Order Number 9224793

Knowledge of mathematical objects

Balaguer, Mark Augustan, Ph.D.

City University of New York, 1992

U·M·I
300 N. Zeeb Rd.
Ann Arbor, MI 48106

A

KNOWLEDGE OF MATHEMATICAL OBJECTS

by

MARK BALAGUER

A dissertation submitted to the Graduate Faculty in Philosophy in partial fulfillment of the requirements for the degree of Doctor of Philosophy, The City University of New York.

1992

This Manuscript has been read and accepted for the Graduate Faculty in Philosophy in satisfaction of the dissertation requirement for the degree of Doctor of Philosophy.

April 28, 1992
Date

Chair of Examining Committee

4/28/92
Date

Executive Officer

Arnold Koslow

Hartry Field

Jerrold Katz

Charles Landesman

Elliott Mendelson
Supervisory Committee

THE CITY UNIVERSITY OF NEW YORK

Abstract

KNOWLEDGE OF MATHEMATICAL OBJECTS

by

Mark Balaguer

Adviser: Professor Arnold Koslow

This dissertation provides a refutation of the epistemological argument against mathematical platonism; that is, it provides an epistemology of abstract objects, in particular, of <u>mathematical</u> objects.

After an introductory first chapter, I formulate (in chapter two) what I argue is the strongest version of the epistemological argument against platonism. It is stronger than Paul Benacerraf's version because the only plausible way for a platonist to respond to it is to actually provide an epistemology of mathematical objects.

In chapters three and four, I argue against the platonist epistemologies of Kurt Gödel and Penelope Maddy, respectively. The main conclusion of these two chapters is

that human beings cannot come into contact with mathematical objects, and that, indeed, it may not even be intelligible to speak in these terms, i.e., to speak of contact between spatio-temporal creatures like ourselves and an aspatial, atemporal mathematical realm.

In chapter five, I argue (following Jerrold Katz and David Lewis) that human beings don't need any contact with the mathematical realm in order to attain knowledge of that realm. The argument is based upon the fact that mathematical truths are necessarily true.

Finally, in chapter six, I develop a second platonist epistemology, i.e., a second refutation of the epistemological argument against platonism. The argument is mostly original, but it relies at one point upon an argument of Michael Resnik's. In a nutshell, my argument is that, since platonists are free to adopt a full-blooded sort of platonism (i.e., a version of platonism which holds that all possible mathematical objects actually exist) it is easy to explain how human beings could attain knowledge of the mathematical realm. For if all possible mathematical objects exist, then any consistent mathematical theory will accurately describe some collection of mathematical objects. Hence, to attain knowledge of the mathematical realm, one must simply formulate a consistent set of mathematical beliefs.

Acknowledgements

I would like to express my appreciation to Adam Vinueza and Stephen Schiffer for reading the final chapter of this dissertation and offering helpful comments; and to Jerrold Katz and Hartry Field for their comments on not only on the last chapter, but also large portions of the rest of the dissertation; and to Charles Landesman and Elliott Mendelson for their helpful comments on the entire work. Most of all, I would like to thank my advisor, Arnold Koslow, who scrutinized the dissertation in the minutest detail and provided innumerable valuable insights.

I would also like to express a different sort of indebtedness to Hartry Field and Jerrold Katz, as well as to Michael Resnik. My philosophical viewpoint has been greatly influenced by the published works of each of these philosophers. Although the argument of the final chapter of the dissertation is original, the position is quite similar to Resnik's, and it was his work which first set me to thinking about this position. Katz's epistemology, while quite different in some respects, is also quite similar to mine; moreover, I have also benefitted a great deal from his general formulation of the platonistic standpoint. Finally, and perhaps surprisingly, my view is quite similar to Field's: he has tried to tell a fairy tale about mathematical entities; I believe large portions of it to be true.

Table of Contents

Chapter One

Introduction

1. Mathematical Platonism

It's not clear what mathematicians do. One view is that, like empirical scientists, they study the world, but that, unlike empirical scientists, they do not study the physical part of the world. Thus, on this view, mathematical theories are descriptions of a mathematical realm, i.e. a non-physical, non-mental aspect of reality that exists outside of space and time. This view (which has been defended by Plato, Gödel, and Frege, among others) is called mathematical platonism.

There are many different versions of mathematical platonism. Perhaps the most popular (and, perhaps, the most naively realistic) is what might be dubbed "object platonism." On this version of platonism, the mathematical realm consists of a collection of abstract, mathematical objects, and the job of the mathematician is to describe the nature of these objects. Thus, for instance, arithmetic is, on this view, the study of numbers and their properties; we learn from arithmetic, for example, that the object 3 has the property of being prime. Mathematical objects, on this view, are abstract.[1] That is, they are non-physical, non-mental, unchanging, eternal entities which existed before the human race evolved and, indeed, before the physical universe was formed. (Platonists might want to refrain from calling abstract objects "eternal" and from claiming that they existed "before" there was a physical universe; the reason would be to emphasize that such objects are atemporal, as well as aspatial.)

Not all platonists, however, believe that mathematics is a study of objects. Some think that the mathematical realm consists primarily of mathematical facts, such as that 2 + 2 = 4. This is still a version of platonism, because these facts are taken to be non-physical, non-mental and non-empirical. This view might be called "fact platonism."

Another version of platonism in which the notion of a mathematical object is questioned is "structuralism." According to this view, the mathematical realm consists of abstract structures, or patterns, and the job of the mathematician is to describe the nature of these things. Thus, on the structuralist account, mathematicians are completely uninterested in the intrinsic properties of objects; all they are concerned with are the relationships that hold between objects. For example, arithmetic is taken to be the study of a certain abstract structure (i.e., a certain "objectless template") which is characterized by Peano's axioms, and which contains an infinite sequence of positions or place holders.[2] Now, any denumerably infinite sequence of objects could "fill" these positions and, thus, "act" as the numbers; but arithmetic, according to structuralists, is not concerned with any of these sequences of objects; it is concerned, rather, with the structure or the pattern that they all share. So, on this view, there is no _one_ thing that _is_ 3. But most structuralists are still platonists, for they take mathematical structures to be real, objective, and abstract.[3]

Now it seems to me that one might undermine the distinction between structuralists and object platonists by arguing that structures _are_ objects. But one could imagine a more full-blooded no-object platonism, in which the mathematical realm is taken to be a "unity." Thus, the mathematical realm would just be an intelligible realm, not intrinsically divided up at all. Stewart Shapiro seems to accept something along these lines. According to his version of platonism,

> the nature of the universe exists pretty much independently of us and our linguistic lives...but...the way the universe is divided into objects depends on linguistic resources.[4]

So, on this view, the divisions in the mathematical realm are all man-made; when we think of a particular number, like 3, or of the structure of Peano Arithmetic, we are thinking (accurately) of certain parts of an objective mathematical realm; but none of these parts are really _separate_ from the rest of the mathematical realm. One might think of this along the lines of the distinction between the Mediterranean Sea and the Atlantic Ocean: we can meaningfully and accurately speak of this distinction, but, in the final analysis, these two bodies of water aren't really _separate_ things.[5]

The present essay is a defense of the most general version of mathematical platonism, i.e., the version that simply doesn't commit to any particular version. All I commit myself to is what all mathematical platonists have in common, namely the belief that our mathematical theories describe some sort of non-physical, non-mental aspect of reality -- i.e., some sort of mathematical realm. I refrain from committing to any particular version of platonism for three reasons. First, I do not want to have to answer any objections which are directed against a particular version of platonism. Second, if it turns out that there is a mathematical realm, and if, moreover, it turns out that our mathematical theories accurately describe parts of this realm, then I would like to be able to say that I was right. Thus, I state my form of platonism so that it will be vindicated by any scenario in which mathematical theories can be construed as descriptions of a non-physical, non-mental aspect of the world. Third, I do not think that there are any good arguments for any of the particular versions of platonism. I believe that all we can know about the mathematical realm is what our mathematical theories tell us about it. Thus, I simply don't know whether the mathematical realm is made up, primarily, of objects, facts, structures, or something else. (I will not discuss why we cannot attain this sort of knowledge about the mathematical realm, but it should become relatively clear -- in chapter six -- when I explain how we can know anything about the mathematical realm.)

But while I only mean to defend the most general version of platonism and don't mean to commit to the existence of mathematical objects or mathematical facts, I will often speak of such things, simply for the sake of brevity. There is no harm in this, for the use of any one of these terms could be eliminated if the need arose. Until then, however, I will help myself to all of them. This is simply for convenience: in some connections, it's easiest (or most appropriate) to speak of "mathematical objects," while in others, "mathematical fact" or "mathematical structure" serve better. But, in spite of this, all I really mean to commit myself to is some sort of mathematical realm.

2. The Project of Establishing Platonism

To establish any philosophical position, one must accomplish two tasks:

(I) The task of giving arguments in favor of the position

and

(II) The task of debunking the arguments against the position.

The present essay is a defense of platonism, rather than an argument for platonism. Thus, I will only be concerned with task (II). Moreover, I will only try to partially accomplish task (II), for I will only concern myself with one argument against platonism, namely the epistemological argument, which has been elaborated by Benacerraf,[6] among others. Now, I actually believe that by debunking this argument, task (II) will be <u>completely</u> accomplished (because I don't think there are any other good arguments against platonism) but I will not attempt to support this claim here. I will concern myself entirely with the refutation of the epistemological argument against platonism.

In chapter two, I will formulate the epistemological argument precisely, but in a nutshell, the argument is that platonism is unacceptable because it claims that mathematicians know things about a mathematical realm to which they have no access. In other words, the argument is that our mathematical theories could not be descriptions of a non-physical part of the world, because, even if there <u>was</u> a non-physical part of the world, we would have no way of finding out what it was like, and so we could not describe it.

Before I turn to the task of refuting this epistemological argument against platonism, I would like to say something about why we ought to be platonists in the first place. That is, before I attempt to accomplish task (II), I would like to at least say a few words about how platonist might go about accomplishing task (I). This, I think, is necessary in order to motivate the discussion of task (II); for if we have no independent reason to accept platonism, then it would seem to be a waste of time to try to defend it against the Benacerrafian challenge. Now, I suppose that one could question this, and take task (II) to be logically prior to task (I). One might argue for this in the following way. A philosophical argument against a theory is an attempt to show that the theory in question is what might be called a "non-starter." The point of the argument is to show that we need not consider reasons for accepting the theory, because there is a flaw in the theory that shows that it couldn't be true. Thus, in this light, it seems that task (II) -- i.e., the task of trying to rebut such arguments against a theory -- ought to be accomplished before we move on to the question of whether the theory is true. In our case, this would suggest that we ought to consider the epistemological argument against platonism before we proceed to consider the reasons for believing that platonism is true.

But whether or not (II) is logically prior to (I), it seems that if we can say something about task (I), it would be welcome. It would, if nothing else, provide additional justification for attempting to accomplish task (II). Thus, what I would like to do in the next two sections is sketch an argument for platonism. I will not try to establish that my argument is conclusive, for (a) such a project would take far too long, and (b) I simply don't need to completely accomplish task (I) in order to motivate a discussion of task (II).

2.1 A Sketch of the Full-Blooded Argument for Platonism

It seems to me that there is only one way to argue for mathematical platonism, namely via the process of elimination. One must take on all comers and show that platonism is the only philosophy of mathematics that can account for everything that needs to be accounted for. This is because platonism commits us to a huge ontology of abstract objects. If we could do without such abstract objects and still account for everything that needs to be accounted for, it would be desirable, because we would be simplifying our ontology, without damaging our explanations. (Of course, many people think it would be desirable for another reason, viz. because they simply don't believe that there are any such things as abstract objects.) The reason it is desirable to simplify our ontology is that we accept Ockham's Razor, which tells us that if we don't have to believe in a group of objects, then we shouldn't, or, more precisely, that an ontologist ought not to "multiply entities beyond necessity," i.e. beyond what is needed for explanation. According to this principle, then, if we could explain everything that requires explanation without positing the existence of mathematical objects, it would be desirable, for it would make our ontology more parsimonious and, perhaps, make our worldview simpler and more elegant.[7]

Now, platonists will not generally deny that if anti-platonism could be made to work, then it would be superior to platonism for reasons of ontological parsimony. But they don't think that anti-platonism can be made to work. That is, they doubt that we will be able to account for mathematical truth (and other things that require explanation) if we don't posit abstract objects. Thus, in order to support this, platonists must consider each anti-platonist philosophy of mathematics in turn and show that it leaves something unexplained. If one of these anti-platonist positions explains everything that platonism explains, then platonists will be obliged to concede the point, for anti-platonists will have shown how our ontology can be made more parsimonious without impeding our ability to explain. But if none of them can explain everything that platonism can, then we will have a positive argument for the latter.

What I would like to do now is briefly consider some of the better known anti-platonist philosophies of mathematics and indicate some of the problems with each of them. I will not consider <u>all</u> anti-platonist positions, nor completely refute those that I <u>do</u> consider. Again, this is because I haven't the space nor the need to do better. All I want to do is provide <u>some</u> motivation for platonism. That is, I want to explain why a scientifically minded person might believe in abstract objects.

One anti-platonist avenue that has been explored by philosophers such as John Stuart Mill is that mathematics is about ordinary physical objects, after all. On this view, mathematics is simply the most general of the natural sciences. Just as astronomy gives us laws concerning the behavior of all astronomical bodies, arithmetic gives us laws concerning the behavior of all objects. The sentence "2 + 1 = 3" says that whenever we add one object to a pile of two objects, we will end up with three objects. Higher mathematics, on this view, is just an abstraction from simple facts like this one. We learn that "2 + 1 = 3" is true from our manipulations of physical objects, and we then abstract away from these objects to form ideas about pure mathematics. Thus, we can state that "17,000 + 1 = 17,001" is true without actually checking it out; we do this by simply extending the practice of addition in the obvious way. But what this sentence really says -- according to Mill -- is that whenever we add one object to a pile of seventeen thousand objects, we will always get a pile of seventeen thousand and one objects. It does not say anything about any abstract, non-physical objects. Mathematical sentences could not be about abstract objects, for, on this view, such objects simply don't exist, and true atomic sentences (such as "three is prime") can only be about existing things.[8]

There are many problems with Millian empiricism, but since there is almost no one that needs to be convinced of this (i.e., since almost no one still holds this view[9]) I will only mention two of them. The first was originally noticed by Frege;[10] it's that Mill's view leaves us with nothing to say about sentences like "-2 + -13 = -15." We obviously can't say that we learn the truth of this sentence through abstraction, but that what it really says is that whenever we push negative-two objects together with negative-thirteen objects, we will always get negative-fifteen objects. It's not clear that we can say anything about this sentence without abandoning Mill's view. Frege's version of this criticism is slightly different. He derides Mill for only defining "3" and not illustrating "the physical facts underlying the numbers 0 and 1."[11] Frege's reason for choosing these numbers is that all other numbers can be derived from them. But I think the difficulty is more conspicuous in connection with negative numbers, for (a) it's even less clear how sentences about such numbers could be about physical objects, and (b) it's not entirely obvious that it matters -- in this connection -- that the negative numbers can be derived from the natural numbers, for it's not clear that Mill could incorporate this fact into his view. The second difficulty with Mill's view derives originally (if I'm not mistaken) from Hempel.[12] The objection here is that we simply don't treat arithmetical truths like laws of nature, as Mill's view suggests that we must. Any statement that we take to be a law of nature (i.e., any <u>lawlike</u> statement) is refutable. For instance, if we found a satellite that followed a circular orbit, we would abandon the law that all satellites follow elliptical orbits. But if we added one object to a pile of two objects and got four objects, we would <u>not</u> abandon the arithmetical truth that "2 + 1 = 3;" rather, we would assume that a fourth object was somehow produced by the process of pushing the objects together. The fact is that empirical evidence could <u>never</u> persuade us to abandon the arithmetical truth that "2 + 1 = 3," and this seems to imply that it is not a natural hypothesis, i.e., a candidate for a law of nature.

A second anti-platonist school of thought that tries to account for mathematical truth without positing abstract objects is psychologism. Unlike Millian empiricism, which holds that mathematics is about ordinary physical objects, psychologism holds that mathematics is about mental objects. Thus, on this view, "3 is prime" is true because there is a mental entity (viz., the idea of 3) that has the property of being prime (and primeness here is, of course, a mentally constructed property which applies only to other mental entities, and, in particular, only to numbers). The biggest difference between psychologism and platonism is that according to the latter, numbers are objective entities about which mathematicians discover facts, whereas according to the former, numbers are human creations, which mathematicians are free (within certain limits) to construct as they please.

There are many problems with psychologism (such as that we seem unable to talk about, for instance, the class of all real numbers, since human beings could never construct them all) but, again, there aren't many advocates of psychologism left, and so I will just briefly mention three arguments, which are all Fregean in spirit.[13] First of all, if psychologism was true, we would need to have five billion arithmetical theories -- one for each person on the planet -- and each of these theories would be true only of the ideas of the person in question. Thus, "3 is prime" might turn out true for Jones but false for Smith. But this is impossible: "3 is prime" is true regardless of what Smith thinks. In fact, it's true regardless of what anybody thinks, and this brings me to a second criticism: psychologism implies that if our brains were structured differently, "2 + 1 = 3" might be false, which seems absurd. Finally, psychologism seems to reduce mathematics to psychology. But this would be to rest the necessary truths of mathematics upon the contingent truths of psychology.

It is conceivable that the first two criticisms could be avoided by coming up with an argument for certain universal hypotheses about minds. And one might try to avoid the third criticism by claiming that while mathematical objects are mental entities, mathematics is not about the mind. But it's not clear, on this view, what mathematics would be about, or why it would be true. Hartry Field has voiced this criticism: he writes that "mathematical idealism...[is] not only too obscure to assert, but also too obscure to deny."[14] And he also claims that it's unclear how such a view differs from fictionalism (the view that mathematics is false). This seems right: is there any difference between saying that a theory T is a fictional story (and, thus, not true) and saying that T is true, because the "characters" of the story exist in the mind?

It might be natural at this point to discuss fictionalism, but before I do that, I would like to discuss conventionalism, a third anti-platonist philosophy of mathematics (i.e., a third attempt to account for mathematical truth without countenancing abstract objects). We have seen that we cannot claim that mathematics is about physical objects or mental objects. Conventionalism (whose proponents have included Carnap, Ayer and Hempel[15]) holds that mathematics is true, but not about anything. The basic idea here is that mathematical propositions are analytically true and, therefore, factually empty. In other words, conventionalists deny that mathematical (and logical) propositions are true in virtue of some sort of correspondence with an objective part of the world; they claim, instead, that such propositions are true solely in virtue of their meanings, i.e., in virtue of certain conventions about the words appearing in them.[16]

Now, clearly, the view here cannot be that each mathematical truth is itself a convention; the claim is, rather, that we accept as conventional the axioms and the rules of inference of a mathematical theory, and that we then derive the rest of the theory. (This must be the case, because while there are only finitely many conventions, there are infinitely many mathematical truths.) Now while this fact may seem quite banal, it is actually the undoing of conventionalists, for they will be unable to derive any propositions that aren't explicitly assumed. To see why, consider a mathematical system in which p and (p --> q) are axioms, i.e., in which these two statements are explicit conventions. Can we now derive q? In "Truth by Convention," Quine borrows the following argument from Lewis Carroll to show that we cannot. Before we can derive q, we will need the following:

$[p \wedge (p \rightarrow q)] \rightarrow q$.

Now, perhaps a particularly slow conventionalist will oblige and write this down as a further convention, or axiom. But, of course, we will still be unable to infer q, for we will be lacking <u>this</u>:

$(p \wedge (p \rightarrow q) \wedge ((p \wedge (p \rightarrow q)) \rightarrow q)) \rightarrow q$.

But it will, by now, be obvious to anyone that it will be futile to claim that this sentence is also a convention, for it is clear that we can carry on this process <u>ad infinitum</u>, and that, therefore, conventionalism will not be able to account for mathematical truths that are not explicitly assumed.

Dummett has suggested[17] that Wittgenstein held a form of conventionalism that avoided this objection, because he held that every mathematical truth is, <u>itself</u>, a convention. I do not think that Wittgenstein actually held this view, and I think Stroud[18] has given convincing arguments against Dummett's interpretation; but, regardless of what Wittgenstein actually thought, I do not think that this view is tenable, for the very reasons that Dummett and Stroud give. In footnote 24, I say a few words about what I take to be the <u>real</u> Wittgensteinian view, but I haven't the space to go into any detail about this view or to discuss any of the problems with it.

Finally, I come to anti-platonists who admit that we cannot account for mathematical truth without positing abstract objects, but who simply deny that there are any (atomic or existential) mathematical sentences that are true. This position is called fictionalism. It takes sentences like "2 + 1 = 3" to be false, simply because mathematical objects don't exist. They are, thus, false for the same reason that "Pegasus flies" is false, namely because their subject terms fail to name existing objects.

It is my opinion that fictionalism is far more appealing than any of the other anti-platonist positions considered above, and, indeed, that it is the only anti-platonist position that is even initially plausible. The main problem with fictionalism is that mathematical theories, such as arithmetic, have been useful in the development of the natural sciences. This is problematic, because it doesn't seem that a fictional story could be indispensable to empirical science.[19] One fictionalist, viz. Hartry Field, has made great progress toward solving this problem.[20] His strategy is to show that (a) all of our empirical theories can be nominalized (i.e., reformulated in a way that avoids quantification over abstract objects) and (b) all of our mathematical theories are conservative. (In Field's words, "A mathematical theory M is conservative if and only if for any assertion A about the physical world and any body N of such assertions, A doesn't follow from N + M unless it follows from N alone."[21])

The main problem with Field's programme is that it is far from clear (and, some might say, highly dubious) that quantum theory can be nominalized. I don't want to discuss this here, for it would take us too far afield, but we can, fairly briefly, raise another problem with Field's programme. Even if all of our empirical theories can be nominalized, if our mathematical theories cannot be nominalized, then one could still rationally adhere to platonism. For recall that the argument for anti-platonism proceeds from Ockham's Razor. The claim is that if everything that requires explanation can be nominalistically explained, then we ought to accept nominalism. Now platonists accept this, but even if Field's nominalization programme can succeed, they could still reject fictionalism, because they could deny that it explains everything that requires explanation (because it doesn't explain mathematical truth). In other words, even if Field's nominalization programme could be made to work, platonists could still claim that mathematical truth is something that requires explanation, and that the only way to explain it is to posit a mathematical realm. Thus, while most anti-platonists can use Ockham's Razor to argue that a workable anti-platonism is superior to a workable platonism, fictionalists cannot use this argument, because they have no grounds for claiming that our mathematical theories are false and no grounds for claiming that ontological parsimony is more important than mathematical truth. Thus, even if Field is right that the only non-question-begging argument for mathematical truth is the Quine/Putnam argument from the indispensability of mathematics to empirical science, and even if Field can rebut this argument by nominalizing physics, he will only have succeeded in producing a standoff between fictionalism and platonism. Charmed by ontological parsimony (or driven by a flat disbelief in abstract objects) fictionalists could claim that we can dispense with mathematical entities by abandoning the commitment to mathematical truth; and unwilling to give up truths like "2 + 2 = 4," platonists will demand that such truths require explanation and that it is, therefore, entirely in the spirit of Ockham's Razor to claim that if arithmetic cannot be nominalized, then we ought to be platonists.

As far as I know, there is no argument in the literature -- on either side -- that could settle this dispute; there are only conflicting intuitions. Therefore, it seems that in order to have a good argument for anti-platonism from Ockham's Razor, one must show that mathematical theory, as well as empirical theory, can be nominalized. (And notice that this is precisely what Mill and the advocates of psychologism and conventionalism tried to do: they tried to show that mathematics could be true, even if there were no mathematical entities.)

But while Field's programme can at best produce a standoff, I don't think that it can even do this, for I don't think that quantum theory can be nominalized.[22] If this is true, then fictionalism -- while it is certainly more appealing than conventionalism, psychologism, and Millian naturalism -- will prove no more successful in avoiding platonism than any of those views. And if no other anti-platonist position (obviously I haven't considered them all) can be made to work, then platonism, it seems, will be forced upon us, at least for the time being. Someone might, in the future, develop a viable anti-platonist philosophy of mathematics, but, if we have no such philosophy now, then it seems that platonism will, at least, be well motivated. Now, of course, I haven't given knockdown arguments against any of the anti-platonist positions considered above, but my purpose here has not been to refute these various schools of thought. I have merely been trying to offer some motivation for platonism, i.e. to explain why a scientifically minded person might believe in abstract objects.

2.2 Condensing the Motivation for Platonism

The argument of the previous section is really just a roundabout version of an argument for platonism that is roughly Fregean. The argument can be stated very simply as follows:

(1) There are true (atomic) mathematical sentences.

(2) Many of these contain abstract singular terms, i.e. terms that seem to refer to abstract objects.

(3) These abstract singular terms are genuinely referential, i.e. the sentences in which they appear are true in virtue of facts about the objects that the terms refer to syntactically.

(4) If a singular term appears in a true sentence and if it genuinely refers, then the object it refers to exists.

(5) Therefore, abstract objects exist, e.g. the referents of

numerals, i.e. numbers.[23]

The anti-platonist positions outlined above all represent different responses to this argument. Field's position is to question the truth of (1).[24] Advocates of psychologism deny (2); they obviously have to admit that mathematical truths contain singular terms, but they deny that these terms are abstract, for they think that they refer to mental entities. Conventionalists question (3): they think mathematical truths are factually empty and are, therefore, true in virtue of meaning (or convention) rather than objective facts. The criticisms that I leveled against these various responses in the last section were, of course, too quick to establish the cogency of the above argument. But, again, my aim has not been to establish platonism. I am merely trying to give a sketch of how one would go about establishing it. It is my opinion, however, that in the end, all anti-platonist responses to the above argument can be met,[25] and that this argument does give us a positive reason to believe in abstract objects.

In what follows I will assume that this Fregean argument for platonism is in place and that we, therefore, have good positive reasons for believing in abstract objects. I do this not to choose the route of theft over honest toil, but rather to apply my toil to a different issue, namely to the question of whether platonists can answer the epistemological argument against their position, i.e., whether they can accomplish task (II). I have sketched the above argument for platonism only to motivate this discussion, not to establish platonism. And this should make the overall purpose of this essay clear: it is not an argument for platonism, but rather a defense of platonism.

3. What's to Come

In chapter two, I will formulate what I believe is the best (and, perhaps, only plausible) argument against platonism, namely the epistemological argument against platonism. It will be my position that the strongest formulation of the argument proceeds as follows:

(1) Human beings exist entirely within space and time.

(2) If there are (abstract) mathematical objects, they exist outside of space and time.

Therefore, we have reason to suspect that

(3) If mathematical objects exist, then human beings could not attain knowledge of them,

and, hence, also

(4) If mathematical platonism is correct, then human beings could not attain mathematical knowledge.

(5) Human beings have mathematical knowledge.

Therefore (from (4) and (5), by an inference similar to modus tollens, which I will justify)

(6′) We ought to have a workable platonist epistemology before we accept platonism.

Most of chapter two will consist of a justification of the claim that this is the anti-platonist's strongest formulation of the epistemological argument. In particular, I will argue that this version of the argument is stronger than the Benacerrafian version, which relies upon the causal theory of knowledge (CTK).[26] The main reason that the above argument is superior to Benacerraf's is that it doesn't allow platonists to respond by simply performing the <u>negative</u> task of refuting CTK, a theory which is not widely accepted. I will argue that the only three plausible ways to attack the above argument all require platonists to provide an epistemology (i.e., a <u>positive</u> account of our knowledge of mathematical objects).

The three plausible ways in which platonists could respond to my version of the epistemological argument -- i.e., to (1)-(6′) -- are to deny (1), to deny (2), and to reject the inference from (1) and (2) to (3). The first is Gödel's strategy, and, in chapter three, I will consider and reject this strategy. The second is Maddy's strategy, and, in chapter four, I will consider and reject it. But in chapters five and six, I will consider the third strategy (which has become very popular among contemporary platonists) and conclude that it can work. That is, I will explain how knowledge of abstract objects is possible, even though (1) and (2) are true, i.e., even though human beings cannot come into contact with, or receive information from, abstract objects. Thus, we will see that there is one version of platonism that is unaffected by the epistemological argument in (1)-(6′).

NOTES

1. A mathematical idealist might use "mathematical object" to refer to a mental (and, hence, concrete) objects, but I will not do this. Whenever I use "mathematical object" in this essay, I will be referring to abstract objects.

2. By calling the structure of arithmetic a pattern which contains an infinite sequence of positions, I follow Michael Resnik (1981).

3. It is possible to be a structuralist and an anti-platonist, but most structuralists are platonists.

4. Shapiro (1989), p. 165.

5. All of this is so metaphorical that it probably isn't of much help. Indeed, I doubt that there is really anything between this and a more standard formulation of platonism, because as soon as we use words like "separate" and "divided up" with respect to abstract objects, we are already speaking metaphorically.

6. Benacerraf (1973).

7. Clearly our ontology would be more parsimonious if we could dispense with abstract objects, but it is not obvious that our worldview would become simpler or more elegant. Most anti-platonists take it for granted that the latter follows from the former, but parsimony need not be thought of as the sole source of theoretical elegance. In any event, the point doesn't seem worthy of argument, for there doesn't seem to be much more to the disagreement than a clash of intuitions about what elegance consists in.

8. Mill discusses his philosophy of mathematics in his (1843), Book II, Chapters 5 and 6.

9. The most notable exception is probably Philip Kitcher, although his view is really a cross between Mill's view and constructivism. See Kitcher (1984), chapter 6.

10. Frege's criticisms of Mill can be found in his (1884), sections 7-10.

11. Frege (1884), section 7.

12. Hempel (1945), section 3.

13. Frege's criticisms of psychologism can be found in his (1893-1903), pp. 12-15.

14. Field (1989), p. 228, footnote 2.

15. See Ayer (1946), chapter IV; Hempel (1945); and Carnap (1934), (1952), and (1956).

16. There is no claim that these conventions are explicitly agreed upon. They can be bound up in linguistic usage, or, as Wittgenstein might say, in forms of life.

17. Dummett (1959).

18. Stroud (1965).

19. This argument is known as the Quine/Putnam argument from indispensability. It can be found in Quine's (1948), especially pp.17-18; Quine's (1951), especially pp. 44-5; in Putnam's (1971), especially p. 347; and Putnam's (1975), especially p. 74.

20. Field (1980).

21. Field (1989), p. 58.

22. For an argument that Field's nominalization programme cannot be applied to quantum theory, see Malament (1983).

23. See Frege (1884). Note, also, that this argument can be modified along Quinean lines in a way that suggests that what <u>really</u> commits us to abstract objects is not merely reference to such objects but rather quantification over them. But it seems unnecessary to do this.

24. I believe that Wittgenstein also attacks (1), although he does not, like Field, claim that all (atomic) mathematical propositions are false. Rather, he claims that there simply <u>are</u> no mathematical propositions, i.e. that sentences like "2 + 2 = 4" are not the sorts of utterances that can be true or false. They are more like rules or imperatives, such as "When you measure an object's length, use a straight and rigid ruler, and hold it parallel to the edge of the object being measured." But I cannot go into Wittgenstein's view here, for it would take too much space to even make sense of what that view is.

25. I have obviously not mentioned all possible responses to the Fregean argument. I have, for instance, said nothing about the attempt to debunk (4). But, perhaps, one might try to do this by advancing a Deweyan pragmatic conception of truth.

26. Benacerraf (1973).

Chapter Two

The Epistemological Argument Against Platonism and the Red Herring of the Causal Theory of Knowledge

1. Forward

In the last chapter, I did three things: I introduced the philosophy of mathematics known as platonism (which holds that mathematics is the study of the nature of an objective, non-physical mathematical realm); I provided some initial motivation for that theory; and I said that I would, in this essay, be defending it against the epistemological argument against platonism (which rejects platonism on the grounds that it precludes the possibility of mathematical knowledge). In the present chapter, I will, again, do three things: I will argue that, contrary to common opinion, this epistemological argument does not depend upon the truth of a causal theory of knowledge (CTK); I will say precisely how anti-platonists ought to formulate the epistemological argument; and I will indicate what the possible platonist responses to this argument are.

The superiority of the new version of the epistemological argument over the old Benacerrafian version[1] derives mainly from the absence (in the new argument) of a reliance upon CTK. The reason this reliance ought to be avoided is that CTK has been widely rejected; thus, by taking the truth of this theory as a premise in their argument, anti-platonists allow platonists to respond by simply giving the standard arguments against that theory.[2] But the point of the epistemological argument is not to elicit a negative response from platonists; the point is, rather, to demand a positive account of our knowledge of mathematical objects. Thus, if the argument is to succeed, it must not rely upon CTK. Now, I don't know of any way to reformulate the argument in a way that will literally force platonists to provide an epistemology; but I do think the Benacerrafian argument can be improved upon. I will argue that, by replacing CTK with a much weaker statement (which I cannot defend, but which is intuitively quite plausible, and which is, I think, quite widely accepted) anti-platonists can provide a prima facie reason for believing that knowledge of mathematical objects is impossible. Moreover, I will argue that platonists who share my intuition about this "weaker statement" will only be able to rebut this prima facie reason by actually providing an epistemology of mathematical objects. Now, admittedly, those who don't share the intuition will not have to reply to the argument at all; but, as we will see, anti-platonists don't have any argument that will move such platonists to provide an epistemology.

In sketch, the epistemological argument against platonism concludes that mathematics cannot be the study of abstract objects, because, while we do possess mathematical knowledge, we could not attain knowledge of abstract objects (because they exist outside of space and time and we exist within space and time). It is my opinion that this argument has its roots in a more primitive objection. The real problem most anti-platonists have with an ontology of abstract objects is that, as Schiffer has put it, "being creatures of darkness, [such entities] are hard to believe in."[3] But this is not a philosophical objection. The anti-platonist needs an argument against platonism, and there seems no way to turn this sort of primitive doubt into a good argument. What the anti-platonist needs to do is derive a consequence of the other-worldliness of mathematical objects and argue that this consequence, as opposed to the other-worldliness, is absurd. This, I think, is precisely what the epistemological argument does. It claims that, even if these other-worldly objects do exist, we couldn't know anything about them, and, therefore, our mathematical knowledge couldn't be knowledge of such objects. But if our mathematical knowledge isn't about abstract objects, then we have no reason to be mathematical platonists, because our only motivation for that theory arises from an attempt to account for the truth of our mathematical beliefs.

But this is only a rough sketch of the argument. We need to formulate it more precisely.

2. Benacerraf's Version of the Epistemological Argument Against Platonism

I begin by laying out a version of the epistemological argument that does rely upon CTK, namely the version expounded by Paul Benacerraf in his 1973 article, "Mathematical Truth." Benacerraf presents his argument as a dilemma. His claim is that our best semantic theory (i.e., truth theory) is incompatible with our best epistemological theory (viz., CTK). It is Benacerraf's opinion, of course, that our best semantic theory is that given by Tarski.[4] Now, Tarski only provided a semantics for certain formalized languages. He thought that, because it is possible to generate the liar paradox within natural languages, it is not possible to provide a semantics for such languages.[5] The reason that natural languages give rise to the liar paradox is that they contain their own truth predicates. One can, in a natural language, say, "This sentence is not true." But in a formalized language that doesn't contain its own truth predicate, one cannot say this, and so the liar paradox does not arise. And, for such a language, Tarski has shown that one can formally define -- in the metalanguage of the language in question -- the concept true in M, where M is a precisely specified model. (Tarski does this by first saying what it means for an infinite sequence of objects to satisfy a sentential function, but this need not concern us here.[6])

But while Tarski did not think that truth could be defined for natural languages, subsequent philosophers have attempted to show that his programme can provide us with a definition of truth in natural languages, after all. The Tarskian programme for defining truth in a natural language is to define truth in a way that satisfies Convention T.[7] What this comes down to is setting up a theory that generates, for each (indicative) sentence of the language in question, a theorem of the form

(T) "p" is true if and only if p.

In the theorem for a particular sentence S, "p" will be replaced by a name of S, and p will be replaced by S itself. Actually, to be more precise, p will be replaced by a translation of S into the metalanguage, but in our particular case, the object language and the metalanguage are the same: they are both English.[8]

Benacerraf is one of those philosophers who think that the Tarskian programme can be carried out for English. He writes:

> But...what of sentences that do not belong to the kind of language for which Tarski has showed us how to define truth? I would say that we need for such languages (if there are any) an account of truth of the sort that Tarski supplied for "referential" languages. I assume that the truth conditions for the language (e.g. English) to which mathematese appears to belong are to be elaborated much along the lines that Tarski articulated.[9]

So Benacerraf is quite clear about the fact that he is assuming that Tarski's programme can be extended to English. And -- since he has already argued that we have to give a uniform semantics for the language of mathematics and the background language in which it occurs[10] -- Benacerraf takes this to include the mathematical parts of English. Thus, he thinks we have not only that "George Bush is a human being" is true if and only if George Bush is a human being, but also that "3 is prime" is true if and only if 3 is prime.

Although it will not be relevant to my discussion of the epistemological argument against platonism, it is worth commenting on the assumption that Tarskian semantics can be extended to natural languages. Benacerraf took this assumption fairly lightly, presumably because there is so much agreement among philosophers about the acceptability of a Tarskian semantics for English. But it isn't at all clear what the reason for this agreement is. After all, there is relatively little agreement about how (and, indeed, whether) the liar paradox can be resolved. Perhaps the agreement stems from a feeling that -- for some reason -- it is acceptable to simply ignore the liar paradox. In any event, it is worth noting that the agreement over the acceptability of a Tarskian semantics for English is not universal. Elliott Mendelson, for instance, questions this assumption; indeed, he thinks that to speak of a "Tarskian semantics" for a natural language is extremely misleading, for it gives a veil of false precision to what is nothing more than what used to be called "the correspondence theory of truth."[11]

But as we will now see, insofar as we are only concerned with the epistemological argument against platonism, we can simply ignore the whole issue of Tarskian semantics. This might seem odd, for Benacerraf claims that it is precisely the acceptance of Tarskian semantics for the language of mathematics which precludes the possibility of our having mathematical knowledge. But the reason he makes this claim is that he thinks that (a) accepting a Tarskian semantics for mathematics is the first step toward a slide into mathematical platonism and (b) there is an incompatibility between platonism and the existence of mathematical knowledge. Thus, since our central concern is whether (b) is true, we can simply look at Benacerraf's argument for this claim and ignore his argument for (a).

(In fact, Benacerraf didn't bother to give an argument for (a), although I suppose he had something like the following argument in mind: if we accept sentences like "There is an integer between -2 and -4" as true -- which, of course, some philosophers, such as Field, reject -- and if we accept a Tarskian semantics for such sentences, we will be committed to the objective existence of a number, namely -3;[12] but if -3 is an existing object, it is an <u>abstract</u> object, because there is no plausible alternative. Now, of course, this is just a <u>sketch</u> of the argument, but, for whatever it's worth, I think that this argument can be filled in and that, therefore, (a) is true. But, again, I need not defend this claim here, because it is irrelevant to the question of whether (b) is true, which is my main concern. Now, if we wish to remain true to Benacerraf's formulation of the problem in terms of Tarskian semantics, the point I am making is that we need only determine whether the acceptance of <u>platonistically interpreted</u> Tarskian truth conditions for mathematics is incompatible with mathematical knowledge, i.e., whether we could ever know when such truth conditions are satisfied.[13])

So the main question I want to consider is whether (b) is true, i.e., whether mathematical platonism is inconsistent with the existence of mathematical knowledge. Although Benacerraf didn't lay the argument out very explicitly at all, it seems to me that the reason he accepted (b) can be captured in the following argument.

(1) Human beings exist entirely within space and time.[14]

(2) If there are (abstract) mathematical objects,[15] they exist outside of space and time.

Therefore,

(3) If mathematical objects exist, then human beings could not attain knowledge of them.

Therefore,

(4) If mathematical platonism is correct, then human beings could not attain mathematical knowledge.[16]

I think it is clear that this argument, if not explicitly Benacerraf's, is at least Benacerrafian. In fact, at one point, he seems to very nearly capture the spirit of this argument in a single sentence: he says that one way of arguing that X doesn't know that p, is to argue that "X's four-dimensional space-time worm does not make the necessary...contact with the grounds" for p.[17] I think it is beyond question that Benacerraf accepts both (1) and (2), although he doesn't explicitly state either. But perhaps the two <u>inferences</u> of the argument require a bit of explanation. Why did Benacerraf think that (1) and (2) lead to (4)? That is, why did he think that X's mathematical ignorance follows from the fact that X's four-dimensional space-time worm does not make contact with mathematical objects? I have formulated Benacerraf's argument here with <u>two</u> inferences (in order to make the logic more clear) but Benacerraf really doesn't distinguish them. This is perfectly acceptable, though, because one of them -- namely the inference from (3) to (4) -- is entirely trivial. This is because we know by <u>definition</u> that

(3.1) If mathematical platonism is correct, then there are mathematical objects;

and because we know (by hypothetical syllogism) that (3) and (3.1) imply that

(3.2) If mathematical platonism is correct, then human beings could not attain knowledge of mathematical objects.

Thus, platonists are committed to the claim that (3) entails (4), because, according the them, (3.2) and (4) are equivalent, because mathematical knowledge just <u>is</u> knowledge of mathematical objects.[18]

But the other inference -- viz., from (1) and (2) to (3) -- is not so trivial, and Benacerraf spends a good deal of time trying to justify it. Now, again, he is not so clear in his formulation, but it seems to me that he has a two-pronged argument for thinking that (1) and (2) <u>do</u> imply (3). The first step, which Benacerraf doesn't really bother to defend, is to argue that (1) and (2) imply that

(2.1) If mathematical objects exist, human beings could not be causally related to them.

This step is, I think, fairly easily obtained; since mathematical objects exist outside of space-time, they could not be involved in <u>any</u> causal relations. Now, of course, if we <u>deny</u> that mathematical objects exist outside of space-time -- as, for instance, Penelope Maddy does -- then we might be able to make out a case for them being causally related to human beings. But this is not to the point; to deny that mathematical objects exist outside of space-time is just to deny (2); but we are, right now, only concerned with whether (1) and (2) imply (3); thus, we must assume both (1) and (2); that is, we must assume that human beings and mathematical objects exist in separate realms.

Now, it might be claimed that if we do not have an overly "strict" notion of causation, then we could take human beings and mathematical objects to be causally related, in spite of the fact that they exist in separate realms. But Benacerraf has a response to this, namely that if we simply <u>pronounce</u> that human beings are causally related to mathematical objects without saying <u>how</u>, then it is no longer clear what "causally related" even <u>means</u>. (Actually, Benacerraf offers this as a response to Gödel: he wrote that "without an account of <u>how</u> the axioms 'force themselves upon us as being true,' the analogy with sense perception...is without much content."[19] The issue here is slightly different, for it's not clear that Gödel would have accepted the claim that human beings and mathematical objects are <u>causally</u> related; but, regardless, it is clear that Benacerraf would make the same response to that claim.)

Another way of putting Benacerraf's response to the suggestion that we simply <u>broaden</u> our notion of "causation," is that we don't have anything to broaden it _to_. We just don't have any idea how human beings and abstract objects could be related in a way that would be anything _like_ what we ordinarily call a _causal_ way. Now, at this point, Benacerraf's opponent might raise the Humean objection, i.e., that we can't say how _any_ two objects are causally related. But we can, at least, say _something_ here. Indeed, we can even say something about how human beings acquire knowledge through their causal interactions with things. When I see a jelly bean, for instance, light bounces off of the candy and into my eyes, triggering a neural signal that travels to my visual cortex, which, in turn, causes another signal to be sent to the part of my brain that stores perceptual beliefs. But we cannot say anything even remotely similar to this about how I could acquire information from mathematical objects. We don't even have the _beginnings_ of a theory here, and this is precisely what's wrong with the suggestion that we broaden our notion of causation. Now, of course, if we don't worry about remaining true to the intuitive meaning of "causation," then it would be trivially easy to redefine that term so that it comes out that human beings and abstract objects are causally related.[20] But this simply won't help matters any. After all, our concern is not with the question of how to define the word "causation," but, rather, with the question of whether human beings can acquire knowledge about mathematical objects. And our _immediate_ concern -- the concern that brought us to consider the notion of causation -

- is whether human beings can acquire knowledge of mathematical objects in a _direct_ sort of way, i.e., whether human beings are capable of coming into some sort of _contact_ with mathematical objects in a way that allows information about the latter to be passed to the former. But since we are presently assuming that (1) and (2) are true -- and, hence, that human beings and mathematical objects exist in separate realms -- it seems safe to conclude that human beings cannot come into _any_ sort of contact with mathematical objects.

So it seems that the first step of Benacerraf's two-pronged argument for the claim that (1) and (2) imply (3) is well-motivated: (1) and (2) _do_ imply (2.1). Thus, the only question is whether (2.1) implies (3), i.e., whether the lack of a causal relationship between human beings and abstract objects implies that the former cannot attain knowledge of the latter. And, of course, the second prong of Benacerraf's argument is just the attempt to show that it does.

Benacerraf's argument for the claim that we cannot attain knowledge of mathematical objects without being causally related to them is that it is entailed by our best epistemological theory, namely CTK.[21] Now, there are actually many versions of CTK. The one that Benacerraf favors places the following necessary restraint on knowledge: in order for S to know that, say (Fa), the fact that a is an F "must figure in a suitable way in a causal explanation of" S's belief that (Fa).[22] Thus, Benacerraf's argument is that even if mathematical objects exist, we could not attain knowledge of them, because (2.1) implies that the above necessary condition on knowledge could not be satisfied with respect to such objects.

Why might one think that CTK is our best epistemological theory? That is, what reason do we have for thinking that the above condition quoted from Benacerraf is really a necessary condition on knowledge? Well, the reason that many philosopher's think that something at least similar to this condition -- i.e. some causal restraint -- must be included as one of the necessary conditions on knowledge is that they think it is the only way to avoid Gettier-type counterexamples to the justified-true-belief (JTB) account of knowledge. Let me expand on this a bit. The traditional account of knowledge is that S knows that p if and only if

(i) S believes that p,

(ii) S is justified in believing that p, and

(iii) p is true.

But in 1963, Edward Gettier produced a counterexample to this account of knowledge.[23] If I have a justified belief that (Fa), then I have a justified belief that (Fa V Fb), even if I think (Fb) is false. But if (Fa) is false and (Fb) true, then I have a justified true belief that (Fa V Fb); but, intuitively, I seem to lack knowledge of it. It seems, then, that JTB is not a sufficient condition for knowledge.

Now, one very natural reaction to this is to simply add a fourth condition that needs to be satisfied in order for

JTB to be considered knowledge. And one suggestion that seems appealing, at first blush anyway, is that this fourth condition must impose a causal restraint on knowledge. The reason it is appealing to supplement JTB with a causal constraint on knowledge is that the Gettier counterexample is generated by imagining a case in which S's belief that p is caused inappropriately, in particular, by some sort of error, rather than by the fact that p. Thus, it seems that one way to avoid this is to claim that, for S to know that p, S must be causally related to p in some appropriate way. Now, there are, of course, many such causal restrictions that could be imposed on knowledge, and so there are many different versions of CTK. Benacerraf's version -- which I will call CTK-B -- is just one of many. As I indicated above, CTK-B takes the fourth condition on knowledge to be

(iv) the fact that p must figure in a suitable way into a causal explanation of S's belief that p.

(Note that CTK-B does not necessarily include the claim that (i)-(iv) is a sufficient condition for knowledge; it may be that further conditions must also be met in order for a belief to count as knowledge. Moreover, this is true of CTK in general; all that's essential to that theory is the claim that one of the necessary conditions on knowledge is causal. CTK-B takes this causal condition to be (iv).)

So we can now see why Benacerraf thought that there must be some sort of causal restraint on knowledge. That is, we can see why he thought that CTK is true. Thus, we can also see why he thought that (1) and (2) imply (3): because he thought that the truth of CTK rules out knowledge of causally inaccessible objects; and because, as we saw above, (1) and (2) imply that mathematical objects are causally inaccessible to us.

Now, while I have said that the first prong of Benacerraf's argument that (1) and (2) imply (3) is well-motivated, I think that there are serious difficulties with the second prong -- i.e., with the appeal to CTK. In the next section (i.e., section 3) we will see that platonists have had a field day with this part of Benacerraf's argument, claiming either that the relevant versions of CTK are false, or that they are compatible with knowledge of mathematical objects, after all. But the success of platonists in attacking Benacerraf's appeal to CTK is of limited importance, because, as we will see in section 4, anti-platonists can argue from (1) and (2) to (3) in a <u>different</u> way -- without relying upon any version of CTK.

But before, we do any of this, I want to complete my formulation of Benacerraf's version of the epistemological argument against platonism. That is, I want to formulate it as an argument <u>against platonism</u>. All we presently have is an argument for (4), i.e., an argument that platonism is incompatible with the existence of mathematical knowledge. But it is not difficult to make this an argument against platonism. We need only supplement the following to (1)-(4):

(5) Human beings have mathematical knowledge.

Therefore, by modus tollens from (4) and (5),

(6) Mathematical platonism is not correct.

This, in fact, is how Benacerraf proceeds. He does not attempt to argue for (5). He simply states that one restraint on any theory of mathematical truth is that the truth conditions offered by the theory "cannot make it impossible for us to know that they are satisfied."[24]

So, while Benacerraf isn't so explicit as I have been, and while his goal -- as he states it, anyway -- is to present us with a dilemma rather than with an argument against platonism, it seems to me that we can, without misrepresentation, claim that he has provided an epistemological argument against platonism and that this argument is the one contained in (1)-(6).

3. Responding to Benacerraf's Version of the Epistemological Argument

It might be suggested that we can attain knowledge about mathematical objects by proving theorems that state truths about the objects in question. But a proof can only increase our knowledge of mathematical objects, given that we already have some, i.e., given that we already have knowledge of the assumptions (i.e., axioms, postulates, hypotheses, etc.) from which the proof proceeds. In other words, our knowledge of theorems depends upon our knowledge of axioms: without an explanation of how we can attain axiomatic, or non-derivative, knowledge of mathematical objects, we cannot claim that we have any knowledge of mathematical objects.[25]

Thus, to answer the Benacerrafian argument, platonists must explain how we can attain knowledge of our axioms, and it will be useless to appeal to the method of proof to account for such knowledge, for to do so would involve us in an infinite regress. Another way of putting this is that the method of mathematical proof is not a bridge to the platonic realm. If we <u>start out</u> with a certain amount of knowledge about mathematical objects, then proofs can give us <u>more</u> knowledge about them. But platonists are not allowed to start with any knowledge. Their task is to explain how we can have <u>any</u> knowledge about mathematical objects, i.e., how our <u>axiomatic</u> knowledge of such objects could arise.

None of this is disputable: platonists recognize that they cannot respond to Benacerraf by appealing to proof. So how <u>have</u> they responded? Well, some platonists (e.g., Gödel and Maddy) have responded by attacking (1) or (2), and arguing that human beings can make contact with mathematical objects. But the most common response has been to attack Benacerraf's reliance upon CTK. There are two ways to do this: one could argue that CTK is false (i.e., that there is no necessary causal restraint on knowledge); or one could argue that some versions of CTK are true, but those that are true are compatible with mathematical knowledge.

Platonists who take the first line will generally admit that there is an incompatibility between platonism and CTK, but claim that the problem lies with the latter and not with the former. In other words, platonists of this sort need not find fault with Benacerraf's argument, for they can simply claim that that argument is a reductio of CTK. This is Jerrold Katz's strategy,[26] and, I think, the strategy of many others. An example is John Burgess, who argues that we cannot accept CTK within naturalized epistemology, for there is no evidence that the scientific community actually requires that knowledge satisfy any causal constraint.[27]

Platonists who take the second line must do two things: (a) argue which versions of CTK are plausible and which are implausible; and (b) argue that all of the plausible versions are innocuous to platonism. Mark Steiner seems to agree with Benacerraf that CTK-B is the best version of CTK, but he argues that our knowledge of the axioms of number theory and analysis satisfies condition (iv), because however we explain our belief that these axioms are true, our explanation will contain these axioms; this is because we explain with theories, and because all theories contain these axioms.[28] Bob Hale also thinks that platonism is compatible with the most plausible version of CTK, but, in his opinion, the most plausible version of CTK -- I will call it CTK-H -- includes as a necessary condition on knowledge not Benacerraf's causal restraint, but, rather,

(iv') For S to know that p, S's grounds for believing p must cause S's belief that p.[29]

It is perhaps worth noting that the Katz/Burgess strategy of simply rejecting CTK outright can be seen as a special case of the Steiner/Hale strategy. Katz and Burgess simply represent the limiting case, in which all the work is done in (a): they think that no version of CTK is plausible (or at least no non-trivial version[30]). And for what it's worth, Maddy's strategy of rejecting (2) can be seen as the other limiting case. For her, all the work is done in (b), because she thinks that all versions of CTK are innocuous to platonism. She even thinks that the strongest (and, in my opinion, most implausible) version of CTK is compatible with knowledge of abstract objects. This implausible version of CTK -- I will call it CTK-I -- takes the necessary causal condition on knowledge to be

(iv'') For S to know that p, the fact that p must cause S's belief that p.

It is my opinion that the best way to respond to the Benacerrafian argument in (1)-(6) is to follow Katz and Burgess and simply refute CTK. My argument that CTK is false can be found in chapters five and six, where I explain how human beings can acquire knowledge of mathematical objects without satisfying any (non-trivial) causal restraint. The reason such an explanation constitutes an argument against CTK is that it provides a counterexample to that theory. I will say a bit more about this in section 7, but the reason I think this is the best way for platonists (or, rather, platonists who reject CTK[31]) to proceed is that, in so doing, they kill two birds with one stone; that is, if platonists have to provide an epistemology anyway (and I will argue in this chapter that they do) then, rather than also arguing that CTK is false, they might as well just point out that their epistemology _is_ an argument against CTK.

Now, many platonists have taken a harder line, arguing that CTK can't even account for our _empirical_ knowledge of _physical_ objects. I think that there are good arguments here, but -- since I have no stake in whether this is true and since these arguments have been already been given by other writers[32] -- I will not try to defend this.

Now if CTK _is_ false, then anti-platonists must either abandon their epistemological argument, or find some way of reformulating it. I will show that they can do the latter by actually doing it myself. I will formulate a version of the epistemological argument which does not rely upon CTK and which comes much closer to _forcing_ platonists to provide an epistemology (i.e., an explanation of how human beings can attain knowledge of mathematical objects). Now, it should be clear that, from an anti-platonist point of view, this is desirable. After all, it seems that anti-platonists will not be dissuaded from their epistemological qualms about platonism until they are shown how a platonist epistemology can be constructed. Thus, it seems that they would want an argument that could _only_ be refuted by a platonist epistemology, i.e. an argument that _forces_ platonists to provide such an epistemology.

4. Reformulating the Epistemological Argument Against Platonism

My version of the epistemological argument does not force platonists to provide an epistemology. That is, I admit that there are other ways for platonists to respond. However, I will argue that (a) the only plausible way to respond to my version of the argument is to provide a platonist epistemology, and (b) anti-platonist cannot do any better (i.e., my version of the argument comes closer to forcing platonists to provide an epistemology than any other version[33]).

In order to mend the epistemological argument against platonism, we need a new way of arguing from (1) and (2) to (3). If particular, we need to replace CTK with another premise, one which still justifies the inference from (1) and (2) to (3), but which is more plausible than CTK. We are led to a way of doing this via the following considerations. First, we must recall that the conclusion of Benacerraf's argument -- i.e., (6) -- follows from (4) and (5). And second, we must notice that all of the platonist rebuttals we've considered so far are attempts to refute (4). Now my point is not that one might reasonably deny (5) -- on the contrary (as I will argue in section 6) I think the existence of mathematical knowledge is beyond question -- but, rather, that by admitting the truth of (5), platonists admit that their ontology cannot preclude the possibility of mathematical knowledge and, thereby, incur a debt. For, if there is even a prima facie reason to think that platonism does preclude the possibility of mathematical knowledge, then platonists will have to show that it does not, because the truth of (5) is looming over their camp like a large black cloud. Thus, what anti-platonists need, in order to establish that platonists must provide an epistemology of mathematical objects, is simply a _prima facie_ reason for thinking that platonism precludes the possibility of knowledge of mathematical objects. But this is just to say that they need a _prima facie_ reason to suppose that (3) is true.

It is worth pointing out that by following this strategy, we conform to a method of criticizing theories that is _generally_ acceptable. One way to criticize a theory T is to argue that it is incompatible with some phenomenon P. Now a criticism of this sort need not be a knockdown argument that T + P leads to a contradiction. All that's needed in order to raise a problem for T is a _prima facie_ reason to think there might be an incompatibility between T and P. (We might put all of this in even more general terms, by relying upon the following principle: whenever we have a _prima facie_ reason to believe p, and no way of rebutting this reason, we ought to at least _suspect_ that p is true.)

Now if anti-platonists only need a _prima facie_ reason to think that (3) is true -- i.e., if they don't need to _establish_ (3) in order to establish that platonists must construct an epistemology of mathematical objects -- then it seems that they need not rely upon CTK in order to argue from (1) and (2) to (3). For it seems that (1) and (2), _by themselves_, provide a _prima facie_ reason to believe (3). We said above that (1) and (2) imply that information cannot pass from mathematical objects to human beings. But even without a theory of knowledge in place, this seems problematic. That is, even if we lack an argument that establishes that human beings could never attain knowledge of inaccessible objects, it seems that we should at least be suspicious of a theory which entails that an entire branch of our knowledge is about a collection of objects that exist in an inaccessible realm from which we cannot receive any information. This is simply because we have no idea how we could acquire such knowledge, i.e., because platonism leaves it entirely unexplained how we could acquire such knowledge.

So we have found our new route from (1) and (2) to (3). Now, it might seem that we haven't replaced CTK in this argument, that we've simply gotten rid of it. But this isn't quite right. I have argued that we only need a prima facie reason to believe (3), and I have pointed out that (1) and (2) imply that mathematical objects are inaccessible to us. But I have simply asserted that this inaccessibility provides us with a prima facie reason to believe (3). Thus, instead of CTK, I am relying upon the following plausible intuition:

(PI) As a general (but not necessarily universal) rule, the total inaccessibility of a collection of objects raises a prima facie problem (which may or may not be solvable in particular cases) about the possibility of attaining knowledge of the objects in question.

Now, I will say a bit more about this in the next section, but, first, I would like to state the rest of my version of the epistemological argument against platonism.

It uses the same five premises as the Benacerrafian argument, but -- because we now have a different justification for the inference from (1) and (2) to (3) -- we also have to restate the justification for the conclusion, and, indeed, the conclusion itself. The Benacerrafian argument proceeded as follows:

(1) Human beings exist entirely within space and time.

(2) If there are (abstract) mathematical objects, they exist outside of space and time.

Therefore, by CTK,

(3) If mathematical objects exist, then human beings could not attain knowledge of them.

Therefore,

(4) If mathematical platonism is correct, then human beings could not attain mathematical knowledge.

(5) Human beings have mathematical knowledge.

Therefore, by modus tollens from (4) and (5),

(6) Mathematical platonism is not correct.

In contract, the new epistemological argument against platonism proceeds like this:

(1) and (2). Therefore -- because we accept (PI) -- we have reason to suspect that

(3), and, hence, also (4). (5). Therefore,

(6′) We ought to have a workable platonist epistemology before we accept platonism.[34]

(I will, throughout this chapter, refer to this new version of the epistemological argument as (1)-(6′). And I will refer to the original version as (1)-(6).)

5. Justifying the New Version of the Epistemological Argument

I return now to a consideration of the new inference from (1) and (2) to (3). It seems that platonists can still respond to this inference in a purely negative way: they can simply deny that (PI) is true.[35] Now, I don't know of any argument for (PI), and so I don't think there is any way of debarring this response. Thus, it must be admitted that platonists are not forced by my argument to provide an epistemology of mathematical objects. Any platonist who fails to see any intuitive correlation between the accessibility of an object and our ability to attain knowledge of it, will fail to see the force of the argument in (1)-(6'). Moreover, since this argument isn't formally valid, anti-platonists will not be able to object.

But in spite of this weakness of the new version of the epistemological argument, I do think that it's an improvement over the Benacerrafian version. I say this because, even though platonists still have a negative avenue of response open to them, they do not have a plausible negative avenue of response. This is because (a) unlike CTK, (PI) is highly plausible -- indeed, it seems to me that most platonists would accept it -- and (b) for those who do share the intuition that (PI) is true, the argument in (1)-(6') can only be rebutted by providing an epistemology of mathematical objects; (I will justify claim (b) in section 6).[36]

Now, I suppose that anti-platonists might object to the fact that I have <u>weakened</u> the epistemological argument. But this would be a misguided apprehension for two reasons. First, it is precisely the weakness of (PI) that makes it so plausible. If we replace (PI) with something stronger, we will almost certainly replace it with something more controversial and, thereby, open the door for platonists to respond in a <u>negative and plausible</u> way. This, of course, is seen very clearly in the case of CTK: it is stronger than (PI), but <u>far</u> more controversial. (And, for whatever it's worth, it seems that any platonist who is unconvinced by (1)-(6') will also be unconvinced by (1)-(6), for it seems that any platonist who would doubt (PI) would almost surely find CTK <u>wildly</u> implausible, falsified by the very existence of mathematical knowledge.) Second, anti-platonists don't <u>need</u> (PI) to be any stronger. This is because, even though we are forced to weaken our conclusion from (6) to (6') when we shift from a reliance upon CTK to a reliance upon (PI), (6') is strong enough. After all, the purpose of the epistemological argument is to provide a justification for the claim that so long as platonism is not accompanied by an adequate epistemology, it is unacceptable. But this is precisely what (6') says.

Now, anti-platonists might feel that while the argument in (1)-(6') is acceptable for now, they ought to search for the "missing premise" to their argument. That is, they ought to search for a premise which, unlike (PI), is a <u>universal</u> claim entailing the impossibility of knowledge of mathematical objects, and which, unlike CTK, is plausible and defensible. I do not think there is such a missing premise. My argument for this can be found in chapters five and six, where I explain how human beings can attain knowledge of mathematical objects. Since I show there that such knowledge is possible, it could not be that there is a missing premise which, together with (1) and (2), entails (3). Now, of course, I could be wrong about this; if such knowledge is, in fact, not possible, then there probably is such a missing premise. But in any event, I have no idea what this missing premise would look like; nor do I know of any attempt -- aside from Benacerraf's -- to state it.

So, it seems to me that there is no way to state the epistemological in a way that will force platonists to provide an epistemology. Moreover, I think that the argument in (1)-(6′) -- or something very similar -- is probably as close as anti-platonists can come to this ideal; this is simply because (PI) is strong enough to justify the inference to (6′), but weak enough to be relatively unobjectionable. To make it stronger would be to open the door to plausible negative responses from platonists; and to make it weaker would leave us incapable of providing even a prima facie reason to believe (3).

6. Responding to the New Version of the Epistemological Argument

I have claimed that there is no way for platonists to respond to the argument in (1)-(6′) in a way that is both negative <u>and</u> plausible, i.e., that the only plausible way to rebut this argument is to provide a positive explanation of how human beings could attain knowledge of mathematical objects. It is now time to justify this claim. Up to now, my only remark in this connection has been the (admittedly unjustified) claim that (PI) is extremely plausible. But in this section, I will consider all of the other premises of the argument in (1)-(6′), and all of the inferences of that argument, and I will argue, for each one of these premises and inferences, that if it is at all dubious, then the only way to use this dubiousness to rebut the argument in (1)-(6′) is to construct a platonist epistemology.

The argument in (1)-(6′) contains three basic premises -- viz., (1), (2), and (5) -- and three inferences -- viz., from (1) and (2) to (3), from (3) to (4), and from (4) and (5) to (6′). I begin with the premises.

I have remarked that I do not think that (5) can be seriously questioned. That is, platonists cannot deny that we possess mathematical knowledge. Let us briefly say why. It seems that the only thing a platonist could mean to deny, by denying the existence of mathematical knowledge, is mathematical <u>certainty</u>. They could <u>not</u> deny the existence of true, justified, mathematical belief. This can be seen very easily. First, the only reason to accept platonism is to explain the fact (or presumed fact) that we have true mathematical beliefs; thus, to claim that our mathematical beliefs are <u>not</u> true (or that we don't have mathematical beliefs) is to give up on platonism. (Presumably, one who gave up on the truth of our mathematical beliefs would accept fictionalism; and, perhaps, one who denied that we have mathematical beliefs would accept some sort of Wittgensteinian position.) Second, a platonists could not claim that our mathematical beliefs are true, but <u>unjustified</u>, for this would undermine the practice of mathematics; in particular, it would be to claim that if mathematicians are more generally correct in their mathematical beliefs than lunatics and imbeciles, it is only a matter of <u>luck</u> that they are.

But if platonists can only deny the existence of mathematical <u>certainty</u>, then the epistemological argument will be safe, because there is no need to interpret (5) as asserting the existence of anything so strong as certainty. Anti-platonists are demanding an account not of mathematical certainty, but, rather, of something like true, justified, mathematical belief.[37] (I am not committing, here, to the claim that knowledge <u>is</u> true justified belief. Even if the two are distinct, anti-platonists could still formulate their epistemological argument in terms of true, justified, mathematical belief; or, assuming we find some other definition of knowledge, they could probably formulate it in those terms.)

I have probably said far too much about (5), for even if there <u>are</u> philosophers who would deny it, I doubt there are <u>any</u> platonists who would deny it. This is simply because, as

I said above, the denial of (5) is tantamount to the denial of platonism.

In contrast to (5), however, it seems to me that (1) and (2) could both be doubted. In fact, they both <u>have</u> been doubted: Gödel sought to reject (1) and Maddy (2). But the question I want to raise here is whether platonists could refute the epistemological argument by <u>merely</u> refuting (1) or (2), i.e., <u>without constructing an epistemology of mathematical objects</u>. That is, could platonists simply argue that (1) or (2) is false and, thereby, refute the argument in (1)-(6′), without saying anything about how human beings can acquire knowledge of mathematical objects? I think that the answer to this question is no. The reason that (1) and (2) lead us to suspect that (3) might be true is that they imply that human beings cannot receive information from mathematical objects. Thus, in order to undermine the case for (3), platonists must be careful that they do not reject (1) or (2) in a way that enables anti-platonists to simply <u>reword</u> their premises and preserve the inaccessibility of mathematical objects. In other words, it would <u>not</u> be enough for platonists to simply show that human beings and mathematical objects exist in the same "realm"; (if (1) was being rejected, this would presumably be the mathematical realm, whereas if (2) was being rejected, it would be the physical realm); it would also be necessary to show that they exist in the same realm <u>in a way that makes it possible for human beings to receive information about mathematical</u>

objects. This is simply because there is no clearly formulated alternative to either (1) or (2). Any conception of human beings as aspatial and atemporal, or any conception of mathematical objects as spatio-temporal, would have to be spelled out in some detail before it could be acceptable. Thus, if platonists wish to adopt a position of either sort for the purpose of establishing the possibility of knowledge of mathematical objects, they must illustrate that their position is, in fact, one that allows for such knowledge. But to explicate such a position and to show that it does indeed allow for knowledge of mathematical objects just is to provide an epistemology of mathematical objects.[38]

It is important to look at (1) and (2) individually here. The problem with the rejection of (1) is that the ordinary immaterialist view does not take the mind to exist in the mathematical realm. Thus, if platonists want to use immaterialism to solve their epistemological problem, they will have to explain how it can even help. But the only way to do this is to explain what their immaterialism consists in and how it allows from knowledge of mathematical objects; thus, it would be necessary, here, to provide an epistemology. The problem with the rejection of (2), on the other hand, is that it ordinarily entails the rejection of platonism. Thus, if platonists seek to solve their epistemological problem by rejecting (2), they will have to explain exactly what they take mathematical objects to be (i.e., how such objects could be spatio-temporal and platonic) and why such objects can be known. (For whatever it's worth, it seems to me that if a platonist could give a coherent account of such "hybrid" objects, and also make out a convincing case for the claim that mathematics is about such objects, then an epistemology would probably follow pretty directly.)

I am now halfway through my argument for the claim that the only plausible way to refute the argument in (1)-(6′) is to provide an epistemology of mathematical objects. I have shown that (5) cannot be questioned and that, while (1) and (2) can be questioned, if platonists try to refute the argument in (1)-(6′) by rejecting (1) or (2), they will have to provide an epistemology of mathematical objects. But I must now consider the inferences of the argument in (1)-(6′). There are three such inferences, but recall that I have already shown (in section 2) that the inference from (3) to (4) is trivially acceptable, so we need only consider the other two.

The inference from (4) and (5) to (6′) is also fairly trivial. It is, after all, almost identical to the inference from (4) and (5) to (6), which is simply an instance of modus tollens. But valid argument forms are not like hand grenades: "almost" doesn't count. The fallacy of affirming the consequent is, likewise, almost a modus tollens, but it is, true to its name, a fallacy. Thus, some elaboration is required.

It seems to me that the inference from (4) and (5) to

(6′) diverges from the form of a modus tollens in an acceptable way. Recall that this inference proceeds as follows:

We have reason to suspect that

(4) If platonism is correct, then human beings cannot attain mathematical knowledge.

(5) Human beings have mathematical knowledge.

Therefore,

(6′) We need a workable platonist epistemology before we can accept platonism.

Notice that (4) and (5) still look like the premises of a modus tollens. The only reason we didn't use modus tollens and infer (6) from (4) and (5) is that we couldn't assert (4) outright. It, therefore, seems that we could have inferred that <u>we have reason to suspect that</u> (6) -- i.e., that we have reason to be suspect that platonism is false -- and still have considered this a modus tollens. If we know that B is false and we have reason to suspect that A --> B, then, surely, we have reason to suspect that A is false. But if it is acceptable to infer that we have reason to suspect that platonism is false, then it also seems acceptable to infer that we ought not to accept platonism until this suspicion is cleared up. But since the suspicion of platonism was generated solely by a suspicion about the possibility of attaining knowledge of platonic objects, this amounts to inferring that we ought not to accept platonism until we have cleared up this <u>latter</u> suspicion, until we have shown why knowledge of platonic objects isn't impossible. But this is essentially what (6′) says.

So it seems to me that the inference from (4) and (5) to (6′) is acceptable. All it's saying is that since there's a *prima facie* (epistemological) problem with platonism, we cannot accept that position unless we can find some way of rebutting this problem.

Now, of course, if we have no alternatives to platonism, we might accept it in spite of its epistemological problems. We might justify this by claiming that *no* theory is problem-free. Thus, platonists could admit that they need an epistemology, but simply not panic about the fact that they haven't found one *yet*. But none of this is to the point; for I am only trying to establish that there is a serious epistemological problem with platonism that needs to be addressed. *How* serious the problem is doesn't really matter; nor is it relevant whether we have any alternatives to platonism, for it might be that *all* of our interpretations of mathematics are false.

I do not know of any platonist who questions the inference from (4) and (5) to (6′), i.e., the inference from the fact that knowledge of mathematical objects *seems* impossible to the claim that platonists ought to explain why it isn't. Hale, however, has said something that sounds similar. He takes knowledge of mathematical objects to be ordinary *a priori* knowledge. And he claims that platonists do not need to argue that we can have such knowledge, because there are good reasons to believe in _a priori_ knowledge that are independent of any platonistic interpretation of what this knowledge is about. He writes that

> The view that some of our knowledge is _a priori_, and specifically, that our logical and mathematical knowledge is typically of that character, is perhaps sufficiently plausible for positive supporting argument to be unnecessary.[39]

I think Hale's reasoning is seriously flawed. If there are reasons to believe in the existence of _a priori_ knowledge that are independent of platonism, then the notion of _a priori_ knowledge being argued for must have more than one interpretation. That is, if these arguments are really independent of platonism, then it will be possible to take this _a priori_ knowledge as being not about abstract objects. But if this is the case, then the arguments supporting this _a priori_ knowledge do not support the existence of knowledge of abstract objects. So Hale is wrong: we do _not_ have independent reasons to believe in knowledge of abstract objects. (Now, it's possible that the "reasons" Hale is thinking of, while not "independent," could turn out to be good reasons to believe in knowledge of abstract objects; but, unfortunately, he doesn't say what these reasons are.) But if we don't have independent reasons to believe in knowledge of abstract objects, then (4) and (5) _do_ seem to imply (6'). For (4) and (5) suggest that there is a _prima facie_ reason to think that such knowledge isn't possible, and (6') merely says that we shouldn't believe in such knowledge until we have an explanation of how it _is_ possible.

So it seems that the only remaining inference of the epistemological argument is the inference from (1) and (2) to (3). I have already said a great deal about this inference; it seems to me that -- so long as we accept (PI) -- this inference cannot be questioned. (1) and (2) do give us reason to suspect that (3) is true. However, platonists can still attack the argument here; for they can argue that the suspicion created by (1) and (2) is an illusion; that is, they can argue that, in spite of the fact that (1) and (2) are true, and in spite of the fact that this gives us reason to suspect that (3) is true, it is nonetheless the case that (3) is false. It seems patently obvious, however, that the only way to do this is to construct a platonist epistemology, i.e., to actually explain how (3) could be false, even if (1) and (2) were true. The reason I say this is that I simply can't think of any other way for platonists to argue their point. In order to argue that purely spatio-temporal creatures like ourselves could attain knowledge of a collection of aspatial, atemporal objects, one would simply have to explain how they could.[40]

So we have seen that while there are three initially plausible ways for platonists to attack the argument in (1)-(6′) -- viz., by denying (1), by denying (2), and by denying that (1) and (2) imply (3) -- all three strategies require them to actually explain how human beings can attain knowledge of mathematical objects.

7. CTK is a Red Herring

I think I have made a fairly convincing case for the claim that my version of the epistemological argument is superior to the Benacerrafian version, which relies upon CTK. But I would like to put one more nail in the coffin of the latter. I will argue that CTK is a red herring, i.e., that (a) platonists cannot help their epistemological situation by refuting CTK, and (b) platonists cannot hurt their epistemological situation by ignoring it, i.e., by not refuting it. The purpose of this is to show that even if CTK could be patched up and made more plausible, the new version of the argument would still be better.[41]

In effect, the argument for (a) was given in sections 4 through 6. For in constructing and defending the argument in (1)-(6′), I have shown that even if platonists can provide knockdown arguments against CTK, their epistemological problem will remain, for they will have done nothing to rebut the argument in (1)-(6′).

And in section 3, I hinted at the argument for (b). I can now state it more clearly. The first point that needs to be made here concerns the fact that there are numerous versions of CTK. We have to be clear that (b) only applies to versions of CTK that platonists disbelieve. Thus, my point is that platonists will not hurt their epistemological situation by ignoring the versions of CTK that they disbelieve. If platonists follow the Steiner/Hale strategy of <u>accepting</u> certain versions of CTK, this will only be <u>some</u> versions of CTK; if they follow the Katz/Burgess strategy, it will be all (non-trivial) versions. (I discussed these two strategies above, in section 3.)

It might seem striking that I would claim that (b) is true. For if CTK is true, then platonists are in serious trouble. Thus, it seems that it would certainly behoove them to refute CTK. The confusion in this reasoning can be seen in the following way. Suppose that platonists do go to the effort to refute CTK. Since (a) is true, they haven't solved their epistemological problem; moreover, if my arguments in this chapter have been cogent, the only way to eliminate this problem is to actually construct an epistemology of mathematical objects. But now notice that if and when they do this, they will, *ipso facto*, have shown that CTK is false. This is because CTK says that there is a *necessary* causal condition on knowledge, and so, if platonists can explain how knowledge of mathematical objects can arise without any sort of causal constraint being satisfied, then they will have given a counterexample to CTK. Again, this is because CTK says that knowledge can *only* arise when a causal constraint *is* satisfied. Thus, my point is that an acceptable platonist epistemology already falsifies CTK (or at least those versions of CTK disbelieved by the platonists who advocate the epistemology in question) and so nothing more needs to be said; that is, there is no need for platonists to *also* provide an explicit argument against CTK.

Now, it *might* be that, in order to make their epistemology plausible, platonists will have to *argue* that no causal constraint needs to be satisfied in order for S to know that p. But there is no reason to think that platonists will have to argue in this way. It is entirely possible that one could give an explanation of how knowledge of mathematical objects arises which is so convincing that we would be willing to admit that human beings can attain knowledge in this way. And if this happened, we would have a counterexample to CTK, and would have no need for a separate argument against it. (As it turns out, platonists can proceed in this fashion. I will give the details in chapters five and six.)

It seems, therefore, that CTK is a red herring of the worst sort: platonists do not help their cause by refuting it and they do not hurt their cause by ignoring it.

8. Field's Version of the Epistemological Argument

At least one anti-platonist agrees that the epistemological argument against platonism must be purged of its reliance upon CTK. Field writes that "Benacerraf's formulation of the challenge relied on a causal theory of knowledge which almost no one believes anymore."[42] This said, Field goes on to give a version of the epistemological argument which is quite similar to the argument in (1)-(6′). He says that the best way to use a Benacerraf-type argument against platonism would be to argue for the following three claims.

(7) Our mathematical beliefs are reliable,

(8) Platonists must explain (7), and

(9) Platonists cannot explain (7).

The use of "reliable" in (7) might seem a bit puzzling. One might think that Field simply used this terminology to avoid making the claim that our mathematical beliefs are true (the thought being that, since Field is a fictionalist, he cannot claim that they are true). But I do not think this is right. The fact is that Field could have stated (7) in terms of truth, because he would not have been forced to accept such truth himself. Thus, (7) could have been stated as follows: "if mathematical platonism is correct, then our mathematical beliefs are true."

So, then, why does Field use "reliable" instead of "true"? The reason, I think, is that he wants to make clear that he is not demanding a simple justification of our mathematical beliefs. In other words, he has chosen not to couch the epistemological argument in terms of "knowledge," because he wants to rule out the possible platonist response that we have knowledge of mathematical objects, because we have true justified belief of such objects. It is clearly acceptable for Field to make this move here; platonists cannot solve their epistemological problems by merely justifying our mathematical beliefs, because everyone is willing to grant that our mathematical beliefs are well-justified. The task is to show that, if these beliefs were about mathematical objects, they would still be well-justified. In other words, the task is to explain how human beings could arrive at reliable beliefs about mathematical objects.

Although Field doesn't give an example, I believe that the sort of response he is guarding against is the Quine/Steiner response that our mathematical beliefs are justified by their applicability to empirical science. The reason (in my opinion) that this sort of response is unacceptable is that mathematicians know that their theories are true before we apply them. Thus, platonists must account for more than the *ex post facto* justification provided by indispensability arguments. I will say much more about this response in chapter five.

Evidence for my interpretation of Field's motivation for demanding an account of reliability rather than knowledge comes from passages like the following:

> The way to understand Benacerraf's challenge, I think, is not as a challenge to our ability to justify our mathematical beliefs, but as a challenge to our ability to explain the reliability of these beliefs.[43]

But all of this is a digression. The main point I want to make is that Field's argument in (7)-(9) is essentially the same as my argument in (1)-(6′). We can see this by simply mapping the two arguments onto one another. The key premise in Field's argument is (9). It corresponds, in my argument, to (3) and (4). This might seem puzzling, for they say very different things. But the following four points about the rest of the two arguments will justify my claim.

First, recall that I support (3) and (4) with (1) and (2), but that I do not claim that there is an entailment here. I only claim that (1) and (2) give us reason to suspect that (3) and (4) are true. Field's argument for (9) is identical. He does not claim to have a knockdown argument establishing that platonists cannot explain the reliability of our mathematical beliefs, but he does think we have reason to suspect that they cannot. Thus, he says:

> I refrain from making any sweeping assertion about the impossibility of the required explanation. However, I am not at all optimistic about the prospects of providing it.

And he uses (1) and (2) to justify his suspicion about the platonist's ability to provide the requested explanation. He writes that

> various facts about how the platonist conceives of mathematical objects collectively rule out all possibility of finding any such explanation. (The relevant facts about how the platonist conceives of mathematical objects include their mind-independence and language-independence; the fact that they bear no spatio-temporal relations to us; the fact that they do not undergo any physical interactions...with us or anything we can observe, etc.)[44]

The second consideration suggesting that Field's (9) is analogous to my (3) and (4) -- and that his argument in (7)-(9) is essentially the same as my (1)-(6') -- is that the truth of (9) and the truth of (3) and (4) would be problematic to platonists for virtually identical reasons: the probable truth of (9) is problematic, because our mathematical beliefs are reliable (i.e., because (7) is true); and the probable truth of (3) and (4) is problematic, because we do have mathematical knowledge (i.e., because (5) is true). Part of the argument is also that the existence of mathematical knowledge -- and, in Field's argument, the reliability of our mathematical beliefs -- is the sort of thing that needs to be explained. Field makes this premise explicit in (8), whereas I assume it. The reason I assume it is simply because I take it as obvious, for, as Field says, the reliability of our mathematical beliefs -- and, in my argument, the existence of mathematical knowledge -- could not be a brute inexplicable fact.[45]

The third similarity between Field's argument and my own is that the conclusion of the two arguments is the same. The conclusion of (1)-(6') was not that platonism is false, but that platonists need an epistemology. We could not conclude that platonism is false, because we were only able to generate a suspicion that (4) is true. But, while Field doesn't clearly state the conclusion of his argument, it should be quite clear that, since he has only generated a suspicion that (9) is true, he cannot conclude that platonism is false, either. All he can conclude is that we cannot accept platonism until we have an explanation of the reliability of our mathematical beliefs. I do not think Field would take issue with this. He refrains from saying that he has established the falsity of platonism, but he does not refrain from saying that platonists must "provide an account of the mechanisms that explain how our beliefs about these remote entities can so well reflect the facts about them."[46]

Fourth, and finally, it is clear that platonists ought to respond to the two arguments in the same way, namely by actually providing the explanation being demanded. The most promising way to respond to (1)-(6′) is to actually explain how human beings could attain knowledge of mathematical entities; and the most promising way to respond to Field's argument is to actually explain the reliability of our mathematical beliefs. But (so long as we disallow explanations of mathematical knowledge which -- like the Quine/Steiner explanation discussed earlier in this section and in chapter five -- provide only an ex post facto justification of our mathematical beliefs) the two explanations are the same; for, since platonists take our mathematical beliefs to be about mathematical objects, to explain how we could attain knowledge of mathematical objects is just to explain how our mathematical beliefs could be reliable.

So it seems that Field's epistemological argument and the argument in (1)-(6′) are just two different versions of the same argument. By couching his argument in terms of "the reliability of our mathematical beliefs" rather than "mathematical knowledge," Field has (to his credit) made explicit the unacceptability of Quine/Steiner-type responses. In spite of this benefit, I will use (1)-(6′), because it is manifestly about the traditionally important philosophical notions of truth and knowledge (and because I think its premises better represent the intuitive flow of the argument).

9. A Taxonomy of Platonist Responses

In what follows, I will assume that the argument in (1)-(6′) is superior to the Benacerrafian in (1)-(6). Indeed, I will not speak of the latter anymore, and, whenever I speak of "the epistemological argument against platonism," I will be referring to the former.[47]

Now, in section 6, I argued that there are three plausible ways of attacking the new version of the epistemological argument. These correspond to the three most plausible strategies for developing an epistemology of abstract objects. (The reason this is so is that if platonists are to be successful in their search for an epistemology, then there must be something wrong with the epistemological argument; thus, since there are only three possible problems with our epistemological argument, there are only three plausible strategies for constructing an epistemology of mathematical objects.) The first strategy platonists could use is to argue that (1) is false and that the human mind is capable of, somehow, forging contact with the mathematical realm and, thereby, acquiring information about that realm. This is Gödel's strategy. Second, platonists could argue that (2) is false and that human beings can acquire information about mathematical objects via normal perceptual means. This is Maddy's strategy. Third, platonists could argue -- by explaining how human beings can know what the mathematical realm is like, even without coming into contact with (i.e., receiving information from) that realm -- that the _prima facie_ plausibility lent by (1) and (2) to (3) is an illusion. This is the most popular strategy among contemporary platonists; its advocates include Steiner, Resnik, Shapiro, Parsons, and Katz.

The remainder of this essay will consist of a consideration of these three strategies. In chapter three, I will consider and reject the first, and in chapter four, I will consider and reject the second. But in chapters five and six, I will argue that the third strategy can effectively refute the epistemological argument against platonism. That is, I will explain how we _can_ attain knowledge about the mathematical realm _without_ coming into contact with that realm.

NOTES

1. Benacerraf (1973).

2. For some arguments against CTK, see Steiner (1975), chapter 4; Hale (1987), chapter 4; Burgess (1990); Brown (1990); Rosen (unpublished); and Nozick (1981), chapter 3.

3. Schiffer (1987), p. 49. Note, however, that Schiffer attributes the phrase "creatures of darkness" to Quine. Exactly where Quine coins this phrase I am not sure.

4. Tarski, A. (1935).

5. See section 1 of Tarski's (1935) for a discussion of the semantics of natural languages.

6. See section 3 of Tarski's (1935) to see how this works.

7. Tarski (1935), p. 187-188.

8. Subtleties would have to be introduced in order to deal with sentences with indexicals and ambiguities, but we need not go into this.

9. Benacerraf (1973), p. 668-669.

10. Benacerraf (1973), pp. 662-663.

11. Mendelson has expressed this worry to me in private communication.

12. For whatever it's worth, this seems to be a special case of the more general point that adopting a Tarskian semantics with respect to the sentence of <u>any</u> domain of discourse entails a realism about the objects that those sentences speak of. If we believe, for instance, that the sentence "At least one human being served as U.S. President between 1985 and 1990," is true, and if we accept a Tarskian semantics for this sentence, then we are committed to the existence of an object which, between 1985 and 1990, was simultaneously human and President of the United States.

13. Benacerraf sometimes conflates two formulations of his objection to the platonist's account of mathematical truth. Sometimes he seems to be saying that if the truth conditions for p are T (where T involves conditions being placed on abstract objects) then it is impossible to know that p. Other times he seems to be saying that if the truth conditions for p are T (where T involves conditions being placed on abstract objects) then it is impossible to know

that T is satisfied. These are not the same: one could know that T is satisfied, but not know that p, for one might not know that the truth conditions for p are T. It is probably not very important which of these we choose, although the second seems better, for it eliminates this possibility. I will, therefore, stick with the second formulation.

14. The "entirely" isn't playing any strange role here. I include it only to guarantee that (1) is incompatible with the belief that human beings possess immaterial minds. Without the "entirely," immaterialists could agree with (1); for, just as they could claim that Frege lived in the twentieth century (because he lived partially in the twentieth century) they could claim that human beings exist in space and time (because they partially do).

15. I put the argument in terms of mathematical objects instead of abstract objects in general, simply because Benacerraf states the problem in this way. However, I think that an exactly parallel argument can be constructed against all abstract objects. Note, too, that if the specialized argument can be answered, then the general argument is also answered, although if the specialized argument cannot be answered, it doesn't mean that the general one can't.

Also, I want to note that when I speak of mathematical objects, I am speaking of abstract objects. Someone like L.E.J. Brouwer might speak of mental entities as mathematical objects, but I am simply not using the term in that way.

16. Perhaps something needs to be said about the modality contained in this argument. The argument is not simply that if mathematical objects exist, then human beings do not, in fact, have knowledge of them, but rather that if mathematical objects exist, then human beings could not attain knowledge of them. This is how Benacerraf actually speaks, and just about every philosopher concerned with the epistemological argument has followed his lead here. I will also follow his lead, although I will interpret the modal somewhat loosely and informally. I want to hang on to the modal, because I think that, in order to respond to the epistemological argument, platonists have only to explain how we could attain knowledge of mathematical objects; they don't have to explain how we actually do attain such knowledge, because this would be too strict a demand (because, insofar as it requires a rational reconstruction of what goes on in the heads of mathematical knowers, it would be a demand that we might have a hard time satisfying with respect to our empirical knowledge). But I interpret the modal loosely and informally because the platonist must do more than explain how it's logically possible that human beings could attain knowledge of mathematical entities. Such an explanation would, surely, be simple to give, for there is a possible world in which God has set up a pre-established harmony between our mathematical

beliefs and the mathematical realm. Clearly, however, platonists must do more than simply point out such a possibility. They must explain how human beings could attain knowledge of mathematical objects, and it must be plausible that a human being actually could attain mathematical knowledge in the specified way. So, to be precise, what (3) should say is that, if mathematical objects exist, then human beings could not attain knowledge of them in a way that might plausibly be a way in which mathematical knowledge actually could arise. But it is too cumbersome to speak in these terms, and so I will simply use the modal claim.

17. Benacerraf (1973), p. 671.

18. Actually, it is not universally true that mathematical knowledge is knowledge of mathematical objects. Even platonists will admit this, because (among other reasons) many of the mathematical propositions that we know to be true are vacuously true, and so the knowledge that such propositions are true is not knowledge of abstract objects. For our purposes, however, we can simply ignore this. If we were going to be really precise, we would have to either reformulate (4) to say that if mathematical platonism was correct, then human beings could only attain a relatively small amount of mathematical knowledge, where "relatively small amount" is appropriately spelled out. But nothing I say will be affected by all of this, and so, in order to avoid needless complication, we can simply assume that (according to platonists) all mathematical knowledge is knowledge of mathematical objects.

19. Benacerraf (1973), p. 674.

20. All we would have to do is make the definition disjunctive and make one of the disjuncts say that two objects are causally related if one of them has thought about the other.

21. CTK is usually traced to Goldman (1967) and Skyrms (1967).

22. Benacerraf (1973), p. 671.

23. Gettier, (1963).

24. Benacerraf (1973), p. 667.

25. That we cannot know that our theorems are true of mathematical objects unless we can know that our axioms are true of mathematical objects can be seen in the following way. Without an explanation of how we can know that our axioms are true, the most a proof will be able to give us is conditional knowledge, i.e. knowledge of the form, "if the

axioms are true and the rules of inference truth-preserving, then the theorems are true." But this sort of knowledge is compatible with the falsity of both the axioms and the theorems.

26. Katz (1981), chapter VI, especially pp. 206-208.

27. Burgess (1990).

28. Steiner (1975), p. 114.

29. Hale (1987), chapter 4, especially sections IV and V.

30. The "non-trivial" hedge is necessary, because some versions of CTK are obviously true, e.g., the version that claims that S can only know that p if S's belief that p is caused by _something_. But while it is trivial that such a version of CTK is true, it is also trivial that it is compatible with platonism, and so we need not worry about it. I'm not so sure that CTK-H isn't a trivial version of CTK; if it is, then Hale would really be closer to Katz and Burgess than to Steiner, but I will not pursue this here.

31. Platonists who _accept_ some version of CTK -- e.g., Steiner and Maddy -- will, of course, not provide a counterexample to CTK by providing an epistemology of mathematical objects. For, according to the epistemologies of such platonists, our mathematical knowledge satisfies some causal restraint (viz., the restraint given by the version of CTK that is accepted).

32. The most common (although, by no means, the only) strategy here is to argue that CTK cannot account for our knowledge of general, nomological truths. For various arguments against CTK, see Steiner (1975), chapter 4; Hale (1987), chapter 4; Burgess (1990); Brown (1990); Rosen (unpublished); and Nozick (1981), chapter 3.

33. I don't mean to imply that my argument couldn't be improved upon. What I mean is that the _sort_ of argument that I am suggesting is the best _sort_ of argument available to anti-platonists.

34. It is worth noting, as an aside, that anti-platonists are not demanding anything extraordinary from platonists here. They are merely demanding something that we demand from _all_ sorts of realists, e.g., common sense and scientific realists. We would not think that our common sense and scientific knowledge was about an objective and external world, unless we believed that we had a reliable method of attaining knowledge of such a world.

Platonists might object here that many of our empirical beliefs involve abstract concepts; for instance, one might claim that in order to know that "my family consists of five people," I would need to have knowledge of the abstract concepts "family" and "five". Thus, it might be argued that if there is a problem seeing how we could attain knowledge of mathematical objects, then there is a problem seeing how we could attain much of our empirical knowledge. I do not think that this is a legitimate response, for anti-platonists do not think that the terms "family" and "five" name abstract objects. Now, it might turn out later that, because of their interpretation of such concepts, anti-platonists cannot account for our ordinary empirical beliefs, but this is not what is at issue right now.

35. Platonists might try to argue for this intuition by claiming that we might also be able to attain knowledge of inaccessible physical objects. To this end, platonists could point out that we might -- in order to simplify our physical theory -- posit a collection of physical objects which are causally inaccessible to us. (E.g., we might, for some reason, posit a parallel universe.) But in such a case, if we could confirm our hypothesis by observing some of its consequences, we might have a justifiable claim to knowledge. (Arnold Koslow suggested this argument to me in private conversation.)

I do not think that this argument weighs against (PI). Rather, it shows how, in certain cases, the epistemic problem raised by inaccessibility might be overcome. It still seems, however, that, in any particular case of inaccessibility, the problem must be addressed.

36. Platonists might try to use a different negative avenue of response, i.e., a response different from the denial of (PI). They might claim that, regardless of whether (PI) is true, they don't have to say anything in response to my version of the epistemological argument against platonism. For they might claim that insofar as we do have mathematical knowledge, (3) must be false. But this is an unacceptable way for platonists to proceed, for it involves the assumption that mathematical knowledge could only be knowledge of mathematical objects. But to assume this is just to assume that platonism is true.

37. We will see in section 8 (and, in more detail, in chapter five) that platonists must account for a bit more than true, justified, mathematical belief. They will have to account for a justification that goes beyond the ex post facto justification provided by the Quine/Putnam argument from the indispensability of mathematics to empirical science. (Field has expressed this by claiming that platonists have to account for the reliability of our mathematical beliefs.) The reason for this is that, just as it is obvious that mathematicians have more justification for their mathematical beliefs than imbeciles do, it is also obvious that they have

such justification <u>before their mathematical beliefs are used in empirical science</u>. Thus, the claim is that platonists have to account for the fact that our mathematical theories are justified <u>as soon as we construct them</u>.

38. Platonists could not simply claim that insofar as we <u>do</u> have mathematical knowledge, (1) or (2) <u>must</u> be false; for this would be to simply <u>assume</u> that mathematical knowledge is knowledge of mathematical objects.

39. Hale (1987), p. 125.

40. I already said this above, but the point is worth repeating. Platonists might try to claim that they do not have to provide an account of <u>how</u> we attain mathematical knowledge, on the grounds that it is <u>obvious</u> that we <u>do</u> have such knowledge. In other words, they might argue that, insofar as we <u>do</u> have mathematical knowledge, (3) <u>must</u> be false, and so nothing at all needs to be said about why (1) and (2) don't justify (3). But platonists cannot make this move, for if they do, they beg the question; in particular, they assume that mathematical knowledge could only be knowledge of abstract objects.

41. It seems to me that the <u>only</u> way the epistemological argument could be made more effective by using CTK would be to provide a <u>knockdown</u> argument for CTK-I. The reason this would be desirable is that anti-platonists could then conclude not just that platonists need an epistemology, but also that they couldn't provide one. (If we could muster a knockdown argument only for CTK-B, then I think (1)-(6') would still be stronger than (1)-(6); for there would still be a plausible response to the latter that doesn't involve the construction of an epistemology, namely Steiner's response that CTK-B is compatible with mathematical knowledge. I'm not saying that this response can definitely work, only that the question of whether it can work is controversial enough to render the reliance upon CTK undesirable, from an anti-platonist point of view.) But we don't have a knockdown argument for CTK-I (or for any other non-trivial version of CTK, either). In fact, we don't have <u>any</u> argument for CTK-I. If we have a knockdown argument for <u>anything</u> in this connection, it's for the <u>falsity</u> of CTK-I.

42. Field (1989), p. 25.

43. Field (1989), p. 25.

44. Field (1989), p. 27.

45. Field makes this point on p. 232 of his (1989).

46. Field (1989), p.26.

47. I don't mean to show any disrespect for Benacerraf, here. I still think of the argument as essentially <u>his</u>. It was simply his misfortune that he wrote his article in 1973, when things looked relatively rosy for CTK.

Chapter Three

Contact With Other Worlds: Gödel

1. Forward

In the last chapter, I sketched an argument which concluded that we cannot accept mathematical platonism unless we can develop a plausible account of how human beings could attain knowledge of mathematical objects. The important part of this epistemological argument against platonism lies in its first three steps:

(1) Human beings exist entirely within space and time.

(2) If there are (abstract) mathematical objects, they exist outside of space and time.

Therefore, we have reason to suspect that

(3) If mathematical objects exist, then human beings could not attain knowledge of them.

We saw (in chapter two) that if (3) can be established, then the epistemological argument against platonism is a good argument, and platonism ought to be abandoned. But it's not clear that (3) <u>can</u> be established, for one could attack the above argument in three different ways, namely, by denying (1), by denying (2), or by denying that (1) and (2) provide us with a good reason to believe (3).

Now, I argued in the last chapter that, regardless of which strategy platonists choose here, in order to adequately respond to the epistemological argument, they will have to actually explain how human beings can attain knowledge of mathematical objects. That is, they have to cooperate with anti-platonists and actually provide the epistemological explanation being demanded. In connection with the third strategy -- i.e., the strategy of denying that (1) and (2) provide a reason to believe (3) -- I admitted that platonists don't really _need_ an argument here, that, since (1) and (2) don't _imply_ (3), platonists can simply deny that the former statements lend any support at all to the latter. But I argued that this move is rather implausible. In particular, I pointed out that platonists who accept a certain very weak and plausible intuition -- which I called (PI) -- have to admit that (1) and (2) provide a _prima facie_ reason to believe (3); and we saw that the only way for such platonists to remedy the situation is to _actually explain_ how (3) could be false while (1) and (2) are true. But to do this just _is_ to provide an epistemology. As far as the first two strategies are concerned -- i.e., the strategies of denying (1) and (2), respectively -- we noticed that platonists could not erase the epistemological suspicion surrounding (3) by merely arguing that human beings and mathematical objects exist in the same realm; we saw that they would also have to argue that they exist in the same realm _in a way that makes it possible for human beings to attain knowledge of mathematical objects_. Thus, we concluded that in order to respond in a plausible way to the epistemological argument against platonism, one must construct an epistemology of mathematical objects.

We also saw in the last chapter that -- since there will have to be something wrong with the argument in (1)-(3) if platonists are going to be able to account for mathematical knowledge at all -- the only three plausible ways of constructing a platonist epistemology correspond to the three ways of responding to the argument in (1)-(3). Thus, the first way in which a platonist might construct an epistemology is to deny (1) and argue that human beings are capable of somehow "elevating" to the abstract level and attaining some sort of epistemological "access" to mathematical objects. (This is Gödel's strategy.) The second way in which a platonist could construct an epistemology is to deny (2) and argue that human beings can acquire knowledge of abstract objects by interacting with them "at home" -- i.e., in the space-time continuum. (This is Penelope Maddy's strategy; she believes that human beings can actually <u>see</u> mathematical objects.) The third way to construct a platonist epistemology is to explain how (3) could be false even if (1) and (2) are true; i.e., to explain how human beings could attain knowledge of mathematical objects in spite of the fact that they cannot interact with such objects. (This is the strategy of Mark Steiner, Jerrold Katz, Charles Parsons, and Michael Resnik, among others.)

In chapters five and six, I will argue that platonists can use this third strategy to debunk the epistemological argument against platonism. In the present chapter and in the next chapter, I will argue that the epistemology given in chapters five and six is the only plausible one available to platonists; I will do this by arguing that the other two epistemologies available to them (namely, the "interaction-based epistemologies"[1] given by the first and second strategies of the preceding paragraph) are both implausible. (Chapter four will be devoted to the second strategy, i.e., Maddy's; and the present chapter will be devoted to the first strategy, i.e., Gödel's.)

I do not have a <u>knockdown</u> argument against Gödel's interaction-based epistemology. What I have are a series of considerations which suggest that his epistemology is implausible and that it <u>might</u> be incoherent. But it is important to note that the main conclusion of this essay -- viz., that human beings can attain knowledge of mathematical entities without interacting with them (in the way in which I will describe in chapters five and six) -- does <u>not</u> depend upon a refutation of interaction-based epistemologies, like those of Gödel and Maddy. If the platonist epistemology that I develop in chapters five and six is plausible and appealing, then it will be acceptable, even if I cannot <u>prove</u> that other platonist epistemologies (such as Gödel's) are unacceptable. In order to motivate the acceptance of the interaction-free epistemology of chapters five and six, it will be enough to show that these other platonist epistemologies are unappealing and implausible.

2. The Notion of Contact

The first two platonist strategies for rebutting the epistemological argument -- i.e., the strategies of denying (1) and (2), respectively -- are, in one respect, very similar: both involve the claim that human beings can _interact_ with mathematical objects, or that they can come into _contact_ with such objects. That is, they both appeal to a faculty of mathematical intuition which provides some sort of _access_ to mathematical objects. Now, ultimately, I'm not certain that this sort of talk makes sense. However, for the sake of argument, we must at least _try_ to make it coherent. In other words, since it's pretty clear that Gödel and Maddy both believe in such contact, we ought to be charitable to them and not rule them out of court for unintelligibility. We ought to try to make sense of their positions.

It seems to me that the easiest way to handle talk of contact between human beings and mathematical objects is to treat "contact"[2] -- in this context -- as a technical term. Now, of course, this doesn't mean that there are no non-technical uses of "contact". Indeed, the reason for choosing this term is that its ordinary non-technical meaning is supposed to play a suggestive role in fixing the technical meaning. Now, there is obviously more than one colloquial meaning of the word "contact." The primary one is probably that which applies to two things that are actually touching each other. (In ordinary usage, the two objects are physical objects, like a book and a table; but this could be extended to abstract objects, like a circle and tangent line.) In another ordinary usage, we say that two people can have eye contact. And philosophers have extended this to humans and visible objects: when we see an object, we are in contact with it. Moving farther away from ordinary language, some philosophers seem willing to say that I am in contact with Elvis Presley, because there is a causal chain leading back in time from me to him. (This causal chain might, for instance, consist in the fact that I have read an article written by someone, who spoke with someone, who remembers actually touching Elvis.) But while all of these meanings of "contact" are suggestive, we ought not to treat them as anything more in thinking about the sort of contact alluded to by Gödel and Maddy. In order to avoid begging any questions, it will be best to simply ignore these other uses and treat "contact" (within this context) as a technical term. To be more precise, I will use it as a relational term, which always takes two arguments, one a human being and one a mathematical object; and to say that human beings can come into contact with mathematical objects will mean nothing more than that information[3] can pass from mathematical objects to human beings.

Now, I have defined "contact" in a fairly broad way here, because I wanted to be true to the intentions of both Gödel and Maddy. But we must keep in mind that (regardless of what Maddy says) these two philosophers are not envisioning the same sort of contact. Whereas Maddy has in mind a relation between two physical objects, i.e. two objects with spatio-temporal location (viz., sets of physical objects and human brains) Gödel was apparently thinking of a relation between two non-physical objects (viz., set-theoretic sets and human minds). But, while different, the two sorts of contact are obviously very similar: both involve information passing from sets to human beings; one involves perception, and the other "something like a perception."[4]

It might be argued that by defining contact as an "information transfer," I have done nothing to fend off the charge of unintelligibility; for "information transfer" might make no more sense in connection with non-physical objects than "contact" does. This objection may ultimately be valid. However, I think I have accomplished at least two things by defining contact as an information transfer. First, I have provided Gödelians with a way of speaking about mathematical intuition which, to my mind anyway, sounds a bit less mysterious than the way in which Gödel himself spoke. Second, and much more important, I have located the absolute weakest thesis that Gödelians must defend. If platonists are to answer the Benacerrafian challenge by appealing to some sort of contact, it seems that they must at least be able to make a case for the claim that information can, in some way or other, get from the mathematical realm to the human/mind brain. Now, again, it might simply be unintelligible to speak of such an information transfer, but I do not want to pursue this objection right now. For this is, in essence, what I will be discussing in the rest of this chapter. My task will be to determine whether we can make sense of

Gödel's belief that human beings can come into contact with mathematical objects, and if so, whether it is plausible to think that they actually do.

But before I move on to consider Gödel's position, I would like to say two more things about the charge of unintelligibility.

The first has to do with Maddy. Since she does not take mathematical objects to exist outside of space and time, it might seem that this objection wouldn't apply to her. This, however, is an illusion; for, insofar as she claims to be a platonist, she does think that mathematical objects are abstract. Thus, she seems to be flirting with a double unintelligibility: it might be unintelligible to talk of "abstract" objects existing in space and time, and it might be unintelligible to speak of information "passing" from abstract to concrete objects. But, again, I will not pursue this here, for when I discuss Maddy in chapter four, one of my primary concerns will be whether Maddy can make this position intelligible. (And, again, my other primary concern will be whether she can make her position plausible.)

My second point about the charge of unintelligibility has to do with those platonists -- myself included -- who accept (1) and (2). I merely want to point out that, for such philosophers, there is absolutely no problem here: such philosophers need not mention contact at all. Since they don't believe in contact between human beings and mathematical objects, it is simply irrelevant -- for their purposes -- whether it is intelligible to speak of such contact.

Now, I suppose it is possible to accept (1) and (2), and also believe in some sort of contact between human beings and mathematical objects; for one might believe in some sort of "cross-realm contact," i.e., contact between physical and non-physical objects. I will not discuss this possibility, simply because I don't know of any philosopher who has attempted to explicate the position, let alone defend it. That no one has tried to explicate this position is, I think, quite important, for, to my mind, it is incoherent. Indeed, we will see in this chapter that it precisely because inter-realm contact seems impossible that Gödel was, in some sense, forced to adopt an immaterialist philosophy of mind. The reason I mention this now is that my discussion will be couched in somewhat different terms and I will not return to the notion of "inter-realm" contact at all. But I would like to point out that while I will not explicitly consider the question of whether Gödel ought to be interpreted as positing an inter-realm contact between human beings and mathematical objects, I will be citing some passages from Gödel which should quell any suspicion that such an interpretation is correct.[5]

3. Two Qualifications to My Argument

I have indicated that, in this chapter, I will try to argue that platonists cannot plausibly rebut the epistemological argument by rejecting (1). Now (at the end of section 1) I qualified this by admitting that my arguments are not knockdown arguments; but two more qualifications are needed.

The first is that one of my two main arguments (viz., the argument of section 5, which concludes that Gödel's view might not be intelligible[6]) is not an in-principle argument: it is directed against a particular advocate of the strategy of rejecting (1), namely Gödel. But (as was the case with the fact that my arguments fall short of proofs) this is acceptable. For it turns out that the argument in question here could be employed with equal success against all versions of the Gödelian position. Let me explain why. My argument is based on the fact that Gödel did not present an intelligible picture of what mathematical intuition (or contact with mathematical objects) is like; in other words, the argument is that he didn't sufficiently explain how human beings could attain knowledge of mathematical objects, even granted that such creatures are, in some sense, immaterial. The reason that this argument would work against any advocate of the Gödelian position is that, to the best of my knowledge, no one else has made the position sufficiently intelligible, either.

One might object that, to make this claim stick, I would have to consider all versions of the Gödelian position. Now I obviously can't do this, but it is worth noting that Gödel's version of the position is (in my opinion, anyway) the clearest and most appealing that there is. In fact, this is the very reason for choosing his version as the paradigm case of the position: it comes the closest to intelligibility. This is striking, because his remarks are brief and rather obscure; but it seems to me that the remarks of others are even more obscure. In fact, it is hard to find philosophers who we can even safely interpret as using the same strategy as Gödel. (This is because the philosophers who have given relatively clear accounts of mathematical intuition do not, in general, follow Gödel in rejecting (1). They take a much more naturalistic approach to intuition, and thus, treatment of their positions would fall more naturally into one of the later chapters of this essay.[7]) Plato, it must be admitted, is quite clear about basing his epistemology of abstract objects upon a rejection of (1); he thought that before human beings are born, their souls have access (in heaven) to the Forms, and that earthly mathematical knowledge is attained via recollection.[8] Beyond this, however, I know of no other clear examples.

Frege, of course, jumps immediately to mind, here, for he often sounds quite like Gödel in this connection: he speaks of "a particular mental capacity, the power of thought," which allows us to "grasp," or "apprehend" abstract objects.[9] But while much of what Frege says about the apprehension of propositions (or "thoughts," as he called them) is remarkably similar to what Gödel says about the intuition of mathematical objects,[10] it is not at all clear that Frege wanted to reject (1) and claim that human beings can actually come into <u>contact</u> with abstract objects. The reason this is unclear is that Frege simply never said anything to reconcile the apparent truth of (1) and (2) with his belief in the faculty of apprehension. But regardless of whether Frege meant to solve this problem as Gödel did -- i.e., by rejecting (1) -- the point is that <u>if he did have this intention</u>, then he was just as guilty as Gödel of not making it clear that the position is intelligible. This is so obvious that it doesn't even require justification; after all, as I've pointed out, it's difficult to ascertain whether Frege even <u>accepted</u> the position. In short, the problem with Frege's few epistemological remarks is quite analogous to the problem with Gödel's: he never gives a positive, non-metaphorical account of what he's talking about. (Frege admits that his use of "apprehension" is a metaphor. His only characterization of that mental faculty is a negative one: he says that it is not like <u>having</u> an idea or like <u>seeing</u> a star.)

The second qualification I want to make concerning my arguments against the rejection of (1) is actually more of a disclaimer. I want to guard (from the start) against responses having to do with exegesis. I will interpret Gödel as solving the epistemological problem with platonism by rejecting (1). Now, one might object to this interpretation. One might think that Gödel wanted to reject (2) or that he wanted to accept (1) and (2) and deny (for some reason -- perhaps because he believed in some sort of inter-realm contact) that they lend support to (3). Now, I think there is good evidence to support my interpretation over these others,[11] but I will not try to support this here,[12] because it is simply irrelevant (in the present context) whether I have interpreted Gödel correctly. My reasoning is as follows: during the course of this essay, I will consider all of the plausible solutions to the epistemological problem with platonism. Thus, it doesn't really matter which of these solutions was really Gödel's. In this chapter, my primary concern will be to show that the strategy of rejecting (1) is implausible; I am concerned with Gödel's view only insofar as I think that this is his strategy.[13] But even if I have interpreted Gödel incorrectly, my argument will not be hurt, for I will be considering the strategies of rejecting (2) and of denying that (1) and (2) provide a good reason to believe (3). Thus (since these are the only responses to the epistemological argument that are even initially plausible) I will not be over-looking any plausible solution by misrepresenting Gödel.[14]

4. Gödel's View of Mathematical Intuition

Gödel never devoted an entire work to explicating the faculty of mathematical intuition. He made only a few scattered remarks in this connection, and these are rather cryptic and metaphorical. What I would like to do is simply list most (if not all) of the remarks that I could find that seemed relevant to the task of understanding his view of intuition. We will then be able to refer back to them.

(Q1) But, despite their remoteness from sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true.

(Q2) It should be noted that mathematical intuition need not be conceived of as a faculty giving an _immediate_ knowledge of the objects concerned. Rather it seems that, as in the case of physical experience, we _form_ our ideas also of those objects on the bases of something else which _is_ immediately given.

(Q3) Evidently, the "given" underlying mathematics is closely related to the abstract elements contained in our empirical ideas. It by no means follows, however, that the data of this second kind, because they cannot be associated with actions of certain things upon our sense organs, are something purely subjective, as Kant asserted. Rather they, too, may represent an aspect of objective reality, but, as opposed to the sensations, their presence in us may be due to another kind of relationship between ourselves and reality.

(Q4) [Russell is correct in claiming that we can] compare the axioms of logic and mathematics with the laws of nature and logical evidence with sense perception, so that the axioms need not necessarily be evident in themselves; but rather their justification lies (exactly as in physics) in the fact that they make it possible for these "sense perceptions" to be deduced.

(Q5) It seems to me that the assumption of [mathematical] objects is quite as legitimate as the assumption of physical bodies and there is quite as much reason to believe in their existence. They are in the same sense necessary to obtain a satisfactory system of mathematics as physical bodies are necessary for a satisfactory theory of our sense perceptions and in both cases it is impossible to interpret the propositions one wants to assert about these entities as propositions about the "data," i.e., in the latter case the actually occurring sense perceptions.[15]

(I have decided not to include in this list a passage about intuition from Gödel (1958) and (1972); my reason is that Gödel seems to be talking, in this passage, about _Hilbert's_ notion of intuition, and not his own.[16])

The striking thing about the above passages is the analogy between sense perception and mathematical intuition that runs through each one of them. In trying to understand Gödel's view of the faculty of mathematical intuition, one cannot stress this analogy enough, because it is the very <u>backbone</u> of Gödel's view: he simply doesn't give us any other picture of mathematical intuition. The only difference that Gödel seems to allow between intuition and sense perception is that the former faculty provides access to mathematical objects, whereas the latter faculty provides access to physical ones. On the other hand, the similarities, on Gödel's view, are numerous. The most <u>important</u> of these similarities, in my opinion, are, first, that both faculties provide us with some sort of <u>contact</u> with objective entities, and, second, that, in both cases, this contact can be thought of in terms of an <u>information transfer</u>. Now, of course, Gödel didn't put it in precisely these terms, but it seems to me that this is what he had in mind.

We can see that Gödel <u>did</u> have this view of intuition by considering his remarks -- most strikingly in (Q2), (Q3), and (Q5) -- about "mathematical data," and "the underlying mathematics," and what's "immediately given." It is clear that, according to Gödel, actual mathematical objects are <u>not</u> what's given in intuition. Now my point here is <u>not</u> that mathematical <u>propositions</u> are what's given. (In other words, my point has nothing to do with Steiner's distinction between intuitions-of -- i.e., representations <u>of</u> mathematical objects -- and intuitions-that -- i.e., beliefs <u>that</u>

mathematical propositions are true.[17] So far as that distinction's concerned, I agree with Parsons that, according to Gödel, the two sorts of mathematical intuition are intimately connected and always occur together.[18]) What I am saying applies equally -- on Gödel's view -- to the intuition of both objects and propositions. The point is that the underlying mathematical data are <u>neither</u> mathematical objects <u>nor</u> mathematical propositions; they are psychological phenomena that are much more amorphous than the objective mathematical entities and propositions that they mirror. The analogy with sense perception is quite strong here: just as actual physical objects are not <u>given</u> to us in perception, so, too, when we intuit objective mathematical entities, these are not immediately given to our minds, either. In both cases, what is immediately given is some amorphous, underlying psychological "data," from which we infer the existence of objective entities which, in some sense, <u>give rise to</u> the "given data." (Further support for this interpretation is leant by the fact that Gödel speaks of "the mere psychological fact of the existence of an intuition."[19])

Thus, we see that, on Gödel's view, sense perception and mathematical intuition both involve a <u>transfer of information</u> from objective entities to human minds. That Gödel wouldn't object to this sort of language in connection with mathematical intuition is clear from (Q3), where he says that the presence in us of mathematical data is due to some sort of relationship between us and reality (where this relationship is presumably similar to, but different from, sense perception). It seems to me that Gödel is fairly clear here: he is saying that, somehow, mathematical data gets from the mathematical realm to the human mind/brain.

It should be clear why Gödel adopted this "representational" view of intuition. For insofar as he took mathematics to be about objective entities, and in so far as a person's intuitions are his or her own, he could not have claimed that the psychological data of intuition are identical with the actual mathematical objects. For this would require the objects of mathematics to actually be inside the minds of people engaged in mathematical intuition (and this would involve a departure from, or perversion of, platonism).

The above remarks have been fairly brief and have not been intended to cover every aspect of Gödel's view of intuition. They are meant only to explicate and support my claims that (a) mathematical intuition -- according to Gödel -- involves contact between human beings and mathematical objects (i.e., a transfer of information from the latter to the former) and (b) this contact is -- again, according to Gödel -- best understood metaphorically (in particular, by thinking of it as similar to the sort of information transfer that occurs in sense perception).[20]

5. Is Gödel's View Intelligible?

I said in section 2 that I would be concerned with answering two questions about Gödel's contact-based epistemology of mathematical objects: "Can it be made intelligible?" and if so, "Is it plausible?" Since the first question is obviously logically prior to the second, I will begin with it.

Now, given that I was able to explain Gödel's view in the last section, it might seem obvious that it _is_ intelligible. But the exposition of the last section leaves many questions unanswered (e.g., "What is the _nature_ of the underlying mathematical data?" and "Precisely what relation do they bear to mathematical objects, on the one hand, and the human mind/brain on the other?"). It seems to me that the presence of some of these unanswered questions generates some suspicion about the intelligibility of Gödel's view. For it's not clear that we can answer these questions (and, hence, make Gödel's epistemology more precise) without also making it unintelligible. Let me explain this.

In section 2, we grew suspicious about the intelligibility of Gödel's view of mathematical intuition by reflecting on the notion of _contact_. In particular, we saw that it isn't obvious that talk of contact between mathematical objects and human beings (which, as we now know, Gödel endorses) makes sense. Before we accept such talk as meaningful, we need an explication of what it means, that is, a description of what such contact is _like_. But given that this obscurity about the meaning of "contact" is the source of our worries about the intelligibility of Gödel's view, it doesn't seem that these worries can be eased by any of the remarks quoted in the last section. For all of those remarks were metaphorical: they all sought to explicate contact with mathematical objects by claiming that it's similar (or analogous) to perceptual contact with physical objects. But while intuition may play a role analogous to that of perception, it's clear that its operation cannot be very similar. (We will see why this is the case three paragraphs below.) Gödel needs to say something about how the faculty of intuition operates, but he hasn't done this. To be more precise, he hasn't said what mathematical data is, or what relation it bears to objective mathematical objects, on the one hand, and human beings on the other.

Benacerraf has stated this objection to Gödel's epistemology in slightly different terms. He seems to think that Gödel's notion of intuition borders on vacuity. He writes that

> without an account of how the axioms "force themselves upon us as being true," the analogy with sense perception...is without much content.[21]

Now a Gödelian might respond to this objection by claiming that, while we might not be able to say anything about the contact between human beings and mathematical objects, we can't really say what the contact between such creatures and physical objects is like, either. But this is just false. While we might not have a high degree of knowledge here, we are not entirely ignorant, either. It seems likely that we attain "contact" with physical objects when (for instance) light bounces off of such objects and into our eyes.

This point serves not only to show that we _do_ have some understanding of the link between physical objects and the human mind/brain, but also to support the claim I made three paragraphs back that the operation of intuition could not be very similar to that of sense perception. For the little that we _do_ know about the contact between human beings and physical objects precludes the possibility that the contact between such creatures and _mathematical_ objects is of the same general sort. This point could be made in connection with any of the five senses, but sticking with the above example, we can say that (visual) contact with physical objects occurs when light _bounces off_ of such objects and travels _through space_ to our _eyes_. But (a) nothing can bounce off of mathematical objects; (b) a "signal" from such objects could not travel through space; and (c) it's not clear how we could "receive" a signal carrying mathematical data, because we're not aware of any non-sensory organs of reception, and even Gödel admits that we don't _sense_ mathematical objects.

The point of all of these remarks is not that Gödel's view of intuition _is_ unintelligible; it is rather that, as of right now, we have no idea how to make Gödel's (extremely vague and imprecise) view more precise without _also_ making it unintelligible. In order to make the view more precise while remaining coherent, one would have to say something intelligible about how mathematical intuition works (i.e., about how human beings could come into contact with abstract objects); in particular, one would have to say what mathematical data is and how it gets into the human mind/brain. To formulate this as an argument against Gödel, the point is that we cannot accept the view that human beings interact with mathematical objects until we have some idea of what such interaction could consist in.

Now one might try to go further and argue that Gödel's view _is_ unintelligible; for it might be argued that we can rule out all possible ways for mathematical data to get into the human mind/brain. We have seen that, insofar as Gödel is a platonist, mathematical objects _themselves_ could not enter the mind/brain. But the only other possibility -- that mathematical data "pass from" mathematical objects and into the mind/brain -- has been ruled out also, because a "signal" from mathematical objects could not pass through space, and so could never "arrive" at the mind/brain. Thus, on these grounds, one might argue that Gödel's view is unintelligible. (One might -- and Katz _has_[22] -- put this argument in more conventional terms: since perception is a _causal_ process, and since human beings and mathematical objects cannot be causally related, it is unintelligible to speak, as Gödel does in (Q1), of "something like a perception of" mathematical objects.)

But the Gödelian could object to this line of argument on the grounds that it assumes that mathematical data would have to pass through space in order to get into the human mind. If the human mind is immaterial and, in some sense, not dependent on the brain -- that is, if (1) is false -- then perhaps mathematical data could, somehow, get from mathematical objects to the human mind without going through space. (In what I call the more "conventional" terminology, this response is that mathematical intuition could be "something like" perception without being a causal process, because mathematical objects and immaterial human minds could be related in "something like" a causal way.) Obscure as all of this sounds, I believe that it is Gödel's position. He remarks several times in the (forthcoming) "Gibbs Lecture," that we are led from mathematical considerations to immaterialism about the mind.[23] Regardless of what Gödel thought, however, the point is that the argument of the last paragraph does not establish that Gödel's view is unintelligible, because it simply ignores the possibility that the human mind could be an immaterial entity.

It's not entirely clear, however, that the adoption of an immaterialist philosophy of mind is of any help in staving off the Katzian argument; for it's not clear how an immaterial mind could come into "contact" with mathematical objects. After all, the two sorts of things don't seem to have very much in common. Thus, there is reason to believe that, even granting immaterialism about the mind, we might be able to revise our argument to show that Gödel's view is unintelligible. For even if my mind is immaterial, it would still exist "inside" (whatever that means in this context[24]) my body. Thus, it seems that, even granting immaterialism, mathematical data would have to pass through space in order to get into the human mind.[25] The alternative -- that the mind is capable of, somehow, leaving the body -- seems (a) wildly implausible and (b) unhelpful. For (a) it is simply too hard to believe that mathematical intuition requires an out-of-body experience; and (b) even if the mind could "leave" the body, it doesn't seem that there's anywhere it could go in order to attain contact with mathematical objects.

In spite of the seeming preponderance here of obscurity and incoherence, I will refrain from claiming outright that Gödel's position <u>is</u> unintelligible, because there might be some way to make sense of all of this that I simply haven't thought of. It seems quite clear, however, that Gödel's view <u>might</u> be unintelligible, i.e., that there might be no way to clear up the obscurity surrounding his view without either abandoning the intuitive idea behind the view (viz., that we are capable of interacting with abstract objects) or collapsing into unintelligibility. Thus, it also seems clear that, before we can accept Gödel's view, it must be shown that there <u>is</u> an intelligible way to make it more precise. In particular, the Gödelian needs an intelligible account of what mathematical data is and how it could get from mathematical objects to the human mind/brain.[26]

It is worth noting that the demand being placed on Gödel here is relatively _weak_. What we would _really_ need -- in order to make Gödel's position clear -- is a genuine psychological (or neurophysiological) hypothesis positing the existence of some (at least fairly) explicit faculty of intuition. My point in this section has been that Gödel is nowhere near this. We have _no idea_ what intuitive contact with mathematical objects could consist in, because we have no idea what mathematical data is or how it could get into the human mind/brain.

But perhaps something needs to be said here to support my claim that Gödel has given no account of mathematical data and its relation to mathematical objects and human minds. For it might be argued that he supplied at least the _beginnings_ of an answer to these questions in (Q3), where he says that "the 'given' underlying mathematics is closely related to the abstract elements contained in our empirical ideas." By "the abstract elements contained in our empirical ideas," Gödel means to refer to ideas such as the concept of _object_, which he thinks isn't received through perception, but which he thinks _is_ "given" to us.[27] Gödel's point, then, is that there is some connection between concept acquisition and the intuition of mathematical data.

If we try to interpret (Q3) as an explication of mathematical data and the process by which we acquire it, then we will run into at least three problems. First, it's not clear that a reliance upon concept acquisition is _helpful_; for we don't know very much about concept acquisition, and Gödel doesn't say anything about how he thinks we acquire concepts. Second, the reliance upon concept acquisition might be circular, because Gödel thinks that concepts are mathematical objects. Thus, he thinks we will be intuiting concepts in intuition. Therefore, concept acquisition might just be a special case of mathematical intuition.[28] Finally, even if these first two objections can be overcome, there is a third problem: Gödel doesn't argue that mathematical intuition has anything to do with concept acquisition. The two might be related, but, prima facie, there's no reason to suppose that they are. We just don't have any reason to believe Gödel here.

Maddy has taken (Q3) as an indication that Gödel had in mind something like the naturalized intuition that Maddy herself favors.[29] In chapter four, I will explicate this theory and then argue that it could not be correct. Thus, there is no need to consider it here. There is also no need to argue against Maddy's interpretation of Gödel, because, as I pointed out in section 3, I am not interested in exegesical issues. I will simply state dogmatically that I think her interpretation is clearly incorrect: Gödel pointed out quite often that the objects of mathematics are located outside of space and time; and all of his allusions to sense perception were intended only to give us an analogical account of intuition. (For whatever it's worth, Maddy has recently taken back some of her claims about the similarities between Gödel's view and her own view.[30])

One might also take (Q3) as an indication that Gödel thought that acquaintance with (or intuition of) mathematical objects arises as some sort of "by-product" of repeated perceptions of physical objects. That is, one might think Gödel was suggesting that just as the concept _horse_ is acquired after repeated perceptions of horses, so the concept _two_ is acquired after many perceptions of piles of two things (or after a certain amount of experience with predicates satisfied by two things). In connection with this interpretation of Gödel, I will say exactly what I said in connection with Maddy's interpretation. First, I will be considering this by-product-of-perception view of intuition later (in chapter five[31]) and so there is no need to consider it here, even if it really _is_ Gödel's view. And second, I do _not_ think it is Gödel's view. He doesn't think that intuition is _grounded_ in sense perception any more than he thinks it _is_ sense perception. Now, he probably _did_ think that intuition is, somehow, _stimulated_ by sense perception (i.e., that perception plays at least _some_ role in getting intuition started); but this just doesn't seem to be very helpful. I think that any sensible theory of mathematical intuition would have to admit that perception plays at least a stimulative role;[32] but this doesn't tell us _anything_ about the way in which intuition actually _works_.

I would like to make one last point about the argument of this section. We have come to the conclusion that it is an open question whether Gödel's epistemology can be made more precise in an intelligible way. This, I think, should make his epistemology rather unappealing. However, it is worth pointing out, in defense of Gödel, that he was certainly aware of this sort of criticism. He was not pretending to have a full-blooded theory of mathematical intuition. (I suppose it is possible that Gödel had such a theory in mind, but refrained from publishing it for one reason or another; I find this highly improbable, but as more of his unpublished works become available, the answer to this question should emerge.)

I believe that we should think of Gödel's remarks on mathematical intuition as providing not a <u>theory</u>, but, rather, a <u>theory scheme together with a research programme</u>. Moreover, I think Gödel would agree to this. He was not providing a mathematical epistemology; he was simply suggesting one way in which we might try to develop one.

We should keep in mind, however, that by interpreting Gödel in this way, we do not simply side-step the problems that I have raised in this section. In arguing that we might not be able to make Gödel's view intelligible, I have, in effect, argued that we might not be able to pursue his research programme. That is, the point of my argument is simply that it is unclear where the Gödelian can go from here. I have refrained from drawing the strong conclusion that Gödel's programme is definitely unpursuable, but at the same time, I simply have no idea <u>how</u> one could pursue it. Moreover, no one else seems to know how to pursue Gödel's programme, either; that is, to the best of my knowledge, no one actually has pursued it (assuming that Maddy's position is not such a case).

6. Is Gödel's View Plausible? (Part I)

That it is an open question whether a Gödelian epistemology can be made intelligible seems to provide significant motivation for a dissatisfaction with Gödel's view. But I think we can make his view seem even more unappealing if we turn to the second question that I said I would try to answer, viz., "Is Gödel's view plausible?" Now it might seem that, since we haven't been able to say what Gödel's view is, we can't turn to the question of whether or not it is correct. But this is an illusion. It turns out to be possible to argue that Gödel's view is implausible, even though we lack an explicit formulation of the view, because, as we have seen, any Gödelian view will involve the adoption of Cartesian dualism (or, to be more precise, it will involve the rejection of (1) and the adoption of an immaterialist philosophy of mind). Thus, what I would like to do in this section is simply provide a direct argument for (1). Since Gödel's epistemology is based upon the denial of (1), we will, by so doing, be arguing against his epistemology. Now, I do not have a knockdown argument against immaterialism. Philosophers have been arguing this issue for centuries, and it would be presumptuous to think that I could settle that dispute in a short digression. I do think, however, that I can show that, as of right now, immaterialism is not the most appealing philosophy of mind.

The first point to be made here is that Gödel's immaterialism cannot be a simple denial of eliminative materialism, or some other strict but implausible version of materialism;[33] for it doesn't seem that the denial of eliminative materialism would serve Gödel's needs. This is because the falsity of eliminative materialism (i.e., the existence of minds) does not imply that there are immaterial minds; that is, it doesn't imply that the human mind is an autonomous entity that exists outside of space and time. (That Gödel needs the mind to be an immaterial thing has already been made clear: he requires mathematical data to, somehow, get from mathematical objects to the human mind, but he cannot allow such data to pass through space and into the brain; thus, it seems that Gödel's only option is to claim that the human mind is an immaterial thing.) So it simply won't help Gödel to deny eliminative materialism and accept some weak sort of dualism (i.e., a dualism that merely recognizes the reality and irreducibility of mental states); for such a stance is compatible with a denial of immaterialism. This should be obvious: many philosophers (e.g., Nagel, Searle, and Fodor, to name just a few[34]) believe that we cannot dispense with our mental terminology, because we need it to account for our behavior and experiences; but almost no one thinks that this implies that there is something immaterial about the mind. Certainly none of the three philosophers mentioned above do. Thus, by merely accepting the irreducibility of the mental, Gödel does nothing to explain how data could get from immaterial mathematical objects to the human mind/brain without passing through space. If Gödel is to have any luck in providing such an explanation, it seems that he must claim that the mind is, in some sense, immaterial.

So it seems that Gödel needs to reject not only strong materialist theories, like eliminative materialism, and not only weak ones, but also weak dualistic theories. In short, he has to reject any view that involves the claim that there is nothing immaterial about the mind. Thus, Gödel's epistemology is incompatible with almost all contemporary views of the mind/brain. He is committed, it seems, to substance dualism, rather than simply property dualism. In other words, he is committed to a version of Cartesianism.

For whatever it's worth, Gödel seems to admit this. He says (in describing the sort of immaterialism that we might be forced to accept) that we are "driven to take some vitalistic viewpoint" and that there is no "possibility of a purely mechanistic explanation of psychical and nervous processes."[35]

Our question, then, is whether this strong version of immaterialism that Gödel must accept -- basically Cartesian dualism -- is true. If I can argue that it isn't, then I will have an argument against Gödelian epistemologies. Now, of course, I cannot diverge at this point to give a full-blooded argument against immaterialism, but I can make a few remarks that suggest that it might be rather implausible. My argument here is based on Ockham's razor: positing an immaterial mind, or soul, is unjustified, because such a posit doesn't explain anything. That is, it doesn't do any theoretical work. We can see that this is true in two different ways: by appealing to cognitive science and by reflecting on the question ourselves.

The former route is the easiest, for it is simply an empirical fact that current scientific theories of the mind/brain do not quantify over immaterial minds.[36] (It is also worth noting, here, that so-called Folk Psychology doesn't quantify over immaterial minds, either. That "theory" explains things like drinking-behavior by appealing to things like desires for liquid; but to quantify over desires is <u>not</u> to quantify over immaterial minds, for it might be that desires can arise from a completely materialistic brain.) Now, admittedly, the current theories of cognitive science (and Folk Psychology) fall far short of explaining all mental phenomena, but this is a function of our ignorance about the brain, and not of our refusal to posit immaterial minds. The fact is that if we <u>did</u> posit such entities, we would do nothing to increase our ability to explain mental phenomena. Therefore, by Ockham's razor, we ought not to posit such entities.

This brings me to the second way in which we can argue that Cartesian minds do no theoretical work. Let us consider whether my claim is, indeed, correct. Is it really true that by positing immaterial minds we would be able to explain no more than we can without that posit? Well, let's ask what immaterialists think they are going to explain by positing such minds, and then see whether minds actually do explain this. It is often said that something like a soul must be posited in order to account for conscious experience. But it seems to me that it is no less puzzling how an immaterial mind could experience, say, pain, than how a physical brain could give rise to pain experience. In fact, it might even be more puzzling, for we at least have the very beginnings of a theory of how a physical brain could give rise to feelings of pain. (There is, for instance, an impact on a nerve somewhere in the body, and this sends a signal to the brain, which....) On the other hand, we can't say anything about how an immaterial mind could feel pain, unless, of course, such minds are simply defined as entities which experience things like pain. But if we simply define minds in this way, our "explanation" of conscious experience would be shamelessly ad hoc.

We might not be able to explain very much about the mind/brain without positing an immaterial mind, but (a) positing a mind doesn't seem to help our ignorance here, and (b) we do seem to be making some progress in neurophysiology by assuming that all mental phenomena are grounded in the physical. (We have succeeded, for instance, in locating the parts of the brain that are responsible for visual perception, and we seem to be making progress in describing the physical processes involved in perception.[37]) Therefore, since we have to believe in brains anyway (because we can see them) there doesn't seem to be any reason to believe in minds also, since they play no explanatory role.

Much more could be said in connection with this argument against immaterialism, but it would not be appropriate to pursue it here, for we could probably never finish the argument.

In addition to providing an argument against immaterialism (as I have now done) it might also behoove me to consider the arguments in favor of immaterialism, and to say what is wrong with them. There are two reasons why I won't do this. First, this would be an enormous project; thus (since it is also a tangential project) we cannot spare the space necessary to properly pursue it. Second, the arguments in favor of immaterialism are only of limited importance, because, even if that doctrine is true, Gödel will still have to face the problem of intelligibility discussed in the last section. In other words, even if the human mind is an immaterial entity, it is still not at all clear how it could come into contact with mathematical objects. So for these two reasons, I will not consider any of the traditional arguments for dualism. However, there is one argument for immaterialism that I would like to briefly consider, namely an argument suggested by Gödel's writings. This argument deserves special consideration not simply because it is Gödel's position that we are considering, but also because looking at his argument might give us a greater understanding of the kind of immaterialism that Gödel accepts, and this might, in turn, give us a greater understanding of his epistemology.

The argument I want to discuss comes from Gödel's (forthcoming) "Gibbs Lecture," but it is only implicitly present in that essay.[38] Gödel's doesn't explicitly state the argument as an argument for immaterialism, but there can be no doubt that he does endorse this argument, because Hao Wang has spoken to him about it.[39]

Gödel begins the discussion which suggests the argument I have in mind by distinguishing objective mathematics (i.e., the body of objectively true mathematical propositions) from subjective mathematics (i.e., the body of humanly provable mathematical propositions). Now, we know from Gödel's first incompleteness theorem[40] (which says that in any recursively axiomatizable and consistent mathematical theory which is strong enough to contain Peano Arithmetic there will always be undecidable propositions, i.e., propositions which can neither be proven nor disproven) that no recursive axiomatization can produce all of objective mathematics. But Gödel points out that it might be possible for a recursive axiomatization to produce all of subjective mathematics.[41] And he also points out that if subjective mathematics is recursively axiomatizable, then this would show that the human mind is like a machine (at least in its mathematical reasoning). Gödel is correct here, because if subjective mathematics is recursively axiomatizable, then any theorem we could ever prove could be derived in a finite number of steps from a recursive list of axioms and rules; but it is provable that anything which can be so derived, can be produced by a Turing machine.[42] Thus, it follows that if subjective mathematics is recursively axiomatizable, then the human mind/brain is reducible to a Turing machine (at least in its mathematical reasoning).

Note that to say that the mind/brain is "reducible to" a Turing machine is not to say that it is a Turing machine; it is only to say that the mind/brain is Turing equivalent, i.e., that it is possible to build a Turing machine which, given the same inputs, will produce the same outputs.

Gödel believes, however, that subjective mathematics is not recursively axiomatizable.[43] And this, he thinks, implies that "the human mind infinitely surpasses the power of any finite machine;"[44] that is, that

> the workings of the human mind cannot be reduced to the workings of the brain, which to all appearances is a finite machine with a finite number of parts, namely, the neurons and their connections.[45]

So Gödel's argument proceeds as follows:

(A) Subjective mathematics cannot be recursively axiomatized.

Therefore,

(B) The human mind cannot be reduced to a Turing machine.

Therefore,

(C) The human mind cannot be reduced to a machine.

That (A) implies (B) follows directly from the proof (mentioned four paragraphs above) that Turing computability is equivalent to partial recursiveness. The inference from (B) to (C), however, isn't quite so certain, because it depends upon the following assumption:

(D) If the human mind/brain is a machine at all, then it is possible to build a Turing machine which, given the same inputs, will produce the same (mathematical) output.

Now, I don't want to discuss the issue of whether or not (D) is true. I only want to point out that, for whatever reason (perhaps because of its close relation to the thesis of Turing equivalency), one might want to question it.[46] Thus, in order to use the argument in (A)-(C) to motivate immaterialism, one would have to provide some sort of justification for (D).

But the real problem with the argument in (A)-(C) is with (A). Gödel has two arguments for (A), but neither is good. The first is a modus tollens: if (A) is false, then there exist absolutely undecidable mathematical propositions; but there aren't any such propositions; therefore, (A) is true. By "absolutely undecidable mathematical proposition," Gödel means to refer to propositions that are

> undecidable, not just within some particular axiomatic system, but by any mathematical proof the human mind can conceive.[47]

Now, there is no question that the falsity of (A) does imply the existence of such propositions. For since objective mathematics is not recursively axiomatizable,[48] it is clear that if subjective mathematics is, then there are propositions which belong to the former but not the latter.

But any proposition which belongs to objective mathematics but not subjective mathematics just is an absolutely undecidable mathematical proposition.

So the cogency of Gödel's first argument for (A) depends upon the truth of his claim that

(E) There are no absolutely undecidable mathematical propositions.

Gödel's argument for (E) comes to us secondhand, from Wang,[49] who claims that Gödel has argued (in private conversation) as follows. If (E) were false, then human beings would be "utterly irrational," because they would be asking questions that they could never answer. But human beings are not utterly irrational in this way, for the parts of mathematics that have already been developed "show an amazing degree of beauty and perfection." Therefore, (E) is true.

This is an extremely bad argument. First of all, it doesn't follow from the falsity of (E) that there are human beings who go around asking unanswerable questions (i.e., inquiring into the truth of absolutely undecidable mathematical propositions). And, second, even if there were such human beings, it doesn't follow that they would be irrational, for they might not know that the propositions whose truth values they seek are absolutely undecidable (even if they do know that some mathematical propositions are absolutely undecidable).

Gödel's second argument for (A) can be found in two different places. In "Some remarks on the undecidability results," he writes that to assume that the mind is reducible to a Turing machine is to assume that

> a finite mind is capable of only a finite number of distinguishable states...[when, in fact, the] <u>mind, in its use, is not static, but constantly developing</u>. This is seen, e.g., from the infinite series of ever stronger axioms of infinity in set theory, each of which expresses a new idea or insight....Therefore, although at each stage of the mind's development the number of its possible states is finite, there is no reason why this number should not converge to infinity in the course of its development.[50]

In "The Gibbs Lecture," we find Gödel pursuing this same line of thought. He does not commit himself here to (A), but it seems to me that we can extract from Gödel's remarks, the following argument for (A).[51] No matter how elaborate an axiom system we construct for set theory, so long as we believe the system to be consistent, we can always come up with new axioms, because the belief that a given axiom system is consistent always gives rise to a new group of axioms. (Gödel writes in this connection that "the very formulation of the axioms up to a certain stage gives rise to the next axiom."[52]) Thus, one might conclude that, since there is no in-principle limit on our ability to formulate new axioms about the set-theoretic hierarchy, we must be capable of outstripping Turing machines in attaining mathematical knowledge.

The obvious response to this argument is that -- while there certainly <u>is</u> an infinite hierarchy of axioms about the set-theoretic hierarchy -- it's dubious that <u>we</u> could keep discovering them. For, sooner or later, we will boggle at the sheer complexity of the system, and so, for instance, we will simply have no belief one way or the other about its consistency. Now, of course, Gödel has anticipated this objection. This is the point of his claim (quoted above) that "although at each stage of the mind's development the number of its possible states is finite, there is no reason why this number should not converge to infinity." The point of this remark is that what's important here are _ideal_ rather than _actual_ people; in other words, the point is to reemphasize that subjective mathematics contains all of the mathematics that we _could_ prove.

But it's time to question the force of this modality. What does it _mean_ to say that subjective mathematics contains all of the mathematical proposition that we _could_ prove? Obviously, subjective mathematics _cannot_ be all of the mathematics that we could prove if we were super-intelligent and had all the time in the world, i.e., from the Big Bang to the end of the universe. For that would still only be a finite amount of mathematics, and so, (A) would still be trivially false. It seems that, in order for (A) to be true, subjective mathematics would have to be defined as that part of mathematics that would be provable if we had no limitations whatsoever, i.e., if we were Gods. But on the face of it, this seems to be _all_ of mathematics, i.e., _objective_ mathematics. But if this is what subjective mathematics is, then (A) certainly doesn't imply (B). If (A) is to imply (B), it will have to be, at least in <u>some</u> sense, about human beings. But so long as (A) is about human beings, it's just downright implausible.

So it seems to me that Gödel's second argument for (A) is no more convincing than his first and that, therefore, the argument in (A)-(C) does not give us a good reason to accept immaterialism about the mind.

Given that we have decided that (A) is probably false, it is worth considering -- as an aside -- the question of why Gödel thinks that the falsity of (A) implies the truth of platonism (i.e., why he thought that we have to either accept platonism or immaterialism about the mind).[53] His argument from the falsity of (A) to platonism depends, again, upon the fact that the falsity of (A) implies the falsity of (E), i.e., that the completability of subjective mathematics implies the existence of absolutely undecidable mathematical propositions. But this is the only part of the argument that Gödel makes clear. He seemed to think it trivial that the existence of absolutely undecidable mathematical propositions implies the truth of platonism. I think his reason must have been that such propositions imply the falsity of the only alternative to the platonistic account of mathematical truth, namely, the so-called "combinatorial" account. Now, this view comes in various guises, but the common thread is the reduction of mathematical truth to provability. The reason that such views seem incompatible with the falsity of (E) is that if p is an absolutely undecidable mathematical proposition, then neither p nor not-p will be provable. But, presumably, either p or not-p will be true. Thus, we will have an unprovable mathematical truth, and, so, we will not be able to reduce mathematical truth to provability.

Now, it might seem that this is just a straightforward argument for platonism, i.e., that it's not an argument for the disjunction of platonism and immaterialism about the mind. But that's wrong. For as it stands, the advocate of the combinatorial view can respond as follows: "When I reduce truth to provability, I do not reduce it to provability in a particular system, but rather to provability in any system." Thus, Curry writes:

> It should be noted that we have not confined mathematics to a single formal system; moreover, metatheoretic propositions are included in mathematics. This answers the objections which might be raised on the ground of the incompleteness theorems of Skolem, Gödel, et. al.[54]

Now the platonist might respond here that it might not be coherent to speak of "provability in any system." But another response is that, since (A) is false, there exist absolutely undecidable mathematical propositions, i.e., propositions that we could never prove, regardless of what system we're allowed to use. Thus, since we need the falsity of (A) to disallow Curry's response, we have an argument for the claim that either platonism or immaterialism about the mind is true.

Now, Gödel admits that anti-platonists can defang this argument by adopting intuitionism and claiming that absolutely undecidable propositions are simply meaningless (or that they simply lack a truth value). But Gödel thinks that intuitionism is so unappealing that it is not worth serious consideration.[55] It is worth noting, however, that anti-platonists might be able to defend the claim that absolutely undecidable propositions are neither true nor false, without adopting intuitionism, by using the method of supervaluations.[56]

But even without making this move, I think there is at least one sort of anti-platonist, namely the fictionalist, who can avoid Gödel's argument. This is not for the obvious reason that fictionalists jettison mathematical truth, and so don't reduce truth to provability. The reason this strategy won't work is that even fictionalists need a way of distinguishing "2 + 2 = 4" from "2 + 2 = 5". Now, they normally do this by claiming that only the former is part of "the story of mathematics,"[57] but if there are absolutely undecidable mathematical propositions, then making this move seems to land fictionalists in the same trap that other anti-platonists are in. For, since there is no principled way of saying whether absolutely undecidable propositions are part of the story of mathematics, it seems that fictionalists are forced into saying that we simply don't know whether they are part of the story. But to admit this is to admit that the story exists independently of us.

Fictionalists, however, have a way out. They can say that there is simply no fact of the matter about whether absolutely undecidable propositions are part of the story of mathematics. And note that this is not to say that such propositions are meaningless or that they lack a truth value; fictionalists can admit that they do have a truth value, because they can claim that they are false (assuming that they commit us to abstract objects). I think this response is quite acceptable, for it is exactly analogous to what we would say of a statement like, "Oliver Twist had a homosexual experience on his thirtieth birthday." There is no fact of the matter as to whether this sentence is true in the story. We do not want to say that it is false in the story in the manner of, say, "Oliver never met the Artful Dodger." But this is not to say that "Oliver had a homosexual experience" lacks a truth value; it is false, because "Oliver Twist" is a vacuous name.

7. Is Gödel's View Plausible? (Part II)

In section 5, I argued that we might not be able to formulate a precise and intelligible Gödelian epistemology. In section 6, I argued that, even if we could state such an epistemology, it would be implausible, because it would force us to accept a fairly strict immaterialist philosophy of mind. In this section, I want to very briefly offer a second argument suggesting that Gödel's epistemology is implausible.

Gödel's appeal to a contact-based faculty of mathematical intuition is unappealing, because it is an ad hoc maneuver. It is ad hoc because Gödel wants to explain our knowledge of mathematical objects by claiming that we can interact with such objects, but our only reason for supposing that we can interact with them is that we need to explain how we could have knowledge of them. In other words, we have no independent evidence for such contact. Thus, even if some philosopher could come up with some speculative theory about how mathematical objects could "send" data to human minds, such a theory would have only a limited value, for we would still be lacking a reason to believe that what this philosopher had "dreamed up" was true.

I do not want to pursue this argument, for it seems to lead pretty quickly to a standoff. Those who follow Gödel will claim that we receive independent evidence for the existence of contact with mathematical objects via first-hand experience. I don't think this is a legitimate response, but I do not have any good argument against it. (Those who think that it's so obvious that it doesn't <u>require</u> argument are probably just assuming that platonism is true. One cannot argue in this context that, insofar as it <u>is</u> obvious to us that, e.g., 3 is prime, we <u>must</u> be capable of intuiting facts about the mathematical realm. For no one wants to deny that we have such mathematical experiences; rather, the claim is that Benacerrafian considerations reveal that these experiences do not consist in intuiting things about a mathematical realm.)

In any event, this whole issue isn't terribly important, because an explanation can be <u>ad hoc</u> and true at the same time. I raise the issue here, merely because I think it is a problem for Gödel's view about which something must be said.

Together, the two arguments against Gödel's epistemological programme show that the motivation to accept his brand of intuition is relatively weak, because the cognitive price of accepting it is relatively high (we'd have to accept an immaterialist theory of mind) and because we have no reason to accept it, save the ad hoc reason that it would solve the epistemological problem with platonism.

8. Conclusion

I would like to reiterate my point that I do not presume to have refuted Gödelian intuition. Again, I take his remarks on this topic to provide us not with an explicit epistemology, but with a suggestion about how we might begin to build one. The purpose of this chapter has been to argue that we ought not to be hopeful that the programme suggested by Gödel's remarks could be fruitful.

I would also like to reiterate the point I made in section 1: the platonist epistemology that I offer in chapters five and six will not depend on a refutation of Gödelian epistemologies. If I wish to show merely how knowledge of mathematical objects could arise, I needn't say anything about Gödel at all. If, on the other hand, I wish to show how knowledge of mathematical objects does arise, then in so far as I can make my own view plausible, I only need to show that Gödel's is not plausible. I do not need to refute his view.

NOTES

1. By this, I simply mean epistemologies that claim that human beings attain knowledge of mathematical objects by interacting with them, or coming into contact with them, or gaining access to them. These terms will be discussed in the next section.

2. Everything I say about "contact" here can be carried over to "interaction," "access," "intuition," and other such terms. The choice of "contact" as the term to be discussed is fairly arbitrary. It is not, after all, a favorite term of either Gödel or Maddy. But there are two reasons why I have chosen it. First, it seems to capture what both Gödel and Maddy had in mind; and, second, while it has appeared periodically in the literature, it hasn't been used for other purposes. "Intuition" is different; as we will see in chapter five, many philosophers that disbelieve in "contact" and "access," still speak of a faculty of mathematical intuition. In fact, this is my own position.

3. I do not mean to commit to any technical notion of "information".

4. Gödel (1964), p. 484.

5. It is perhaps worth noting that talk of "contact" between the physical and non-physical is not entirely detached from ordinary usage. After all, there are people who claim to be in contact with Elvis not because they are at the end of a causal chain leading back to him, but, rather, because they are at the end of some sort of New Age telephone line leading to his Spirit.

6. My second argument (which is found in section 6 and which is directed against the plausibility of Gödel's view, rather than its intelligibility) is just an argument against immaterialist philosophies of mind; thus, obviously, it counts against anyone who rejects (1).

7. There are two sorts of naturalistic intuition. The first is Maddy's version, in which human beings can actually see mathematical objects. (I consider this position in chapter four.) The second is advocated by Steiner, Parsons, Katz, Resnik, myself, and others. Here, it is denied that intuition requires any contact with mathematical objects at all. (I consider this position in chapters five and six.)

8. See The Meno and The Phaedo. It might be argued that Plato's epistemology is clearer than Gödel's. But we wouldn't be doing platonists any favor by taking Plato,

rather than Gödel, as the paradigm case of the rejection of (1). For regardless of how clear Plato's position is, almost nobody thinks it's correct.

9. Frege (1919), p. 530.

10. For instance, Frege agrees with Gödel that in order to perceive ordinary physical objects, something more than sensation is required, something "non-sensible." And, just like Gödel, he thinks that this non-sensible element could also provide us with apprehensions of thoughts (or, in Gödel's words, intuitions of mathematical objects). Frege writes (1919, p.532): "since the answer lies in the non-sensible, perhaps something non-sensible could also lead us out of the inner world and enable us to grasp thoughts where no sense-impressions were involved."

Palle Yourgrau (1989) has made this same observation. It is possible, of course, that Gödel came to his view by reading Frege, but I doubt this, for it seems that he would have provided a reference. Thus, I agree with Yourgrau that this similarity is downright "remarkable."

11. Most of the evidence for the fact that Gödel wanted to reject (1) comes from two sources. First, it comes from some of Gödel's unpublished works -- e.g., the (forthcoming) "Gibbs Lecture" -- many of which will appear in volume III of his Collected Works. Second, evidence also comes from the reports of Hao Wang, who spoke with Gödel about these issues. (See pp. 324-325 of Wang's (1974).)

12. Actually, I hope that my remarks will provide a sort of inadvertent support for my interpretation of Gödel. This is because, despite my lack of concern with exegesical issues, I will still provide various quotes from Gödel's writings. It is my belief that these passages support my interpretation of Gödel.

13. One might be puzzled at this point. For, above, I said that I will argue against the strategy of rejecting (1) by arguing against Gödel, and I am now saying that I am really uninterested in Gödel, i.e., that I am only interested in the rejection of (1). There is, however, no problem here. I will argue against the strategy of rejecting (1) by arguing against Gödel's position as I have interpreted it (and, as I've indicated, other versions of this strategy are not immune to the criticisms I will give). All I am pointing out now is that, for the present purposes, it is irrelevant whether Gödel's real position is different from my interpretation.

14. I would like to make one more remark about what Gödel's real view is. Scholars of Gödel's work who don't have access to all of his unpublished writings are at a disadvantage in

piecing his view together, for almost <u>none</u> of Gödel's philosophical writings have been published. Fortunately, this is about to change. Many of Gödel's unpublished works are about to appear in volume III of his <u>Collected Works</u>. Moreover, within a few years, most of the rest of his writings will probably also be published. But for now, we can only work with what we have. Now, I think there is good reason to think that Gödel <u>did</u> want to solve the epistemological problem with platonism by rejecting (1), and I do not think this will change as more of his writings become accessible. However, various intricacies of his epistemology may very well emerge in the future. We can only wait and see.

15. (Q1) is found on pp. 483-4 of Gödel (1964); (Q2) and (Q3) are on p. 484 of the same work; (Q4) is on p. 449 of Gödel (1944); and (Q5) is on pp. 456-7 of the same work. In addition to these two articles, the (forthcoming) "Gibbs Lecture," is also very relevant to a study of Gödel's epistemological views. But there is nothing explicitly about <u>intuition</u> in this article, and so I haven't included any passages from it here. I will, however, be quoting from that article below, in sections 5 and 6.

16. On p. 241 of his (1958) -- or p. 272 of the later (1972) version of this paper -- Gödel says that intuitive concepts are those which "involve properties or relations of <u>concrete objects</u> (for example, of combinations of signs)." Those that do <u>not</u> involve such properties or relations Gödel calls "abstract," or "nonintuitive". But the first time he uses "intuitive" in this passage, Gödel drops a footnote (viz., footnote 2) which reads: "See Hilbert's formulation in his <u>1926</u>, pp. 171-173." Then, in the (1972) version, he appends to this reference another footnote (viz., footnote b) which includes the following remark: "it is Hilbert's insistence on <u>concrete</u> knowledge that...excludes many things that are just as incontrovertibly evident to everybody as finitary number theory."

Because of these footnotes (as well as the context in which the above characterization of intuition occurs) I think it is clear that Gödel's remarks in this article simply have nothing to do with his own view of intuition. Gödel was simply making a point about <u>Hilbert's</u> notion of intuition.

Hao Wang (1987, p. 288) seems to agree that Gödel is not using "intuition" here as he normally does. Wang writes that "In this context, 'intuitive' is understood in a restricted sense so that, correspondingly, much of what is taken to be 'intuitive' elsewhere (certainly in set theory) becomes 'abstract,' i.e., going beyond concrete intuition."

17. Steiner (1975), chapter four, section V.

18. Actually, Parsons only claims that, for Gödel, intuition-that always involves intuition-of. He says (unpublished, p. 18) "that even the most elementary perception of truths involves perception of concepts"; (recall that, for Gödel, concepts <u>are</u> objects). Parsons says nothing about the converse claim that intuition-of always involves intuition-that, and for good reason: it is a much more controversial claim. Regardless, the distinction between intuition-of and intuition-that is not very important for our present purposes, and so I will not pursue it.

19. Gödel (1964), p. 484.

20. I already said this above, but it is worth repeating. It is possible that, as more of Gödel's unpublished writings on intuition become accessible to us, we will have to modify our reconstruction of his view.

21. Benacerraf (1973), p. 674.

22. Katz (1981), p. 201.

23. Gödel (forthcoming) claims that his incompleteness theorem forces upon us the following tripartite space of possibilities: either (a) mathematical platonism is true, or (b) immaterialism about the mind is true, or (c) both are true. Now, Gödel never really <u>commits</u> himself in this essay to (c), but it is quite clear that he believes it. I don't think this interpretation is controversial, but, for whatever it's worth, Hao Wang has indicated that, in private conversations, Gödel confirmed this interpretation. See Wang (1974), pp. 324-325.

24. One might argue that it is simply unintelligible to speak of a non-physical object existing inside of some physical space, and that this renders the concept of a Cartesian Mind unintelligible. If this were the case, then the adoption of immaterialism would <u>clearly</u> do nothing to stave off unintelligibility; in fact, it would simply create a <u>double</u> unintelligibility. I am not unsympathetic to this criticism of immaterialism, but, for the sake of argument, I will assume that some sort of sense can be made out of the claim that immaterial minds exist <u>in</u> our heads.

25. I take it for granted, in this context, that it is unacceptable to claim that mathematical data can pass through space. But, of course, it is not so obviously unacceptable in <u>all</u> contexts. We will see in chapter four -- in connection with the rejection of (2) -- that this was essentially Maddy's position.

26. I suppose that platonists might respond here as follows: we _do_ require a more explicit account of our contact with mathematical objects, but we need not worry that such an account is impossible to give, for insofar as we _do_ have mathematical knowledge, we _must_ be capable of interacting with mathematical objects. I think this response is clearly unacceptable, for it simply assumes that mathematical knowledge could _only_ be attained via contact with mathematical objects.

27. The reason Gödel thinks we must _receive_ the concept of _object_ is that he thinks we are incapable of coming up with it on our own. See Gödel (1964), p. 484.

28. Gödel's platonistic attitude toward concepts is clear in his (1944). On p. 458, for instance, he says that "concepts are supposed to exist objectively." Also, on pp. 84-86 of Wang (1974), we get evidence not only for the fact that Gödel had a platonistic attitude toward concepts, but also for the fact that he thought that concepts are objects of intuition.

29. Maddy (1980).

30. Maddy (1990), p. 78.

31. The philosopher who comes the closest to endorsing this view is Charles Parsons. However, we will see in chapter five that once the platonist accepts (1) and (2) -- and, hence, gives up on contact-based theories of intuition -- the details of how intuition works will no longer be very important.

32. To see that this is true, consider a human being raised in the wild by wolves, without ever _hearing_ language, or _seeing_ a teacher do arithmetic on a blackboard. Would such a creature have any mathematical intuitions at all?

33. Roughly, eliminative materialism is the view that there is no such thing as a mind or a mental state or a mental property. For an explication of eliminative materialism, see Churchland (1984).

34. See, for instance, Nagel (1974), Fodor (1987), and Searle (unpublished).

35. Gödel (forthcoming), p. 8 and p. 9.

36. It might be argued that psychology commits us to _propositions_, but that is a separate issue.

37. See, for instance, Hubel and Wiesel (1977) and (1979). The same remarks could also be made in connection with visual _recognition_; for an example here, see Alexander and Albert

(1983).

38. In particular, the argument can be found on pp. 6-9 of Gödel (forthcoming). It is perhaps worth noting that, on p. 9 of this essay, Gödel explicitly gives another argument for immaterialism. It is an appeal to authority: he writes that immaterialism "is in good agreement with the opinions of some of the leading men in brain and nerve physiology." Unfortunately, however, I cannot assess this claim, because I do not know who or what Gödel was referring to.

39. See Wang (1974), pp. 324-325.

40. Gödel (1931).

41. We must keep in mind that, by "subjective mathematics," Gödel means all humanly provable mathematics. He does not mean all the mathematics that we actually have derived, for it is trivially true that that portion of mathematics is recursively axiomatizable.

42. I don't think anyone knows who was the first to realize (and/or prove) that (partial) recursiveness and Turing computability are coextensive. In all likelihood, it was either Turing, Church, or Kleene, who were, of course, pioneers in this area of mathematics. Regardless, for a version of the proof, see Boolos and Jeffrey (1989), chapters 6-8.

43. Gödel doesn't really commit himself to this in the "Gibbs Lecture," but Wang (1974, p. 324) reports that this is Gödel's view.

44. Gödel (forthcoming), p. 7.

45. Gödel (forthcoming), p. 8.

46. I know of no one who has ever argued one way or the other about (D), but there are those who would deny the more general thesis of Turing equivalency. See, for instance, Dreyfus (1972).

47. Gödel (forthcoming), p. 7.

48. This is just Gödel's incompleteness theorem.

49. See Wang (1974), pp. 324-325 and (1987), p. 193. All the quotes I provide here can be found in either place.

50. This passage comes from Gödel's two-page (1972a). It is part of a paragraph which he intended to be appended to his (1934), as a footnote to the word "mathematics," on line 3 of p. 73 of that article. The entire footnote is also reprinted

on pp. 325-326 of Wang (1974).

51. See Gödel (forthcoming), pp. 2-4.

52. Gödel (forthcoming), p. 3.

53. Gödel writes on p. 7 of his (forthcoming) that either "the human mind...infinitely surpasses the power of any finite machine, or else there exist absolutely unsolvable Diophantine problems." Then, on p. 8, he says that the second alternative seems to imply "some form or other of Platonism or 'Realism'."

54. Curry (1954), pp. 204-5.

55. Gödel (forthcoming), p. 7-8.

56. The method of supervaluations was developed by Bas van Fraassen to handle languages with presuppositions. Very briefly, the supervaluation induced by a set of presuppositions X is the valuation which (a) assigns true to just those sentences which are true in all models which satisfy X, (b) assigns false to just those sentences which are false in all models which satisfy X, and (c) assigns no truth value at all to other sentences. (See, for instance, van Fraassen (1969), p. 72.)

57. See, for instance, Field (1989), pp. 2-3.

Chapter Four

Contact in This World: Maddy

1. Forward

In chapter two, I formulated what I called the epistemological argument against platonism. That argument proceeded as follows:

(1) Human beings exist entirely within space and time.

(2) If there are (abstract) mathematical objects, they exist outside of space and time.

Therefore, we have reason to suspect that

(3) If mathematical objects exist, then human beings could not attain knowledge of them,

and, hence, also

(4) If mathematical platonism is correct, then human beings could not attain mathematical knowledge.

(5) Human beings have mathematical knowledge.

Therefore

(6′) We ought to have a workable platonist epistemology before we accept platonism.

Now, I argued in chapter two that if platonists are to have any hope of derailing this argument, they must do so at the beginning, before we get to (3). In other words, I argued that platonists must either reject (1), reject (2), or deny that (1) and (2) provide a good reason to believe (3). The first of these strategies, however, has already been ruled out. For in chapter three I argued that (a) it's not clear how we can even make sense of the Gödelian strategy of rejecting (1) -- i.e., of salvaging platonism by adopting an immaterialist philosophy of mind and claiming that human minds are capable of somehow forging contact with mathematical objects -- and (b) even if we could somehow make this view intelligible, it would not be very plausible anyway.

In this chapter, I would like to shift from a consideration of this attempt to elevate humanity to platonic heaven to a consideration of the reverse strategy of bringing the inhabitants of the platonic heaven down to earth. Less metaphorically, I will consider the view that we can answer the epistemological argument against platonism by rejecting (2) -- i.e., by adopting a materialistic conception of mathematical objects -- and arguing that human beings can attain knowledge about mathematical objects by coming into contact[1] with them in the ordinary physical world. I will argue against this view in much the same way that I argued against the Gödelian view in the last chapter; that is, I will argue that (a) it -- i.e., the view that platonism can be salvaged by rejecting (2) -- is unintelligible, and (b) even if it was intelligible, it would not be very plausible.

2. Qualifications and Disclaimers

In chapter three, I did not provide in-principle arguments against the strategy of rejecting (1). Rather, I argued against a particular advocate of that strategy, namely Gödel. It didn't matter that I proceeded in that fashion, however, because (as I pointed out) the arguments I used against Gödel could have been employed against _any_ advocate of the view that platonism can be salvaged by rejecting (1). The situation is the same in this chapter. I will only argue against _one_ advocate of the strategy of rejecting (2) -- namely, Penelope Maddy -- but the arguments I will use could be employed against _any_ advocate of this strategy.

The central argument will aim to establish that Maddy's view is unintelligible. In particular, I will argue that she cannot so radically shift the platonist position (taking mathematical objects to have spatio-temporal location, so that the epistemological argument can be answered) without simply _abandoning_ platonism; but, the argument continues, Maddy cannot afford to collapse into anti-platonism, for the particular form of anti-platonism she would represent is utterly implausible. Thus, Maddy falls into the contradiction of both accepting and not accepting platonism. Benacerraf might put Maddy's dilemma as follows: she wants to have her cake (i.e., reap the traditional -- semantic -- benefits of platonism) and eat it too (i.e., reap the traditional -- epistemological -- benefits of naturalism); but this is not possible, because her _naturalized platonism_ is incoherent.[2]

Now it seems to me that this argument could be employed _generally_ against any naturalized platonist -- i.e., any advocate of the strategy of rejecting (2). But it might be argued that in order to really justify this claim I would need to actually run the argument against all such advocates; that is, I would have to argue that <u>no one</u> has shown how to naturalize platonism without contradiction. Moreover, one might claim that there are <u>numerous</u> advocates of the rejection of (2) and, hence, that my task would be a lengthy one. I have two responses to this. First, among the philosophers whom we might reasonably interpret as advocating a rejection of (2) -- e.g., Aristotle, Locke, and Mill, to name just three[3] -- very <u>few</u> could be reasonably interpreted as trying to defend <u>platonism</u>. Thus, while such philosophers might be naturalists, they are not naturalized platonists, and so we could not very easily look to them for an argument that that view is intelligible. Now, to be sure, Maddy is not the <u>first</u> naturalized platonist. (One might, for instance, interpret David Armstrong[4] along these lines.) But it seems to me -- and this is my second response to the above line of argument -- that Maddy's presentation and defense of naturalized platonism is the clearest and most convincing in the literature. Indeed, this is why I have chosen her writings as the target of my arguments. What makes Maddy the obvious choice here is that, so far as I know, she is the only philosopher who (a) accepts naturalized platonism and (b) takes the construction of a platonistic epistemology as her primary task. The epistemological remarks of other naturalistically inclined platonists -- such as Armstrong -- tend to be quite brief and open to divergent interpretation.

In light of all of this, it seems acceptable to claim that if I can show that Maddy's version of naturalized platonism is unintelligible, then, even though I won't have given a knockdown, in-principle argument against the general position of naturalized platonism, I will have nonetheless given a good reason for being highly suspicious of that position. Moreover, I don't need a knockdown argument against naturalized platonism, i.e., against the strategy of rejecting (2). The reason for this is the same as the reason I gave for not needing a knockdown argument against Gödel's position: the arguments that I provide in chapters five and six -- i.e., the arguments that support the platonistic epistemology developed there -- do not depend upon the falsity of Maddy's (or Gödel's) epistemology. Now, to be sure, if Maddy's epistemology is correct, then mine couldn't be;[5] but in order to argue that my epistemology is in fact the correct one, I only need to establish that it is plausible and that Maddy's is not. I do not need a knockdown argument against the latter.

3. Maddy's View of Mathematical Intuition

I can support my claim that Maddy clearly does want to reject (2) and begin my explication of her position with a single quotation:

> I intend to reject the traditional platonist's characterization of mathematical objects; I will bring them into the world we know and into contact with our familiar cognitive faculties.[6]

Her use of "contact" here is in line with what I said about that term in the last chapter: she thinks that information can pass from mathematical objects to the human mind/brain. Unlike Gödel, however, Maddy is quite clear about how this information transfer arises. She thinks that sets of physical objects are located in space and time and that human beings can actually perceive them (i.e., see, hear, smell, feel and taste them in the usual way[7]). She claims, for instance, that if I have three eggs in a carton, then the set of these three eggs "is located in the egg carton -- that is, exactly where the physical aggregate made up of the eggs is located."[8] Her argument for this is that it seems intuitive to think that the set of eggs comes into and out of existence with the eggs themselves.

In spite of her adoption of this spatio-temporal conception of sets, however, (and in spite of her claim that we can see sets in the ordinary way) Maddy still considers herself a platonist. Now, presumably, this means that she takes sets to be -- in some relevant sense -- abstract objects; but as we'll see in the next section, there is some ambiguity in Maddy's position here. But in any event, Maddy does take her view to be platonistic, in spite of the fact that she takes mathematical objects to be sensible and located in space and time. As I indicated in the last section, I will call this view naturalized platonism.[9]

We now come to Maddy's account of mathematical intuition. It is her view that, after having repeated experiences with sets of physical objects, children acquire the concept set -- just as they acquire the concept horse after repeated perceptions of horses and the concept _triangle_ after repeated perceptions of triangles. Maddy also thinks that once we have acquired a concept, we can notice certain subtleties about it; i.e., we can notice certain semantic facts that are, "in a sense, 'built into' the" concept we have acquired.[10] So, just as it is built into the concept _horse_ that horses are animals and just as it is built into the concept _triangle_ that triangles have three sides, it is also built into the concept _set_ that, for instance, "the number property of a set is not changed (barring mishap) by moving its elements."[11] Thus, according to Maddy, when we acquire the concepts _horse_ and _set_, we become aware of certain semantic facts and, hence, arrive at beliefs about horses and sets. Maddy calls these beliefs _intuitions_. (Note that Maddy does not claim that such intuitions are infallible. Intuitions are just _beliefs_ about x's that arise because of what "x" means.)

Maddy's claim is that the axioms of set theory are intuitive in this sense. (Recall that it is only knowledge of _axioms_ that the platonist needs to account for, because knowledge of theorems arises via proof.) We acquire the concept _set_ via repeated perceptions of sets and, thereby, become convinced that the axioms of set theory are true. If this is correct, then Maddy will have explained how knowledge of mathematical objects is just as well-founded as knowledge of physical objects, for she will have explained how our set-beliefs are _caused_ by sets. (Sets, she claims, cause perceptions of sets, which, in turn, cause the acquisition of the concept set; this, then, gives rise to intuitive set-beliefs, which lead to derivative set-beliefs.) Thus, if Maddy's view is correct, then we have a response to even the strongest version of the epistemological argument against platonism (i.e., the version that relies upon the sort of causal theory of knowledge that claims that, in order for S to know that p, there must be a causal chain leading from the fact that p to S's cognitive faculties).

It is worth noting that, according to Maddy, set-theoretic axioms can also receive what she calls extrinsic, or theoretical, justification. Such evidence for axioms stems essentially from holistic considerations. We would be more inclined to accept an axiom if it had "verifiable consequences," if it provided "powerful new methods for solving pre-existing open problems," if it simplified and systematized theory, if it implied "previous conjectures...[and] 'natural' results," etc.[12] In fact, Maddy seems willing to admit that some axioms (e.g., the axiom of infinity) are justified almost exclusively by such considerations.[13] I will return to a discussion of extrinsic justification in section 6.

It is also worth noting that Maddy doesn't take numbers to be identical with sets. Rather, she takes them to be universals. Thus, three is a property of three-membered sets in the same way that redness is a property of fire engines. Maddy, however, doesn't commit to the existence of numbers.

She claims (quite justifiably, I think) that the question of the existence of numbers is just a special case of the question of the existence of universals. If physicists can get by without properties, then so can mathematicians get by without numbers.[14]

I think that Maddy's view is somewhat ingenious, but, in the end, I do not think it is intelligible. In the next two sections, I will try to establish this conclusion. Then, in section 6, I will argue that even if Maddy's view could somehow be made intelligible, there are reasons to doubt its truth.

4. Is Maddy's View Intelligible? (Part I)

One response to Maddy's epistemology is to claim that mathematical platonists (i.e., those who think that mathematics is about abstract objects) cannot deny (2), because it is simply part of the definition of "abstract object" that such things are aspatial and atemporal. Thus, if we deny (2), as Maddy suggests we should, we will, in effect, be denying that there are any abstract objects. But if this is true, then platonists would be quite justified in considering Maddy to be the sort of friend association with whom eradicates the need for enemies. We set out looking for a response to the epistemological argument against platonism, and it appeared at first glance that one avenue which might lead to such a response was the denial of (2). But it seems that the denial of (2) entails the denial of platonism, and if this is so, then we will certainly not find an acceptable platonistic solution to our problem here.

So this response to Maddy is that if she takes sets to have spatio-temporal location, then she doesn't take them to be abstract objects, and, hence, she isn't a platonist. Maddy responds to this in the obvious way:

> On some terminological conventions, this means that sets no longer count as 'abstract'. So be it; I attach no importance to the term.[15]

I think that this sort of response is often acceptable. Philosophers get to formulate their theories however they want to; and once a view has been formulated the only interesting question is (usually) whether it is true. Questions about whether a view gets counted as xism or yism are, in general, not interesting. Thus, *prima facie*, it seems that we ought not to be concerned with whether or not Maddy's view is a brand of platonism; we ought only to be concerned with whether her view of mathematics and mathematical knowledge is acceptable. In other words, Maddy's "so be it" response is just this: we clearly *can* take sets to be the kind of thing that can be seen by looking at a pile of objects; the important question is *not* whether such perceptible objects are "abstract," but whether they can plausibly be taken as the objects of set theory.

But while it *usually* doesn't matter whether a theory gets counted as xism or yism, it *is* important -- in the present context -- whether Maddy's view is platonistic. For recall that, in this essay, I am trying to find a solution to the epistemological problem *with platonism*. Thus, if Maddy's position is a brand of anti-platonism, then I shouldn't even be considering it here. (Instead, her position should be added to the list of anti-platonist positions -- discussed in chapter one -- which would have to be refuted before platonism could be accepted.[16]) Maddy's epistemology is only of interest to us (in the present context) if it is an epistemology of abstract objects; thus, it is only of interest to us if her sets are -- in some (relevant) sense of the term -- abstract.

So it would be quite acceptable (in the present context) to simply assume that Maddy is a platonist, i.e., to assume that her sets are, in some sense, abstract. Thus, our question would then be whether this is compatible with her claim that sets are spatio-temporally located and perceivable by the senses.

But I think it is worth digressing for a moment -- even though I don't need to -- to discuss the question of whether Maddy's view would be plausible if it were thoroughly anti-platonistic, i.e., if Maddyan sets weren't abstract in any (relevant) sense of the term. The reason this question is worth consideration is that Maddy sometimes writes as if this is her view.[17] Now, Maddy certainly doesn't write this way all the time, and if her view is anti-platonistic, then it would raise several puzzling questions. For instance, why would Maddy, if she were a thorough-going anti-platonist, bother with the Benacerrafian epistemological problem? This is a problem for platonists only; indeed, a great deal of the motivation for anti-platonism is to avoid this problem. Maddy spends a lot of time and effort trying to show that human beings can acquire information about sets. But if, according to her view, sets were ordinary concrete objects, it would seem that she wouldn't have to argue at all for the claim that we can acquire information about sets. It seems that Maddy could just say that we literally perceive sets and be done with it; it doesn't seem that she would have any need for an elaborate theory of mathematical intuition.

For whatever it's worth, I believe that Maddy does want to hang onto the idea that sets are (in some sense) abstract; for (a) she admits that, "strictly speaking," we do not interact with sets;[18] and (b) she seems to imply that whether or not she countenances abstract objects depends "on our definition of 'abstract'."[19] But she doesn't say any more about this, and so we don't know which senses of "abstract" apply to Maddyan sets.

In any event, I want to argue now that if Maddy's view is thoroughly anti-platonistic, then it is implausible. The easiest way to see this is to notice that Maddyan anti-platonism (i.e., the view that would be Maddy's if she were an anti-platonist) is dangerously similar to Millian anti-platonism. But we have to be careful here, because Maddy is very cagey in this connection.

If we reject platonism and take sets to be concrete objects, then it might seem natural to say that a set is identical with the aggregate of the "physical stuff" that makes up the elements of the set in question. I will call this view the aggregate theory of sets, or ATS. Now, Maddy realizes that ATS is quite similar to Mill's view and, more importantly, that it's implausible. The argument she usually uses against ATS is Frege's argument that no determinate number applies to an aggregate: the sum of the egg-stuff in Maddy's carton, for instance, consists of "three eggs, but many more molecules, [and] even more atoms";[20] thus, since sets are supposed to have a determinate number of members, they cannot be physical aggregates.

Now, it might seem that ATS is the only theory available to Maddy-qua-anti-platonist. For consider a singleton containing an egg. According to Maddy, it has the very same spatio-temporal location as the egg itself. Thus, if both are concrete objects, it seems that the egg and its singleton are identical and that both are identical with the aggregate of the physical stuff making up the egg. Thus, it seems that Maddy-qua-anti-platonist has no option but to accept ATS. Now, Maddy says that she could solve this problem by appealing to platonism and taking sets to be abstract; but she leans, instead, toward identifying the egg and its singleton in a way that enables her to avoid ATS. Her suggestion is that we can identify the set containing the egg not with the aggregate of the egg-stuff, but with the egg as individuated thing.[21] (It is worth noting that this suggestion doesn't commit Maddy to anti-platonism, or to the claim that all singletons are identical with their elements.[22])

I think there are numerous problems with Maddy's suggestion here which would have to be solved before we could grant that Maddy-qua-anti-platonist can avoid ATS (and the Fregean objection to it). I will only just indicate what these problems are, however, because it seems to me that, independently ATS and the Fregean objection, there are other problems with Millian anti-platonism which Maddyan anti-platonism doesn't avoid. Some of the questions I want to ask in connection with the above suggestion are as follows. First, what, precisely, is an egg as individuated thing? (I ask this question not only to inquire whether such things are abstract or concrete, but also because it seems to me that Maddy's position here might psychologize sets and, hence, jeopardize the objectivity of mathematics.) Second, why isn't Maddy forced to take the same line with pairs, triples, quadruples, etc.? (It seems to me that an argument similar to that of the preceding paragraph can be used to show that Maddy needs some way to distinguish a set of three eggs from the corresponding aggregate of the physical stuff of the eggs; thus, it isn't sufficient for Maddy to say why she isn't forced to accept ATS in connection with singletons; she must also say why she isn't forced to accept it in connection with non-singleton sets. Now, if Maddy tries to use the same strategy here that she used with singletons, she will say that the set of three eggs is something like the three eggs as individuated things taken together; but -- assuming this even makes sense -- Maddy certainly cannot make this claim, because, if she does, she will not be able to get the iterative hierarchy going.[23] Thus, it seems that Maddy can only avoid ATS in connection with non-singleton sets by going platonist.) Third, isn't it odd that some sets are utterly different in kind from others? (While a singleton containing an egg is, according to Maddy, an ordinary physical object, {{Madonna, Quine}} is not; moreover, although it's a singleton, it's not identical with its element, for its element is not a singleton.)

Even if Maddy-*qua*-anti-platonist can answer all of these questions, she is not out of the woods. For there are other problems with Millian anti-platonism which she cannot avoid. One problem has to do with sets that contain infinitely many elements: it seems that Maddy has no option but to take such sets as abstract, for -- assuming the physical world is finite[24] -- there is no concrete object that could be an infinite set. Thus, the axiom of infinity will simply be false on any anti-platonist view; thus, it seems that fictionalism is the only tenable version of anti-platonism.

A related problem arises in connection with the axiom of the null set. Maddy tries to solve this problem by biting the bullet, i.e., by admitting that there is no such thing as the null set and, hence, that the axiom of the null set is false. To motivate this claim, she points out that (a) one can construct a set theory without the null set which is just as mathematically powerful as standard set theories, e.g., Zermelo-Fraenkel set theory (ZF); and (b) we can still use theories like ZF, so long as we recognize that the null set is just a notational convenience. Technically speaking, there is nothing wrong with this revision of mathematics, but it seems undesirable to have to say that ZF is false.

In addition to these problems, Maddyan anti-platonism also seems to fall prey to a very old objection to Millianism. If sets were physical objects, then mathematical truth would be physical truth, and mathematics would be an empirical science, providing us with falsifiable laws of nature. But (as I pointed out in section 2.1 of chapter one) it seems clear that this is an incorrect picture of mathematics. Mill tries to avoid this problem by claiming that mathematical propositions are falsifiable and that the only reason that they seem not to be is that we see them confirmed so frequently that it creates an illusion of necessity and independence from experience.[25] Maddy makes a similar remark:

> In time, generalizations like these become so well-entrenched that our observer is inclined to deal with seeming counterexamples by assuming hidden abnormalities rather than to doubt the generalization.[26]

Among the many things wrong with this response is the fact that if Maddyan anti-platonism were correct, then mathematics would depend not just upon trivially true and constantly confirmed empirical hypotheses, but also upon highly controversial ones, such as the axiom of infinity. (This is why I said above that it won't help Maddy-qua-anti-platonist if the axiom of infinity turns out merely true according to her view; what she needs is for this axiom to be true independently of any controversial empirical hypotheses.)

So, in conclusion, Maddy's "so be it" response to the claim that her view entails anti-platonism is unacceptable to us for more than one reason. It's not just that we are presently engaged in the search for a platonist epistemology (and, hence, unconcerned with anti-platonist positions). It's also that there is good reason for Maddy to avoid anti-platonism; for, while I've only sketched the argument for this claim, it seems that the particular version of anti-platonism that she's flirting with is implausible. Maddy needs sets to be abstract in some sense of that term. Our question, then, becomes this: is the abstractness of sets consistent with Maddy's claim that sets are perceivable by the senses and located in space-time (in the same location as the aggregate of the physical stuff of its members)?

We can put all of this in Benacerrafian terms.[27] Maddy was driven by epistemological considerations to naturalize platonism. By bringing mathematical objects into the physical world and into contact with our cognitive faculties, she may well make it easier to construct an epistemology, i.e., to provide an account of the link between our cognitive faculties and the objects known; but in so doing she may also lose the semantic benefits of traditional -- i.e., non-naturalized -- platonism. Maddy wants to have her cake and eat it too; that is, she wants to go between the horns of the

Benacerrafian dilemma. But she has to make it clear that this really can be done. If the epistemic horn is avoided by adopting naturalism and the semantic horn is avoided by adopting platonism, then she cannot shrug off as "terminological" the objection that she is not really a platonist. (Otherwise, she runs the risk of not going between the horns at all, but being gored by the semantic horn.) Thus, Maddy has to show that naturalism and platonism can go together; that is, she has to show that her naturalized platonism (i.e., the view that sets are in space-time and perceptible <u>and also</u> -- in some sense -- abstract) is intelligible.

To really have a knockdown argument against Maddy's naturalized platonism, we would need to derive a contradiction between the abstractness of her sets on the one hand and the spatio-temporality and perceptibility of her sets on the other. But let me point out that even if we can't present such an argument, we have already given reasons to be dubious of naturalized platonism. For the burden of proof is clearly on Maddy here. If she wants to be taken seriously, she has to make it clear that her position is intelligible; that is, she has to say in what sense her sets are "abstract"; she has to say <u>how</u> sets differ from ordinary concrete objects. But Maddy hasn't clarified any of these things; thus, since they need clarification, we have reason to doubt her view.[28]

But I think we can go a long way toward arguing that naturalized platonism is unintelligible. I will not say that we can formally derive a contradiction from that view, but I think we can make it seem highly probable that it leads to a contradiction. I turn to that task now.

5. Is Maddy's View Intelligible? (Part II)

It might seem that since Maddy is clearly shifting the meaning of "abstract object," it would be difficult to show that abstractness is inconsistent with spatio-temporal locality and perceptibility. For it seems that Maddy could simply define "abstract object" however she wanted to and, hence, make sure that abstractness is not inconsistent with spatio-temporal locality and perceptibility. I admit that Maddy could avoid incoherence by stipulation in this way. For instance, if she took "abstract object" to mean "red object", then she would clearly be able to claim that abstract objects have spatio-temporal location. However, if she wants "abstract object" to mean anything even remotely similar to what it ordinarily means (which she obviously does, if she is going to avoid the problems of Maddyan anti-platonism that I specified in the last section) then she will not be able to avoid incoherence. I am led to this conclusion by the following considerations.

If "abstract" is to mean anything remotely similar to what it usually means, then it will not apply to ordinary physical objects -- i.e., objects made up of matter, or physical stuff. Notice that I am giving Maddy a great deal of freedom here to define "abstract" as she pleases. She can distort it beyond recognition, if she likes; all I demand is that she not take it to mean precisely the opposite of what it is traditionally taken to mean. That is, it cannot apply to ordinary physical objects. This constraint is so lenient that it would not be very difficult to justify. But it doesn't matter, because I don't <u>need</u> to justify it, because it simply wouldn't do Maddy any good to define sets as physical <u>and</u> abstract; if, on her view, sets are physical objects, then her view will collapse into the untenable brand of anti-platonism described in the previous section. Whether or not sets are <u>also</u> "abstract" would then be irrelevant. (Moreover, since we're allowing Maddy to distort the meaning of "abstract," the question would also be uninteresting, for Maddy could easily procure whichever answer she desired. Sets <u>would</u> be "abstract" if she took that term to refer to all and only things that aren't round squares; and they <u>wouldn't</u> be "abstract" if she took it to refer to all and only round squares.)

So Maddy cannot define "abstract" in a way that allows it to apply to physical objects, i.e., to objects made of matter. Thus, since Maddy needs sets to be abstract, she cannot allow them to be made of matter. But insofar as Maddy claims that sets are perceptible (i.e., that they are objects of sense perception) she seems forced to claim that they <u>are</u> made of matter. For if they're <u>not</u> made of matter, then they can't reflect light and, therefore, can't be seen.[29]

So it seems that (even if we allow Maddy to shift the meaning of "abstract") we can derive something of a contradiction between abstractness on the one hand and perceptibility (and spatio-temporal locality) on the other. Precisely formulated, this argument proceeds as follows:

(i) Anything that can be seen can reflect light.[30]

(ii) Anything that can reflect light is made up of matter (or physical stuff).

Now, according to Maddy's naturalized platonism,

(iii) Sets can be seen.

Therefore, according to Maddy's naturalized platonism,

(iv) Sets are made of matter.

(v) Ordinary physical objects -- i.e., objects made up of matter or physical stuff -- are not abstract objects.

Now, according to Maddy's naturalized platonism,

(vi) Sets are abstract.

Therefore, according to Maddy's naturalized platonism,

(vii) Sets are not made of matter.

But (iv) and (vii) contradict each other.

In different places Maddy writes as if she would attack this argument in utterly different (and incompatible) ways. One response which she sometimes seems to favor is the denial of (vi). In the last section, I dealt with this at length, arguing that (a) Maddy cannot adopt anti-platonism and (b) even if she does, her position will (in the present context) be of no interest to us. Thus, for now, I will assume that Maddy would accept (vi). Moreover, I have also already argued that Maddy must accept (v). Finally, it is quite clear that Maddy would accept (iii), that (iv) follows from (i)-(iii), and that (vii) follows from (v) and (vi). Thus, it seems that Maddy's only option is to deny (i) or (ii). But both of these seem obviously true.

Maddy's strategy -- when she's not writing as if she would deny (vi) -- is to deny (i), although, of course, she doesn't put it in precisely these terms. Her claim is that in perceiving the members of a set (which, of course, can reflect light), we also perceive the set itself. Her argument for this claim is quite strange and, I think, unconvincing. She says that the fact that we do not strictly interact with sets shouldn't deter us from claiming that we perceive them, because there is a similar problem involved with the perception of physical objects. When we perceive a physical object, Maddy says, we don't really interact with it, either. The interaction is rather with "the front side of a time slice of the" object.[31] Her point here is that

> the platonist could argue that the relation of element to set is no more objectionable than the relation of fleeting aspect to temporally extended object.[32]

Bob Hale has argued that the relation of element to set is more objectionable than the relation of fleeting aspect to temporally extended object. He writes in this connection that by speaking of the front side of the time slice of a physical object, we give "a more detailed description of [the] causal interaction...not a reason for denying that it occurs."[33] Thus, the point is that in perceiving the front side of a time slice of x, we perceive x, whereas, in contrast to this, in perceiving the members of x, we do not perceive x. In other words, the members of a set are different from that set, but the front side of a time slice of a physical object is part of that object.

I think that Hale's criticism is a good one. Another objection to Maddy's claim that the platonist could argue that the element/set relation is no more objectionable than the object/fleeting aspect relation is that she says nothing about how the platonist could argue this, and moreover, that it looks for all the world as if the platonist couldn't argue it. For there just doesn't seem to be any connection at all between the element/set relation and the object/fleeting aspect relation. Maddy is, of course, right that we only perceive the front sides of time slices of physical objects, and this means that anyone who wants to develop a theory of perception will have to take this into account. But how does it follow from this that such a theorist would also have to concern him or herself with the element/set relation? We need a independent argument for the claim that in perceiving elements, we also perceive sets; for, on the face of it, this seems utterly unconnected with the claim that one can only perceive the front side of a time slice of a physical object.

Now perhaps what Maddy had in mind is that the object/fleeting aspect case shows that we do perceive objects with which we don't interact. Thus, the point is that the general claim that we can only perceive those things with which we interact is wrong. I have three things to say to this. First of all, if Hale's criticism is correct (and I think it is) then the object/fleeting aspect case doesn't provide a counterexample to the general claim; for if Hale is right, then we do interact with physical objects. Second, even if Hale is wrong and the general claim is false, it does not follow that (i) is false. Moreover, it seems that the object/fleeting aspect case simply doesn't provide a counterexample to (i), for physical objects can reflect light. Third, even if our general claim is not strictly true, it seems to be almost true; thus, even if we can perceive physical objects without strictly interacting with them, one would need an argument to claim that we can perceive some other class of objects without interacting with them. One cannot claim that because we can perceive physical objects without (strictly) interacting with them, we can also perceive sets without interacting with them. To make this claim, one would need either (a) an argument providing some connection between the two cases (i.e., the cases of physical objects and sets) or (b) an independent argument concluding that we do perceive sets. If we didn't demand either of these, then we could claim that, since we can perceive physical objects without interacting with them, we can perceive everything. We could claim, for instance, that we can perceive God in perceiving His creations and that we can perceive the Big Bang in perceiving events that it has caused. Thus, Maddy needs to provide either (a) or (b). But she hasn't done this.

So it seems that Maddy's attempt to reject (i) fails. Therefore, it also seems that my reductio ad absurdum of her position goes through. She cannot take sets to be abstract, because she claims that they are perceptible; but she must take sets to be abstract in order to avoid collapsing into an untenable version of anti-platonism. Therefore, her view is unintelligible.

But perhaps the naturalized platonist could retreat here and claim that while human beings cannot, strictly speaking, perceive sets, their acquaintance with sets is, nonetheless, grounded in perception. Perhaps, that is, human beings acquire the concept set after repeated perceptions of, not sets themselves, but physical aggregates. I will call this view the by-product-of-perception view of mathematical intuition. The problem with it (in this context) is that to accept it is to abandon the naturalized version of platonism.[34] (Actually, this isn't precisely correct. One could adopt a brand of "naturalized platonism" in which mathematical objects are located in space-time, but, yet, not perceptible. There doesn't seem to be much motivation for such a position, however, because the only reason to naturalize platonism is to reap the epistemological benefit of being able to claim that we can perceive mathematical objects.) It seems to me, therefore, that insofar as this chapter is concerned only with naturalized platonism -- i.e., only with the strategy of rejecting (2) -- I ought not to consider the by-product-of-perception view of intuition in this chapter. But I will not simply ignore this position; I will consider it at length in chapter five, where I consider the platonist attempt to build an epistemology by denying that (1) and (2) give us a good reason to believe (3).[35]

6. Is Maddy's View Plausible?

What I want to do in this section is assume that Maddy could somehow avoid the problems that I've raised for her view in the last two sections. That is, I will assume that it is intelligible to claim that sets of physical objects are located in space-time and perceptible, but, yet, still abstract (in some sense of that term). I want to argue that even if this is true, and even if human beings can perceive such sets of physical objects, Maddy has not provided a plausible epistemology for mathematics. That is, I will argue that she has not shown how we can proceed from perceptual knowledge of sets of physical objects to mathematical knowledge.

Of course, Maddy only needs to account for our knowledge of the axioms of mathematics, because she can claim (like everyone else) that knowledge of the theorems arises via proof. Moreover, we can grant that she only has to account for knowledge of the axioms of set theory -- in particular, ZF[36] -- since the rest of mathematics can be reduced to it. (Actually, I think this point could be argued, for pre-Cantor mathematicians surely didn't think that all of their mathematical knowledge was really about sets. I won't press this issue, but if it turns out that Maddy does have to account for our knowledge of other branches of mathematics in non-set-theoretic terms, she will be in trouble, for -- with the possible exception of geometry -- it's not clear that her apparatus could be applied to these other branches of mathematics.) But in spite of these reductions of the problem, I do not think that Maddy can accomplish her task, because I do not think it is plausible that we could gain knowledge of the axioms of ZF by merely acquiring the concept set and then noticing certain semantic facts about this concept.

According to Maddy, the semantic facts that we can intuit about the concept set, include

> beliefs that might be expressed as 'sets have number properties', 'sets (other than singletons) have many proper subsets', 'any property determines a set of things which have that property', the number property of a set is not changed (barring mishap) by moving its elements'."[37]

All of these various propositions, however, are irrelevant. The only thing we need to determine is whether Maddyan intuition (i.e., the process of acquiring concepts and noticing semantic facts about them) could plausibly engender knowledge of the axioms of ZF. And I think it is clear that the answer to this question is "no": at best, Maddyan intuition could only account for our knowledge of three or four of the axioms of ZF.

Maddy would not deny any of this. That is, she does not claim that we attain knowledge of all of the axioms of ZF via intuition. I will discuss how she intends to solve this problem below, but first, I think it is worth discussing the extent of the impotence of her brand of intuition. So far as I know, Maddy only discusses the intuitive appeal of two of the axioms of ZF, viz., the axioms of pairing (which says that any two objects make a set) and union (which says that any two sets can be combined to form a third set). Now it seems to me that she might very well be able to argue that these two axioms could be known via Maddyan intuition. (In other words, it is plausible to think that anyone who understood "set" would believe these two axioms.[38]) Beyond union and pairing, however, the situation is rather bleak. With the exception of the axiom of extensionality (which says that any two sets with the same elements are identical) it seems implausible to think that knowledge of any of the other axioms of ZF could arise via Maddyan intuition.

Maddy's only hope for an exception here is with the axiom of the power set (which says that, for any set x, its power set exists, i.e., the set whose members are the subsets of x exists). The reason I am dubious of our ability to attain knowledge of this axiom via Maddyan intuition is that, in order to even understand it, one needs the concept set of sets, which we might not be able to acquire by perceiving sets of physical objects. Regardless, the other axioms are even less accessible to Maddyan intuition. To understand the axiom of replacement (which says that, for any set x and any functional property P,[39] there is a set y whose members are the values of P(z), where z is a variable ranging over the members of x) one needs the concept <u>functional property</u> -- not to mention the concept <u>range of a functional property</u> -- but it doesn't seem likely that these concepts are going to be acquired in anything like the way that Maddy suggests. More importantly, one also needs a fair amount of mathematical "know how" to understand what the axiom of replacement even <u>says</u>. But Maddy can't assume that we already have any prior mathematical knowledge, because the whole point of her theory of intuition is to explain how we acquire mathematical knowledge.

I will not discuss them here, because they aren't really full-fledged members of ZF, but the axioms of choice and regularity are in the same boat as the axiom of replacement.[40]

The situation gets even worse with the axiom of the null set (which asserts the existence of a set with no members) and the axiom of infinity (which asserts the existence of a set with infinitely many members). How could existence claims be "built into" a concept?[41] To claim that anyone who understands "set" believes that there is an empty set and an infinite set is like claiming that anyone who understands "horse" believes that there are horses with no teeth and with infinitely many teeth. I'm not even sure that anyone who understood "set" would <u>understand</u> "null set" and "infinite set", let alone believe that such terms refer to real things.

It is worth noting here that Maddy's view gives rise to another problem in connection with the axioms of infinity and the null set: it's not clear that, on Maddy's view, these axioms are even true. I raised this problem above, in section 4, in my discussion of the difficulties with what I called Maddyan anti-platonism. I pointed out that Maddy might be able to avoid this problem in connection with the axiom of the null set, but I could not see any way for her to avoid it in connection with the axiom of infinity. It's not clear whether Maddy-qua-platonist will still have this problem, for we don't really know what Maddyan platonism is. Recall that we are assuming that Maddy will be able to solve the problems of the previous two sections; that is, we are assuming that she can provide a coherent account of sets of physical objects according to which they are abstract objects and also located in space-time and perceptible by the senses. But, since we have no idea how she could do this, we have no idea what -- according to Maddyan platonism -- an infinite set is; thus, we have no idea whether the axiom of infinity will come out true under Maddyan platonism. It seems to me that the best way to handle this situation is to state that Maddy has a further desideratum to meet. She has to provide an account of sets of physical objects according to which they are abstract objects but yet located in space-time and perceptible by the senses and according to which the axiom of infinity is true independent of empirical hypotheses about the physical world.[42]

Regardless of considerations of particular axioms, it seems quite wrong-headed, in general, to claim that the axioms of set theory are "built into" the concept set. Such a claim seems analogous to the claim that the entire theory of horses is built into the concept horse. By merely acquiring a concept, one does not attain knowledge of the theory of the things falling under that concept. Maddy might respond here that she makes no such claim, because she admits that our intuitions are fallible. But this is irrelevant, because my point here is not really about knowledge. My claim is that Maddyan intuition couldn't even provide us with an understanding of set theory, let alone knowledge of it. Moreover, this claim seems to me to be rather obvious. To really understand set theory requires years of training, but a seven-year-old child could acquire the concept set.

Maddy is aware of all of these problems. She admits that some of the axioms of ZF are not at all intuitive. She writes, for instance, that "there is nothing obvious about" the axiom of infinity.[43] To solve this problem, she supplements her account of intuition (i.e., of the intrinsic justification that the axioms of ZF can receive) with an account of various theoretical considerations that can move us to accept an axiom (i.e., of the extrinsic justification that the axioms of ZF can receive). These theoretical considerations are holistic: axioms are justified

> by their consequences, by their ability to systematize and explain lower-level theory.[44]

To be a bit more specific, Maddy claims that we might accept a hypothesis as an axiom if it provided a solution for one or more open questions, or if it entailed the truth of previous conjectures or intuitively pleasing results.[45]

I do not wish to deny that axioms can be justified in this "extrinsic" way, by purely pragmatic considerations. Instead, I would like to point out that this fact raises some problems for Maddy's epistemology. Maddy writes as if she can merely state that some axioms are justified solely extrinsically, but this is a misconception. She must also provide an explanation of why axioms can be justified in this way. To see that such an explanation is required here, all we have to do is recall that Maddy is trying to respond to the epistemological argument against platonism. For in order to respond to that argument, platonists must explain how human beings arrive at non-derivative mathematical knowledge. Nobody would think that platonists could respond to the epistemological argument by simply stating that we arrive at non-derivative mathematical knowledge via mathematical intuition; it is obvious that they also have to explain how intuition does this, i.e., how it provides us with knowledge (as opposed to false belief) of mathematical objects. And, indeed, Maddy has not skirted her responsibility here: she has devoted a great deal of effort to explaining how we arrive at intuitive mathematical knowledge. But Maddy does not think that all of our non-derivative mathematical knowledge arises via intuition. She thinks that some of it arises from extrinsic considerations. Thus, just as Maddy had to explain how intuition leads to knowledge (as opposed to false belief) of mathematical objects, she also has to explain how extrinsic considerations lead to knowledge (as opposed to false belief) of mathematical objects.

Another reason for thinking that the platonist has to explain how pragmatic considerations could engender knowledge of mathematical objects is that there is a prima facie reason to think that the claim that such considerations could engender such knowledge is incompatible with platonism. If mathematics is about objective entities (as Maddy and all other platonists claim), then it is unclear what the fruitfulness (for instance) of a hypothesis could have to do with its acceptability. Why should we consider a hypothesis to be true merely because it provides an answer to an open question?[46]

Unfortunately, Maddy has not provided an explanation here. After spending a great deal of time explaining how some of the axioms of ZF can be justified intrinsically (i.e., by their intuitive appeal) she merely states that the rest of the axioms are justified extrinsically (i.e., by their pragmatic appeal). Thus, Maddy has left it unexplained how we attain knowledge of most of the axioms of ZF, e.g., the axiom of infinity. Therefore, she has provided neither an adequate epistemology of ZF nor an adequate response to the epistemological argument against platonism.

Now, I want to emphasize again that I do not disagree with Maddy's claim that mathematical axioms can be justified purely extrinsically. My point is simply that she cannot account for this fact, i.e., that her view makes it puzzling why this should be a fact. And it is worth pointing out as an aside that her inability to provide an explanation here exposes not just the inadequacy of her epistemology, but also the implausibility of her entire view of mathematics. For insofar as mathematical axioms can be justified pragmatically, and insofar as platonism seems to be incompatible with this fact, platonists are on the hook to explain why this seeming incompatibility is an illusion. Thus, if Maddy cannot explain why pragmatic considerations are relevant to the acceptability of a mathematical proposition, then we have reason to abandon her entire philosophy of mathematics.

But not all platonist positions are equally vulnerable here. In particular, from within the platonist position developed in chapter six, it is easy to explain why pragmatic considerations are relevant to acceptability.[47]

7. Conclusion

I have argued in this chapter that Maddy's naturalized platonism is unintelligible, and that, even if it could somehow be made coherent, it is not very plausible. Now, since my arguments could be used equally well against any version of naturalized platonism,[48] I conclude that the denial of (2) cannot lead to a rebuttal of the epistemological argument against platonism. If we combine this with the result of the last chapter -- that a denial of (1) cannot lead to a rebuttal of that argument, either -- we arrive at the conclusion that platonists cannot appeal to <u>any sort of contact</u> between human beings and abstract objects in their attempt to construct an epistemology.[49] Thus, the only remaining hope for platonists is to argue that (1) and (2) do not provide a good reason to believe (3), i.e., that in spite of their lack of contact with the mathematical realm, human beings can nonetheless acquire knowledge about the nature of that realm.

It is worth noting that Maddy has unwittingly provided an argument for the claim that this is, indeed, the strategy that platonists ought to pursue. She correctly points out that certain axioms are justified extrinsically, without the aid of intuition. Thus, if platonism is correct, we must be able to arrive at <u>some</u> non-derivative mathematical knowledge <u>without coming into contact with mathematical objects</u>. But if it is possible to argue that we arrive at <u>some</u> non-derivative mathematical knowledge in this way, it ought to be possible to argue that we arrive at <u>all</u> non-derivative mathematical knowledge in this way.[50] But if we can argue that <u>all</u> non-derivative mathematical knowledge can arise without the aid of contact with mathematical objects, why should we try to argue that there <u>is</u> such a thing as contact between human beings and mathematical objects?

In the next two chapters, I will clear all of this up. That is, I will explain <u>how</u> non-derivative mathematical knowledge arises without the aid of contact with the mathematical realm.

NOTES

1. I am using the notion of "contact" -- and related notions such as "interaction," "access," "link," etc. -- in the same way that I did in chapter three. For a discussion of this, see section 2 of chapter three.

2. On p. 669 of his (1973), Benacerraf claims that "'combinatorial' theorists...have usually wanted to have their cake and eat it too: they have not realized that the [epistemologically motivated] truth conditions that their account supplies for mathematical language have not been connected to the referential semantics which they assume is also appropriate for that language."

3. See Aristotle's Metaphysics (remarks on mathematics are scattered throughout); Locke (1706), Book II, section XVI; and Mill (1843), Book II, chapters V and VI.

4. Armstrong's view is clearly different from Maddy's, but one could think of them both as "naturalized platonists," for they both seem to think that there are abstract objects which exist within space-time. See, for instance, Armstrong (1978), chapter 18, section V.

5. This actually needs some qualification. My position is consistent with some of Maddy's more general claims, e.g. that non-demonstrative mathematical justification can be intrinsic (i.e., intuitive) and extrinsic (i.e., based upon holistic considerations). What I disagree with is the way in which Maddy explicates these two sorts of evidence.

6. Maddy (1990), p. 48.

7. Intuitively, there doesn't seem to be any important differences among the various senses here. Thus, I will, in general, speak in terms of vision. But everything I say could be applied to the other senses as well.

8. Maddy (1980), p. 179.

9. It is worth noting that Maddy is not the first to postulate abstract objects with spatio-temporal location. Her sets seem at least somewhat similar to Aristotelian Forms and to what we might call quasi-abstract objects (e.g., the equator). Some people would take both Aristotelian Forms and the equator to be abstract and yet located in space-time.

10. Maddy (1980), p. 185.

11. Maddy (1980), p. 185.

12. Maddy (1990), p. 145. For a general discussion of her position on extrinsic justification, see chapter 4 of her (1990).

13. Maddy (1990), p. 125.

14. For a discussion of this position, see Maddy's (1990), chapter 3, especially section 2.

15. Maddy (1990), p. 59.

16. Recall that, in chapter one, I pointed out that in order to establish platonism, one must argue that all anti-platonist positions are untenable. The reason is that if any anti-platonist philosophy of mathematics could account (at least as well as platonism does) for mathematical theory and truth, then -- by Ockham's Razor -- it would be preferable to platonism.

17. See Maddy's (1990a) and chapter five of her (1990). One might wonder why, if this were Maddy's view, she would call herself a platonist. The answer is that she might take platonism to be the view that mathematics is about objective (but not necessarily abstract) entities.

18. See Maddy (1980), pp. 167-168; and Maddy (1990), p. 49.

19. Maddy (1990a), p. 260.

20. Maddy (1990), p. 60. Frege's objection to Mill can be found in his (1884), section 23.

21. Maddy (1990), pp. 152-153.

22. While the identification of singletons with their elements sounds anti-platonistic, Maddy may still be a platonist. For she countenances not just sets of physical objects, but sets of sets of physical objects, sets of sets of sets of physical objects, etc. Moreover, she accepts the axiom of infinity. So there is reason to think that even if Maddy takes a singleton containing an egg to be a concrete object, she still takes some sets to be -- in some sense -- abstract.

Maddy does not identify all singletons with their elements; this identification only holds for singletons which contain urelements. (On Maddy's view, this is just to say that it only holds for singletons which contain physical objects, for she doesn't acknowledge any other kind of urelement.)

23. Maddy needs {x, y} to be different from the two physical objects x and y as individuated things taken together. For she needs to be able to get {{x, y}}, {x, y, {x, y}}, etc.

But if {x, y} is identical with the two objects x and y as individuated things taken together, then it is also identical with {{x, y}}. Thus, Maddy will never be able to get off the ground floor.

24. Even if the physical world isn't finite, it won't help Maddy-_qua_-anti-platonist; for the truth of mathematics can't depend on such a controversial empirical hypothesis.

25. Mill (1843), Book II, chapter V, section 5.

26. Maddy (1990a), pp. 276.

27. Benacerraf (1973).

28. One might think that Maddy could argue that only sets of physical objects (perhaps just singletons containing physical objects) are concrete. Thus, it might be thought that the epistemological problem could be solved by arguing that we can attain knowledge of concrete sets via perception and knowledge of abstract sets via some sort of inference from our knowledge of concrete sets. But this doesn't solve the epistemological problem; for since abstract sets will be utterly different in kind from concrete sets, anti-platonists will demand an explanation of why perception of the latter tells us anything at all about the former.

In any event, I do not think that Maddy would take this line. See her (1990a).

29. There might be a counterexample to the claim that we can only see things which are capable of reflecting light. On some uses of "see," we might be able to say that I can see an afterimage. I do not want to squabble over the meaning of "see," and it is clear that I don't _need_ to. For when Maddy claims that we can see sets, she is speaking of the perception of objective entities which actually exist outside of us; which can be seen at the same time by _other_ people -- people who aren't malfunctioning in any way; and which can be felt, tasted, smelled, and heard. Thus, we can simply ignore afterimages. Now, I think that Maddy _will_ want to object to my claim that all sensible objects are made of matter, but it will be for entirely different reasons.

30. An analogous claim could be made about objects that can be heard, tasted, smelled, and felt. Objects that can be heard, for instance, must be capable of initiating a sound wave.

31. Maddy (1980), p. 167. The same argument is given on p. 49 of Maddy (1990).

32. Maddy (1990), p. 49. The same argument is given on p. 168 of Maddy (1980).

33. Hale (1987), p. 81.

34. It is for precisely this reason that Maddy wouldn't accept the by-product-of-perception view. To do so would represent a departure from her position: human beings would no longer be gaining knowledge of sets by coming into contact with them; rather, they would be attaining knowledge of sets via contact with physical objects.

35. The philosopher who comes the closest to accepting the by-product-of-perception view of intuition is Charles Parsons. I will say more about this in chapter five.

36. I choose ZF not only because it is so widely accepted as the "standard" set theory, but also because this is the set theory which Maddy herself chooses. That is, it is ZF for which she is trying to construct an epistemology. It should be noted, however, that the choice of ZF will not affect my arguments against Maddy's epistemology. I could make the same point (or, at least, an analogous point) in connection with the axioms of any set theory robust enough to provide a basis for mathematics.

For a good, quick explication of the axioms of ZF, see section 1.11 of Malitz (1979).

37. Maddy (1980), p. 185.

38. What I mean when I say that it's plausible that we could attain knowledge of the axioms of pairing and union via Maddyan intuition is that this claim is not obviously false. In the end, however, I do not agree with Maddy's semantic account of intuition. (Now, this is not simply because I reject naturalized platonism. Maddy's semantic account of intuition is independent of her naturalism: one could deny that sets are spatio-temporal and perceptible and still claim that mathematical intuition is semantic intuition about mathematical concepts, or vice versa.)

The problems with Maddyan intuition are numerous, but I will indicate just two. First, Maddy's theory of intuition leaves too much unintuitive. Mathematicians have sometimes uncanny mathematical intuitions. Gödel, for instance, had a quite strong intuition about the falsity of the continuum hypothesis; and -- according to Jerrold Katz (1981), p. 196 -- Ramanujan intuited that 1729 is "the smallest number expressible as the sum of two cubes in two ways." (Katz's remark derives from Hardy (1940), p. 37.) It seems to me that such "advanced" intuitions couldn't derive from the meaning of "set" or, for that matter, from the meaning of any other term. A second (and related) problem with Maddy's theory of intuition is that it doesn't account for differences in intuitive ability. How is it that Ramanujan has better intuitions than most mathematicians, who have better intuitions than I, who have better intuitions than most 12-year-old children? If Maddy's theory were correct, then everybody completely familiar with the meaning of "set" should have comparable intuitions.

39. According to Malitz (1979, p. 45) "a functional property P is one such that for every x there is at most one y such that Pxy." In other words, there is at most one y such that <x, y> has the property P.

40. The axiom of choice says that every set S of non-empty sets has a choice function, i.e., a function that chooses a particular member of each of the members of S. The axiom of regularity says that every non-empty set x has a "least" member y such that nothing is in the intersection of y and x. The reason that neither of these axioms is knowable by Maddyan intuition is identical to the reason that the axiom of replacement isn't so knowable: one needs a fair amount of mathematical knowledge to even understand them.

41. I raise this objection in connection with the axioms of infinity and the null set simply because the problem is particularly striking in connection with them. But since almost all of the axioms of ZF involve some sort of existence claim, I could raise the same objection to these other axioms. For instance, even if Maddy could make a plausible case for the claim that one could come to understand the concept of <u>power set</u> via her brand of intuition, it is arguable that such a person might puzzle at the claim that the power set of any set <u>exists</u>.

42. There is a related problem about what the negative and real numbers are. Maddy says that the natural numbers are properties of sets, but since there are no sets with -2 members, she cannot take the same line on negative numbers. Now, I suppose she could make use here of the normal set-theoretic reduction of such numbers and claim that -2 is a property of certain ordered pairs of sets. This seems more than a bit unnatural to me, but, in any event, Maddy cannot use this strategy to solve the analogous problem with the reals. For it would seem not only unnatural but also <u>wrong</u> to say that the real numbers are properties of Dedekind cuts; this is simply because, since Dedekind cuts have infinitely many members, the number property associated with them is infinity. Maddy recognizes this and doesn't take this line. Rather, she says that "the real numbers are the property of continuity." Her motivation for this claim is that, according to her, Dedekind -- in his (1872) -- was trying to account not for the real numbers, but for continuity. She writes -- in her (1990), p.94 -- that "the question Dedekind faced was not 'what are the real numbers?', analogous to Frege's 'what are the natural numbers?', but rather, 'what is continuity?'"

It seems to me, however, that regardless of whether this is correct and regardless of whether, taken together, the reals are the property of continuity, none of this is relevant to the question of what a particular real number is. Maddy would presumably admit that, say, π is a particular thing (because she would admit that the sentence "π is irrational" is true). We are thus led to the question 'what is π?', and we cannot answer it by saying what the reals are, taken as a whole. I do not want to dwell on this issue; I just want to point out that it's not clear how Maddy could solve this problem.

43. Maddy (1990), p. 125.

44. Maddy (1990), p.107.

45. Maddy discusses extrinsic justification in chapter 4 of her (1990). On pp. 145-6, she gives a quick rundown of some of the theoretical considerations that can result in the acceptance of an axiom.

46. It is also worth pointing out that if it were acceptable for a platonist to simply state that human beings can arrive at non-derivative mathematical knowledge via extrinsic -- or holistic -- considerations, then Benacerraf's argument would never have been very troubling. Platonists would simply have argued that all axioms are justified extrinsically, and that would have been the end of the problem. Indeed, I wonder why Maddy didn't take this line. She could have avoided all of the problems with naturalized platonism if she had. She could still have claimed that the axioms of pairing, union, and extensionality are intuitively obvious, but she would have had to admit that this doesn't justify them, that they are only justified by extrinsic considerations. For whatever it's worth, the view that mathematical axioms are justified purely extrinsically seems to be the view of Mark Steiner (1975, chapter IV), and perhaps also Quine. It seems to me, however, that Steiner's view of extrinsic justification is far less plausible than Maddy's, for he takes the justification to be doubly extrinsic. That is, he thinks that mathematical axioms are justified by their fruitfulness in empirical theories, not mathematical ones. I will say more about this (and why it is implausible) in chapter five, section 5.

47. It would be much easier to justify this claim after I lay out the version of platonism which I favor. However, since I will only be concerned in chapter six with showing that this version of platonism gives rise to an adequate solution to the epistemological problem, I will not pause there to explain how the position can also account for the connection between pragmatic considerations and acceptability. Thus, I will attempt to provide the explanation now. Consider a consistent first-order theory T and a new axiom-candidate A.

Once it has been shown that A is consistent with T, it follows that T+A has a model. Thus, for any such T and A, so long as A doesn't contradict T, we can justify its adoption extrinsically, or pragmatically. This is simply because the acceptance of A merely represents the decision to study a particular model. It is important to note that this is true even if T+not-A is _also_ consistent. In that case, the adoption of not-A could _also_ be given a pragmatic justification. Thus, the point is that, so long as A is independent of T, one need not worry about _truth_ at all, for it will be legitimate to add either A or not-A to T, because T+A and T+not-A both have a model in which they are true. Hence, since either course of action would be legitimate, the decision as to which course to follow (i.e., which model to study) can be supported extrinsically, i.e., by pragmatic considerations.

The reason Maddy can't use this explanation is that she doesn't think that, in such a case, there actually exists models of _both_ T+A and T+not-A. Thus, it is a mystery, on her view, how we could know which of these theories to believe, for it is a mystery how pragmatic considerations could be relevant to the question of what models actually exist, i.e., the question of which of T+A and T+not-A has an actually existing model.

48. In section 2, I said a few words about why my _reductio_ of Maddy's view could be applied generally, but it also seems that my argument in section 6 could be applied generally. For that argument just rests on the fact that by merely perceiving sets of physical objects, one could not gain an understanding of set theory. But it seems that this would be true however naturalized platonists spelled out the process by which we acquire perceptual knowledge of sets.

49. One might object here that, since I haven't interpreted either Gödel or Maddy as defending an "inter-realm" contact between human beings and mathematical objects, there remains a contact-based epistemology which I haven't yet refuted. But recall that, in arguing in chapter three that Gödel had to adopt an immaterialist philosophy of mind, I gave an argument (based on causal inaccessibility) which suggested that "inter-realm" contact is impossible. But even without this argument, we would be quite justified in moving on. For, _prima facie_, inter-realm contact seems impossible, and no one has ever said anything to suggest otherwise; that is, no one has ever tried to explain _how_ inter-realm contact might occur.

50. One might think that this is wrong, that platonists might only be able show how human beings can arrive at non-derivative mathematical knowledge without the aid of contact with mathematical objects, if they had some prior knowledge to work with. But this would be a mistake, for, insofar as

the mathematical knowledge in question is non-derivative, its justification could not rely upon any prior mathematical knowledge.

Chapter Five

Knowledge Without Contact

1. Forward

I argued in chapter two that mathematical platonism cannot be accepted unless we can find a flaw in the following argument:

(1) Human beings exist entirely within space and time.

(2) If there are (abstract) mathematical objects, they exist outside of space and time.

Therefore, we have reason to suspect that

(3) If mathematical objects exist, then human beings could not attain knowledge of them.

Moreover, I argued in chapters three and four that platonists cannot respond to this argument by rejecting (1) or (2) and that, therefore, they cannot hope to build an epistemology for mathematics by claiming that human beings are capable of coming into contact with abstract objects (i.e., receiving information from such objects). Therefore, if platonists are to escape the epistemological argument against platonism, they must argue, somehow, that (1) and (2) do not provide a good reason to believe (3). Now, of course, platonists <u>could</u> do this very easily; for, since (1) and (2) do not <u>logically imply</u> (3), platonists could simply claim that nothing needs to be said about this inference at all. But we saw in chapter two that such a stance would not be very plausible. In particular, we saw that it is at odds with the following plausible intuition:

(PI) As a general (but not necessarily universal) rule, the total inaccessibility of a collection of objects raises a prima facie problem (which may or may not be solvable in particular cases) about the possibility of attaining knowledge of the objects in question.

Among platonists who share the intuition that (PI) is true, something needs to be said about why (1) and (2) do not provide a good reason to believe (3). And, as I pointed out in chapter two, the only way to do this is to simply explain how (3) could be false while (1) and (2) are true, i.e., to explain how human beings could attain knowledge of abstract objects, given that they cannot come into contact with such objects.

Now it might seem as if there are innumerable ways of trying to do this. In other words, while we have said that, by eliminating the strategies of Gödel and Maddy (i.e., of denying (1) and (2), respectively), we have narrowed the platonist alternatives to one, what we have really done is narrowed their alternatives to one schema. Moreover, it is a very loosely defined schema -- the only restraint is that the truth of (1) and (2) must be respected -- and so it seems that one could fill in the details of this schema in many different ways. Thus, it might seem that we have not made much progress in our job of searching the logical space of possible platonist responses to the epistemological argument. But from both an historical and a logical perspective this is false. From an historical perspective, there has been relatively little variation in the epistemological theories that have been offered by platonists who accept (1) and (2). And from a logical perspective, we can see that there is good reason for this historical fact: the platonist really has only one promising way to proceed. (The reason for this will be made clear below.)

2. The No-Contact Theory of Intuition

The one remaining way for the platonist to proceed in his attempt to provide an acceptable epistemology for mathematics is to adopt what I will call the no-contact theory of intuition, or for short, NCTI. Why this is the only plausible avenue left to the platonist, I will make clear below. For now, I simply want to explain what the theory says and how platonists have, in recent years, arrived at it.

In the face of difficulties like the ones outlined in chapters three and four, contemporary platonists felt obliged to abandon the Gödelian view that human beings possess a faculty of mathematical intuition which provides contact with abstract objects. But they did not want to abandon the faculty of intuition all together, because they did not want to lose the great explanatory power that such a faculty brings to epistemology. Thus, in an effort to rescue the baby from the bath water, many platonists have, in the last two decades, attempted to provide an epistemology of abstract objects by laying out a theory of mathematical intuition which avoids the crippling assumption that such a faculty must involve some sort of contact between the human mind/brain and abstract objects. The theory in question is, of course, NCTI, and the platonists who have advocated it include Mark Steiner, Michael Resnik, Stewart Shapiro, Jerrold Katz, Charles Parsons, and David Lewis.[1]

Now, depending on which of these philosophers we read, we will get a different version of NCTI; that is, the details of the theory will vary from one treatment to the next. But the basic idea is always the same: all versions of NCTI involve the claim that mathematical intuitions are the products of a psychological apparatus which is capable of producing (usually true) beliefs about mathematical objects (or structures or patterns) without the benefit of any sort of contact with the objects in question.[2]

Three points need to be made about the way I have formulated NCTI. First of all, there is nothing controversial about my use of the term "contact" here. In chapter three, I tried to spell out what is meant by "contact between human beings and mathematical objects," and it was important to do so there, because I was considering the position of Gödel, who <u>does</u> believe in such contact. But, here, nothing needs to be said to justify the use of "contact," because advocates of NCTI <u>don't</u> believe in such contact. Thus (in this context) it simply doesn't matter if, in the end, it is meaningless or incoherent to speak of contact with mathematical objects, because advocates of NCTI needn't use the term "contact" at all. If they want, they can formulate NCTI as follows: from ordinary perceptual stimuli alone, human beings are capable of generating thoughts and beliefs about mathematical objects.[3] Thus, whatever is meant by "contact with the mathematical realm," advocates of NCTI not only reject it, but they needn't talk about it. My original formulation of NCTI was couched in terms of "contact" merely to emphasize the fact that advocates of this theory disbelieve in it. But it wasn't necessary to do that, for, again, I could have said that such philosophers think that we only have perceptual (and introspective) information coming in, but that, in spite of this, mathematical knowledge is still possible, because the human mind/brain is so structured that it doesn't need contact with the mathematical realm in order to generate (usually true) beliefs about it.[4]

The second point to be made about the way I have formulated NCTI is that I haven't used the term "faculty". Thus, NCTI does not depend upon a faculty theory of mind. NCTI merely says that one of the abilities of the human mind/brain is the ability to proceed from ordinary perceptual stimuli to beliefs about mathematical objects. It is not claimed that this ability is the result of a special faculty. Whatever cognitive scientists decide to say about mental abilities is what I will say about the ability to formulate mathematical beliefs. In the mean time, however, for the sake of convenience, I will speak of the faculty of no-contact mathematical intuition.

The third and final point that I want to make about my formulation of NCTI is that it leaves open which of our mathematical beliefs will be counted as intuitions. Advocates of NCTI think that all of our mathematical beliefs arise without the aid of any contact with the mathematical realm, but this does not commit them to saying that all of our mathematical beliefs are intuitions. To be true to ordinary usage, advocates of NCTI need only say that intuitions are mathematical beliefs which are (a) generated from some introspective or perceptual stimulus, but (b) not generated by reading, hearing, or thinking of a proof, or by holistic considerations, or, etc. To be briefer, they could simply say that to be counted as an intuition, a mathematical belief must be immediately obvious.

I do not mean to give the impression that NCTI is a new and radical development. What it is is a modernized version of the seventeenth-century rationalist conception of a priori knowledge. I said above that the impetus for NCTI came from a reaction to Gödel's theory of intuition, and I think that is correct; but, in spite of this, the content of NCTI comes from Kant's theory of intuition. In fact, NCTI really just provides a platonized version of Kant's notion of "pure intuition." That faculty, according to Kant, was responsible for concept construction,[5] but if we take a platonistic view of concepts, then what we construct in pure intuition will be precisely what NCTI-platonists claim that we construct in mathematical intuition -- namely internal representations of abstract objects. Now, NCTI-platonists also think that no-contact intuition provides us with _propositional_ beliefs of abstract objects,[6] but again, this is describable in seventeenth-century terms. Such propositional beliefs will simply be knowledge of concept relations, which is precisely what the rationalists held _a priori_ knowledge to be.[7]

Hale has also pointed this out. After abandoning the Gödelian view that we can attain knowledge of abstract objects through a faculty of mathematical intuition which is analogous to perception,[8] Hale claims that we must return to the traditional seventeenth-century notion that mathematical knowledge is _a priori_. In this context, he says that

> The notion of [mathematical] intuition might, to be sure, be developed in ways that play down the supposed analogy with sense perception...But I am far from sure that, when elaborated along these lines, it is a genuinely distinct alternative to the view that basic _a priori_ knowledge is got by recognition of conceptual linkages.[9]

So according to NCTI, intuition involves a sort of _construction_: given certain ordinary inputs, certain mathematical concepts and beliefs are simply spun out of the machinery of the human mind/brain. Thus, NCTI is not so dissimilar from what a mathematical intuitionist, or conceptualist, might say. The difference is that, according to platonistic advocates of NCTI, the intuitions that we construct in our minds are not actual mathematical objects (as someone like Brouwer would say) but rather _internal representations_ of mathematical objects.[10] Mathematical objects themselves, according to the platonist, exist quite independently of any human faculty of intuition -- they are objective abstract objects.

NCTI is also in agreement with what other sorts of anti-platonists would say about mathematical intuition and knowledge. In fact, I would like to now argue that anti-platonists must accept NCTI, although, of course, they will not formulate in a way that commits us to abstract objects. The reason they must accept it is that it merely states that from ordinary perceptual stimuli, human beings are capable of arriving at mathematical beliefs. Since anti-platonists are the first to claim that human beings only receive ordinary perceptual stimuli, and since they must admit that we do have mathematical beliefs,[11] they cannot deny NCTI.[12]

It shouldn't be surprising that anti-platonists can accept NCTI, because there is nothing particularly platonistic about that theory. NCTI is a psychological (or perhaps neurophysiological) theory: it says that the human mind/brain has the ability to proceed from ordinary perceptual stimuli to mathematical beliefs. Thus, NCTI is neutral as to whether platonism is true or false. Now, to be sure, platonists and anti-platonists will formulate NCTI differently, but this is irrelevant. The important thing is that they will agree about the psychological facts involved.

It is worth considering how platonists and anti-platonists would differ in their formulations of NCTI. According to platonistic advocates of NCTI, there are two sorts of intuition: intuition-that (which provides intuitions that propositions about abstract objects are true) and intuition-of (which provides intuitions of abstract objects). I have already alluded to this distinction: in my discussion of Kant, I spoke of concept construction as a process of building internal representations of abstract objects. These internal representations are just intuitions-of. Intuitions-that, on the other hand, are just propositional beliefs about abstract objects.

Some platonists -- e.g., Parsons and Steiner -- write as if human beings could possess one of these kinds of intuition without possessing the other.[13] This seems to me to be a mistake. If I have an intuition that, say, "b is F" is true, then what's to stop me from saying that I have an intuition of the object b? It seems entirely arbitrary to say that I don't.[14] The purpose of distinguishing intuition-that from intuition-of is not to jettison either one; the purpose is simply to facilitate communication. So long as we accept NCTI and deny that human beings can have direct contact with the mathematical realm (as Parsons does) there is nothing controversial about either form of intuition. The faculty of intuition-that simply yields propositional mathematical beliefs and the faculty of intuition-of simply yields representations of (or -- for those who balk at the very notion of representations -- thoughts about) mathematical objects. One cannot seriously question that human beings possess either faculty. Now, to be sure, one might be an anti-platonist and deny that any of our representations of mathematical objects actually <u>represent</u> anything outside the mind. Such philosophers would, of course, not call the products of intuition-of "representations," but this is only a terminological issue.

This brings me to the question of how anti-platonists would formulate NCTI. Such philosophers could also refer to both sorts of intuition, but they would simply describe them in different terms. Intuition-of would still be a process of concept acquisition, but anti-platonists would not say that the products of intuition-of are "representations of mathematical objects." How they would describe them depends upon what particular version of anti-platonism they accept. Idealists, for instance, would say that the products of intuition-of <u>are</u> mathematical objects; fictionalists, to pick another example, would say that they are thoughts about fictional objects (and, of course, "about fictional objects" is going to be handled nominalistically here). As far as intuitions-that are concerned, anti-platonists would simply take these to be propositional beliefs about the products of intuition-of. There are two possible exceptions to this. If fictionalists wanted intuitions-that to be true, they would have to say something a bit different, e.g., that intuitions-that are beliefs about the <u>possible truth</u> of propositions about fictional entities. The second exception is Wittgenstein, who might say that to have an intuition-that is to accept a <u>rule</u>. I'm no Wittgenstein scholar, so I'm not certain about this, but it doesn't matter. The only important thing is that Wittgenstein would have to accept _some version of NCTI or other_, for he must admit that human beings have mathematical ideas _of some sort_ and he must admit that they arrive at these ideas via ordinary stimuli, i.e., in a nominalistically kosher way.

If we had to formulate NCTI in a way that both platonists and anti-platonists would agree to we could say that human beings have the ability to proceed from ordinary perceptual stimuli to mathematical beliefs. Perhaps a more interesting way of putting it is that human beings can proceed from such stimuli to thoughts _which they at least think are about mathematical objects_. Anti-platonists could not object to this, for the very existence of a single human platonist verifies that humans _do_ have this ability.

The point of all of this is that NCTI is a fairly trivial theory. The only people who would object to it are those who think that human beings could only arrive at mathematical beliefs via some sort of Gödelian or Maddyan contact with mathematical objects. There are two reasons, however, why it is acceptable (for the present purposes) to simply _ignore_ the possibility of contact with mathematical objects. The first is that I have already argued (in chapters three and four) that such views are incoherent. The second, and more important, reason is that platonistic advocates of NCTI are only trying to convince _anti-platonists_ that they can construct a platonistic epistemology for mathematics. But anti-platonists certainly cannot object to

NCTI on the grounds that contact with mathematical objects is a necessary condition for mathematical belief; this is simply because anti-platonists believe in the latter but not the former. Thus, for our present purposes, we can simply assume that NCTI is true.[15]

I do not know why platonistic advocates of NCTI have failed to recognize the triviality of NCTI. All of them feel as if they have to argue that we possess a faculty of no-contact mathematical intuition. It seems to me that this is a tactical mistake, for it gives the impression that the doctrine in question requires argument; and this can only reduce the clarity and credibility of what is being said. Only one advocate of NCTI -- viz. Shapiro -- comes close to recognizing this. He says that

> Whatever the level of knowledge concerning the relevant psychological mechanisms, pattern recognition is not philosophically occult. We clearly do have the ability to apprehend and characterize patterns.[16]

His talk here of patterns is not important for our purposes. This is merely a function of his ontological structuralism: he thinks (following Resnik) that mathematics is a science of patterns, rather than objects. But while Shapiro realizes that it is not contestable that human beings possess the capacity for pattern recognition, he thinks there is something of a problem about how we could have beliefs about mathematical patterns, since these are infinite, and, presumably, could not be grasped via pattern recognition.[17] I don't understand how Shapiro could realize that pattern recognition is not philosophically occult and still think that mathematical belief is. It is as if Shapiro had the right idea but just didn't take it far enough. For we can say exactly the same thing about mathematical belief that he said about pattern recognition: whatever the level of knowledge concerning the relevant psychological mechanisms, mathematical belief is not philosophically occult. We clearly do have the ability to form mathematical beliefs and thoughts which (we at least think) are about mathematical objects, structures, and so forth. Now, as I said above, it might be that there are no mathematical objects, but it is still obvious that we are capable of believing things that we take to be about such objects.[18]

3. Various Versions of NCTI

I said above that, while it might seem that platonists could proceed in numerous different ways, their last real hope of constructing an acceptable epistemology for mathematics is to adopt NCTI. Now, I have yet to justify this claim, but one might object that, even with the adoption of NCTI, we still only have a strategy schema, because NCTI is only a theory schema. It is not a complete theory, for there is room within NCTI for different conceptions of what, precisely, intuitions are and how they are produced. But while this is obviously true, I will argue in this section that the details of NCTI are not important for our epistemological purposes. That is, the way in which mathematical intuitions are actually produced -- so long as they are _not_ produced via contact with abstract objects -- will not be relevant to our question of whether these intuitions can provide the basis for mathematical _knowledge_, or, to be more precise, the basis for an acceptable epistemology of mathematical objects. To show why this is the case, we probably don't need to be familiar with any particular version of NCTI. However, the argument will be much clearer if we have, if nothing else, at least a sketchy idea of how the various advocates of NCTI think the theory should be filled in. Moreover, we will, by looking at the "nuts and bolts" of various versions of the faculty of no-contact mathematical intuition acquire a better understanding of just what this faculty _is_. For these two reasons, then, I will now look at some of the different versions of NCTI.

Since there are so many versions of NCTI, I will try to be fairly brief in what I say about them. (At times, my remarks might seem _too_ sketchy, but this won't necessarily be my fault; most of the writings on NCTI from which I will be quoting are _themselves_ very sketchy.)

Perhaps the most popular way to explicate the faculty of no-contact intuition is in terms of _abstraction_. Steiner, for instance, thinks that intuitions of sets are generated by looking at a group of concrete bodies and abstracting away from their spatial relations. In other words, if we see three dots arranged in a triangle, we can abstract away from this particular arrangement of the dots and think of them as an unordered, unarranged set of three objects. (Actually, Steiner might balk at this way of putting it, for he thinks that we can only intuit the relations between various abstract objects. That is, he thinks that we can intuit the structure of ZF sets, but not particular sets.[19] I will say a bit more about this below.) Steiner writes that

> One imagines or looks at material bodies, and then diverts one's attention from their concrete spacial arrangement. One gathers up in one's mind the objects into a manifold, and then has an intuition of their structure. This is how one might become familiar with the standard model of ZF set theory -- by abstracting from dots on a blackboard arranged in a certain way. Thus one arrives at an intuition of the structure of ZF sets.[20]

It is clear that Steiner accepts a no-contact theory of intuition from his remark that "intuition, of course, is not considered to be initiated from without but self-induced."[21]

Resnik also refers to his conception of mathematical intuition[22] as an "abstraction process,"[23] and he also denies the existence of "contact between ourselves and mathematical objects."[24] For him, though, the abstraction process consists of several discrete stages. First, we perceive a group of physical objects; second, we perceive these objects as patterned; third, we recognize equivalence relations between different groups of patterned objects; and fourth, we introduce names for abstract patterns[25] and develop theories about them. Resnik summarizes this process as follows:

> We go through a series of stages during which we conceptualize our experience in successively more abstract terms. At the last stage we leave experience far enough behind that our theories are best construed as theories of abstract entities.[26]

Finally, Shapiro also models his account of intuition (at least partially) on the process of abstraction. He writes that

> One way to grasp, or understand, or refer to a structure is through a process of abstraction, or pattern recognition. One observes a system, or several systems with the same structure, and focuses attention on the relations among the objects. For example, one can come to understand a baseball defense by going to a game (or several games) and noticing the spatial relations among the players wearing gloves, ignoring things like height, hair color, and batting average.[27]

Shapiro allows for a second method of grasping structures: description. That is, one can simply be told that a baseball defense consists of three outfielders, four infielders, and a battery of two, arranged in such-and-such a way.

Parsons gives a conception of no-contact intuition which isn't founded on abstraction, at all. Rather, it is founded on ordinary perception and imagination.[28] He suggests that in perceiving concrete tokens, we often intuit abstract types. For instance, although we can only hear a token of a sentence, when we do so, we intuit the corresponding sentence type. Taking a mathematical case, Parsons claims that, in perceiving a token of a string of strokes, we intuit the type. This counts as a mathematical intuition, because one model for arithmetic is given by a particular sequence of strings of strokes, viz. |, ||, |||, etc. Now while perception of tokens will, according to Parsons, suffice to provide intuitions of singular propositions about types (such as that ||| is the successor of ||) it will not be sufficient to provide intuitions of general propositions about types (such as that any string of strokes has a successor). For this, Parsons thinks we need imagination. He says that "one has to imagine _an arbitrary string of strokes_"[29] and intuit that _it_ has a successor.[30]

Katz gives a different (and slightly more complicated) picture of no-contact intuition, which is based on Chomskyan nativism (i.e., the view that there are "psychological or biological constraints on how children learn a language"[31]). According to Katz, there are three innate conditions that allow for the construction of intuitions. They are

> competence-rules expressing information about the grammatical structure of a language, a knowledge of the relation 'x knows y', and knowledge of the characteristics of the category of abstract object."[32]

Katz thinks that these three components of the faculty of intuition allow it to construct representations of particular abstract objects. To obtain _general_ knowledge, he says we must construct scientific theories that "are _a priori_ systematizations of" the particular facts arrived at in intuition.[33]

The question I want to consider now is whether, _from an epistemological point of view_, any of these details are important. It seems to me that they are not. From an epistemological point of view, the only important question about no-contact intuitions is whether we should count them as instances of _knowledge_ (i.e., whether they can provide the basis of a platonist epistemology). Now, _prima facie_, there are only two reasons to think that we could make progress toward finding an answer to this epistemological question by providing an explication of the inner workings of the faculty of no-contact intuition. First, we might think that we need to describe how the faculty of intuition works in order to justify the claim that we do possess such a faculty. But as we saw in the last section, platonists do not need to establish that NCTI is true, because anti-platonists have to admit that it is. If the existence of a faculty of no-contact intuition was in question, then platonists would be wise to say how it could exist (i.e., how it might work); but, as we have seen, its existence is not in question.

Second, one might want to show -- by elaborating the inner workings of the faculty of no-contact intuition -- that this faculty operates in an epistemically praiseworthy way. But this would only be a good reason to provide such an explication of the faculty of no-contact intuition if the epistemological status (i.e., worth) of our no-contact intuitions depended upon the inner workings of the mental apparatus that produces them. It seems to me, however, that this is not the case. Once we have given up the Gödelian dream that intuitive mathematical data is acquired as the result of some sort of contact with mathematical objects, once we admit that the sole source of such data is the mind/brain (i.e., that our intuitions are produced wholly within our bodies) it is clear that the epistemological status (or well-foundedness) of this data will be the same, no matter how the mind/brain actually produces it. After all, since the mind/brain is constructing these intuitions by itself, without "checking its work against the mathematical facts," why should the epistemologist care how it does it? It seems that all routes to mathematical beliefs that don't involve any access to mathematical facts are equally epistemically praiseworthy -- or unpraiseworthy as the case may be. For all we care, the faculty of mathematical intuition could be a process of creative writing or dreaming up, instead of anything so respectable-sounding as abstraction. (We will see later on, in chapter six, why mathematical knowledge could be based on a faculty of intuition that consisted in a process of dreaming up.)

So it seems that neither of the (epistemological) reasons for explicating the faculty of intuition are good reasons; thus, platonists do not have to explicate it. The only thing that matters is that we can form beliefs about mathematical objects, and this, as we have seen, is entirely obvious.[34]

Now, I don't mean to suggest that the question of how we construct mathematical intuitions is of no interest, at all. If our aim is a psychological or neurophysiological one (i.e., to attain knowledge of the workings of the human mind/brain) then the question is of great interest.[35] I am only pointing out that, for our specific epistemological purposes, the answer to that question is irrelevant. Now, in defense of the philosophers I have been discussing, it is clear that at least some of them have had other motives in mind (in addition to the motive of trying to answer the

Benacerrafian challenge) when they set out to give an account of how the faculty of intuition operates. Katz, for example, was at pains to convince Chomskyan philosophers and linguists, who favor a conceptualistic interpretation of linguistic theory, and who are already quite convinced that there is such a thing as intuition, that they need not abandon their reliance upon intuition if they adopt a platonistic interpretation of linguistic theory. In other words, he wanted to show that the notion of intuition makes just as much sense within platonism as within conceptualism, and that, indeed, we need not alter our view of intuition very much at all, since, on his view of it, that faculty still involves mental constructions, rather than contact with abstract objects.

Now, as far as the other advocates of NCTI are concerned, they might have had other motives in mind, also. Or, perhaps, they merely meant to clarify the workings of intuition for the mere sake of increasing our understanding of it. But my point remains: if our purpose in advocating NCTI is to solve the epistemological problem with platonism, then we need not go out on any limb concerning the inner workings of the faculty of intuition. In fact, I think I can make a stronger claim: if this is our purpose, then we ought not to go out on such a limb, because if we do, and if, moreover, we are wrong, then our credibility will be needlessly brought into question. It is worth noting, too, that, in light of the fact that we are currently very ignorant about the workings of the brain, withholding judgement here is especially important. For it seems that anything we could possibly say right now about the workings of the faculty of no-contact intuition would be so speculative as to be virtually worthless, anyway. In other words, while it is virtually certain that some version of NCTI is true, any particular version that we could formulate right now is probably false. What we have here is a difficult question about the operation of the brain which cognitive scientists seem nowhere near ready to answer. Thus, to think that philosophers could answer the question via armchair speculation seems rather presumptuous.

In light of all of this, it should not come as any surprise that I do not favor any particular version of NCTI. From now on, when I speak of NCTI, I will be referring only to the general theory, and I will not worry at all about the actual workings of the faculty of no-contact intuition.

I have now given arguments to support the claim I made at the outset of this section that the adoption of NCTI constitutes not merely a strategy schema for responding to the epistemological argument against platonism, but a single strategy. The reason is that, even though NCTI is merely a theory schema, rather than a complete theory, all versions of it are on a par when it comes to the question of whether NCTI is relevant to the epistemological problem with platonism.

And I would now like to return to another piece of unfinished business. I said in section 1 that, from an historical perspective, the platonists who have rejected the inference from (1) and (2) to (3) have sought to motivate this move by developing an epistemology that begins with the adoption of NCTI. But I also said that there is a good reason for this, that NCTI is the platonist's last hope for an epistemology. I can now say why. The task is to show that we can have knowledge of abstract objects. Now we know that (1) and (2) are both true (from chapters three and four, respectively) so we know that we cannot come into contact with abstract objects. But we also know from the arguments of the last two sections that we are able to form beliefs and theories which we at least think are about abstract objects. Thus, platonists finally have something to work with. But if they cannot show that these beliefs about abstract objects are instances of knowledge of such objects, then they will be back to square one. And they won't be in a very happy position. For if we cannot come into contact with abstract objects, and if having beliefs of such objects cannot engender knowledge of such objects, then how could platonists proceed in constructing an epistemology? I don't have a knockdown argument for the claim that there would be no way to proceed, but I for one cannot imagine how they could proceed. In other words, I simply cannot imagine how knowledge of abstract objects would be possible, if it were established that (a) we cannot come into contact with such objects (and we've already established this) and (b) no-contact intuitions of such objects cannot lead to knowledge of them. It seems to me that the only hope for platonism, in this case, would be to show that there is something wrong with the Benacerrafian demand for a platonist epistemology, because the prospects of providing an epistemology would be dim at best.

4. A Prima Facie Reason to Doubt that No-Contact Intuitions are Instances of Knowledge

Having accepted NCTI, the hard work is still in front of us. We now have to show that our ability to construct no-contact intuitions of mathematical objects engenders knowledge of such objects. Now, we found in section 2 that there can be no doubt that human being do have the ability to construct such intuitions. In spite of this, however, there can be great doubt about whether this trivial fact is sufficient to provide a platonistic epistemology for mathematics.

On the face of it, there is reason to think that no-contact intuitions could not provide us with knowledge of mathematical objects, for there is reason to question the reliability of the psychological mechanisms responsible for the construction of intuitions. Given that these mechanisms have no access to the actual objects which they are supposedly constructing representations of, what reason is there to suppose that these representations will be at all accurate? I can examine my representation of the number 3 and decide that that number is prime, but how do I know that my representation accurately depicts the true nature of 3?

That is, what reason is there to suppose that the mechanisms responsible for the construction of this representation are reliable, given that they have no access to the number 3?

Field has put this objection in the following way:

> But special 'reliability relations' between the mathematical realm and the belief states of mathematicians seem altogether too much to swallow. It is rather as if someone claimed that his or her belief states about the daily happenings in a remote village in Nepal were nearly all disquotationally true, despite the absence of any mechanism to explain the correlation between those belief states and the happenings in the village.[36]

We can take Field's point to be this: while it might be trivially true that human beings are capable of believing things about an abstract mathematical realm, it is very hard to swallow the claim that almost everything human beings believe about the mathematical realm is true (even though they have no access to that realm). The reason that such reliability about the mathematical realm is so hard to swallow is that it would seem to entail that the human mind/brain is innately endowed with some sort of preestablished harmony with the mathematical realm.

Benacerraf also voices this objection. His main criticism of Gödel's view of intuition was that it failed to account for "the link between our cognitive faculties and the objects known."[37] Now, while this objection is raised in connection with Gödelian intuition, it seems to me that the objection carries directly over to NCTI. To say that we are able to construct no-contact intuitions of mathematical objects is to say something about our cognitive faculties.

But precisely because these are <u>no-contact</u> intuitions, we still lack an account of the "link between our cognitive faculties and the objects known." This is why I said, in the last section, that it is pointless to elaborate the inner workings of the faculty of intuition: to do so is simply to explicate a theory of our cognitive faculties, which is irrelevant to the problem at hand. What we need is precisely what Benacerraf demands, namely a <u>link</u> between the mental intuitions we construct and the objects that they are supposed to be representations of. Or, in Field's terminology, what we need is an account of the <u>reliability</u> of the faculty of intuition (i.e., of the mechanisms responsible for the construction of our representations of abstract objects).

So our <u>prima facie</u> reason to doubt that no-contact intuitions are instances of knowledge is just that they're <u>no-contact</u> intuitions. But this is only a <u>prima facie</u> objection. I do not mean to imply that platonists <u>cannot</u> use NCTI as the basis of their epistemology. My point is simply that <u>if</u> they can say nothing to rebut this <u>prima facie</u> objection, <u>then</u> they cannot use NCTI as the basis of their epistemology.

None of the platonistic advocates of NCTI have tried to rebut the above <u>prima facie</u> consideration by arguing that there <u>is</u> a link between our cognitive faculties and the mathematical realm. Instead, they have all tried to argue that such a link does not need to be given, after all. It is clear that this _must_ be their strategy, for if it wasn't, then they would be abandoning NCTI. The central claim of NCTI is that we arrive at our mathematical beliefs without the aid of any contact with the mathematical realm. But this is just another way of saying that there is no "link" between our cognitive faculties and mathematical objects. To argue that there _is_ such a link is just to argue that human beings can come into contact with the mathematical realm. But to argue this is to abandon NCTI and return to a rejection of either (1) or (2).

One advocate of NCTI, namely Parsons, doesn't attempt to rebut our _prima facie_ consideration, at all.[38] This is simply because he wasn't really doing epistemology; he was doing philosophy of mind. Now, to be sure, Parsons intends his version of NCTI to provide the basis of an account of mathematical knowledge; he just doesn't attempt to _argue_ that it can in fact provide such a basis. He is only interested in explicating the faculty of no-contact intuition. Now, it seems to me that this is a puzzling agenda for a platonist to follow, because the _prima facie_ consideration spelled out above _requires_ a rebuttal, and the faculty of no-contact intuition does _not_ require explication. Again, this is simply because we're _clearly_ capable of constructing beliefs about mathematical objects; the only important question is whether such beliefs can be trusted. Once we've abandoned the Gödel/Maddy programme of trying to establish the existence of some sort of contact between the human mind and the mathematical realm, the task is to show that our mathematical beliefs could, nonetheless, be reliable. It is epistemologically pointless to show that we can have mathematical beliefs, for that is not contestable. But, as I said, Parsons was not doing epistemology. He was simply trying to understand the faculty of intuition.

Other platonistic advocates of NCTI, however, have tried to rebut our prima facie consideration; that is, they have tried to argue that a link, or contact, between the faculty of intuition and the mathematical realm is unnecessary for the genesis of mathematical knowledge. What I would like to do now is consider three such arguments. The first is Steiner's and Quine's; I consider this in section 5. The second was advanced by Katz and Lewis; I consider this in section 6. And the third is mine, although one of the steps of the argument (as well as the underlying position from which the argument emanates) derives partially from Resnik and Shapiro; I consider this argument in chapter six.[39]

5. Steiner's (Quinean) Rebuttal

Steiner does not explicitly face our prima facie consideration, but it is quite clear what he would say to it. He thinks that a link between the faculty of intuition and the mathematical realm is unnecessary for mathematical knowledge, because, as it turns out, the beliefs that we come to via intuition can be justified after the fact by Quinean "holistic considerations and semantic ascent."[40] In other words, he thinks that intuition is responsible only for the genesis of our beliefs and that the justification of our beliefs arises from a consideration of the role that our mathematical theories play in our "web of belief." Thus, Steiner says that "the holist...can justify such [mathematical] knowledge ex post facto."[41] In other words, we can be confident that our mathematical theories are true, because they are indispensable to science, because we are unable "to say what we want about the world without...them."[42]

There is an initial plausibility to this view. To show that we can have mathematical knowledge, platonists must show that we can have true justified mathematical belief. The acceptance of NCTI gives us mathematical belief; the acceptance of platonism gives us true mathematical belief; and Steiner's claim is that Quinean holistic considerations give us justified true mathematical belief.

I think it is clear, however, that our mathematical beliefs are not justified in the way that Steiner claims that they are. If they were, it would suggest that the reliability of these beliefs is a mere coincidence. In other words, Steiner's view implies that, first, we construct our mathematical theories, and then, we notice that, lo and behold, they must be true, because they are useful in empirical science. But it seems to me that this can't be right: whatever account we give of mathematical knowledge, we have to be able to say that mathematicians possess such knowledge when they first construct their theories, i.e., before their theories are used for anything.

That platonists must account for this "early" mathematical knowledge[43] can be seen by noticing that if our mathematical theories could <u>only</u> be justified after the fact, then the process of constructing a mathematical theory would be a process of making a "stab in the dark." That is, mathematical theories would have to be constructed through a process of elimination. This is simply because working mathematicians wouldn't have any way of knowing whether the mathematical theories they were constructing were true; in order to have any justification for their theories, mathematicians would have to wait and see whether these theories turned out to be useful in empirical science. But if this were the case, it would be a miracle of Biblical proportions that our mathematical theories almost always turn out true. If Steiner's view was correct, we would expect most of our mathematical theories to be false. But they almost never <u>are</u> false.[44]

It is worth noting here, too, that, insofar as he is a platonist, Steiner could not simply deny that our mathematical theories almost always turn out true. Fictionalists, of course, think that our mathematical theories are false (and that it is the <u>conservativeness</u> of these theories that makes them useable). But Steiner is a platonist, and so he is committed to the truth or our mathematical theories. Moreover, as I've pointed out, he must also admit that mathematicians <u>know</u> that such theories are true when they first construct them. But if this is so, then platonists must explain how mathematicians attain such knowledge. And Steiner's appeal to holism does nothing to explain this. It only explains how mathematicians can attain after-the-fact mathematical knowledge.

This argument shows that Steiner's epistemology runs counter to mathematical practice, because it implies that mathematical knowledge only arises when we notice that our mathematical theories are useful to empirical science. But it can also be shown that this implication of Steiner's epistemology runs counter to the practice of empirical science as well. For when scientists use mathematical theories in constructing empirical theories, the last thing on their minds (or anyone else's) is that if the empirical theories in question turn out to be true, then we will have confirmation for the mathematical theories being used. In fact, the opposite is the case. When we use a mathematical theory in constructing an empirical theory, we assume that we already have knowledge of the mathematical theory. Indeed, this is precisely why empirical scientists feel entitled to use mathematical theories.[45]

The opposite side of this coin also suggests that Steiner's epistemology is flawed. If an empirical theory turns out false, we don't think that this throws doubt upon the mathematical part of that theory. On the contrary, we assume that it is the empirical part of the theory that is flawed. Again, this is simply because it is assumed that the we already have the mathematical knowledge in question. Now, there _are_ exceptions to this. It has been suggested, for instance, that the best way of handling certain difficulties with quantum theory would be to adopt an alternative logic, or an alternative probability theory. But this is quite rare. When an empirical theory is disconfirmed, suspicion is almost never transmitted to the mathematical part of the theory. So this argument provides us with another reason for thinking that a platonist epistemology must account for "early" (or pre-application) mathematical knowledge, namely, that empirical scientists assume that we _already_ know that mathematical theories are true when they use them. And, again, Steiner's appeal to holism doesn't account for this.

Steiner might think that he can respond to these criticisms by pointing out that he is _not_ making the simple-minded claim that the faculty of intuition provides us with mathematical belief and that these beliefs can then be justified via holistic considerations. On his view, the faculty of intuition does not start from a stand still; it does not generate beliefs about the mathematical realm from scratch. Modeling his view of intuition on Quine's view of perception, Steiner says that the faculty of intuition can only operate (and can only be trusted) within a theoretical framework. Now, it might seem that this gets the cart before the horse, that intuitions cannot require a theory in which to operate, because the construction of our _first_ mathematical theory required intuition. But while Steiner doesn't mention this, it seems that he could simply adopt some version of the view that intuition and theory grow up together.

But none of this constitutes a response to the criticisms I have leveled against Steiner's reliance upon ex post facto justification, because none of it explains why our "early" mathematical beliefs are already known to be true, before they are applied in empirical science. We can grant for the sake of argument that the faculty of mathematical intuition developed simultaneously with the ability to construct mathematical theories, and we can grant that neither of these can be trusted alone and that both are necessary to arrive at "good" mathematical theories; but none of this will provide an explanation of our "early" mathematical knowledge, because none of it is relevant to the question of justification. For this, Steiner still must appeal to holism. Thus, my point is that no matter how elaborate our method of theory construction is (and no matter how intricate its relation to mathematical intuition) if we cannot justify our theories until we see their usefulness in empirical science, then it will still just be a coincidence that this method of theory construction (whatever it is) is reliable.

There is another way of saying why Steiner's appeal to the partnership of intuition and theory construction cannot explain why mathematicians know that their theories are true when they first construct them. I argued in section 3 that it is irrelevant to try to describe the inner workings of the faculty of no-contact intuition, because to do so is to simply elaborate a theory of our cognitive faculties, when what is needed (in order to account for mathematical knowledge) is either an account of the link between our cognitive faculties and mathematical objects, or an argument that such a link is unnecessary. Now, by claiming that the faculty of intuition operates (and grows up) together with the ability to construct theories, Steiner has done nothing but provide further elaboration of our cognitive faculties. He has said nothing about the link between these faculties and mathematical objects, nor about why such a link is unnecessary for mathematical knowledge. Therefore, Steiner's appeal to the partnership of intuition and theory construction does nothing to account for any mathematical knowledge <u>at all</u>, let alone the "early knowledge" that, as we've seen, must be explained.

That a discussion of the relationship between intuition and theory construction is only a discussion of our cognitive faculties suggests that it is, for our epistemological purposes, pointless to even use two terms here. We could think of <u>the ability to construct mathematical theories</u> as a part of <u>the faculty of mathematical intuition</u>. That is, by "the faculty of intuition" we might simply refer to <u>all</u> of the psychological mechanisms that give rise to our (non-derivative) mathematical beliefs, <u>whatever these mechanisms are</u>. Now I admit that it will often be useful to distinguish between these two abilities -- i.e., the ability to construct particular intuitions, and the ability to construct general theories from these particular intuitions -- and so, it will be useful to have two different terms.[46] But the point I am making is that, from an epistemological point of view, the two abilities are on a par. Both are responsible for providing mental constructions, and both do this without the aid of any contact with the mathematical realm. Thus, for our epistemological purposes, we might as well use one term. We could use "intuition" or "theory construction" (or some other term, for that matter) to refer to all of the mental processes (whatever they are) which give rise to our full-blooded (non-derivative) mathematical beliefs. But if this is right, then, in so far as he is trying to rebut our prima facie consideration, Steiner might as well not say anything about the relationship between intuition and theory construction.

I conclude, then, that the appeal to holism is Steiner's only argument for the claim that human beings don't need a link with mathematical objects in order acquire knowledge of them. But we have already seen that this is not a good argument, for (a) it says nothing about why we don't need such a link in order to acquire early (i.e., pre-application) knowledge of mathematical objects, and (b) something must be said about this, because mathematicians do possess such early knowledge.

Before we leave our consideration of Steiner, it is worth noting that another issue that he devotes a great deal of attention to is irrelevant to our epistemological problem. He seems to think that one of the reasons for introducing the notion of mathematical intuition is to "reconcile the remoteness of mathematical objects with the firmness of mathematical knowledge."[47] The reason he thinks this is necessary is that, ordinarily, we are very uncertain of theories that treat of remote objects. Steiner goes on to argue that holism can account for mathematical certainty, because our mathematical theories are at the <u>core</u> of our "web of belief" and because "a single change in mathematical theory would result in wholesale chaos in scientific laws across the board."[48] Now I do not want to discuss whether intuition was ever introduced in order to account for certainty, or whether holism can do this job. The point I want to make is that certainty is irrelevant to the epistemological argument against platonism. Benacerraf and Field (and other anti-platonists) are not demanding an account of the <u>certainty</u> of our mathematical knowledge; they're demanding an account of the <u>existence</u> of such knowledge, i.e. of how we can know -- with <u>any</u> degree of certainty -- when the truth conditions of mathematical statements are satisfied. Now, of course, if the Benacerrafian challenge is met, then anti-platonists could mount a second attack, demanding that platonists account for mathematical <u>certainty</u>. In this case, platonists might want to appeal to the fact that mathematics is at the core of our belief system, or, preferably (in my opinion) to the fact that mathematical truths are necessary. But whatever we ultimately decide to say; I do not think that mathematical certainty would be very difficult to account for, given that we had already accounted for mathematical knowledge.

6. Katz's and Lewis's Rebuttal

I think that Katz has a much more effective way of arguing that the prima facie consideration of section 4 is misguided and that no-contact intuitions can provide us with knowledge in spite of the fact that they are constructed without the aid of any contact with the mathematical realm. Like Steiner, Katz also believes that holistic considerations can take some of the philosophical weight off of intuition. That is, if the task is to show that we have true, justified mathematical belief, part of the justification for these beliefs will come from holistic considerations. If our intuitions are systematized into a whole theory, then, according to Katz, we can justify this theory by showing that it satisfies all of the standard criteria that we use for evaluating theories.[49] Now, this isn't the only way that Katz tries to argue for the fact that a link with the mathematical realm is unnecessary for knowledge. But, even if it was, he would have a better account than Steiner, for he is suggesting that we justify our theories with mathematical criteria, not with the criteria of empirical usefulness.[50] It seems to me that this is better, because when the criteria for theory evaluation are moved to within mathematics, the feeling of coincidence discussed in the last section seems to evaporate, at least partially, because we no longer have to wait to see if our theories will be useful in science in order to evaluate them.

But, by itself, this appeal to holism still seems inadequate, because it does not solve the mystery of how our intuitions originally came to accurately depict the mathematical realm. Again, this is because holistic considerations are simply part of our cognitive faculties. We need to say something to eradicate the need for a link between these faculties and the mathematical realm.

Unlike Steiner, Katz does have something to say about this. His main argument for the claim that no-contact intuitions can provide us with knowledge, even without a link between them and the mathematical realm, is based not on holism, but on necessity. In the case of empirical knowledge, we do require a link between our cognitive faculties and the objects known, but this is only because a link (i.e., perception) is our only means to such knowledge. To know that a fire engine is red, we must look at it. This is because the fire engine could have been blue. It is only contingently red. The situation with respect to mathematical knowledge, however, is not the same. For instance, 4 is necessarily the sum of two primes; it could not have been otherwise. Therefore, to know that 4 is the sum of two primes, we don't need any link with it, for, as Katz says,

> if we construct a sufficiently articulated concept of

> the number four in intuition, we will be able to see that the concept is a concept of an object that is the sum of two primes.[51]

We can put this into terms that make it a direct response to Field. We do not trust beliefs about the daily happenings of a remote village in Nepal, unless the believer has some access to these happenings, because those happenings can be different in different possible worlds. But we can trust our mathematical beliefs without any access to the mathematical realm, because that realm is not different in different possible worlds. We simply don't need to look, in this case, in order to have knowledge.

Putting the discussion in terms of possible worlds brings us to the philosophy of David Lewis, who has also argued that the necessity of mathematical truths implies that we can attain mathematical knowledge without any sort of contact or link with the mathematical realm.[52] He says that in order for our beliefs about contingent matters to be of any epistemological worth, we must be, somehow, causally acquainted with that which we believe. This is because, if our empirical beliefs are to count as knowledge, they must depend counterfactually on the facts that they correspond to. But Lewis points out that such counterfactual dependence is not necessary in the mathematical case, because

> nothing can depend counterfactually on non-contingent matters. For instance nothing can depend counterfactually on what mathematical objects there are....Nothing sensible can be said about how our opinions would be different if there were no number seventeen[53].

Field has responded to this appeal to necessity. He thinks there are four reasons why it doesn't sufficiently establish that human beings can attain knowledge of mathematical entities without there being any link between their cognitive faculties and the objects known.[54]

First, Field points out that not all facts about the mathematical realm are necessary. For instance, "there are 9 planets" is only contingently true. It seems to me, however, that (for the present purposes, anyway) this is irrelevant. If NCTI-platonists can explain how we can attain knowledge of purely mathematical truths, then they will have solved their epistemological problem. That is, they will have explained how human beings can attain knowledge of the mathematical realm in spite of their lack of access to that realm. Another way of putting this is that in order to respond to the epistemological argument, platonists do <u>not</u> have to provide a <u>complete</u> epistemology. They only have to explain how human beings could attain <u>some</u> knowledge of the mathematical realm, for they only have to rebut the conclusion of the epistemological argument, which says that, if platonism were true, then human beings couldn't attain <u>any</u> such knowledge.[55] So while the question of how we acquire knowledge of mixed sentences (i.e., sentences which refer to mathematical objects but which aren't theorems of any mathematical theory) may be interesting, it is not a point of epistemological worry for platonists. (But in spite of the fact that I don't <u>need</u> to say anything here, in chapter six (section 5.2) I will offer a view of how knowledge of mixed sentences -- as well as knowledge of impure mathematical theories -- could arise without any contact with the mathematical realm.)

Field's second objection to the Katz/Lewis appeal to necessity is that, according to Field, it's not clear what the platonist _means_ when he says that mathematical truths are necessary. Field states the problem as follows:

> mathematical facts...are not logically necessary, nor do they...reduce to logically necessary truths by definition. (And Lewis clearly did not mean to depend in this passage on the logicist programme...) They are of course _mathematically_ necessary in the sense that they follow from basic laws of mathematics. Similarly, the existence of electrons is presumably _physically_ necessary, i.e., follows from basic physical laws. But Lewis does not think that the epistemological problem of explaining how our 'electron' beliefs can reliably indicate the existence of electrons is really a pseudo-problem, just because the existence of electrons is physically necessary; so why should the fact that the existence of numbers is mathematically necessary show that the corresponding epistemological problem about numbers is a pseudo-problem?[56]

Field follows this passage up by considering the possible platonist response that mathematical necessity (as opposed to physical necessity) is _absolute_, or _metaphysical_, necessity. Field claims that this response is unhelpful, because whether or not we decide to hang the tag "absolute" or "metaphysical" on mathematical necessity, platonists have to explain _why_ mathematical necessity is epistemologically relevant while physical necessity is not. It seems to me, however, that platonists _can_ provide such an explanation. The mathematical necessity of following from basic mathematical laws is absolute necessity (and, hence, eliminates the epistemological problem with numbers that Field refers to) because basic mathematical laws are themselves necessary. On the other hand, the physical necessity of following from the basic laws of physics is not absolute necessity, because the basic laws of physics are not themselves necessary. In other words, there are possible worlds with different laws of physics, but there are no possible worlds in which the axioms of mathematics are false. Therefore, mathematical truths are absolutely, or metaphysically, necessary, because they are true in all possible worlds.

It is clear that this is how Katz would respond to Field's second objection. In the passage quoted above, he says that once we've constructed our basic intuition of 4 (i.e., once we realize that 4 is the successor of 3) we can see that it must be the sum of two primes. But he doesn't merely believe that "3 + 1 = 4" follows necessarily from "4 is the successor of 3"; he also thinks that 4 is necessarily the successor of 3. Now, I think it is obvious what Field would say to this. He would admit that human beings could know that 4 is the sum of 1 and 3, given that they had some initial knowledge of what 4, 3, and 1 are, but he would point out that platonists must explain how human beings could get their initial, or basic, knowledge of these (and other mathematical) objects. In Fieldian terms, the response is that platonists must account for the reliability of our basic intuitions of 4, 3, and 1.

I do not want to respond to this demand just yet, for what I want to say will also be a response to Field's fourth objection. Thus, I will put objection number two on hold for a moment; I will return to it five paragraphs below.

Field's third point against the appeal to necessity is that, in some cases, it does seem to make sense to speak of counter-to-fact mathematical conditionals. He admits that nothing sensible can be said about how things would be different if there were no number 17, but he claims that

> even those who think that there is some sort of 'absolute necessity' to mathematics may find [it intelligible to speak of]...how things would be different if the axiom of choice were false.[57]

I think the easiest thing for platonists to do here is simply grant Field's point. The reason they can do this without giving up on the above appeal to necessity is that they can deny that necessity ought to be explicated in terms of counterfactuals. That is, they can claim that the reason we can attain knowledge of the mathematical realm without any access to that realm is that mathematical truths are necessary; but they can deny that this has anything at all to do with counter-to-fact mathematical conditionals. Thus, they can admit that such counterfactuals make sense.

I think this is how Katz actually would respond.[58] As far as Lewis is concerned, I think there may be a way for him to cling to his formulation of the view in terms of counterfactuals, although I will only just mention how he might do this. I think that Lewis might be able to simply <u>deny</u> Field's claim. That is, he might be able claim that counter-to-fact mathematical conditionals <u>never</u> make sense. Field says that "if the axiom of choice were false, the cardinals wouldn't be linearly ordered, the Banach-Tarski theorem would fail and so forth."[59] But one might claim that this is simply to speak of how things <u>actually are</u> in models in which the axiom of choice (C) is false. To speak of how things would be <u>different</u> if C were false, one would have to speak of how things would be different if it were false in a model in which it is actually true. But one might argue that nothing sensible could be said about this.

Now, in the face of such a response, Field might claim that to speak of how things would be different if C were false is to speak of how things would be different if it were false in the <u>actual</u> set-theoretic universe, i.e., of how things would be different if the actual set-theoretic universe were the universe of a model of ZF+not-C, rather than ZFC. I do not think this is a good response, for Lewis does <u>not</u> have to admit that the <u>entire</u> set-theoretic universe is the universe of a model of ZFC <u>or</u> of ZF+not-C. One might claim that ZFC universes and ZF+not-C universes exist "side-by-side," so to speak. But I do not want to defend this position any further, because, again, I don't think there is any need to couch the Katz/Lewis appeal to necessity in terms of counterfactuals.

Field's fourth objection to the Katz/Lewis appeal to necessity is really just a follow-up to my response to his third objection. He says that even if counter-to-fact mathematical conditionals don't make sense (i.e., even if we cannot demand that the platonist explain how our mathematical beliefs would be different if the mathematical realm was different) "there is still a problem of explaining the <u>actual</u> correlation between our" mathematical beliefs and the mathematical realm.[60]

We can actually narrow this problem a bit. We saw in our discussion of objection number two that platonists can easily account for how human beings attain "parasitic" mathematical knowledge. If we have some initial knowledge of 4, 3, and 1 -- e.g., that 4 is the successor of 3, and so forth -- then we can know that 3 + 1 = 4.[61] So the problem is not really to explain the correlation between our mathematical beliefs and the mathematical facts, but to explain this correlation in connection with our <u>initial</u>, or <u>nonparasitic</u>, mathematical beliefs. In Katzian terms, the problem is to explain how human beings could know how to construct their <u>bottom-level</u> representations of mathematical objects. Given that we <u>already have</u> representations of 4, 3, and 1, we can, of course, know that 3 + 1 = 4. The problem is to explain how we knew what to do when we constructed our representations of these numbers in the first place. (Recall that this was precisely the problem raised by objection number two; thus we need only one response to these two objections.)

At first glance, this problem might seem ominous. For it might seem that, since human beings have no access to the mathematical realm, they could no more know how to proceed in constructing their bottom-level representations of mathematical objects than could a sculptor know how to proceed in constructing a statue of somebody whom he had never seen or felt. But in spite of first appearances, our problem is easily solved. For the question of how we knew what to do in constructing our representation of 4 reduces to the question of how we knew that 4 is the successor of 3. But the latter question is confused. We were thinking of the successor of 3, and we simply hung the tag "4" on it. Thus, only someone who was confused could ask how mathematicians knew to characterize 4 as the successor of 3.

Another way of putting my response here is as follows. Anti-platonists cannot ask how we know what to do in constructing our bottom-level representations, because, in such cases, there is simply nothing to know, because these "bottom-level representations" are merely <u>definitions</u>. For instance, our bottom-level representation of 4 is that it is the successor of 3; our bottom-level representation of 3 is that it is the successor of 2, and so forth.

The obvious anti-platonist response here is to ask how we know that there is anything answering to the description "the successor of 3". In other words, how do we know, when we define a term (i.e., when we construct a bottom-level intuition) that there is any abstract object corresponding to the definition (or intuition) in question? That is, how do we know that there are any abstract objects at all? I do not want to answer this question here, for an exactly analogous question will arise in connection with my own rebuttal to the prima facie consideration of section 4. Thus, I put the answer off until chapter six (section 6), where I argue that platonists must be allowed to assume that mathematical objects exist when they are constructing their account of how human beings could attain knowledge of the nature of such objects.

It is worth pointing out, however, that Katz and Lewis must not only assume here that platonism is true, i.e., that mathematical objects exist; they must assume that all possible mathematical objects exist.[62] For to defend their epistemology, they have to claim that any definition (i.e., bottom-level representation) of a mathematical object that we can come up with is a representation of an actually existing mathematical object. For this is the only way to justify the claim that our knowledge that, for instance, 4 is the successor of 3 derives from the way in which we have defined our terms (i.e., constructed our bottom-level representations).

7. Conclusion

So the Katz/Lewis argument is that we can know that mathematical propositions are true, because they are necessarily true. This manifests itself in different ways for different sorts of sentences. We know that the theorems of our various mathematical theories are true, because they follow necessarily from the axioms and rules of these theories; we know that the _axioms_ of our theories are true, because they are parasitic on the definitions of the terms appearing in them; and we know that these _definitions_ accurately depict actual mathematical objects, because all possible mathematical objects exist.

I think that the Katz/Lewis explanation of why no-contact intuitions count as knowledge successfully rebuts the _prima facie_ consideration of section 4. Thus, I think that we have refuted the epistemological argument against platonism, because we have shown that (1) and (2) do not provide a good reason to believe (3). We have done this by simply explaining _how_ human beings can attain mathematical knowledge in spite of the fact that they have no access to the mathematical realm.

What I would like to do in the next chapter is provide _another_ refutation of the epistemological argument against platonism, one which is motivated by some remarks of Resnik.[63] It will proceed in the same way that the Katz/Lewis argument does, viz., by explaining _how_ human beings can attain mathematical knowledge in spite of the truth of (1) and (2). In other words, it will explain _why_ no-contact intuitions count as knowledge and, hence, why the _prima facie_ consideration of section 4 is misguided.

At first glance, the explanation I will provide might seem to be utterly unconnected with the Katz/Lewis explanation from necessity, but I think that the two explanations are deeply related. If this is true, then perhaps platonists ought not to pit these two epistemological explanations <u>against</u> each other; perhaps they can use them <u>together</u>. Much could be said about the connection between these two explanations, but all I will say at this point is that one of the main differences between the consistency-based explanation that I lay out in chapter six and the Katz/Lewis necessity-based explanation is that, in the former, I make <u>explicit</u> the assumption that all possible mathematical objects exist, and that I try to make a greater <u>use</u> of this assumption in motivating the claim that contact with mathematical objects is unnecessary for knowledge of such objects.

NOTES

1. See Steiner (1975), chapter four; Resnik (1982) and (1990); Shapiro (1989); Katz (1981), chapter VI; Parsons (1980); and Lewis (1986), section 2.4. Note that Lewis' remarks here are extremely sketchy and that he doesn't use the word "intuition". Moreover, with Resnik, the situation is even worse, for he explicitly distances himself from the term. But whether or not these philosophers would use the word "intuition," themselves, it is quite clear that they accept what I am calling NCTI here.

2. One might respond here that this is really what Gödel had in mind, and that any suggestion in Gödel's writings of contact with abstract objects was purely metaphorical. Thus, one might think that the distinction between Gödel's contact-based theory of intuition and NCTI is merely terminological. I do not think this is an accurate interpretation of Gödel, but I will not support this claim here. It is worth noting, however, that even if Gödel really did hold NCTI, it will not affect my arguments. Chapters three and four would still provide a reason for rejecting the literally interpreted contact-based theory of intuition; and we would still be in a position, at this point, to move on to a consideration of NCTI. The only thing I will have gotten wrong is my exegesis of Gödel.

3. Actually, advocates of NCTI could claim that introspective stimuli (as well as perceptual stimuli) can play a role in the production of mathematical intuitions. There is nothing objectionable about this, for introspection is just as "ordinary" (i.e., naturalistic or nominalistically kosher) as perception. On pp. 194-202 of his (1981), Katz makes a few remarks about the relationship between intuition and introspection. I will, in general, just speak of perceptual stimuli, but this is merely for the sake of convenience.

4. I suppose that most advocates of NCTI would make the Chomskyan claim that the mind/brain is innately structured in this way. So far as I know, Katz is the only one who actually says this; see pp. 4-8 and 204-5 of his (1981). I think that Katz is right here, although I do not think that I have to justify this claim, because I do not think that anything relevant to the present discussion turns on this issue. I am not certain whether other advocates of NCTI also agree with Katz.

5. Kant (1781), p. A713, B741.

6. The two sorts of intuition -- propositional beliefs about abstract objects and internal representations of abstract objects -- correspond to Steiner's (1975, chapter four, section V) distinction between intuition-that and intuition-of. I will say more about this below.

7. Note that this does <u>not</u> mean that all intuitive propositional knowledge is <u>categorical</u> knowledge. Just because an intuition is about concepts, it doesn't follow that it isn't about particulars. For instance, an intuition that 3 is prime could be said to be about the concept three and the concept primeness.

8. Hale abandons the Gödelian view for the same reason that I do, viz., because it is incompatible with the acausal nature of abstract objects. See Hale (1987), section 4.I.

9. Hale (1987), p. 124.

10. The practice of referring to certain kinds of intuitions -- viz., <u>intuitions-of</u>, which I will discuss below -- as "representations" of mathematical objects derives from Katz (1981, p. 203). It is worth noting that this way of talking does <u>not</u> commit us to a representational theory of mind. If it is determined that there are no such things as mental representations, then I could reformulate this description. I could say something like this: an intuitions-of is a thought (or idea) about what some mathematical object <u>is</u>.

11. One anti-platonist -- viz., Wittgenstein -- deserves special mention here, for I think he would deny that we have mathematical beliefs, at least if beliefs are taken to be propositional attitudes. But in spite of this, Wittgenstein would still accept NCTI. He would just formulate it in a somewhat peculiar way. He would say that from ordinary perceptual stimuli, human beings are capable of formulating and living by certain rules which could be called "mathematical".

12. One might object that while anti-platonists admit that we have mathematical beliefs <u>and</u> that we receive only perceptual stimuli, they don't have to admit that our mathematical beliefs are <u>caused by</u> perceptual stimuli. But a moment's reflection reveals that anyone who accepts the above conjunction has almost no alternative to accepting the causal claim; for if perceptual stimuli are our <u>only</u> inputs, then there is nothing else that could cause our mathematical beliefs. Moreover, the causal claim seems <u>correct</u>: we come to our mathematical beliefs by, for instance, <u>seeing</u> proofs in books and <u>hearing</u> teachers explain things.

13. Parsons (1980, p. 146) accuses Gödel of committing a _non sequitur_ in inferring the existence of intuition-of from that of intuition-that. Steiner (1975), p. 131 claims that no one (with the possible exception of Gödel) would countenance intuition-of. Obviously, Steiner thinks that intuition-of goes hand-in-hand with a contact-based theory of intuition. I believe that this is mistaken.

14. We can say the same thing in response to Steiner's claim that we possess _neither_ intuition-of _nor_ intuition-that. He comes to this conclusion via the claim that we cannot intuit _particular_ sets, but only relations _between_ sets. Thus, he thinks that our intuitive knowledge is of _structures_, rather than of objects _or_ of truths. But again, it seems arbitrary to say that I can have an intuition of the relationship between two objects, but that I can't have an intuition of the objects themselves, or of truths about the objects. Moreover, one might argue that a structure is just a special kind of object; if this is right, then an intuition of a structure just _is_ an intuition-of; and a propositional intuition about a structure is an intuition-that. (It is worth noting that, in his (forthcoming), Parsons also argues that intuition is of structure, rather than of objects.)

15. The reason that I couch this trivial theory in terms of "mathematical intuition," the existence of which is ordinarily thought to be anything _but_ trivial, is merely to be true to the usage of mathematicians and philosophers. We routinely say that we have intuitions about mathematical questions. I want to preserve this terminology without claiming that intuitions arise from any mysterious faculty of mind.

16. Shapiro (1989), p. 165.

17. Shapiro's view is that we can acquire beliefs of infinite structures by merely understanding the language of the theory of the structure. But, again, I think it is entirely unnecessary to provide an account of how we can attain mathematical beliefs, because it is obvious that we can. Note that I am interpreting Shapiro charitably here by saying that he thinks we can attain _beliefs_ about infinite structures via language understanding. He says that we can "grasp" structures in this way; if he means that we can attain _knowledge_ of them in this way, then he is merely adopting the traditional view that mathematics is analytic -- i.e. that we attain knowledge of mathematical propositions by merely understanding what they _mean_. I don't think Shapiro wants to accept this position, but if he does, he will have to argue for it.

18. It is worth noting here that if what I've argued is correct, then accepting structuralism is of no epistemological help. Thus, structuralists like Resnik and Shapiro have falsely advertized. I suppose the reason they think that structuralism is epistemologically helpful is that pattern recognition is not philosophically occult. But, since mathematical belief isn't philosophically occult, either, structuralism isn't needed. However it is that we come to our mathematical beliefs, the fact is that we can come to them. Thus, for the simple reason that NCTI is a trivial theory, it follows that structuralism isn't epistemologically helpful. (Now in claiming that NCTI is trivially true, I've assumed that we can't have Gödelian contact with mathematical objects; but if this turns out to be false, then structuralism won't be needed anyway, because we'll have contact.)

19. Steiner seems to have two reasons for making this claim. First, mathematicians speak of intuition in a way which suggests this structural view of intuition. Second, the only thing of value to know about mathematical objects is what relations they bear to other such objects. (See Steiner (1975), p. 134.) I am dubious of both of these claims.

20. Steiner (1975), p. 134-135.

21. Steiner (1975), p. 133.

22. Note that Resnik does not like the term "intuition," because he does not think that we possess a special faculty of intuition. As I have defined intuition and NCTI, however, there is no commitment to such a special faculty. The only claim is that human beings have the ability to proceed from ordinary perceptual inputs to beliefs about mathematical objects. Since Resnik would clearly agree to this, I will speak of him as advancing a theory of "intuition," even though he wouldn't use this term.

23. Resnik (1982), p. 99.

24. Resnik (1990), p. 42.

25. An abstract pattern, on this view, would just be the pattern common to every group of patterned objects in an equivalence class, where equivalence classes are defined by the equivalence relations of the third stage.

26. Resnik (1982), p. 99.

27. Shapiro (1989), p. 146.

28. Parsons (1980), especially sections 5 and 6.

29. Parsons (1980), p. 156.

30. Recall that, in chapter four, after arguing that Maddy could not claim that we can perceive abstract objects with the senses, I asked whether she could retreat and claim that intuition of abstract objects is grounded in perception. I said that Maddy wouldn't accept this view and that I would consider it in chapter five. For whatever it's worth, I am now considering it, for this view is just Parsons' view.

31. Katz (1981), p. 5.

32. Katz (1981), p. 212.

33. Katz (1981), p. 212.

34. A corollary of this is that it is entirely irrelevant whether the faculty of no-contact intuition is a "special faculty." Resnik and Parsons deny that it is. Now, it's not entirely clear just *what* they are even denying here -- although I think they're denying that intuitions are produced by a faculty which is responsible *only* for producing mathematical intuitions -- but, regardless, it doesn't matter (for our purposes) whether intuition is a special faculty, or not, for it doesn't matter (for our purposes) how intuition works.

35. The question might also be important for mathematical reasons. If we knew how our mathematical intuitions were constructed, we might be able to improve our intuitive abilities, and thereby answer some open questions.

36. Field (1989), p. 26-27.

37. Benacerraf (1973), p.674.

38. Parsons gives his conception of mathematical intuition in his (1980). But in his (forthcoming), he *does* broach the subject of our *prima facie* consideration. He says (pp. 1-2) that "the step from the givenness of numbers to knowledge of their existence is not quite so trivial as it appears." But for some reason Parsons doesn't think that this is a problem that platonists have to take seriously. He immediately writes that "intuition has to have the character that at least in typical cases, it follows that" intuition engenders knowledge. I suppose that one might take this as an indication that Parsons accepts a contact-based theory of intuition after all. This would account for the fact that he simply *ignores* the question of whether our intuitions are trustworthy. But I do not think this interpretation of Parsons is defensible. Everything else he says suggests that he doesn't believe in contact between human beings and mathematical objects. For instance, on p. 148 of his (1980),

he writes that "It would be implausible to suppose that in mathematical intuition there is a causal action of a mathematical object on us..."

39. There is a fourth platonist argument that might also be considered in this connection, namely that of Crispin Wright (1983, section xi). I will not consider this argument, however, because I think it can be quickly and easily dispensed with. Wright's argument is that we can set statements involving abstract objects equal to statements not involving them and, thereby, attain knowledge of the former. For instance, since the direction of line a = the direction of line b if an only if a is parallel to b, and since we can know whether a is parallel to b (because it is, according to Wright, about concrete objects, viz., lines), we can, therefore, attain knowledge of the directions of a and b, which are abstract objects. The problem with Wright's view is as follows. Even if we grant that the right-hand side of the above biconditional is nominalistically kosher, and even if we grant that it's true just when the left-hand side is true, the platonist is committed to the claim that we cannot, in general, give such biconditionals. For if we could always give nominalistic equivalencies of sentences which are about abstract objects, then the way would be open to accept a reductionist version of nominalism, and (by Ockham's razor) we would be obliged to do so. That is, if we could always give such nominalistic equivalencies, we would have a way to paraphrase away our commitment to abstract objects.

40. Steiner (1975), p. 124.

41. Steiner (1975), p. 130.

42. Steiner (1975), p. 122-3.

43. By "early mathematical knowledge" I do not mean to refer to any particular theory. I mean rather to refer to the knowledge that mathematicians have of a theory in its infancy, before it is applied.

44. It seems to me that the only mathematical theories that are false are those that are inconsistent. I am not simply ignoring the case of alternative geometries here. It seems to me that, in so far as we take two incompatible geometries to be mathematical theories, there is nothing wrong with taking both to be true, albeit of different spaces. Now, I grant that only one geometry could be a true theory of the physical space of our world, but when we take geometries to be theories of actual physical space, they cease to be purely mathematical theories. I will say much more about this in chapter six, in connection with my own rebuttal of the prima facie consideration of section 4. (Note, however, that even if we took certain geometries to be false, it would still be

the case that <u>most</u> of our mathematical theories are true.)

45. These considerations suggest another way in which Steiner's epistemology runs counter to the way we think about mathematics. If mathematical theories were confirmed by their usefulness in empirical science, then mathematics would be (at least partially) empirical. Now, I don't want to take the space to <u>argue</u> that mathematics is <u>not</u> empirical (primarily because most people take the non-empirical nature of mathematics to be quite obvious) but I do not think it would be very difficult to provide such an argument.

46. It will, of course, also be convenient to be able to distinguish various <u>sorts</u> of mathematical beliefs. We want to be able to say, for instance, that many or our mathematical beliefs (e.g., those which we only accept because they have been proven) are <u>unintuitive</u>.

47. Steiner (1975), p. 130.

48. Steiner (1975), p. 130.

49. For a discussion of these issues, see Katz (1981), pp. 14-16 and 212-214.

50. It is worth noting that this is similar to Maddy's claim that the axioms of ZF can be justified "extrinsically," by considering their fruitfulness in answering open mathematical questions, their entailment of previous conjectures, etc. See Maddy (1990), chapter four.

51. Katz (1981), p. 207.

52. See Lewis (1986), section 2.4. Note, however, that Lewis has only a peripheral interest in mathematical knowledge. His main concern is to show that the modal realist can attain knowledge about possible worlds without any contact with these worlds.

53. Lewis (1986), p. 111.

54. Field (1989), p. 233-242.

55. I am not familiar with Lewis' view in this connection, but from private conversations with Katz, I have found that his position is essentially the one that I have suggested here.

56. Field (1989), p. 235.

57. Field (1989), p. 237.

58. He has conveyed this to me in private conversation.

59. Field (1989), p. 237-238.

60. Field (1989), p. 238.

61. All I mean, when I call my belief that 3 + 1 = 4 "parasitic," is that it is dependent upon <u>other</u> beliefs, viz., the beliefs about what 4, 3, and 1 <u>are</u>. I avoid calling the belief that 3 + 1 = 4 "derivative," because that word is ordinarily reserved for beliefs which arise via <u>proof</u>. Thus, I call this belief parasitic only because I <u>also</u> want to call it obvious, intuitive, etc.

62. I do not mean to suggest here that there are things which are possible mathematical objects; all I mean is that the actual mathematical objects exhaust all the possibilities.

63. Resnik (1982), p. 101.

Chapter Six

A New Platonist Epistemology

1. Forward

In chapters two, three, and four, I constructed an argument for the claim that platonists have to find fault with the inference from

(1) Human beings exist entirely within space and time

and

(2) If there are (abstract) mathematical objects, they exist outside of space and time

to

(3) If mathematical objects exist, then human beings could not attain knowledge of them.

But in chapter five, we saw that platonists can disallow this inference on the grounds that mathematical truths are <u>necessarily</u> true.

In this chapter, I would like to offer <u>another</u> argument against the inference from (1) and (2) to (3), one that is partially inspired by Michael Resnik. In other words, I will provide another rebuttal of the <u>prima facie</u> consideration of chapter five, section 4. It is my hope that the argument in this chapter will be more convincing than the Katz/Lewis argument from necessity which I sketched in the last chapter and that the epistemological explanation I provide here will eliminate any lingering feeling of mystery surrounding our knowledge of mathematical objects. But I do not mean to repudiate the Katz/Lewis argument. I think that that argument can be used together with the one I will provide here.

2. The Fundamental Intuition

The fundamental intuition behind my refutation of the epistemological argument is quite simple. If platonists adopt a full-blooded platonism, according to which any mathematical object which could exist actually does exist (or, as I will often put it, according to which all possible mathematical objects exist[1]) then there can be no problem about attaining knowledge of such objects. On this view, all we have to do in order to attain mathematical knowledge is conceptualize, or think about, or even "dream up," a mathematical object. Whatever we come up with, we will have formed an accurate representation of some mathematical object, because, according to platonists, all possible mathematical objects exist.

We can put this in terms of Field's example of the Nepalese village. To recall, he formulates the example as follows:

> But special 'reliability relations' between the mathematical realm and the belief states of mathematicians seem altogether too much to swallow. It is rather as if someone claimed that his or her belief states about the daily happenings in a remote village in Nepal were nearly all disquotationally true, despite the absence of any mechanism to explain the correlation between those belief states and the happenings in the village.[2]

Now I admit that I could not have knowledge of the happenings of a Nepalese village without any access to that village.

However, if all possible Nepalese villages existed, then I _could_ have knowledge of the happenings of these villages, even without any access to them. To attain such knowledge, I would merely have to dream up a possible Nepalese village. Since (by assumption) all possible Nepalese villages exist, it would follow that the village that I have imagined exists. Moreover, since my belief states about this village would correspond to the facts about this village (and since, if I am aware of the assumption that all Nepalese villages exist, I could _know_ that my belief states correspond to these facts) it also follows that I have _knowledge_ about the village. Now, of course, all possible Nepalese villages don't exist, and so we cannot attain knowledge of them in this way. Platonists, however, are quite free to claim that all possible mathematical objects exist. Therefore, they _can_ claim that we can attain mathematical knowledge in this way.[3]

3. Skeleton of the Refutation of the Epistemological Argument

Despite the simplicity of the fundamental position here, the argument I will use to show that this position provides an adequate refutation of the epistemological argument against platonism (i.e., an adequate rebuttal of the _prima facie_ consideration of chapter five, section 4) is quite long and complicated. Thus, to make the discussion as clear as possible, I will first present a skeleton of the argument. It has three steps.

Step One: argue that if human beings can attain knowledge of mathematical consistency, then platonists can adequately account for <u>how</u> human beings can -- without the aid of any contact with the mathematical realm -- attain such knowledge. I do this in section 4. I admit that, if we're sufficiently strict about what is to count as knowledge, then, perhaps, <u>nobody</u> (platonists nor anti-platonists) has knowledge of mathematical consistency; but I argue that anti-platonists cannot use this in their epistemological argument against platonism.

<u>Step Two</u>: argue that if platonism is true, then any consistent, purely mathematical theory is an accurate description of part of the mathematical realm (and that human beings can know this). I do this in section 5. It is here that my argument relies partially upon an argument of Resnik's.

So the argument to this point proceeds as follows. Human beings can believe things about the mathematical realm (because NCTI is true). Moreover, they can know (with an acceptable degree of certainty) that these beliefs are consistent (in spite of the fact that they do not have any access to the mathematical realm). But if platonism is true, then any consistent mathematical theory is an accurate description of part of the mathematical realm (and human beings can know this). Therefore, if platonism is true, then human beings can attain mathematical knowledge (in spite of the fact that they do not have any access to the mathematical realm).

<u>Step Three</u>: argue that anti-platonists cannot disallow

platonists from assuming that platonism is true (in so far as platonists are merely responding to the Benacerrafian argument -- i.e., merely trying to construct an epistemology). I do this in section 6.

4. Step One: Consistency

In this section, I will argue that if human beings can attain knowledge of mathematical consistency (i.e., knowledge of the syntactic consistency of their various mathematical theories and beliefs[4]) then platonists can adequately account for how human beings can -- without the aid of any contact with the mathematical realm -- attain such knowledge.

That this is precisely what needs to be established (i.e., that platonists ought not to argue that we can have knowledge of mathematical consistency) is clear. The epistemological challenge facing platonists is to explain how it could be that the mathematical knowledge that we do have is knowledge of an inaccessible mathematical realm. Thus, since the epistemological argument assumes that we have mathematical knowledge (this was statement (5) of the argument, as I laid it out in chapter two) it will not do platonists any good to argue this point; it is not what is at issue. What platonists need to do in order to satisfy anti-platonists is assume that we have mathematical knowledge and explain how this knowledge could be knowledge of an abstract mathematical realm.

The reason I bring this up is to make clear from the start that one cannot object to my argument on the grounds that, if we are sufficiently strict about what is to count as knowledge, then it might be that we simply don't know whether our mathematical theories are consistent. Such an objection would simply not be to the point, for if we don't have such knowledge, then platonists don't need to account for it. Now, platonists might try to use this to their advantage by arguing that human beings don't really have any mathematical knowledge at all. But, of course, this would be a hopeless project, for, regardless of whether we have knowledge of, say, Zermelo-Fraenkel set theory (ZF), we do know that 2 + 2 = 4. Thus, platonists are not going to get off the hook here: there are cases of mathematical knowledge which need to be accounted for. Moreover, they are going to have to account for knowledge of mathematical consistency as well. For, again, regardless of whether we can know that our theories are consistent, we do know that, for instance, "4 is even" and "4 is positive" are consistent.

So the fact that we might not have knowledge of the consistency of our mathematical theories is simply irrelevant to what I will be arguing in this section. Now, it seems to me that the best thing to say here is that we can have knowledge of the consistency of our mathematical theories. We might not be able to attain certainty here, but it seems to me that we do have very good reasons to think that most of our theories are consistent.[5] Thus, I will assume that we do know (with at least an acceptable degree of certainty) that our mathematical theories are consistent, and so I will assume that platonists need to account for such knowledge. This assumption, however, is quite irrelevant to my argument.

In any event, I now turn to the task at hand. I will give five reasons for thinking that anti-platonists must admit that platonists can adequately account for how human beings can -- without the aid of any access to the mathematical realm -- attain knowledge of mathematical consistency.

4.1 The First Reason

The first reason is that anti-platonists must <u>also</u> account for knowledge of mathematical consistency (because, they, too, have to account for mathematical knowledge -- just like platonists do -- and, in order to do so, they will have to account for our knowledge of mathematical consistency).

Anti-platonists might respond in two ways here. The first response, which I will put on the back burner for a moment, is that anti-platonists <u>don't</u> have to account for knowledge of mathematical consistency, because they needn't admit that we <u>have</u> such knowledge. The other response, which I will consider first, proceeds as follows. Who cares if anti-platonists have to account for knowledge of mathematical consistency? They have to account for mathematical knowledge in general, but this doesn't mean that platonists are off the hook. The claim is that anti-platonists <u>can</u> account for mathematical knowledge and platonists <u>can't</u>.

The problem with this response is that, if we are only trying to account for knowledge of mathematical <u>consistency</u>, then platonists (or, rather, NCTI-platonists) can give any account that anti-platonists can. That is, if anti-platonists can account for knowledge of consistency, then NCTI-platonists can too, because they can simply borrow the anti-platonist account. Now, of course, this isn't generally true; platonists can't always use anti-platonist accounts of mathematical knowledge, because the two groups of philosophers have different conceptions of what mathematical knowledge is; (platonists think that such knowledge is about mathematical objects, and anti-platonists don't). But this situation changes in the special case of our knowledge of mathematical consistency, because there is no difference in opinion, among platonists and anti-platonists, about what such consistency is. Both groups of philosophers think that a collection of mathematical sentences is (syntactically) consistent if it is impossible to derive a contradiction from it. Moreover, NCTI-platonists and anti-platonists have the same conception of how we come to believe that a certain set of sentences is consistent; that is, both accept NCTI, both think that such knowledge arises naturalistically. Thus, there is simply no account that anti-platonists could give of our knowledge of mathematical consistency that platonists couldn't simply use for their own.

I now come to the second response to my first reason, namely the response that anti-platonists don't have to account for knowledge of mathematical consistency, because they don't have to admit that we have such knowledge.[6] Anti-platonists might mean two things here, but neither of them poses a threat to my argument. First, they might mean to deny that we have knowledge of the consistency of complex mathematical theories, but admit that we have knowledge of the consistency of sentences like "4 is even" and "4 is positive." But if this is what is meant, then the response is simply irrelevant. For if anti-platonists admit that we know that "4 is even" is consistent, then they have to account for this knowledge; but, again, whatever account they give will be available to platonists. Second, anti-platonists might mean to claim that human beings don't have any knowledge of mathematical consistency. But if they take this line, they will have to deny that we have any mathematical knowledge at all; for if we can't know that "4 is even" is consistent, then what can we know? But if anti-platonists deny the existence of mathematical knowledge, then they cannot use the epistemological argument against platonism, because that argument depends upon the existence of such knowledge, i.e., upon statement (5). In other words, if anti-platonists disbelieve in mathematical knowledge, they cannot demand that platonists account for such knowledge.

Now, I suppose that anti-platonist could try to block this response by claiming that, while there is, in fact, no mathematical knowledge, platonists are committed to such knowledge. That is they could abandon (5) in favor of

(5′) According to platonists, human beings have mathematical knowledge.

But I don't see how (5') could be motivated, for it seems to me that if anti-platonists could account for mathematical practice and still deny that there is mathematical knowledge, then platonists could too. The only reason platonists are committed to the existence of mathematical knowledge is that there obviously _is_ such knowledge. But if we suddenly became aware of a reason to doubt the existence of mathematical knowledge, then platonists could jettison their commitment to such knowledge as easily as anti-platonists could, for there is no _other_ reason -- aside from the reason that we clearly _do_ have mathematical knowledge -- for thinking that platonists are committed to the existence of such knowledge.

I suppose that, in order to be really thorough, I would have to say a bit more by way of justification for some of the claims of the preceding paragraph; but I will not do that, because I think this entire discussion is a farce. That is, I don't think _anyone_ can account for mathematical practice, while denying that we have mathematical knowledge. It just seems to fly directly in the face of the facts to deny that we know that "4 is even" and "4 is positive" are consistent.[7] Moreover, in point of actual fact, I do not think that there are many anti-platonists (if there are any at all[8]) who would deny that we have knowledge of mathematical consistency or that they have to account for such knowledge. Field, for one, has admitted his responsibility here. His view is that mathematical knowledge is really logical knowledge, and that this is of two sorts.

The first is knowledge that (the axioms of) our various theories are consistent; and the second is knowledge that various theorems follow from various axiom systems.[9] The really interesting thing about Field's epistemology is that it is virtually _identical_ to the one which I am suggesting that platonists should adopt. I think that our mathematical knowledge consists of the same two parts that Field does: we know (with an acceptable degree of certainty) that our various axiom systems are consistent (I will explain _how_ we know this in the same way anti-platonists do); and we know that we can prove certain theorems from these axioms. The only difference is that I believe that these axioms are true and Field thinks that they are false. (I will say more about this difference, and the epistemological repercussions of it, in section 5. In particular, I will explain _how_ we can know -- without the aid of any contact with the mathematical realm -- that our axioms accurately describe this realm.)

4.2 The Second Reason

My second reason for thinking that anti-platonists must admit that platonists can adequately account for knowledge of mathematical consistency is that, in general -- whether we are speaking of mathematical or physical objects -- we do not need any access to a set of objects in order to know whether a set of sentences about them is consistent. The reason this is so important is that the whole force of the epistemological argument against platonism derives from the fact that, at least _prima facie_, it appears that -- in general -- one _does_ need contact with a set of objects in order to attain knowledge about them; thus, the lack of contact with mathematical objects was a source of alarm. But if we are only concerned with knowledge of _consistency_, there is no need to be alarmed, because knowledge of the consistency of a set of sentences does _not_ depend upon having access to the objects that the sentences are about, and so platonists will have no more difficulty than anti-platonists in accounting for our knowledge of mathematical consistency.

I can support my claim that knowledge of consistency does not depend upon access by merely considering examples. I do not need access to the seventh child born in 1991 in order to know that the sentences asserting it to be female and Italian are consistent with each other; likewise, I don't need access to this child to know that the sentences asserting it to be female and male are _in_consistent. The same is true of mathematical sentences. I do not need any access to the number 4 in order to know that "4 is even" and "4 is positive" are consistent, or that "4 is even" and "4 is odd" are inconsistent.

Now, one might claim that the mathematical case really is different from the physical case after all. That is, one might claim that if we are dealing with a complex mathematical theory, then we will need some sort of access to the objects of the theory in order to attain knowledge of its consistency. The reason one might think this is that it will often be the case that the sentence which asserts a mathematical theory to be consistent is equivalent to an arithmetical sentence. Thus, knowledge of mathematical consistency is often just knowledge of arithmetic truth. But since we presumably need access to mathematical objects in order to attain arithmetical knowledge, it would seem that we would also need access to such objects in order to know whether certain of our theories are consistent.

The problem with this objection is that, on my view, we don't need access to mathematical objects in order to attain arithmetical knowledge. Now, of course, I have yet to explain how we can attain arithmetical knowledge without access to the mathematical realm, but, by the end of this paper, I will have done that. But even if I didn't have this response available to me, the above objection would be irrelevant; for my claim is not that human beings will always be able to determine whether some theory is consistent. Such a claim would be obviously false -- whether we are speaking of mathematical or empirical theories, and whether human beings have access to the mathematical realm or not -- because there are theories which are so long and complicated that no human being could even understand them. My claim is simply that access to the mathematical realm is not necessary for knowledge of mathematical consistency.[10] And this is shown by considering the above "easy" examples about 4.

It should be clear that this weak conclusion is all that I need to establish. The epistemological argument against platonism is designed to show that if platonism were true, then human beings could not get started, mathematically. Thus, if I can show that we can (without the aid of any access to the mathematical realm) attain some knowledge of that realm, then I will have refuted the epistemological argument. Further mathematical knowledge will be attainable via proof (and, perhaps, via inductive methods and holistic considerations).

Now one might respond here that the initial knowledge that needs to be accounted for is knowledge of the axioms of our theories. But I just deny this. If platonists can account for how human beings can attain a pool of elementary, non-derivative knowledge of the mathematical realm, then (a) the appeal of the epistemological argument will have entirely evaporated, and (b) knowledge of the axioms of our various theories (if not part of this pool of knowledge) can be justified in some other way, such as by holistic considerations.[11]

I have argued that access to the mathematical realm is not necessary in order to attain knowledge of mathematical consistency. But it is worth noting that it's not sufficient, either: if we could come into contact with mathematical objects, this wouldn't necessarily help us attain knowledge of consistency. Again, this follows from the general truth that contact with any group of objects (physical or mathematical) is unhelpful in determining whether a given set of sentences about those objects is consistent. Here, the example of complex theories serves.

We could be uncertain of whether some complex empirical theory was consistent, even if we had access to the objects of that theory, because, again, the theory might be so complicated that we couldn't even understand it.

Now, given these two facts -- i.e., that we can have knowledge of consistency without contact and contact without knowledge of consistency -- we can draw the general conclusion that the question of whether or not we can come into contact with mathematical objects is entirely irrelevant to the question of whether we can attain knowledge of the consistency of our mathematical theories. And note that the situation here is, again, parallel to the situation with physical objects and empirical theories. This has been pointed out by Tarski. He writes that our knowledge of consistency

> cannot by influenced in any way by empirical knowledge, and in particular by knowledge of the objects to which the sentences...[in question] refer.[12]

Again, the reason all of this is important is that the epistemological argument against platonism depends heavily upon there being a _deep_ connection between knowledge of the mathematical realm and access to that realm. Thus, since there is no such connection in the case of knowledge of mathematical _consistency_, the epistemological argument cannot establish that if platonism were true, then human beings couldn't attain such knowledge.

4.3 Three More Brief Reasons

The third reason why anti-platonists must admit that platonists can adequately account for knowledge of mathematical consistency is closely related to the second, and so I will say very little about it. The reason is that in order to <u>formulate</u> a system of consistent beliefs about a realm of mathematical entities, I do not need access to those entities. Again, this follows from the corresponding general truth: to formulate a consistent system of beliefs about <u>any</u> set of entities, I do not need any access to them. I can formulate the (consistent) beliefs that the smallest village in Nepal is primitive and east of the largest village in Nepal, even if I have never had any contact with either village. Likewise, I can formulate consistent beliefs about numbers, even though I've never had any contact with them.[13]

The fourth reason is related to the first <u>and</u> the second. While the first reason was that anti-platonists will have just as hard a time accounting for our knowledge of the consistency of our mathematical theories as platonists will, my fourth reason is that scientific realists will have just as hard a time accounting for our knowledge of the consistency of our <u>empirical</u> theories. This is because -- as we saw in connection with the second reason -- the perceptual access that we have to physical objects is of no help when it comes to determining whether our beliefs about such objects are consistent. It therefore seems that if we cannot account for our knowledge of the consistency of our mathematical theories, then we cannot account for our knowledge of the consistency of our empirical theories, either. But this suggests that if the former provides a reason to be anti-realists with respect to mathematics, then the latter provides a reason to be anti-realists with respect to empirical science. But anti-platonists do not, in general, want their epistemological argument to work equally well against all forms of realism; for much of the force of their epistemological argument derives from the appearance that we will have no problem meeting their epistemological demand with respect to our empirical theories.

Finally, my fifth reason why anti-platonists must admit that platonists can adequately account for knowledge of mathematical consistency is that to deny that they could account for such knowledge would be a dead-end strategy. The epistemological argument against platonism reasons that since we could never know whether our mathematical theories are true (in the naive sense) we oughtn't to think that they are true (in the naive sense). Thus, it would seem that the point of arguing that we cannot know whether our theories are consistent would be to conclude that we oughtn't to believe that our theories are consistent. But surely anti-platonists don't want to jettison all of mathematics in order to get rid of platonism. Fictionalists, of course, want to jettison mathematical truth in order to get rid of platonism, but they still preserve a place for mathematics by arguing that it is conservative with respect to all empirical theories (and that all true empirical theories can be nominalized).

5. Step Two: Satisfiability

At least one anti-platonist -- viz., Field -- seems willing to admit that platonists can account for our knowledge of mathematical consistency. But he doesn't think this does much to solve the epistemological problem with platonism, because he thinks there is "a big gap" between knowledge of mathematical consistency and knowledge of actual mathematical objects.[14] I will argue that this isn't the case. In this section, I will argue that

(P) If platonism is true, then any consistent (purely[15]) mathematical theory accurately describes some part of the mathematical realm (i.e., some collection of actual mathematical objects).[16]

And in section 6, I will argue that the assumption that platonism is true is -- in this context -- legitimate. Thus, since (as I argued in section 4) platonists can account for how human beings can attain knowledge of mathematical consistency, and since (as I will argue in this section) human beings can easily recognize the truth of (P), it follows that platonists can account for how human beings can acquire knowledge of the nature of the mathematical realm.

Now, as I've formulated my strategy, I have to do two things in this section. First, I have to argue for (P) and, second, I have to argue that human beings could recognize the truth of (P). But, of course, both of these tasks can be accomplished at once by simply giving an argument for (P) which human beings can follow.

In order to see that (P) is true, one must understand that, by "platonism," I mean the full-blooded sort of platonism referred to above (in section 2). Thus, my claim is that if all possible mathematical objects exist, then all consistent (purely) mathematical theories accurately describe some collection of mathematical objects.[17] Moreover, it seems to me that if one appreciates that this is what I mean by "platonism," then there can be no question about the truth of (P). If all possible mathematical objects exist, then in order to attain knowledge of some collection of these objects, all I have to do is dream up some consistent (i.e., possible) set of purely mathematical sentences. For if all possible mathematical objects exist, then <u>any</u> consistent set of purely mathematical sentences will accurately describe <u>some</u> set of mathematical objects, i.e., <u>some</u> part of the mathematical realm.[18]

Resnik uses a similar argument (in connection with his structuralist view that mathematics is a science of patterns). He states the argument very quickly, however, and seems to underestimate its importance. This is probably because (a) he thinks his version of NCTI is highly speculative, and (b) he doesn't think he has sufficient arguments for the consistency of our mathematical theories. But, as I have shown, both of these problems are irrelevant. In chapter five (sections 2 and 3) I argued that anti-platonists cannot object to NCTI and that platonists need not go out on any limb concerning the inner workings of the faculty of no-contact intuition; and, in section 4 of this chapter, I argued that it is simply irrelevant whether our mathematical theories are consistent. The real problem with Resnik's argument is neither (a) nor (b); it's that he only performs step two of the argument, i.e., that he doesn't perform step one or step three. In connection with step one, he tries to argue that our mathematical theories are consistent (and that we can know this);[19] what he should argue is that if we have knowledge of mathematical consistency, then platonists can adequately account for this knowledge. In connection with the step three, Resnik just doesn't acknowledge (much less justify) his assumption that platonism is true.

But in any event, it is clear that Resnik wants to defend the same general position that I do. He writes that

> a pure [mathematical] theory can [only] be falsified by showing that it fails to characterize any pattern at all, that is, that it is inconsistent.[20]

Resnik contrasts this with applied theories, which, because they contain empirical hypotheses, can be falsified. (An applied theory, according to Resnik, is a pure theory together with an empirical hypothesis to the effect that the pure theory in question is a theory of some part of the physical world.[21]) Thus, for instance, an applied grammar[22] can be falsified by showing that no population actually speaks the language characterized by the grammar; likewise, an applied geometry can be falsified by showing that physical space is not characterized by the geometry. But -- so long as it's consistent -- a pure grammar cannot be falsified at all; for, regardless of whether it characterizes any natural language, it <u>does</u> characterize <u>some</u> collection of abstract objects (or, as Resnik would say, <u>some</u> pattern) which could correctly be called a language.[23] And the same goes for pure geometries: so long as they're consistent, they cannot be falsified.

Although Resnik never explicitly says it, it is clear that, in making these claims, he is assuming the truth of a full-blooded platonism, i.e., a platonism which countenances all possible mathematical objects (or patterns). For in order to believe that any consistent grammar characterizes a language, we have to believe that <u>all possible languages exist</u>. (And, of course, the same holds for consistent geometries and all possible geometric spaces.) Thus, it seems that Resnik is defending the same claim that I am, namely, that <u>if platonism is true</u>, then mathematical consistency is sufficient for accurate mathematical depiction. In other words, he is defending (P).

I have so far supported (P) with only informal, philosophical argument, but, for first-order theories, it has been mathematically proven. As part of his proof of the Completeness Theorem, Gödel proved what has been called the Fundamental Theorem of Quantification (FTQ),[24] which says that any consistent first-order theory has a (denumerable) model. But if all possible mathematical objects (and, hence, all possible mathematical models[25]) actually exist, then to say that some consistent, first-order, purely mathematical theory T has a model is just to say that there actually exists a set M of mathematical objects which satisfy T, i.e., which are <u>accurately described</u> by T (under an appropriate interpretation).[26] Thus, any proof of FTQ is a proof that, so far as first-order theories are concerned, (P) is true.

One might object to this argument for (P) on the grounds that the proof of FTQ relies upon platonistic assumptions. Now, I admit that it would be unacceptable to rely in my argument for (P) upon assumptions about the <u>nature</u> of <u>particular</u> mathematical objects. (The reason it would be unacceptable is that human beings are supposed to be able to come to know that (P) is true so that they can attain knowledge of the mathematical realm; thus, since (P) is needed to arrive at mathematical knowledge, it cannot be that one needs mathematical knowledge in order to gain knowledge of (P).) But it is <u>not</u> unacceptable to rely in my argument for (P) upon the assumption that there <u>are</u> mathematical objects. (This is simply because (P) is a <u>conditional</u>: it only says that <u>if platonism is true</u>, then...) But the only platonistic assumption needed for the proof of FTQ is that there <u>are</u> models, i.e., that platonism is true.[27] Thus, there is nothing wrong with using the proof of FTQ to justify the claim that (P) holds for first-order theories.

Now, it might be claimed that -- precisely because I have formulated (P) as a conditional -- I have only given an argument for the claim that <u>if platonism is true</u>, then human beings could attain knowledge of the mathematical realm. But I have already indicated that I will respond to this objection in section 6; I will argue there that, in the present context, there is nothing wrong with assuming that platonism is true. That is, I will argue that, in explaining how human beings can attain knowledge of mathematical objects, platonists are justified in assuming that there are mathematical objects.

But one might object that even if platonists can assume that platonism is true when they are constructing an epistemology of mathematical objects, they cannot make this assumption a necessary condition on the acquisition of mathematical knowledge. The intuition behind this objection is (I suppose) that mathematicians don't make any such assumption in order to arrive at mathematical knowledge. There are two ways to respond to this objection. The first is to claim that it doesn't matter whether mathematicians actually assume that platonism is true, and the second is to claim that, in fact, mathematicians do assume that platonism is true, albeit subconsciously.

The first response proceeds as follows. In order to refute the epistemological argument against their view of mathematics, platonists only have to specify a route by which human beings could come to acquire knowledge of the mathematical realm; that is, they only have to show that such knowledge isn't impossible for spatio-temporal creatures like ourselves.[28] Thus, it simply doesn't matter (in the present context) whether the epistemology that platonists adopt is an accurate description of what goes on in the heads of mathematicians; thus, it doesn't matter, in my particular case, whether mathematicians assume that platonism is true. I think this response can be made quite plausible by reflecting for a moment upon mathematical practice. For it's not only the case that most mathematicians don't assume that platonism is true; it's also the case that most of them don't think about such matters at all. What mathematicians do, when they acquire knowledge, is acquire familiarity with mathematical theories; most mathematicians never stop to ask themselves what these theories are true _of_. Thus, insofar as (a) platonists think that mathematical knowledge is knowledge about the nature of an abstract realm and (b) most mathematicians don't even cognize that they are, or might be, thinking about an abstract realm, one cannot expect platonists to worry about what goes on in the heads of mathematicians. In fact, if platonists _did_ simply give a theory of the mental processes of mathematicians, this theory would _not_ be useful in rebutting the epistemological argument; for such a theory wouldn't even broach the topic of abstract objects. To respond to the Benacerrafian challenge, platonists must discuss the relationship between our mathematical theories and the mathematical realm; thus, they must go beyond the mental processes of most mathematicians.

It is worth noting, also, that what I have said thus far explains _why_ mathematicians can attain knowledge of the mathematical realm by _merely_ acquiring theoretical familiarity, i.e., without ever cognizing that their work has anything to do with an abstract mathematical realm. The explanation is just this: on my view, all theoretical familiarity (or, rather, all familiarity with consistent mathematical theories) is familiarity with the mathematical realm.[29]

The second way of responding to the above objection is to simply deny the claim that mathematicians don't assume that platonism is true. One could claim that, while they might never cognize it, most mathematicians do assume that their singular terms refer to objective entities. Moreover, this line of response seems quite plausible, for an exactly analogous situation seems to obtain in empirical science and ordinary experience. There seems to be nothing wrong with saying that a biologist can know that the rat in her hand is white, and that a stock broker can know that the pencil in his hand is yellow, even though neither of them has ever considered the question of whether there is an external world, and neither could justify the subconscious assumption that there is an external world. Likewise, there is nothing wrong with saying that a mathematician could know that the number he is thinking of is prime without ever having considered the question of whether numbers exist.

Now, to be sure, some mathematicians do think about platonism, and, indeed, some disbelieve it; but this sort of person should be no more problematic to the platonist than a solipsist/physicist is to the scientific realist. Although the solipsist/physicist and the anti-platonist/mathematician both consciously disbelieve in the objects that their theories are taken by realists to be about, the realist can still claim that there is a sense in which such disbelievers have knowledge, for they have theoretical familiarity.

5.1 Mathematical Truth

In section 5.2, I will consider various objections to (P), but before I do this, I want to comment on how (P) effects our view of mathematical truth. If we combine (P) with a correspondence theory of truth, we arrive at

(P') If platonism is true, then any consistent, purely mathematical theory is true.

If we adopt this claim, then consistency (or, in other words, satisfiability or truth in a some particular model M) will, among first-order, purely mathematical theories, be sufficient for truth. Resnik and Shapiro have hinted that they endorse this striking conclusion. The former says that "A pure theory can [only] be falsified by showing that it fails to characterize any pattern at all".[30] The latter says that

> It is conceivable (barely) that the language of arithmetic may not be satisfiable and thus no structure is characterized....But it is nonsense to claim that the language of arithmetic does refer to a...structure...but says hardly anything true about it.[31]

Moreover, Field seems willing to grant that platonists can make this move. He writes that

> If only the acceptance of purely mathematical sentences is at issue, then any interpretation under which a mathematical theory is true meets all the conditions for being an intended interpretation; so that any consistent mathematical theory comes out true on an interpretation intended by its advocates.[32]

The first thing I want to point out here is that, for the present purposes, I don't have to take a stand on whether (P′) is true. The epistemological argument against platonism is supposed to show that human beings couldn't attain knowledge of the nature of mathematical objects; but as I have shown, (P) is sufficient to rebut this. If full-blooded platonism is true, then as soon as we have knowledge of the satisfiability of a purely mathematical sentence, we have knowledge of a sentence that accurately describes some part of the mathematical realm, and so we have knowledge of the nature of the mathematical realm. Thus, I need not worry about truth at all.

But the question of whether (P′) is true is interesting enough to merit a short digression. The reason that one might want to resist (P′) is that, if we accept it, then there will be cases in which a sentence and its negation are both true. For instance, since there are models in which the axiom of choice (C) is true (viz., models of ZFC) and models in which not-C is true (viz., models of ZF+not-C)[33] it seems that, if we accept (P′), then we will be forced to say that C and not-C are both true. But this seems unacceptable, because it implies that truth is not closed under conjunction, because "C and not-C" is not satisfiable and, hence, false.

I think that there are two ways in which an advocate of (P′) might try to deal with this problem. The first is to embrace the conclusion that truth is not closed under conjunction. This might sound crazy, but I think it can be made at least somewhat plausible by pointing out that the notion of _true in M_ (where M is a precisely specified model) _is_ closed under conjunction, because a sentence and its negation can never both be true in the _same_ model. Thus, the person who holds that the ordinary notion of truth isn't closed under conjunction would argue that it is the notion of _true in M_ (as opposed to the notion of _true_) which is mathematically important. And I think this is quite plausible on _independent_ grounds; for when mathematicians say that a sentence is "true," what they mean is that it is _true in the standard model_. But, of course, the notion of _true in the standard model_ is just a special case of the notion of _true in M_. A second reason for thinking that it is the notion of _true in M_ (as opposed to that of _true_) which is mathematically important is that it is the former notion that Tarski precisely defined. (It is also worth pointing out that it need not be any part of this view that the notion of truth is _totally_ uninteresting; for, even if it isn't _mathematically_ interesting, it could be _philosophically_ interesting.)

The second way in which an advocate of (P') might try to handle the above problem is to _resist_ the conclusion that truth isn't closed under conjunction. It is difficult to see how this could be made to work, however, for since C and not-C are both satisfiable, platonists who accept (P') must admit that both are true. Thus, if they want truth to be closed under conjunction, they must admit that the sentence "C and not-C" is true, even though it is <u>not</u> satisfiable (i.e., not true in <u>any</u> model). In other words, they must admit that, while truth in some model M is a sufficient condition for the truth of a purely mathematical sentence, it is not a necessary condition.[34] Now, despite the bizarre sound of this, I think that this view <u>might</u> be defensible. I am far from sure that, in the end, it can be made to work, but here is how one might <u>try</u> to make it work. The general strategy is to claim that, since ZFC and ZF+not-C are true in <u>different</u> models, C (as it appears in ZFC) and not-C (as it appears in ZF+not-C) are talking about <u>different</u> things (i.e., different parts of the mathematical realm) and, hence, do not contradict each other. This might be expressed by saying that C (and ZFC) is about the universe of <u>sets</u>, whereas not-C (and ZF+not-C) is about the universe of <u>*sets</u>.[35] Thus, the argument continues, there is nothing wrong with admitting that "C and not-C" is true, for, when we make this claim, we interpret "C" in two different ways and, hence, do not contradict ourselves. So, on this view, "C and not-C" is exactly analogous to "Aristotle married Jackie Kennedy and Aristotle didn't marry Jackie Kennedy." As long as we use the two tokens of "Aristotle" to refer to different people, this sentence can be true, because, insofar as the two conjuncts are talking about different things, they do not contradict each other.

Now, I do not want to pretend that this view is problem-free. First of all, since there will be sentences which are true, but which are not true in <u>any</u> model (because they are not satisfiable), the advocate of (P′) will still have to claim that it is the notion of <u>true in M</u> (as opposed to that of <u>true</u>) which is mathematically important. As I've already indicated, however, I do not think this is a disastrous result. But the ability to say that truth is closed under conjunction does not come without cost; in purchasing it, advocates of (P′) saddle themselves with numerous other problems about which it is not clear what ought to be said. I will not discuss these problems here, though, because I am not committed to this view.

Now, one might wonder why the platonist would even bother with all of this. That is, since (P) is sufficient to solve the epistemological problem, why would the platonist go to so much effort to motivate (P′)? The answer to this question is that there are good reasons to <u>accept</u> (P′). One reason is that, if we deny it, then we will have to abandon the correspondence theory of truth. For if (P′) is false, then there will be sentences which accurately describe parts of the world (in particular, they will describe "non-standard" models) but which are <u>not</u> "true."

A second (and more important) reason to accept (P′) is that, if we don't, then the distinction between true and satisfiable theories might be entirely arbitrary. As I've just indicated, if (P′) is false, then there will be theories which accurately describe parts of the world, but which are, nonetheless, false. But it seems that there could be no principled way of saying <u>which</u> of our satisfiable theories are false and which are true. If ZFC and ZF+not-C both describe a part of the mathematical realm, what reason could we have for calling one true and one false? What reason could we have for singling out certain parts of the mathematical realm as <u>special</u>, i.e., for saying that theories that describe some models (viz., "standard" models) are true, while those that describe others are false? It seems that the fact that some parts of the world are considered "standard" is merely an accident of human history; this should not have anything to do with which of our theories are true and which are false.[36]

Now, a platonist who was opposed to (P′) might respond as follows. C is true of the set-theoretic universe and not-C is false, even though it is satisfiable, because, while ZF+not-C has models, none of these models correspond to the actual set-theoretic universe. That is, the set-theoretic universe is the universe of a model of ZFC and not of ZF+not-C.

But, in the present context, this move to non-full-blooded platonism is illegitimate; for recall that this whole discussion is prefaced on the assumption that all possible mathematical objects exist, i.e., on the assumption of full-blooded platonism. Now, one might wonder why a platonist would accept full-blooded platonism at all, rather than a form of platonism which takes the <u>entire</u> set-theoretic universe to be the universe of a model of ZFC. The reason is that the only kind of platonism that doesn't fall prey to the epistemological argument is full-blooded platonism. If the above non-full-blooded sort of platonism were true, it would be a mystery how we could know what the mathematical realm was like, i.e., how we could know that it had one nature rather than another, e.g., how we could know that the set-theoretic universe was the universe of a model of ZFC rather than of ZF+not-C. Thus, since we clearly do have mathematical knowledge, non-full-blooded platonism is false.

It's also not clear that non-full-blooded platonism solves the problem of arbitrariness that I introduced it to solve. For, insofar as one must admit that models of ZF+not-C exist, it doesn't seem that non-full-blooded platonists have any non-arbitrary way of saying that the "actual" set-theoretic universe is the universe of a model of ZFC. It seems that this is just another way of saying that models of ZFC are "standard".

So platonists cannot motivate a rejection of (P′) by abandoning full-blooded platonism. But perhaps the two problems with the rejection of (P′) -- i.e., the problems I raised four and five paragraphs back -- can be solved in the same way in which I indicated that the problems with the acceptance of (P′) might be solved; that is, these problems might be solved by pointing out that it is the notion of true in M (as opposed to that of true) which is mathematically important. If this is right, then perhaps the best thing for platonists to say about (P′) is that it simply doesn't matter how we decide to use "true" in purely mathematical contexts. But I do not want to discuss this issue any further, for, again, I need not take a stand on (P′). On my view, ZFC is an accurate description of sets and ZF+not-C is an accurate description of *sets.[37] Thus, if I can account for our knowledge of this, I will have accounted for knowledge of the mathematical realm. Thus, I needn't bother with (P′); in order to refute the epistemological argument against platonism, it will be sufficient to accept (P).

5.2 Objections

What I want to do now is consider various objections to (P). The first is that if we argue for (P) via FTQ, then we will only have reason to believe that (P) is true for first-order theories (because FTQ only holds for first-order theories, because only first order theories are complete). But if this is true, then we will be left without an account of how we attain knowledge of higher-order theories.

First of all, I didn't just argue for (P) via FTQ; I also gave a philosophical argument for (P). Second, even if FTQ is the only good argument for (P), it doesn't matter, because even if (P) only holds for first-order theories, it will still suffice to refute the epistemological argument against platonism. As I indicated above, the epistemological argument will be rebutted if platonists can explain how human beings can attain <u>some</u> mathematical knowledge. Now, to be sure, platonists will not have a complete epistemology if they don't have an account of our knowledge of higher-order theories. But to answer the Benacerrafian challenge, one need only explain how it is possible for human beings to attain <u>any</u> knowledge of mathematical objects; thus, an account of our knowledge of first-order mathematical theories is sufficient for the task at hand.

A second objection to (P) might arise in connection with the notion of <u>categoricity</u>. If, in addition to being consistent, a theory is categorical, then all of its models are isomorphic to one another. It seems to me -- as opposed to Shapiro and Resnik -- that categoricity is simply irrelevant for our purposes. If a mathematical theory is consistent but not categorical, it will have a plurality of (non-isomorphic) models. But it will still give us knowledge of the mathematical realm, because it will still accurately describe part of that realm. It won't describe a <u>unique</u> part, but that's simply irrelevant. (This had better be the case, because it's arguable that <u>none</u> of our mathematical theories correspond to a unique part of the mathematical realm, including categorical ones. This is one way to interpret the difficulty raised by Benacerraf's 1965 paper, "What Numbers Could Not Be." Even if a theory is categorical, it can have a plurality of models. All of these models will be isomorphic to each other, but they will still be different models.)

I will state the third objection to (P) as an objection to (P'). This is merely to make the discussion more lucid;

the same objection could be raised against (P). The objection is that I have left unexplained our knowledge of impure mathematical theories.[38] Field has shown that (P') cannot be extended to cover impure mathematical theories (i.e, that impure theories can be consistent but false) by showing that such theories can imply falsehoods about the physical world. To this end, he considers a theory M, which is generated in the following way:

> start with Zermelo-Fraenkel set theory, modified so as to allow for sets with non-sets as members and so as to include the assertion that there is a set of all non-sets; and interpret the separation and replacement schemata in such a way that the empirical vocabulary that is ordinarily used to describe non-sets is allowed to appear in the instances of the schematas....Now, to obtain M, drop the axiom of infinity and replace it by its negation.[39]

My response to this objection is the same as my response to the objection about higher-order theories. To respond to the epistemological argument, platonists need only explain how human beings can attain some knowledge of mathematical objects. Thus, it is sufficient to explain how we attain knowledge of pure, first-order mathematical theories. Now, again, a complete platonist epistemology would include an explanation of how we could attain knowledge of impure theories, given knowledge of pure theories, but this task need not be accomplished in order to answer the Benacerrafian challenge.

But in spite of the fact that I don't need to say how we could attain knowledge of impure mathematical theories, I do think that there is a natural line for me to take here.[40]

Anyone who accepts (P') could also accept

(P'') If platonism is true, then any consistent, mathematical theory is true, so long as it doesn't imply any falsehoods about the physical world.

Now by adding this clause, I clearly do not affect what I have said about pure mathematical theories, because such theories don't imply anything about the physical world.[41] But in the case of impure theories, this modification of our principle gives us precisely what we need. Human beings can attain knowledge of an impure theory by knowing that it is consistent and by knowing that it doesn't imply any falsehoods about the physical world. Now to attain such knowledge, we would, of course, need to know something about the physical world, but this is to be expected. In order to know that some impure theory is true, one does need knowledge of the physical world.[42]

6. Step Three: On the Assumption that Platonism is True

We have, thus far, erected the following argument. Platonists can account for how human beings can -- without coming into contact with mathematical objects -- attain knowledge of mathematical consistency. But if platonism is true, then any consistent mathematical theory or set of beliefs accurately describes some collection of mathematical objects. Thus, since human beings can easily recognize this fact, platonists can (on the assumption that platonism is true) account for how human beings can attain mathematical knowledge. That is, platonists can provide an epistemology of mathematical objects.

The epistemology platonists will provide proceeds as follows. Human beings can believe things about the mathematical realm (because NCTI is true). Moreover, without coming into contact with the mathematical realm, they can know (with an acceptable degree of certainty) that these beliefs are consistent (because their lack of access to the mathematical realm is simply irrelevant to their ability to attain knowledge of the consistency of their mathematical beliefs). Finally, provided that platonism is true, human beings can -- again, without any access to the mathematical realm -- know that all of their consistent mathematical beliefs accurately describe the mathematical realm. Hence, they can know things about the mathematical realm.

But anti-platonists might respond to all of this by asking how platonists can simply <u>assume</u> that platonism is true, when this is the very thing that is at issue. It must be admitted that I have said nothing to justify the assumption of the existence of mathematical objects, and that it is possible that there are no such things. This, however, does <u>not</u> constitute a telling response to the epistemology that I have provided. For, so long as I am merely trying to respond to the epistemological argument against platonism, anti-platonists must allow me to assume that platonism is true. There are two arguments for this claim.

6.1 The First Argument

In chapter one, I distinguished two tasks that platonists have to perform: task (I) was the task of providing positive arguments for the existence of a mathematical realm, and task (II) was that of rebutting the arguments against the existence of a mathematical realm. Now, after long analysis, it appears that the epistemological argument against platonism has collapsed into a demand that platonists satisfy task (I). Anti-platonists have to admit that -- if we assume that there is a mathematical realm -- then, even without any access to this realm, we can know that our mathematical theories accurately describe parts of it. Thus, the anti-platonist's only hope of salvaging the epistemological argument is to demand that platonists justify their assumption that there are mathematical objects. But to demand that platonists justify this assumption just is to demand that they perform task (I). But platonists already acknowledge that they have to perform task (I), and so the epistemological argument is doing nothing. Therefore, if the epistemological argument is the only good argument against platonism, then task (II) has been accomplished.

It is perhaps worth noting here that platonists do have arguments that they can provide for the existence of mathematical objects. Two examples are Frege's argument from the existence of true sentences with genuinely referential mathematical singular terms and the Quine/Putnam argument from the indispensability of mathematics to empirical science.

6.2 The Second Argument

The second reason why anti-platonists cannot demand that platonists justify their assumption that abstract objects exist -- when platonists are responding to the epistemological argument -- is that this argument *itself* assumes that they exist. The epistemological argument against platonism is a *reductio ad absurdum*: it proceeds by arguing that *if platonism were true*, then mathematical knowledge would be impossible. (This was statement (4) of the argument, as I laid it out in chapter two.) Thus, the challenge to platonists is to show that *if platonism is true*, then mathematical knowledge is *not* impossible. But this is exactly what I've done.

The illegitimacy of preventing platonists from assuming that platonism is true can be seen by considering a mathematical proof by contradiction. Imagine Jones assuming p, deriving a contradiction, and concluding not-p. And imagine Smith assuming p, showing that Jones' derivation of a contradiction is flawed, and concluding that p might still be correct. Obviously, in this case, Jones cannot claim that Smith's assumption of p is illegitimate, since p is what is at issue.

It is worth noting that my formulation of the epistemological argument is not the only one that is a *reductio*. Field and Benacerraf also state it in this way. They are both very careful to state that there is only a problem with mathematical knowledge *if the platonist is right about the nature of mathematical objects*.[43] Now, it seems to me that the argument *must* be stated in this way, for, otherwise, anti-platonists would be forced to claim that mathematical knowledge is impossible whether we accept platonism or not. But, as we saw above, anti-platonists do not want to deny the existence of mathematical knowledge.[44]

Finally, it is worth noting that this isn't just a point about the epistemological argument against platonism. Rather, it is a general point about epistemology. In trying to explain how human beings can attain knowledge that p, one needn't argue that p is true.

6.3 A Possible Response to the Two Arguments

Anti-platonists might respond to these two arguments by claiming that, even if platonists can assume that mathematical objects exist, they must explain how human beings could know that they exist. This response is suggested by a passage from Field, although I doubt that he was thinking of precisely this issue. He writes:

> if there are mathematical entities of the sort that the platonist believes in...then there seems to be a difficulty in seeing how we could ever know that they exist, or know anything about them.[45]

If this is right, then we should distinguish two different epistemological demands on platonists, viz.,

(ED1) The epistemological demand that platonists explain how -- if mathematical objects exist -- human beings could acquire knowledge of the nature (or properties) of such objects.

and

(ED2) The epistemological demand that platonists explain how -- if mathematical objects exist -- human beings could acquire knowledge of the existence of such objects.

Now, I have argued at length in this chapter that (ED1) can be satisfied. Thus, the only thing that needs to be considered is (ED2). The first thing to notice here is that, as of right now, anti-platonists do not have an argument for the claim that platonists must satisfy (ED2). The epistemological argument only establishes that platonists must satisfy (ED1), because it only establishes that platonists must account for mathematical knowledge. But the knowledge that there are mathematical objects -- i.e., the knowledge that platonism is true -- could hardly be counted as mathematical knowledge. This is simply because the claim that platonism is true is not a mathematical claim.

Now, I suppose that one might question this, but it is not worth arguing, for it is clear that the demand that platonists satisfy (ED2) is really no different from the demand that they perform task (I). To perform task (I), platonists need to argue that mathematical objects exist, and to satisfy (ED2) they must explain how human beings could know that mathematical objects exist. But the two projects are the same, for human beings could attain knowledge of the existence of mathematical objects by using the very same arguments that platonists use to perform task (I).

So there are two reasons why -- in the present context -- I do not need to satisfy (ED2).[46] In spite of this, however, I will argue that (ED2) is no more difficult to satisfy than is its empirical correlate (i.e., the corresponding demand on our empirical knowledge). But this implies that the failure to satisfy (ED2) -- if, indeed, we can't satisfy it -- only gives us an argument against platonism if we also have a corresponding argument against scientific realism. But, as I will now argue, this is unacceptable.

The force of the epistemological argument against platonism derives not just from its attempt to put platonistically interpreted mathematical theories on unsteady epistemological footing, but also from its attempt to establish that the epistemological bases of such theories are <u>less steady than the epistemological bases of empirical theories</u>. When anti-platonists claim that we lack any means by which to acquire knowledge of mathematical objects, they are assuming that we <u>do</u> have a way of acquiring knowledge about <u>physical</u> objects -- namely via perception. Allusions to this pervade the writings of both Benacerraf and Field. The former is quite clear about his opinion that "our knowledge about medium-sized objects" satisfies his causal restraint on knowledge;[47] and the latter is equally clear about his opinion that we can sketch an account of the mechanisms responsible for the reliability of our empirical beliefs. He writes, for instance, that

> we can sketch the route whereby the assumed properties of, say, the electromagnetic field lead to various observable physical phenomena, and thereby affect our perceptual beliefs, and thereby indirectly affect our beliefs about the electromagnetic field. But nothing remotely analogous to this seems possible in the case of mathematics.[48]

Now, it seems to me that this is a crucial element of the epistemological argument against platonism. For if that argument placed an epistemological demand on mathematical platonists whose empirical correlate couldn't be met by scientific realists, then, by parity of reasoning, anti-platonists ought to admit that their argument only provides a reason for disbelieving in abstract objects if it also provides a reason for disbelieving in concrete objects. But it is clear that this is not the anti-platonists' intention; they do not mean to jettison concrete objects along with abstract ones. Moreover, if it was their intention, then this would, for all practical purposes, establish that their demand was not legitimate, for almost none of us are willing to jettison physical objects from our ontology.

Now if the epistemological argument against platonism was only being used to establish that platonists have to satisfy (ED1), then there would be nothing wrong with the above remarks, for there are prima facie reasons to think that (ED1) is more difficult to satisfy than is its empirical correlate. But this is not the case with (ED2). It is no more difficult to satisfy than is its empirical correlate. If we interpret (ED2) strongly, so that platonists will have a hard time satisfying it, then scientific realists will also have a hard time trying to satisfy the empirical correlate of (ED2). But if we interpret (ED2) weakly, so that scientific realists will be able to satisfy its empirical correlate, then there will not even be a prima facie reason to think that platonists cannot satisfy (ED2) itself.

Let us state the two interpretations clearly. The strong demand is

(SD) The demand for a full-blooded demonstration of our belief in the existence of mathematical objects.

And the weak demand is

(WD) The demand for a reason to think that our belief in the existence of mathematical objects is correct.

In connection with (WD), there is not much to say, because we clearly do have reasons to believe in abstract objects. These reasons include the Fregean and Quine/Putnam arguments already mentioned. Benacerraf and Field both admit that there are good reasons to believe in mathematical objects. In fact, Benacerraf thinks that the semantic considerations that favor platonism are just as strong as the epistemological considerations that weigh against it. This is clear from the fact that he presents his paper as a dilemma caused by a conflict between our best semantic theory and our best epistemological theory. Thus, Benacerraf's reason for thinking that platonism might be correct is that it allows us to use a straight forward semantics for mathematical discourse. He says that platonism

> assimilates the logical form of mathematical propositions to that of apparently similar empirical ones: empirical and mathematical propositions alike contain predicates, singular terms, quantifiers, etc.[49]

Field does not think that the arguments for platonism are as good as the epistemological argument against it, but he doesn't think that they can be dismissed lightly.[50] He writes, for instance, that

> there can be strong reasons to believe in mathematical entities, having to do with the apparent

> indispensability of mathematical entities to important theories outside of mathematics.[51]

So it seems that I need only support my claim that (SD) is no more difficult to satisfy than is its empirical correlate. To see why this is the case, one need only notice that "the empirical correlate of (SD)" is just the traditional skeptical demand that we justify our belief in an external world. Once we notice this, we will realize that the empirical correlate of (SD) will, by no means, be easily satisfied. For, as anyone who has had even an introductory course in philosophy knows, the skeptic is not easily refuted. After all, the history of philosophy is chock full of failed attempts at such a refutation.

Now, I have no stake in whether or not the skeptic can, in fact, be answered (i.e., in whether (SD) and its empirical correlate can be satisfied). My only point is that we have no reason to suppose that we will be able to meet the skeptic with respect to our belief in physical objects, but not with respect to our belief in mathematical objects. The reason for this is that, since the skeptic does not recognize perceptually acquired information as trustworthy evidence for the existence of physical objects, there is not going to be any evidence which supports the existence of physical objects but which does not support the existence of mathematical ones (at least as far as the skeptic is concerned). The justification for both, if it is to be forthcoming at all, will have to come from holistic consideration arising from the acceptance of certain theories as the best explanations available, i.e., as our best means of saving the phenomena. But if this is so, then our empirical and mathematical theories are on equal footing here: they will stand or fall to the skeptic together. This point has been made by Katz:

> Empirical knowledge, as we have shown, has no advantage over a priori knowledge in encounters with the skeptic. Each will survive or succumb, not on the basis of its own special features, but on the resourcefulness of the philosophical believer in meeting the challenge of the philosophical skeptic.[52]

7. Conclusion

I conclude that the epistemological argument against platonism does not accomplish its task, because (1) and (2) do not provide a good reason for believing (3). I have given two explanations of why this is so, namely the Katz/Lewis necessity-based explanation of the last chapter and the consistency-based explanation of the present chapter. It is my opinion that these two explanations stem from the same epistemology and that they are parallel explanations of the same fact.

NOTES

1. In speaking of "possible mathematical objects" I do not mean to speak of objects which are different in kind from ordinary, garden-variety mathematical objects. All mathematical objects are ordinary, actually-existing mathematical objects. When I say that "all possible mathematical objects exist," this is just a loose way of saying that the actual mathematical objects exhaust all of the possibilities, i.e., that the actual mathematical objects are all the mathematical objects there could be. So while I will speak in different places of "mathematical objects," of "possible mathematical objects," and of "actual mathematical objects," I only do this for the purpose of emphasizing different things. I do not mean to imply that these terms pick out different kinds of objects; rather, I take all three terms to be coextensive.

2. Field (1989), p. 26-27.

3. There is one response to my position that can be dealt with immediately. It might be objected that this full-blooded platonism that I am advocating is not the standard form of platonism. Quinean platonism, for instance, seems quite opposed to my suggestion that all possible mathematical objects exist. Quine only wants to recognize extensional abstract objects, such as sets. (This attitude is quite apparent, for instance, in chapter six of his (1960).)

My response to this objection is that it is simply irrelevant. My claim is that there is no epistemological problem with the full-blooded platonism that recognizes all possible mathematical objects. If this isn't the "standard" version of platonism, then so much the worse for the standard version. The only thing that needs to be kept in mind here is that when platonists go to perform what -- in chapter one -- I called task (I) (i.e., when they go to provide arguments in favor of their position), they must argue not just for any version of platonism, but for the full-blooded version. Thus, there is a sense in which the adoption of my epistemology makes task (I) a bit more difficult to perform.

Two points need to be made here. First, the Katz/Lewis epistemology of chapter five is also an epistemology for the full-blooded version of platonism only, and so task (I) will be a bit more difficult for them also. Second, it doesn't matter that task (I) has become a bit more difficult, because there are arguments which favor the full-blooded version of platonism over Quinean platonism. For while Quinean platonism can account for mathematical truth, it cannot account for linguistic truth. To do this, we need meanings, sentence types, etc. (Also, if Quinean platonists refuse to recognize certain mathematical objects, they will not be able to account for _all_ mathematical truths.)

4. There are two notions of consistency. A theory, or set of sentences, is _semantically_ consistent if it has a model, i.e., if it is satisfiable. A theory, or set of sentences, is _syntactically_ consistent if it is impossible to derive a contradiction from it. It is syntactic consistency that I am concerned with in this section.

5. It is clear that we have good reasons to believe that at least most of our theories are consistent. Resnik (1982) has given three reasons for this confidence. We have (a) "proofs that if one theory...is consistent or categorical, then other theories are or are not;" (b) reductions of theories to other theories, which shows that we have arrived at the same results in different ways; and (c) "several consistency proofs for number theory". Resnik admits, however, that none of these provide knockdown arguments for consistency: (a) relies on the assumption that an initial theory is consistent; (b) could be true even if all the theories involved in the various reductions were inconsistent; and the proofs in (c) begin from assumptions, which, for our purposes, ought not to be allowed.

Field (1989, p. 232) has given another reason to be confident here: if one of our mathematical theories weren't consistent, "someone would have probably discovered an inconsistency in it by now."

6. Note that anti-platonists could _not_ claim that we _do_ have such knowledge but that they needn't account for it. For if anti-platonists needn't account for it, then platonists needn't either. If the one can claim that knowledge of consistency is a brute fact, then the other can, too. But this, of course, is ridiculous. Knowledge of consistency is _not_ a brute fact. Anyone who accepts it, must explain it; moreover, anyone who _can't_ explain it, can't deride platonists for not being able to explain it.

7. The anti-platonist might try to argue that knowledge of the consistency of "4 is even" is not so trivial as I make out; for one might claim that we cannot know that this sentence is consistent without knowing that some larger piece of theory, of which this sentence is a part, is consistent. But I think this is wrong. To know that "4 is even" is consistent, we only need to know that we don't contradict ourselves by saying this sentence; but to know this, we only need to know what "4" and "even" mean.

It doesn't matter, however, if this response is inadequate, for, again, if we don't know that "4 is even" is consistent, then platonists need not account for such knowledge. And in this case, we wouldn't have any mathematical knowledge at all; for if knowledge of a background theory is required for knowledge of the consistency of "4 is even," then it is also required for knowledge of the truth of "2 + 2 = 4".

8. Even Wittgenstein, who denies that mathematical sentences are propositions, has to find a place for something at least corresponding to mathematical knowledge. For he has to be able to distinguish Gödel from the mathematical dunce.

9. Field (1989), essay 3, especially pp. 84-85. Note, too, that there is a second reason why Field is committed to the claim that we can establish that our mathematical theories are consistent. In order for fictionalism to be a tenable philosophy of mathematics, it must be shown that our mathematical theories are conservative, i.e., that they are "consistent with every internally consistent theory...about the physical world." (See Field's (1989), p. 240.) But since conservativeness entails consistency, Field must establish that our mathematical theories are consistent. But if he can do this, then a platonist can too.

10. Again, the reason this is important is that, at least prima facie, it appears that -- in general -- access to a group of objects is necessary in order to attain knowledge of them.

11. We should also remember here that platonists only have to account for knowledge that we do have. If some theory T is so complex that we can't know whether it's consistent, then T is irrelevant to the present discussion.

12. Tarski (1936), pp. 414-15. Note that I have "cheated" just a bit with this quotation. Tarski was actually talking about logical consequence, and not consistency. On his view, however, the two concepts are trivially inter-definable.

13. One might wonder how we could come to understand the mathematical concepts involved in these beliefs, but the answer to this question just isn't important for our purposes. For (a) however we come to understand them, we clearly do, and (b) in order to understand a concept, one does not need access to the objects in the extension of the concept. This is obvious from the fact that many of our empirical concepts are uninstantiated. For instance, we understand "unicorn," even though there are no such animals. Now, one might object that while we have no access to unicorns, we do have access to horses and horns. But, of course, platonists can make a similar response in connection with mathematical concepts. If we can come to understand "unicorn" via our experience with horses and horns, why can't we come to understand "two" via our experience with piles of two things.

14. Field (1989), pp. 230-232.

15. One might mean several different things by "purely mathematical." The easiest way to illustrate this is to list the various sorts of theories which people call "impure." These include, for instance, applied geometries (i.e., geometries which are intended to be about some _physical_ space) and set theories which allow for physical objects as elements. It is also sometimes said that a set theory with urelements (i.e., a set theory with non-sets as elements) is impure. But provided that these non-sets are still _mathematical_ objects, I will allow such theories to count as "purely mathematical." My reason is that such theories speak of nothing but the mathematical realm; that is, they do nothing but predicate mathematical properties and mathematical relations of mathematical objects. I will take this to be a _rough_ mark of the "purely mathematical," but I don't want to defend this usage, nor make it any more precise.

It is worth pointing out, however, that this sort of "purely mathematical theory" will be _conservative_ (or, assuming that it's true, _necessary_). For to say that a theory speaks only of the mathematical realm is to say that it doesn't imply anything about the physical world. But this is just to say that it's consistent with every internally consistent theory of the physical world; and this is just to say that it's _conservative_, in Field's sense of that term. But as Field has pointed out, the only difference between a conservative theory and a necessary one is that the former need not be true. (See Field's (1989), pp. 58-59.) It does not follow, however, that because a theory is impure, it is not conservative. Set theories which allow for physical objects as elements _can be_ conservative, provided they do not assert the existence of any physical objects.

16. Field couches his discussion of these issues in terms of the distinction between mathematical consistency and mathematical _truth_. I have purposely avoided the use of "mathematical truth" in my formulation of (P); I have spoken only of a theory "accurately describing some part of the mathematical realm." Now, I think these two ways of speaking are equivalent, but in section 5.1, I will explain why someone might resist this identification. I will also argue there that, for the present purposes, I need not take a stand on this issue. It is for this reason that I avoid talk of truth.

17. Recall that I do not mean to distinguish actual mathematical objects from possible mathematical objects. These two collections of objects are one and the same collection. I refer to them differently in different contexts merely to put a different emphasis on what I am saying.

18. In chapter five, I said that mathematical knowledge could be based upon a faculty of no-contact intuition which consisted of a process of creative writing, or dreaming up. We can now see why this is the case. Now, of course, I don't think that this is what mathematical intuition is _really_ like; I'm just saying that it _could_ be like this, i.e., that such a faculty of intuition could engender mathematical knowledge.

19. Resnik (1982), pp. 101-102.

20. Resnik (1982), p. 101. Note that Resnik's talk of "patterns" here is merely a function of his ontological structuralism.

21. I have represented Resnik here as accepting the claim that applied theories always contain a metatheoretical element. Now, he doesn't put it in exactly this way. He writes (1982, p. 101) that "An applied theory of a pattern will consist of a pure one together with claims stating how the pattern is instantiated, etc." Thus, I suppose that Resnik might try to maintain that there is nothing metatheoretical about this.

In any event, I would like to emphasize that I am not committing myself to Resnik's definition of applied theories.

22. There is nothing strange about this example, because, on this view, grammars are mathematical theories. Resnik simply assumes this view of grammars. Katz has dedicated an entire book -- viz., his (1981) -- to establishing it.

23. It is worth noting that this view of languages has been confirmed by philosophers of language, who are entirely unconcerned with our epistemological question, i.e., who are interested only in understanding language. David Lewis (1972, p. 163) says that a language is a function from "strings of types of sounds or marks" to meanings. There are an infinite number of such things which have never been spoken or used by human beings in any way. Lewis then says that the problem for a semantic theorist of a natural language like English is to say which of the infinity of languages is the _actual_ English language.

24. Gödel (1930).

25. By a "mathematical model," I just mean a model of a purely mathematical theory, i.e., a model containing only mathematical objects.

26. One might wonder how we know that T describes M _accurately_. The answer is easy: any model which is inaccurately characterized by T is not T's model. To say that M is a model of T just _is_ to say that (under an

appropriate interpretation) T accurately characterizes M.

27. The proof I am referring to is not Gödel's; it is Leon Henkin's (1949). (For a good, short statement of this proof, see Mendelson (1987), p. 71.) The proof proceeds by showing how to construct a model for an arbitrary consistent first-order theory. Thus, the only assumption that platonists need to make in order to use this proof in their argument for (P) is that any mathematical model that can be described by a Henkin-type construction actually exists. But there's nothing wrong with this assumption, because it follows from what we're already assuming, viz., that all possible mathematical objects exist.

28. Actually, they must do more than this. If this were all they had to do, the problem would be trivially solvable. For since it is logically possible that there is a God who has created a pre-established harmony between the mathematical realm and our belief states, it is not impossible for us to have mathematical knowledge. Clearly this doesn't refute the epistemological argument. But the point still stands. Platonists do not have to provide a rational reconstruction of how we actually do acquire mathematical knowledge. They must provide something in between this and an account of how we logically could acquire such knowledge. They must provide an account of how a human being could acquire knowledge of the mathematical realm such that it is plausible that a human being actually could acquire mathematical knowledge in the specified way. (I also discuss this issue in chapter two, section 2.)

Now, one might object to the peculiarity of this. That is, one might wonder why such a project is necessary in constructing an epistemology for mathematics when nothing analogous is necessary in constructing an epistemology for empirical science. But there's nothing peculiar going on here. The reason this project is necessary in the mathematical case and not in the empirical case is that it is only in the former case that there is an argument -- viz., Benacerraf's argument -- which raises a question about the possibility of knowledge. Thus, in short, the main reason why platonists only have to show how a human being plausibly could attain knowledge of mathematical objects is that they are responding to the claim that human beings couldn't attain such knowledge.

29. Another way of putting this is that my view explains why a rational reconstruction of what goes on in the heads of mathematicians is not needed. Since all familiarity with consistent mathematical theories is familiarity with the mathematical realm, we need not demand -- as a necessary condition on mathematical knowledge -- that a person cognize that his or her mathematical beliefs are beliefs about a mathematical realm. It is as if theoretical familiarity is

"good enough." Thus, given this, what goes on in the heads of mathematicians -- aside from theoretical familiarity -- is epistemologically unimportant; therefore, a rational reconstruction of the mental processes of mathematicians is not needed for an adequate platonist epistemology.

30. Resnik (1982), p. 101. Note that Resnik's use of "pattern" instead of "model" is irrelevant for our purposes.

31. Shapiro (1989), p. 167-168.

32. Field (1989), p. 278.

33. We know this because C has been proven independent of the other axioms of ZF. Gödel (1939) proved that C is consistent with ZF; and Paul Cohen (1963) proved that not-C is consistent with ZF.

34. This does not mean that the philosopher who takes this line is left without a definition of "true". On this view, a sentence (whether it is mathematical or empirical) is true just in case it accurately describes some part of the world. Now, in connection with purely mathematical atomic sentences (and negations of such sentences) we have another definition: they are true just in case they are satisfiable, i.e., just in case they are true in some model M. But since the philosopher who takes this line salvages the thesis that truth is closed under implication, this also gives us a definition of truth for any purely mathematical sentence: such a sentence is true if it is constructed in an appropriate way from purely mathematical atomic sentences (or negations of such sentences) using "and," "not," "some," etc.

35. By using "set" to refer to the objects of ZFC, I am insinuating that it is ZFC which describes the "standard model"; but nothing turns on this, and advocates of the view presently under discussion need not commit themselves to it. Moreover, it is not relevant for the present purposes whether there is any agreement among mathematicians about the truth of C in the "standard model."

36. This problem can actually be made worse; for if only theories of "standard" models are true, then it seems that a theory can become true without being changed. This is simply because models can become standard. For instance, non-Euclidean geometries are, today, considered to be on a par with Euclidean geometry, at least from a purely mathematical perspective; but this was not always the case.

37. It should be noted that this feature of my position enables me to explain numerous phenomena. For instance, Maddy claims (in various places in her 1990) that every position in the philosophy of mathematics faces the

epistemological problem of saying how we could know how to settle certain open questions such as the continuum hypothesis. But on my view, this problem is easily solved, for on my view it is always legitimate to accept any (consistent) answer to any open question. (And note that this also explains why extrinsic modes of justification -- discussed in section 6 of chapter four -- are acceptable in mathematics.)

38. As I pointed out above, one might mean various things by speaking of a "pure mathematical theory." What I mean, when I say that a theory is purely mathematical is, roughly, that it speaks only of the mathematical realm, i.e., that is does nothing but predicate mathematical properties and mathematical relations of mathematical objects.

39. Field (1989), p. 56.

40. I thank Hartry Field for bringing to my attention this view of our knowledge of impure theories.

41. This is just to say that pure mathematical theories are conservative, in Field's sense of the term. For if a theory T doesn't imply anything about the physical world, then it is consistent with every internally consistent description of the physical world. (See Field (1989), pp. 58-59.)

42. It is worth noting that platonists could account for our knowledge of "mixed" theories (i.e., empirical theories which, at least prima facie, quantify in an ineliminable way over mathematical objects) in the same way. For (P'') holds for empirical theories (whether they're mixed or nominalistic) as well as mathematical theories. Now, again, in order to know whether an empirical theory implies any falsehoods about the physical world, one needs knowledge of the physical world; but, of course, this is exactly what we'd expect: knowledge of an empirical theory does require knowledge of the physical world.

43. Neither says that their argument is a reductio, but it's clear that both of their arguments are. Field, for instance, writes (on p. 25 of his 1989) that "if there are mathematical entities of the sort that the platonist believes in...then there seems to be a difficulty in seeing how we could ever" attain mathematical knowledge. (My emphases.) As far as Benacerraf's argument is concerned, I haven't been able to find a single quote that's quite so illuminating, but (on p. 667 of his 1973) he says (a) we must assume "that we have mathematical knowledge," (b) "An acceptable semantics for mathematics must fit an acceptable epistemology," and (c) a platonistic semantics does not fit with an acceptable epistemology, because it "will depict truth conditions in terms of conditions on objects whose nature...places them

beyond the reach of the better understood means of human cognition."

44. The anti-platonist might try to formulate the argument without assuming platonism in the following way:
(i) It is impossible to attain knowledge of a platonic realm.
(ii) We have mathematical knowledge.
Therefore,
(iii) Mathematical knowledge is not knowledge of a platonic realm.

But anti-platonists cannot use this argument. For the only way to claim that (i) is true without implying that there *is* a platonic realm, is to claim that it's *vacuously* true. Thus, the only way that anti-platonists can put (i) forward as a true premise is to put it forward as a vacuously true premise. But to do this is to beg the question. The anti-platonist's argument for (i) relies upon the falsity of platonism. Therefore, anti-platonists cannot use (i) in an argument against platonism. It seems to me that the only way to avoid this problem is to use a *reductio*, i.e., to state that *even if platonism is true*, then knowledge of a platonic realm is *still* impossible.

45. Field (1989), p. 25.

46. There is actually a third reason. The epistemological argument generates a suspicion about our ability to satisfy (ED1), because it seems *prima facie* that the mathematical realm could have any one of an infinity of possible configurations, and so our lack of access to this realm seems to make it a mystery how we could know which of these configurations it actually has. But the epistemological argument does *not* generate a suspicion about our ability to satisfy (ED2), because there are only two possible answers to the question of whether there *is* a mathematical realm, and so there is no great mystery about how we could answer this question. For even if we lack access to the mathematical realm, it is plausible that we could have good philosophical reasons for believing in it.

47. Benacerraf (1973), p. 672.

48. Field (1989), p. 232.

49. Benacerraf (1973), p. 668.

50. In fact, he wrote an entire book -- his (1980) -- trying to rebut one of the arguments for platonism, namely the Quine/Putnam indispensability argument.

51. Field (1989), p. 238.

52. Katz (1981), p. 212.

Bibliography

Alexander, M.P. and Albert, M.I.
(1983) "The Anatomical Basis of Visual Agnosia," in Kertesz (1983), pp. 393-415.

Aristotle
Metaphysics. See, for instance, the translation by H.G. Apostle, Peripatetic Press, Grinnell, IA, 1979.

Armstrong, D.M.
(1978) A Theory of Universals, Cambridge University Press, Cambridge.

Ayer, A.J.
(1946) Language, Truth and Logic, second edition, Dover Publications, New York. (First published 1936.)

Bealer, G.
(1982) Quality and Concept, Oxford University Press, Oxford.

Benacerraf, P.
(1973) "Mathematical Truth," Journal of Philosophy, vol. 70, pp. 661-679.

(1965) "What Numbers Could Not Be," reprinted in Benacerraf and Putnam (1983), pp. 272-294.

(1981) "Frege: The Last Logicist," Midwest Studies in Philosophy, vol. 6, pp. 17-35.

Benacerraf, P. and Putnam, H. (eds.)
(1983) Philosophy of Mathematics, second edition, Cambridge University Press, Cambridge.

(1964) Philosophy of Mathematics, first edition, Prentice Hall, Englewood Cliffs, NJ.

Bernays, P.
(1935) "On Platonism in Mathematics," reprinted in Benacerraf and Putnam (1983), pp. 258-271.

Block, N.
(1978) "Troubles with Functionalism," reprinted in Block (1980a), pp. 268-305.

(1980) "Introduction: What is Functionalism," in Block (1980a), pp. 171-184.

(1980a) Readings in the Philosophy of Psychology (edited by Block), Harvard University Press, Cambridge, MA.

Bohm, D.
(1980) Wholeness and the Implicate Order, Routledge & Kegan Paul, London.

Boolos, G.
(1984) "Nominalistic Platonism," Philosophical Review, vol. 94, pp. 327-344.

Boolos, G. and Jeffrey, R.
(1989) Computability and Logic, third edition, Cambridge University Press, Cambridge. (First published 1974.)

Brown, J.R.
(1990) "Π in the Sky," in Irvine (1990), pp. 95-120.

Burgess, J.
(1990) "Epistemology and Nominalism," in Irvine (1990), pp. 1-15.

Carnap, R.
(1934) Logische Suntax der Sprache. Translated by A. Smeaton as The Logical Syntax of Language, Harcourt Brace, New York, 1937.

(1952) "Meaning Postulates," Philosophical Studies, vol. 3, pp. 65-73.

(1956) "Empiricism, Semantics, and Ontology," reprinted in Benacerraf and Putnam (1983), pp. 241-257.

Church, A.
(1936) "An Unsolvable Problem of Elementary Number Theory," reprinted in Davis (1965), pp. 89-107.

Churchland, P.
(1984) Matter and Consciousness, MIT Press, Cambridge, MA.

Cohen, P.
(1963) "The Independence of the Continuum Hypothesis," part 1, Proceedings of the National Academy of Sciences, vol. 50, pp. 1143-1148. (Part 2 in vol. 51 (1964), pp. 105-110.)

Curry, H.B.
(1954) "Remarks on the Definition and Nature of Mathematics," reprinted in Benacerraf and Putnam (1983), pp. 202-206.

Davis, M. (ed.)
(1965) The Undecidable: Basic Papers on Undecidable Propositions, Unsolvable Problems, and Computable

Functions, Raven Press, Hewlett, NY.

Dedekind, R.
(1872) Stetigkeit und irrationale Zahlen. Translated by W.W. Beman as "Continuity and Irrational Numbers," in Dedekind (1901), pp. 1-27.

(1888) Was sind und was sollen die Zahlen? Translated by W.W. Beman as "The Nature and Meaning of Numbers," in Dedekind (1901), pp. 31-115.

(1901) Essays on the Theory of Numbers, Open Court Publishing, Chicago, IL. Translated by W.W. Beman.

Dewey, J.
(1925) Experience and Nature, Open Court, Chicago, IL.

Dreyfus, H.
(1972) What Computers Can't Do, Harper and Row, New York.

Dummett, M.
(1959) "Wittgenstein's Philosophy of Mathematics," Philosophical Review, vol. 68, pp. 324-348.

(1973) "The Philosophical Basis of Intuitionistic Logic," reprinted in Benacerraf and Putnam (1983), pp. 97-129.

Field, H.
(1980) Science Without Numbers, Princeton University Press, Princeton, NJ.

(1989) Realism, Mathematics, and Modality, Basil Blackwell, New York.

Frege, G.
(1884) Der Grundlagen die Arithmetic. Translated by J.L. Austin as The Foundations of Arithmetic, Basil Blackwell, Oxford, 1953.

(1893-1903) Grundgesetze der Arithmetik. Translated (in part) by M. Furth as The Basic Laws of Arithmetic, University of California Press, Berkeley, CA, 1964.

(1919) "Der Gedanke." Translated by A.M. and M. Quinton as "The Thought," in Klemke (1968), pp. 507-535.

Fodor, J.
(1987) Psychosemantics, MIT Press, Cambridge, MA.

George, A. (ed)
(forthcoming) Mathematics and Mind, Oxford University Press, Oxford.

Gettier, E.
(1963) "Is justified true belief knowledge?" Analysis, vol. 23, pp. 121-123.

Gilles, D.A.
(1982) Frege, Dedekind, and Peano on the Foundations of Arithmetic, Van Gorcum, Assen, The Netherlands.

Gödel, K.
(1930) "Die Vollstangdigkeit der Axiome des Logishen Funktionkalküls." Translated by S. Bauer-Mengelberg as "The Completeness of the Axioms of the Functional Calculus of Logic," in Gödel (1986), pp. 103-123.

(1931) "Uber formal unentscheidbare Satze der Principia Mathematica und verwandter Systeme I." Translated by J. van Heijenoort as "On Formally Undecidable Propositions of Principia Mathematica and Related Systems I," in Gödel (1986), pp. 145-195.

(1934) "On Undecidable Propositions of Formal Mathematical Systems," reprinted in Davis (1965), pp. 41-74.

(1939) "Consistency-Proof for the Generalized Continuum Hypothesis," Proceedings of the National Academy of Sciences, vol. 25, pp. 220-224.

(1944) "Russell's Mathematical Logic," reprinted in Benacerraf and Putnam (1983), pp. 447-469.

(1958) "Über eine bisher noch nicht benützte Erweiterung des finiten Standpunktes." Translated by S. Bauer-Mengelberg and J. van Heijenoort as "On a hitherto unutilized extension of the finitary standpoint," in Gödel (1990), pp. 241-251.

(1964) "What is Cantor's Continuum Problem?" reprinted in Benacerraf and Putnam (1983), pp. 470-485.

(1972) "On an extension of finitary mathematics which has not yet been used," in Gödel (1990), pp. 271-280. (Note: this essay is a revised version of Gödel (1958).)

(1972a) "Some remarks on the undecidability results," reprinted in Gödel (1990), pp. 305-306.

(1986) Collected Works, Volume I, Oxford University Press, Oxford.

(1990) Collected Works, Volume II, Oxford University Press, Oxford.

(forthcoming) "Some Basic Theorems on the Foundations of

Mathematics and their Implications," to appear in Volume III of Gödel's Collected Works. (Note: this essay is commonly referred to as "The Gibbs Lectures;" it was delivered at Providence on December 26, 1951 as the 25th Josiah Willard Gibbs Lecture.)

Goldman, A.
(1967) "A Causal Theory of Knowing," Journal of Philosophy, vol. 64, pp. 357-372.

Goodman, N.
(1951) The Structure of Appearance, Harvard University Press, Cambridge, MA.

(1956) "A World of Individuals," reprinted in Benacerraf and Putnam (1964), pp. 197-210.

Goodman, N. and Quine, W.V.O.
(1947) "Steps Toward a Constructive Nominalism," Journal of Symbolic Logic, vol. 12, pp. 105-122.

Hale, B.
(1987) Abstract Objects, Basil Blackwell, Oxford.

Hardy, G.H.
(1940) A Mathematician's Apology, Cambridge University Press, London.

Hempel, C.
(1945) "On the Nature of Mathematical Truth," reprinted in Benacerraf and Putnam (1983), pp. 377-393.

Henkin, L.
(1949) "The Completeness of the first-order functional calculus," Journal of Symbolic Logic, vol. 14, pp. 159-166.

Hubel, D. and Wiesel, T.N.
(1977) "Functional Architecture of the Macaque Monkey Visual Cortex," Proceedings of the Royal Society of London, series B, vol. 198, pp. 1-59.

(1979) "Brain Mechanisms of Vision," Scientific American, vol. 241, number 3, pp. 150-162.

Irvine, A. (ed.)
(1990) Physicalism in Mathematics, Kluwer Academic Publishers, Norwell, MA.

Kant, I.
(1781) Kritik der reinen Vernunft. Translated by N.K. Smith as Critique of Pure Reason, Macmillan, London, 1929.

Katz, J.

(1981) Language and Other Abstract Objects, Rowman and Littlefield, Totowa, NJ.

Kertesz, A. (ed.)
(1983) Localization in Neurophysiology, Academic Press, New York.

Kitcher, P.
(1984) The Nature of Mathematical Knowledge, Oxford University Press, Oxford.

Kleene, S.
(1936) "General Recursive Functions of Natural Numbers," reprinted in Davis (1965), pp. 237-253.

Klemke, E. (ed.)
(1968) Essays on Frege, University of Illinois Press, Urbana, IL.

Lakatos, I.
(1976) Proofs and Refutations, Cambridge University Press, Cambridge.

Lambert, K. (ed.)
(1969) The Logical Way of Doing Things, Yale University Press, New Haven, CT.

Lewis, D.
(1972) "Languages and Language," reprinted in Lewis (1983), pp. 163-188.

(1983) Philosophical Papers, volume 1, Oxford University Press, New York.

(1986) On the Plurality of Worlds, Basil Blackwell, Oxford.

Locke, J.
(1706) Essay Concerning Human Understanding, fifth edition, London. (First published 1690.)

Malitz, J.
(1979) Introduction to Mathematical Logic, Springer-Verlag, New York.

Maddy, P.
(1980) "Perception and Mathematical Intuition," Philosophical Review, vol. 89, pp. 163-196.

(1989) "The Roots of Contemporary Platonism," Journal of Symbolic Logic, vol. 54, pp. 1121-1144.

(1990) Realism in Mathematics, Oxford University Press, Oxford.

(1990a) "Physicalistic Platonism," in Irvine (1990), pp. 259-289.

Mendelson, E.
(1987) Introduction to Mathematical Logic, third edition, Wadsworth, Belmont, CA. (First published 1964.)

Mill, J.S.
(1843) A System of Logic, reprinted in Mill (1950).

(1950) Philosophy of Scientific Method, Hafner Publishing, New York.

Nagel, T.
(1974) "What is it Like to be a Bat?" Philosophical Review, vol. 83, pp. 435-450.

Nozick, R.
(1981) Philosophical Explanations, Harvard University Press, Cambridge, MA.

Parsons, C.
(1980) "Mathematical Intuition," Proceedings of the Aristotelian Society, vol. 80, pp. 145-168.

(1983) Mathematics in Philosophy, Cornell University Press, Ithaca, NY.

(forthcoming) "Intuition and Number," to appear in George (forthcoming).

(unpublished) "Gödel and Quine on Analyticity."

Plato
The Meno and The Phaedo. Both translated by G.M.A. Grube in Five Dialogues, Hacket Publishing, Indianapolis, IN, 1981.

Post, E.
(1936) "Finite Combinatory Processes," reprinted in Davis (1965), pp. 289-291.

Putnam, H.
(1967) "The Nature of Mental States," reprinted in Block (1980a), pp. 223-231.

(1967a) "Mathematics Without Foundations," reprinted in Benacerraf and Putnam (1983), pp. 295-311.

(1971) "Philosophy of Logic," reprinted in Putnam (1979), pp. 323-357.

(1975) "What is Mathematical Truth?" reprinted in Putnam (1979), pp. 60-78.

(1979) Mathematics, Matter and Method: Philosophical Papers Volume 1, second edition, Cambridge University Press, Cambridge, MA. (First published 1975.)

(1980) "Models and Reality," reprinted in Benacerraf and Putnam (1983), pp. 421-444.

Quine, W.V.O.
(1936) "Truth by Convention," reprinted in Benacerraf and Putnam (1983), pp. 329-354.

(1948) "On What There Is," reprinted in Quine (1961), pp. 1-19.

(1951) "Two Dogmas of Empiricism," reprinted in Quine (1961), pp. 20-46.

(1960) Word and Object, Wiley, New York.

(1961) From a Logical Point of View, second edition, Harper and Row, New York. (First published 1953.)

(1963) Set Theory and its Logic, Harvard University Press, Cambridge, MA.

(1969) Ontological Relativity and Other Essays, Columbia University Press, New York.

(1970) Philosophy of Logic, Prentice Hall, Englewood Cliffs, N.J.

Resnik, M.
(1981) "Mathematics as a Science of Patterns: Ontology and Reference," Nous, vol. 15., pp. 529-550.

(1982) "Mathematics as a Science of Patterns: Epistemology," Nous, vol 16, pp. 95-105.

(1990) "Beliefs About Mathematical Objects," in Irvine (1990), pp. 41-71.

Rosen, G.
(unpublished) "Causation and the Case for Nominalism."

Russell, B.
(1919) Introduction to Mathematical Philosophy, Allen and Unwin, London.

Schiffer, S.
(1987) Remnants of Meaning, MIT Press, Cambridge, MA.

Searle, J.
(1980) "Minds, Brains and Programs," The Behavioral and Brain Sciences, vol. 3, pp. 417-457.

(Unpublished) The Rediscovery of the Mind.

Shapiro, S.
(1989) "Structure and Ontology," Philosophical Topics, vol 17, 145-171.

Skyrms, B.
(1967) "The Explanation of 'X Knows that p'," Journal of Philosophy, vol. 64, pp. 373-389.

Steiner, M.
(1975) Mathematical Knowledge, Cornell University Press, Ithaca, NY.

Stroud, B.
(1965) "Wittgenstein and Logical Necessity," Philosophical Review, vol. 74, pp. 504-18.

Tarski, A.
(1935) "The Concept of Truth in Formalized Languages," reprinted in Tarski (1956), pp. 152-278.

(1936) "The Concept of Logical Consequence," reprinted in Tarski (1956), pp. 409-420.

(1956) Logic, Semantics, and Metamathematics. Papers from 1923-1938, Clarendon Press, Oxford.

Turing, A.
(1937) "On Computable Numbers, with an Application to the Entscheidungsproblem," reprinted in Davis (1965), pp. 116-153.

van Fraassen, B.
(1969) "Presuppositions, Supervaluations, and Free Logic," in Lambert (1969), pp. 67-91.

van Heijenoort, J. (ed.)
(1967) From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931, Harvard University Press, Cambridge, MA.

Wang, H.
(1987) Reflections on Kurt Gödel, MIT Press, Cambridge, MA.

(1974) From Mathematics to Philosophy, Routledge and Kegan Paul, London.

Wittgenstein, L.

(1921) Logisch-philosophische Abhandlung. Translated by D.F. Pears and B.F. McGuinness as Tractatus Logico-Philosophicus, Routledge and Kegan Paul, London, 1974.

(1953) Philosophical Investigations, Macmillan, New York. Translated by G.E.M. Anscombe.

(1956) Remarks on the Foundations of Mathematics, Basil Blackwell, Oxford. Translated by G.E.M. Anscombe.

Wright, C.
(1983) Frege's Conception of Numbers as Objects, Aberdeen University Press, Aberdeen, Scotland.

Yourgrau, P.
(1989) "Review Essay: Reflections on Kurt Gödel [by H. Wang]," Philosophy and Phenomenological Research, vol. 1., pp. 393-408.