] Dummett concludes, therefore, that our understanding of statements such as Goldbach's Conjecture cannot be truth conditional.[44]

(iv). Finally, I turn briefly to the question of what kind of theory of meaning Dummett thinks best accounts for our understanding of our mathematical language. Dummett thinks our understanding of our mathematical language is accounted for by a proof-conditional theory of meaning, which is a special case of a more general assertability-conditional theory of meaning.[45] An assertability-conditional theory of meaning associates with each (declarative, sufficiently non-vague) statement of the language a condition such that that statement is assertable (or, as it is sometimes put, "warrantedly" assertable) just in

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43 I take it that Dummett would reject an appeal to compositionality on similar grounds. That is, it seems to me that Dummett would reject a truth conditional account of how we understand the universal quantifier as it features in Goldbach's Conjecture which appeals to our understanding of the quantifier as it features in statements involving only finite or potentially infinite totalities. In the latter cases there are ways to connect the required observations to our actual observational powers. In the former case there is not. Note the strong conclusion: Our understanding of the quantifier can only be as that ranging over a finite or potentially infinite domain.

44 It is in this context which Dummett introduces the principle that if a statement is true, it must be in principle possible to know it is true (Dummett [2] p.99). I find this principle quite mysterious. I take it that Dummett adopts this principle because he thinks that if the notion of truth is to do any work, it must be possible in principle to know when something is true. Yet I know of no reason why everything in the world should be cognitively transparent.

45 See, e.g., Dummett [5] pp.225-226.

case canonical evidence shows that that condition obtains. What counts as canonical evidence for a mathematical statement is possession of a proof of that statement, or possession of a method by which we could, in principle, obtain a proof of that statement.[46] Thus, Dummett concludes, a mathematical statement S is assertable just in case we have a method for obtaining a proof of S, and its negation is assertable just in case we have a method for obtaining a disproof of S.

There are three points to note about a proof-conditional theory of meaning. First, S is assertable only if we have a method for obtaining a proof of S. Therefore, whether S is assertable is tied to the speaker's knowledge in such a way that assertability, unlike truth, cannot transcend a speaker's knowledge.[47] Second, a proof-conditional theory of meaning satisfies Dummett's requirements (A) and (B). What X must know in order to understand any mathematical statement S is what counts as a proof of S, and what counts as having this knowledge is his ability, in principle, to recognize a proof of S when confronted with one. Third, a proof conditional

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[46] For a discussion of canonical versus conclusive evidence, see appendix A.

[47] It is not the case, however, that an assertability conditional theory supports any kind of individual relativism. You and I may have differing beliefs about some state of affairs P. But our having these different beliefs does not thereby warrant our assigning different assertability values to statements about P. There will be a commonly accepted set of beliefs about P. Call this, loosely, a theory of P. A statement about P is thus warrantedly assertable if generally accepted canons of theory evaluation (whatever they turn out to be) lead us to assert that statement. If these canons should lead us to revise our be'iefs, or, if we should somehow change the canons themselves, we would then be warranted in revising our assignment of assertability values accordingly. Relativism threatens only if there are irreconcilable canons of theory evaluation.

theory of meaning supports the choice of intuitionistic, not classical logic for mathematical reasoning. According to the intuitionist, a statement S is intuitionistically true just in case we have an effective means of producing a proof of S. Therefore, unrestricted bivalence fails. Although I would like to say more about this aspect of Dummett's program, it is difficult to do so since, to my knowledge, no one has a reasonable idea what a proof conditional theory of meaning looks like in detail. Therefore I will stop here.

To summarize section 3: Dummett thinks our classical mathematical practices depend on our acceptance of classical logic, and that acceptance of classical logic for mathematics depends on the acceptance of a truth conditional theory of meaning for our mathematical statements. He argues that our understanding of our mathematical statements cannot be truth conditional. (More precisely, he argues that a truth conditional theory of meaning for mathematics cannot be complete.) He contends that only a proof-conditional theory of meaning can satisfactorily account for our understanding of our mathematical statements, and that a proof-conditional theory of meaning supports an intuitionistic logic for mathematics. He concludes, therefore, that we should abandon classical mathematics in favor of intuitionism.

4. Why Dummett's argument for mathematical antirealism fails.

There are many objections one could raise against Dummett's argument for antirealism. I shall focus, however, on the single issue of why Dummett thinks a truth-conditional theory of meaning is incomplete. I will argue that his reason for thinking a truth conditional theory is incomplete is not well-founded, and that we are therefore free to reject his mathematical revisionism.

We have seen that Dummett thinks a truth-conditional theory is incomplete because he thinks that behavior constitutes understanding, and because he thinks there is no behavior which constitutes an understanding of statements which, when analyzed truth conditionally, are non-effectively decidable. But as I argued in section 2, Dummett has given us no reason why we should accept the claim that behavior constitutes understanding. This alone is sufficient to call into question his conclusion that a truth conditional theory of meaning is incomplete. I think, however, that there is additional reason to think his claim that behavior constitutes understanding is mistaken.

We have also seen that (2), Dummett's claim that behavior constitutes understanding, depends on (1), his claim that X's understanding of E must be publicly manifestable. Now, if Dummett is to have (2), the modality in (1) had better not be some kind of epistemological possibility. If it is, then the claim that X's understanding of E must be publicly manifestable reduces to the unexceptional claim that in order to know what X understands by E, what X understands by E must be publicly manifestable. But as I have already noted, this epistemological reading does not rule out the (metaphysical?) possibility that X understands E in some way that X cannot publicly manifest. Therefore, as I see it, Dummett's argument that a truth conditional theory of meaning is incomplete depends on the claim that X cannot have a private understanding of E.

Consider the following scenario which I introduced in section 2 and which I intend to serve as a counterexample to Dummett's claim that X cannot have a private understanding of E. Suppose X and Y have a sufficiently similar psychological endowment such that each has an innate grasp of a concept c. (Recall that Dummett does not reject this possibility.) Similar training in the use of language leads X and Y to associate an expression E with concept c. Thus, although X and Y understand E in the same way, they need not be able to publicly manifest that understanding. It follows, on this view, that understanding cannot legitimately be identified with behavior.

I am not claiming that the above scenario represents the way understanding works. I am, however, claiming it is possible that it represents the way understanding works, and that before we take antirealism seriously an argument is owed why it cannot be the way understanding works. Dummett offers no such argument. (As I showed in section 2, Dummett's manifestation and acquisition arguments depend on the assumption that there can be no private understanding of E.) I conclude that until Dummett offers some such argument, we need not accept the claim that behavior constitutes understanding, and we are therefore free to reject his mathematical antirealism.[48]

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[48] Burgess makes a similar point on somewhat different grounds. According to Burgess, Dummett thinks that because we do not have conclusive, or skeptic-proof evidence that X understands S when S is non-effectively decidable, we cannot conclude that X understands S. (For more on this issue of skeptic-proof evidence, see appendix A.) Thus, Burgess writes:

> [Dummett] seems to claim that [the absence of skeptic-proof evidence] somehow makes communication between

5. The Private Language Considerations.

I have claimed that Dummett's arguments for antirealism rest on (1), and that (1) rests on Wittgenstein's private language considerations. Here, finally, I had better say something about how I understand these considerations. Although what I write here will be brief, I hope to give the reader some reason to suspect that (1) remains unsecured. I will focus not on the private language considerations as such, but rather on the rule following considerations which, according to at least one interpretation, underlie the private language considerations.

Recall the skeptical paradox.[49] Wittgenstein writes:

> mathematicians impossible, and hence makes mathematics as an activity involving communication impossible. Surely such a claim would be mistaken. For whether mathematicians X and Y succeed in communicating through their use of the expression E surely depends only on whether X and Y do in actual fact attach the same meaning to S, and not on whether they possess skepticism-proof guaranteed knowledge that they do. One hesitates to accuse a distinguished authority on modal logic of arguing from $\Diamond -p$ to $-\Diamond p$, but Dummett does almost seem to wish to move from the (epistemic) possibility that X and Y do not succeed in communicating to the (metaphysical) impossibility of X and Y not succeeding in communicating" (Burgess [1] p.183).

I think Burgess is correct that Dummett has confused the metaphysical with the epistemological, but not for the reason he gives. As I argue in appendix A, Dummett's rejection of a truth-conditional theory of meaning has nothing to do with a requirement that in order to ascribe an understanding of E to X we must have skeptic-proof evidence that X understands E.

49 Here I follow Kripke [2]. For present purposes I will assume that Kripke has interpreted Wittgenstein's views correctly, even though I recognize

> This was our paradox: no course of action could be determined by a rule, because every course of action can be made out to accord with the rule. The answer was: if everything can be made out to accord with the rule, then it also can be made out to conflict with it. And so there would be neither accord nor conflict here (Wittgenstein [1] #201).

Kripke spells this out with the following example.[50] We suppose that "+" denotes the addition function which we may define in the usual way:

$$(\forall x)\ x+0=x$$
$$(\forall x)(\forall y)\ (x+y)'=x+y'$$

Now, suppose X has never added numbers greater than 56, and that he is now to add 57+68. X does this and gets 125. But X is now invited to imagine a skeptic who points out that as X used "+" in the past, the result of his computation should be 5, not 125. The skeptic claims that in the past X used "+" to denote another function which we will call "quus", symbolize by "⊕", and define as:

$$(\forall x)(\forall y)\ x\oplus y = \begin{cases} x+y, & \text{if } x,y<57 \\ 5 & \text{otherwise.} \end{cases}$$

Now, we think there must be some fact about X's past usage to which X can appeal in order to show that he does not mean quus. Yet X's past usage is compatible with his following either rule. Therefore, since there is nothing to which X may appeal in order to show that X was not following the quus rule, Wittgenstein concludes that there is nothing about X which determines which rule he is following. He concludes, therefore, that there

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that this is a controversial matter.

[50] See Kripke [2] pp.7-21.

are no facts about what X meant by "+".[51]

What the previous example is taken to show is that when faced with a novel situation, X proceeds blindly and without justification.[52] But if X proceeds blindly, then whatever strikes him as right at the moment is right, and in that case Wittgenstein claims there is no sense to be made of the claim that X is engaged in a meaningful practice.[53] Wittgenstein concludes, therefore, that the individual can only engage in a meaningful practice within the wider context of a community of practitioners; X proceeds correctly only when he proceeds in accordance with his community.

I see a problem with this view.[54] Let us assume that X's past usage of E is compatible with his following either rule. Yet the only way I see of getting from the assumption that X's past usage is compatible with his following either rule to the conclusion that there is no rule which X follows is by assuming that X's usage constitutes his following a rule. (As Kripke quite correctly points out, the skeptical paradox is not meant to be an epistemological problem.[55] If it was simply an epistemological

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51 Kripke [2] p.77. There is nothing particular to mathematics here. The conclusion is supposed to be general for all expressions.

52 See Kripke [2] p.87.

53 See Wittgenstein [1] *202.

54 I set aside the many problems I see with trying to explain what a linguistic community is.

55 See Kripke [2] p.21.

problem, then the fact that X might not know which rule he was following would not license the conclusion that he is not following a rule.) But how can usage constitute rule following? Here it may be useful to review the way I claim Dummett reaches the conclusion that behavior constitutes understanding.[56]

Recall that Dummett's argument for (2) proceeds by claiming that we should not ascribe an understanding of E to X without evidence. Then, because the only relevant evidence is X's linguistic behavior, the account of what it is for X to understand E can be given in purely behavioral terms, hence (2). Suppose we try the analogous argument for the claim that usage constitutes rule following: We should not think that X is following a rule without evidence. Then, because the only relevant evidence that X is following a rule is X's past usage, the account of what it is for X to follow a rule can be given in terms of usage (that is to say, in terms of X's behavior). But, because X's usage is compatible with his following different rules, and usage constitutes rule following, we may conclude that X is not following a rule, at least when X acts in isolation from the community.

The problem I found with Dummett's argument is that it rests on the assumption all understanding must be manifestable. I see a similar problem with the analogous Wittgensteinian argument. The Wittgensteinian argument depends on the assumption that what rule X is following must be discoverable through X's usage. (Otherwise, the fact that we cannot tell

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[56] See section 2iii.

which rule X is following does not license the conclusion that X is not, in fact, following a rule.) But it is unclear to me what the grounds for this last assumption are. I suspect there are none that do not somehow beg the question at hand. I contend, therefore, that much more needs to be said about why the private language considerations should be taken seriously, and much more needs to be said about how they might provide a foundation for antirealism. In lieu of this account, it seems to me that Dummett's antirealism remains unsecured.

Appendix A: Dummett on conclusive verifiability.

There is some confusion about the role "conclusive verifiability" plays in Dummett's account of when a statement is true. Consider, for example, the following passages:

> [...] an understanding of a statement consists in a capacity to recognize whatever counts as verifying it, i.e. as conclusively verifying it as true (Dummett [2] p.111).

> A verificationist theory represents an understanding of a sentence as consisting in a knowledge of what counts as conclusive evidence for its truth (Dummett [2] p.132).

There are two ways one might understand what Dummett means by "conclusive verifiability" here. The first way involves the claim that, for example, I have conclusively verified the truth of S and I hold that claim to be indefeasible. Call this <u>strong</u> conclusive verification. The second way involves my claim to have conclusively verified the truth of S but hold that claim to be defeasible. Call this <u>weak</u> conclusive verification.

The failure to distinguish strong from weak conclusive verification has led to what I take to be some serious misunderstandings of Dummett's position. Burgess, for example, has argued that Dummett's antirealism is based (in part) on the mistaken requirement that to ascribe an understanding of a statement S to a speaker X, we must be able to strongly conclusively verify that X understands S.[57] Now, because Dummett restricts evidence of X's understanding of S to how X has used S in the past, the only evidence that X understands S in a particular way is just

57 Burgess [1] pp.182-83.

that X has used S in that way. But, as Burgess correctly points out, no amount of past evidence will guarantee how things will go in the future; X may use S in a particular way up to any point, yet it is always possible that X's future use of S will diverge sufficiently from past use to reveal that X does not understand S in the way previously ascribed to him. Therefore, Burgess concludes, the hypothesis that X understands S in a certain way can never be strongly conclusively verified. Therefore, if X's understanding of S depends on our ability to strongly conclusively verify that X understands S, X must not understand S. Furthermore, this conclusion holds independently of whether X's understanding is characterized by a truth conditional theory of meaning or an assertability conditional theory of meaning.

I interpret Dummett's use of "conclusive verifiability" using the weak reading. My interpretation is supported by the fact that, as I understand it, weak conclusive verifiability functions in the same way as assertability. Recall that according to the assertability-conditional theorist, we are to accept the accumulation of canonical evidence that X attaches a certain meaning to S as reason to ascribe that understanding of S to X. If X's future use of S reveals a divergence from this use, we revise our ascriptions accordingly. Note, therefore, that the weak reading does not undermine an assertability conditional account of what it is for X to understand E. Only when we do not know what behavior would count for understanding – as Dummett thinks is the case with truth-conditionally characterized undecidable sentences – is the case different. In that case there can be no strong or weak conclusive verifiability since, according to Dummett, there is no behavior which exhibits such understanding, hence no such understanding.[58]

Although I think Burgess has misinterpreted Dummett's use of "conclusive verifiability", much of the blame for this misunderstanding must rest with Dummett. Surely the more natural way to read "conclusive verification" is as strong conclusive verification. Nevertheless, I think the issue of how we understand Dummett's use of "conclusive verifiability" is really just one of terminology. If we go through Dummett's writings and replace "conclusive verification" by some other phrase (perhaps "canonical verification"), certain confusions may be cleared up. But this change will not effect the more serious objections to Dummett's program presented in the body of this paper.

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58 Burgess may have been misled by a comment of Chihara's. Burgess notes Chihara's comparison of Dummett's views about understanding with Malcolm's views about dreams (Burgess [1] p.180n4). Malcolm claims that the only evidence we have of X's dreaming is his behavior, his willingness to tell dream stories upon awakening. Malcolm concludes that there are no dreams, only a disposition to tell dream stories. Although Dummett claims the only evidence we have that X understands S is X's behavior, he does not conclude that there are no mental phenomena underlying understanding as Chihara seems to think. Rather, Dummett claims only that reference to these mental phenomena can play no role in an account of what it is to understand a language. Therefore, as I understand it, Chihara's comparison is misplaced.

Appendix B: Platonism.

As I understand it, platonism is the doctrine that there are abstract mathematical objects whose existence is independent of our existence, and whose various properties and relations make our mathematical statements true or false independently of what we judge to be the case. Now, if we assume that a statement S is true just in case there is some state of affairs by virtue of which S is true, it is then easy to see why acceptance of unrestricted bivalence for our mathematical statements pushes one naturally (and perhaps inevitably) in the direction of platonism. If, for example, Goldbach's Conjecture is true, then there must be some state of affairs by virtue of which it is true. And if we think that the truth of Goldbach's Conjecture is independent of what we judge to be the case, then the state of affairs by virtue of which Goldbach's Conjecture is true or false must be independent of what we know to be the case. What, then, is more natural than a platonic realm of mathematical objects to make up this state of affairs?

Some brief comments are in order here. First, as we have seen, Dummett thinks our acceptance of classical mathematics does not turn on a decision about whether or not there are these platonic objects. Rather, he thinks that metaphysical questions about the existence and nature of mathematical objects are best addressed by considering what kind of theory of meaning best represents our understanding of our mathematical statements. Once we have answered that question, he thinks the "right" metaphysical" position falls out.[59] (And, because Dummett thinks our

[59] See Dummett [5] p.229.

understanding of our mathematical statements is best represented by an assertability conditional theory of meaning, he concludes that the "right" metaphysical position is constructivist.)

I think Dummett's view that semantics precedes ontology gives us some insight into Kreisel's dictum that the important issue in the philosophy of mathematics is not the existence of mathematical objects but the objectivity of mathematical truth.[60] As I understand it, Kreisel's point is that we must first give an account of what it is for a mathematical statement to be true independently of what we judge to be the case. Whether we should then accept the view that there are mathematical objects depends on the account of objectivity. Platonism offers the clearest solution to this problem; our arithmetical procedures give us insight into the way the platonic arithmetical realm is structured, but the way it is structured is independent of our procedures. As I see it, therefore, the important challenge facing the antirealist (and other constructivists) is whether there is viable anti-platonist account of mathematical objectivity. I do not know of one.[61]

My second point is that the acceptance of a platonist ontology may not be an inevitable consequence of accepting unrestricted bivalence for our mathematical statements. One obvious way of avoiding commitment to a specific realm of platonic objects is to argue that numerical singular

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[60] See, e.g., Dummett [5] p.228-29.

[61] I evaluate some ways the intuitionist might attempt to develop an antiplatonist account of mathematical objectivity in chapter 3.

terms function syntactically but not semantically as singular terms. This, I take it, is the gist of Benacerraf's suggestion that arithmetical statements should be interpreted as statements about any progression which satisfies the Dedekind-Peano axioms.[62] Interpreted in this way, for example, an expression such as "the number 4" is a kind of dummy name for the 5th element in any such progression. Thus, following Benacerraf, one might argue that every arithmetical statement is true or false yet deny the existence of specifically arithmetic objects inhabiting a platonic realm. I will not attempt to evaluate this view here.

Finally, we might think of one's rules for, say, addition as reflecting how things stand platonically. Yet one consequence of Wittgenstein's skeptical paradox is that there are no such rules. Note, therefore, that the skeptical paradox is _prima facie_ incompatible with platonism. Yet if the arguments I presented in section 5 are correct, the Wittgensteinian still owes an account of why there cannot be these private rules which regulate our mathematical practices. I conclude that without further argument, platonism is not undone by the rule following considerations.

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62 Benacerraf [1].

5. CONCLUDING REMARKS

It may seem to the reader as if very little progress has been made in answering the questions with which I began this essay. I have, for example, addressed the ontological question of whether there are numbers simply by assuming that there are. With respect to the metaphysical question of what kind of objects the numbers are, I have argued that they are neither descriptions of mental processes nor antirealist constructions. I have not, however, put forward a claim as to what I think the numbers are. One might conclude on the basis of what I have written that I believe platonism wins by default. That conclusion, however, would be premature at best. It is not clear to me that we have a reasonable idea of what the platonist position really comes to. The rather crude picture of an abstract domain of mathematical objects which we explore through our mathematical cogitations is fraught with difficulties. (To name just the obvious one, it remains completely mysterious how we might have knowledge of these objects.) Moreover, Dummett's point that the "right" metaphysical account of the numbers may be only a kind of by-product of how we understand our mathematical statements has some appeal. One issue that needs to be considered in much more depth, therefore, is what the proper relation between semantics and metaphysics is.

In chapter 1 I noted that if a causal theory of knowledge is truly incompatible with our having arithmetical knowledge, then the obvious conclusion to draw from that incompatibility is that arithmetical knowledge is not causal knowledge. (I know of no reasonable grounds for abandoning mathematics for a questionable epistemology.) Abandoning the causal picture, however, does not leave us without alternatives. It is clear that some knowledge is not causal. Consider, for example, the statement:

> (1) No book on my desk is simultaneously red and blue all over.

(1) is clearly true. There is, moreover, good reason to think that my knowledge that (1) is true is not causally acquired, at least in the way Benacerraf thinks. If my knowledge that (1) is true is causal, then, following Benacerraf, there would be some causal relation obtaining between the books on my desk and my belief that (1) is true. But my belief that (1) is true no more involves the books on my desk than my knowledge that no books in the Venerable Bede's library were ever simultaneously red and blue all over involves any causal knowledge of his library. Rather, I know (1) is true because I know apriori that nothing can be simultaneously red and blue all over. The moral of this story is not, of course, that mathematical knowledge is apriori knowledge. Rather, the moral is that some knowledge is not causal, and if mathematical knowledge is apriori in this way, then Benacerraf's causal model is clearly inapplicable. It seems to me, therefore, that a reasonable strategy to pursue at this point is to re-examine the traditional view that mathematical knowledge is apriori knowledge, and to consider what metaphysical picture of the numbers emerges from that examination. I will not attempt to undertake this study here. I will, however, briefly mention some relevant issues, especially as they relate to the work presented in the earlier chapters of this essay.

As Frege observed, the apriori/aposteriori distinction has to do with the grounds used to justify one's judgment that a statement is true.[1] In discussing _apriori_ truths, therefore, I will take as a starting point the view that _apriori_ truths are truths the justification of which do not depend on evidence from sense experience. (As Frege warned, one may confuse the logical grounds for holding a statement true with the psychological grounds by which one may come to believe that the statement is true. The psychological grounds may, of course, involve particular sense experiences. The logical grounds do not.)

There are, roughly speaking, two ways we might know a statement to be true _apriori_. First, we might know a statement is true immediately or non-inferentially. Second, if we accept the view that deductive logic preserves the _apriori_ nature of truth (a claim which I accept but am aware may require defending), then we may also come to know that a statement is true _apriori_ by deducing the truth of that statement from premises which are known to be true _apriori_ in the first sort of way. It seems to me, therefore, that the obvious point on which to concentrate our attentions is on truths known immediately or non-inferentially.

So far as I am aware, there are two ways we might know certain statements to be truth immediately or non-inferentially. The first way involves intuition. Parsons has spelled out one way intuition might work, although if the arguments I presented in chapter 2 are correct I do not see how Parsons's account of intuition could give us the numbers. (I should note that Parsons does not tie his account of intuition to the claim that

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[1] Frege [1] #3.

mathematical statements are true apriori.[2]) I suspect that any other account of intuition developed along similar lines would face similar difficulties.[3] Finally, it appears to be the case that one's intuition of an object is an aposteriori process. It is unclear, therefore, how intuition can lead to apriori knowledge.[4]

The second way we might know that a statement is true apriori is by reflection on the logical interconnection of concepts known apriori. (For present purposes I will simply assume that the apriori/aposteriori distinction is applicable to concepts. I am aware that this assumption needs some spelling out, but I do not think it is too controversial.[5]) For example, we might know that a statement is true apriori through our innate possession of the concepts required to understand that statement. (It may not be the case that a concept known apriori must be innate. For the moment, however, I will use the example of an innately held concept to illustrate my point.) Then, by ruminating on the relations between these concepts using our (apriori-preserving) logical machinery, we come to know the truth of new statements apriori. If these basic concepts include

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2 See, for example, Parsons [3] p.159.

3 It is not clear to me whether one should develop Godel's account of intuition in a similar way. It is not even clear to me that Godel's account can be coherently developed. (See Parsons [3] pp.145-146.)

4 This last question surely calls for a Kantian answer. I suspect that in order to fully understand Parsons's theory of intuition a detailed study of this aspect of Kant's theory will be required.

5 Because of what he saw as the interdependence of concepts and intuition, Kant would see these two ways of knowing truths apriori as inseparable.

those sufficient for basic arithmetic, then we are well on the way to demonstrating our arithmetical truths are, or can be, known apriori.

I am aware that any attempt to develop the view that arithmetical knowledge is apriori conceptual knowledge faces formidable difficulties. One obvious difficulty involves giving an account of how concepts known apriori relate to objects. That is, if we suppose that we possess those concepts which are responsible for our basic arithmetical knowledge, what is it for there to be objects corresponding to these concepts? (Here I suspect the path may lead back to Frege.) A second, though related difficulty is that considered in earlier chapters having to do with how we know the number series is infinite. I do not see how our knowledge that the number series is infinite could involve only innately held concepts. Moreover, as is well known, there is nothing in logic (at least as logic is currently conceived) which tells us what there is or what there can be. It is unclear to me, therefore, how our knowledge that there is an infinity of numbers could be apriori.

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