STRUCTURE AND THE CONCEPT OF NUMBER

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Preface

Do numbers exist, and if so, how might we refer to them? That is the topic of the present essay, and is thus an essay in the philosophy of mathematics. But it is also an essay in the philosophy of language and metaphysics. Just as Frege was driven to the philosophy of language by his mathematical investigations, I have been driven to the philosophy of mathematics by my interest in the nature of reference and ontology. I have been moved by the conviction that if sweeping generalizations are to be made about reference or what manner of things exist, then we must take a hard look at the special cases lest our philosophy suffer from a meager diet of examples.

Many issues directly relevant to this topic have either not been discussed or only cursorily so. I discuss at length the ontological commitments of arithmetic without having much to say about the nature of ontological commitment. The problems in this area are familiar, but I believe that they pose no insurmountable obstacle to the discussion of specific cases. Messy though these issues are, it is a mess that we have learned to live with. I also assume that analyticity is a coherent notion though I give no explicit defense of this. All that I can say in my defense is that after the forty year fallout of "Two Dogmas" an adequate treatment of analyticity would be the topic of another, very different, dissertation. Modal issues loom large in this essay.

Our arithmetic assertions are claimed to be conceptually equivalent to certain second-order modal generalizations. A complete treatment of my topic would also then include an explicit discussion of the metaphysics of modality and the ontological commitments of second-order logic. If I may be forgiven a parody of Wittgenstein: I should have liked to produce a good dissertation; this has not come about, but the time has past in which I could improve it.

The help and encouragement of many people were invaluable to the production of this essay. I would like to thank Alex Byrne, Ned Hall, David Lewis, and Michael Thau for useful comments and discussion. Special thanks are also due to three others. I would like to thank Gideon Rosen for his freindship and discussion of these and related issues over the course of a number of years. I would like to thank John Burgess for his patience and tutelage. And finally I owe an invaluable debt to Paul Benacerraf whose skepticism and charm saw me through the writing of this essay. On a more personal note, I should also thank my wife, Virginie Strub, for her inestimable patience, support, and love.

CHAPTER ONE

Naive Platonism

1.1 *The Strategy*

Platonism, in the philosophy of mathematics and elsewhere, has been name to a number of importantly different if sometimes intimately related doctrines. In the sense that will concern us here, it is the claim that numbers exist. It is towards the defense and elaboration of this doctrine that the present essay is dedicated.

At first blush, arithmetic appears to be a theory about the properties and relations of a special domain of objects, the natural numbers. On such a conception, the language of arithmetic is interpreted at *face-value*--our ordinary numerals are understood to be the unambiguous names that they appear to be, apparent quantification over the natural numbers is genuine, etc. Surface grammar is conceived to be in no way misleading or devious: the ontological commitments of our arithmetic assertions, as explicitly represented by surface syntax, are understood to be genuine. I contend that this is our natural, pre-theoretical conception of arithmetic. Or rather, that such a conception makes explicit what is implicit in our pre-theoretic understanding. Not only is a face-value interpretation part of our naive view of arithmetic, but so is a kind of *minimal realism*: the arithmetic assertions we accept are believed to be true.

Minimal realism about a theory or discourse is the view that the central beliefs in the disputed area are more or less true. There are two components to minimal realism and hence two corresponding ways to oppose minimal realism. First of all, the minimal realist holds that the target class of statements are genuinely assertoric, that they normally express belief, and that they have a truth-evaluable content. An anti-realist about the target class of statements may deny this and insist that their semantics must be understood in some other way--perhaps as the expression of some non-cognitive attitude. *Non-factualism*, as the view is fashionably known, is the denial that a range of apparent assertions have genuine truth-conditions and thus don't function as representing a putative domain of fact. Non-cognitivism in ethics is an example of a non-factualist thesis.[1] In the philosophy of mathematics, there is a traditional interpretation of Wittgenstein where statements of pure arithmetic don't have genuine truth-conditions, but rather are prescriptions for the use of mathematical symbols in applications.[2] (Thus to say that 2 + 2 = 4, for example, is just to say that if two disjoint groups are

[1]Examples of contemporary non-cognitivists are Allan Gibbard (1990) and Simon Blackburn (1984).

[2]Cf. D.A.T. Gasking (1964).

counted such that there are two of each, then if you count them together you *should* count four of them.) In addition to conceiving of the relevant discourse as genuinely assertoric, normally expressing belief, and having a truth-conditional content, the minimal realist must also believe that enough of the central beliefs in the disputed area are *true*. Another way to oppose minimal realism, then, is to grant that a range of assertions have a truth-evaluable content, but claim further that such assertions are subject to widespread and systematic error. John Mackie's error theory is an example of this kind of anti-realism as is Hartry Field's mathematical fictionalism.[3] The qualification 'minimal' is necessary in order to distinguish this notion from other issues that come under the rubric *realism*. Very often, once it has been established that the central beliefs of some subject matter are true, a question may arise as to whether their truth is *objective*--whether the truth of such assertions are suitably independent of *us* (with respect to our minds or to our social, linguistic, or epistemic practices). Thus Michael Dummett's celebrated case for anti-realism doesn't question whether our arithmetic assertions are truth-evaluable, or whether they are subject to widespread error; rather, the case is a sustained attack on the notion that arithmetic truth may transcend

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[3]John Mackie (1977) and Hartry Field (1980).

our proof procedures.[4] Though an anti-realist about arithmetic (in the sense that an intuitionistic semantics is a correct representation of its content), Dummett is nonetheless a minimal realist about arithmetic. The notion of realism that concerns me here is *minimal* in the sense that the issue of objectivity is only intelligible once minimal realism has been settled. Disputes concerning the objectivity of a certain subject matter *presuppose* that there are *facts* that comprise that subject matter (though, of course, the disputants disagree about how such facts should be conceived).

If the arithmetic assertions we accept, when interpreted at face-value, imply the existence of the numbers, then our natural, realistic attitude towards arithmetic involves us in a commitment to the natural numbers. Field's example is instructive in this regard. Unlike traditional nominalists, Field believes that mathematical language should be interpreted at face-value, but claims that since there are no abstract objects, there are no numbers, functions, and the like.[5] As a consequence he claims that our mathematical theories are systematically false. Since Field

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[4]Cf. Dummett (1978).

[5]The origin of both the contemporary tradition of nominalism, understood as the rejection of non-spatiotemporal entities, *and* the nominalist strategy of re-interpretation is Nelson Goodman's and Willard Van Orman Quine's "Steps Towards a Constructive Nominalism," (1972).

denies that the mathematical assertions we accept are true when interpreted at face-value, he doesn't undertake the ontological commitments involved in a face-value understanding of them. A face-value interpretation of arithmetic is thus *in*sufficient to secure a commitment to the natural numbers: arithmetic must also be believed.

This collection of attitudes (belief in arithmetic under a face-value interpretation) may be described as a kind of minimal platonism--*platonism* insofar as it involves a commitment to the natural numbers, and *minimal* insofar as nothing special is assumed about them. Arithmetic talk is taken at face-value and our practice of fixing mathematical opinion is understood to at least deliver reasonable belief. What the minimalist lacks is any kind of detailed philosophical *theory* about the nature of the natural numbers and our relation to them. I claim that minimal platonism is our natural, pre-philosophical starting point. If platonism is understood as the bare supposition that numbers exist, then far from being a distinctively philosophical doctrine, it is an attitude we bring with us to philosophy.

Philosophers have registered three intimately related but separable doubts concerning this conception of arithmetic. The first is an *epistemological* doubt. Questions have been raised about the very possibility of arithmetic knowledge given a platonist understanding of its content. Given certain epistemological assumptions, even if numbers were to exist, it would be impossible for us to know about them.[6] The second, and perhaps least developed of the three doubts, is *metaphysical* in character. The metaphysical doubt concerns whether the natural numbers objectually understood can intelligibly exist.[7] In this essay, however, I will discuss and critically examine a third *semantical* form of doubt. *Semantic skepticism* is the view that, given certain inevitable theses about meaning or reference, our arithmetic assertions can't mean what they appear to mean on a face-value interpretation of them: Given the nature of meaning or reference, even if the numbers were to exist it would be impossible for us to refer to them.[8] Despite appearances, it is impossible for us to form a determinate representation of the natural numbers. Our arithmetic opinions, insofar as they are understood to concern the natural numbers, turn out, on reflection, not to be determinate opinions after all.

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[6]The locus classicus of this kind of worry is Paul Benacerraf (1973); but the worry has been extended and refined most notably by Hartry Field in Field (1991).

[7]Nelson Goodman's complaint that mathematical objects are "ethereal, platonic pseudo-entities" is an example as is the metaphysical argument occurring toward the end of Benacerraf (1965).

[8]Cf., for example, Benacerraf (1965); Hodes (1984); and Jubien (1977).

If I am right in contending that our naive view is correctly described as a kind of minimal platonism, then all three sorts of doubts are kinds of *skepticism*. In order to succeed, any argument with minimal platonism as its target must rationally compel us either to abandon, or at least suspend, our naive belief in the natural numbers. One of the many norms governing our practice of fixing opinion is a kind of *conservativism*: Given that we hold a certain belief we are entitled to retain that belief as long as we have no positive reason to change our minds. Given such a norm the burden of proof is on the skeptic to rationally persuade the minimal platonist to suspend or disavow altogether any belief in the existence of the natural numbers. As long as such a belief remains *rationally permissible*, then such skepticism need not persuade the believer in numbers.

This epistemological observation underlies one sort of reaction one may have to semantic skepticism about the natural numbers. Such a skeptic contends that given the nature of representation, it is impossible for us to refer to the natural numbers in our thought and talk. But why is this an objection to a face-value interpretation of arithmetic, as opposed to an interesting observation about the special nature of numerical reference? (This reaction might be described as the "So what?" response.) Consider Berkeley's analogous argument against materialism. Suppose only an idea can resemble an idea. Suppose, further, that ideas represent by resemblance. It follows that no idea can represent a non-mental thing. I would hazard to say that to most contemporary philosophers, this looks like a better argument against Locke's theory of representation, than an argument against material existence. Similarly, given the positive, though defeasible, presumption in favor of belief in the numbers, semantic skepticism may be avoided if we give up the relevant assumptions made about meaning and representation. The epistemic situation is actually stronger than this: If our unsophisticated platonism is a sufficiently entrenched commitment, semantic skepticism may provide a *positive* reason to abandon the operative semantic assumptions.

This then is the strategy I pursue in the present essay: It may be described as a kind of *methodological conservativism*. My starting point is that of the minimal platonist. I then take up Paul Benacerraf's skeptical argument of "What Numbers Could Not Be." Insofar as conservativism provides pressure to retain our pre-theoretic conception of number, it provides us a reason to critically examine the assumptions made in Benacerraf's case. I contend that in doing so what we learn is not that numbers could not be, but, rather, something important about the nature of the natural numbers and our ability to refer to them. The object of this exercise, then, is not to further *skepticism*, but to further *understanding*; and so to arrive at what the minimal platonist lacks: a philosophical theory about the nature of number and the nature of arithmetic meaning.

Before proceeding to the details of Benacerraf's case, I want to discuss a *prima facie* difficulty for semantic skepticism about the natural numbers. The worry can be put rather simply: How can an argument to the effect that it is impossible to refer to a range of objects issue in an *ontological* conclusion? The target of such skepticism is an ontological thesis--that numbers exist. But if successful, all that would be established is that if numbers were to exist, it would be impossible for us to refer to them. Even if we were to succumb to semantic skepticism, the numbers could still very well exist, even if it is in principle impossible for us to bring them within our referential ken. It would seem, then, that the negative ontological conclusion (which is semantic skepticism's natural ambition) is precluded by the very *kind* of considerations that comprise the case for it.

In response, the skeptic could contend that his case, if successful, would leave the platonist in an unstable dialectical position. While, strictly speaking, the existence of the natural numbers is untouched by such skepticism, belief in the numbers would turn out not to be a determinate belief after all. Platonism, insofar as it is belief in the natural numbers, would not be intelligible and, indeed, be no belief at all. But notice if belief in the natural numbers is unintelligible, then so is its *denial*. The proper conclusion of semantic skepticism, then, could not be the negative ontological thesis that numbers could not be; but, rather, it could only issue in a kind of *quietism*--where quietism, in the present context, is understood as the view that there is no determinate issue dividing the platonist and the anti-platonist. I don't take my remarks here to be decisive. How this worry is properly addressed depends on the details of the skeptical argument and cannot be settled in this very general setting. Nevertheless, it is useful to bear in mind when assessing semantic skepticism about the natural numbers.

1.2 *The Argument*

In the *Foundations of Arithmetic* Frege complains of the scandalous state of our arithmetic knowledge that we are "unclear about the first and foremost of its objects..."[9] This unclarity is revealed by the vague and uninformative answers we are tempted to give to the question what is 1, or what does '1' refer to? According to Frege, this unclarity would be alleviated if we were in a position to evaluate identity statements of the form:

$$n = q$$

[9]Frege (1980), p.*I*.

where 'n' is a numeral and 'q' is a singular term that is neither a numeral nor of the form 'the number of so-and-so's.'

That the analysis of number should take this form for Frege is testimony to his belief that our number words function as unambiguous singular terms. If numerals didn't function in this way, there would be nothing to be discovered and, hence, nothing amiss in our arithmetic knowledge. If our number words didn't purport to refer uniquely, then how could the vague and uninformative answers we are tempted to give be an embarrassment? Given that Frege thought that evaluating such identity statements was a matter of conceptual *discovery*, an adequate analysis of number was conceived by him to be a hermeneutic task--a matter of uncovering what we meant all along as opposed to investing new meaning in our old way of speaking.[10] In "What Numbers Could Not Be" (henceforth, WNCNB), Paul Benacerraf argues that Frege's task was fundamentally misguided. That we are unable to give an adequate answer to Frege's question is no scandal for the simple reason that there is nothing to be known. Our number words don't function as unambiguous singualr terms--indeed, there is nothing denoted and, hence, nothing to be discovered. But as contrasted with the view that uniqueness is a genuine part of our pre-theoretic understanding of number, Benacerraf's argument is, as I have emphasized, a kind of *skepticism*.

[10]The terminology is John Burgess's, cf. Burgess (1990).

In chapter one I discuss and critically examine the argument of WNCNB. There are two components to Benacerraf's case. The first component consists of an argument that numbers could not be sets. Benacerraf argues that if, in an analysis of number, the numbers are identified with a progression of sets, then no *unique* such identification is warranted by the facts of usage. This argument can be extended to anything you like as long as you are not naively tempted to give the uninformative answer that Frege disdained--that 1 is simply itself and not another thing. The first component thus consists in a *general* argument against any reductive proposal identifying the numbers with a range of things "not already known to be the numbers."

The second component consists in a metaphysical argument that numbers could not be objects at all--that (sui generis) numbers, objectually understood, are *queer*. Benacerraf argues that, given a structuralist analysis, a face-value interpretation involves a conception of the natural numbers that violates what he contends is an a priori principle. According to Benacerraf, the difficulty with a face-value interpretation "lies in the fact that the 'elements' of the structure have no properties other than those relating them to other 'elements' of the same structure." The semantic skepticism of WNCNB is thus *impure*, in that its successful completion essentially involves a direct *metaphysical* argument for the non-existence of the natural numbers. It is this impurity that keeps Benacerraf's skepticism from succumbing to the quietism described in the last section.

The metaphysical argument crucially depends on the proper formulation of structuralism. There are two sorts of structuralist analyses: (i) those that directly quantify over the property of being a progression, and (ii) those that generalize in some suitable manner over progressions. Benacerraf's argument is formulated in terms of the former style of analysis. Unfortunately, as I argue in chapter two, structuralist analyses involving a special sort of entity, a *structure*, are in some deep sense *circular*. In order to properly assess the metaphysical argument, the structuralist analysis must be reformulated as generalizing over progressions.

How such an account should be formulated is the topic of chapter three. There I argue that given what Benacerraf contends are meaning-determining constraints on arithmetic usage and given a further feature of arithmetic discourse (that negation in the language of arithmetic is classical negation and that the surface grammar correctly represents negation as receiving wide-scope), some form of *modal structuralism* is a correct representation of arithmetic meaning. Modal structuralism is the view that arithmetic admits of a meaning-preserving reduction to a collection of generalizations about *what would be true if there were* progressions. According to the modal structuralist, arithmetic truth doesn't presuppose the existence and uniqueness of the natural numbers but, rather, the mere possibility of instantiating the structure of the number sequence.

I then reconsider Benacerraf's metaphysical argument explicitly reformulated in terms of modal structuralism. Unfortunately for the skeptic, I argue that the argument fails when so reformulated. I contend that *more* has been accomplished than a rational defense of the commitments of minimal platonism--I believe that at this point we have learned something important about the nature of arithmetic meaning. In particular, I endorse the apparent conclusion of chapter three--that some form of modal structuralism is a correct representation of the content of arithmetic.

But how can this be if our pre-theoretic platonism is to be retained? After all, the modal structuralist contends that there is a meaning-preserving *reduction* of arithmetic to a theory that doesn't seem to presuppose the existence of the natural numbers. Why doesn't the truth of modal structuralism establish that, contrary to appearances, we never were minimal platonists? In particular, why isn't modal structuralism a straightforward example of an *ontological reduction*? The ontological reductionist holds that if a theory or discourse is apparently commited to a range of controversial entities, the F's, but admits of a meaning-preserving reduction to a theory or discourse which is not apparently committed to the F's, then the original theory or discourse is not really burdened with such a commitment. If modal structuralism is a correct representation of what we meant all along by our arithmetic talk, then our naive view has been misdescribed as a kind of platonism.

The nature and limits of ontological reduction is the topic of chapter four. There I argue that a face-value interpretation of arithmetic can be squared with a modal structuralist analysis of its content. This involves denying certain assumptions about the nature of reference and ontological commitment made by the ontological reductionist. The resulting view about arithmetic I dub *modal platonism*. According to modal platonism, arithmetic may be interpreted at face-value while being conceptually equivalent to a collection of modal generalizations. The metaphysically significant distinction between (i) modality and (ii) the natural numbers (objectually understood) is understood to be genuine, but *facts* concerning the natural numbers are nonetheless identical with certain modal facts. There is a single domain of fact with two ways of representing it--as being composed of certain objects, the natural numbers, or as not involving the existence of progressions, but, rather, their mere possibility. The natural numbers are constituted by the distribution of certain complex modal properties, and facts about the natural numbers *just are* certain modal structural facts. If modal platonsim is coherent, structuralism has been misdescribed in the philosophical literature as an *alternative* to platonism. Rather, the best platonist analysis is a form of *structuralism*. Or so I shall argue. Finally, chapter five assesses the argument of this essay and discusses some of the consequences of modal platonism.

CHAPTER TWO

The Skeptical Argument

2.1 *The Setting*

If numbers are objects, which objects are they? This is a natural question and one that Frege took quite seriously. If an answer to this question isn't to be trivially devoid of content, we can't just say that the numbers are simply the numbers. If the question is *substantive* an appropriate answer must take the form of a reductive identification of the natural numbers. In "What Numbers Could Not Be", Benacerraf argues that to pose this question at all is to badly misconceieve how mathematical language functions--it presupposes, among other things, that our number words function as denoting singular terms. The task of the skeptical argument is to show that our number words designate nothing and hence that no reductive identification is possible.

2.2 *Why Numbers Could Not Be Sets*

The argument begins with a list of conditions claimed to be individually necessary but jointly sufficient for an adequate analysis of number (where the analysis purports to capture what we meant all along and takes the form of an explicit definition):

(i) *The structural constraint*: The candidate objects ought to form a progression.

(ii) *The cardinality constraint:* The cardinality relation ought to be suitably coordinated with the candidate progression--something like the following:

Nx(Fx) = n iff there is a 1-1 correspondence between the F's and the numbers preceding n

ought to be a provable consequence of the axioms governing the candidate objects and the proposed definitions;

I will explain these in turn.[1]

(i) An analysis of our arithmetic concepts in terms of set-theoretic (or other) notions ought *inter alia* to provide a definition of what it is to be the natural numbers, what it

[1]The author of WNCNB also required that the candidate progression be recursive. He has since recanted, cf. Benacerraf (1995).

is to be 0, and what it is for one number to be the successor of another. A successful reduction of number ought also to provide a deduction of the basic laws pertaining to these notions (from the set-theoretic or whatever laws and the proposed definitions):

(1) $\exists x\, N(x) \wedge x = 0;$

(2) $\forall x[N(x) \supset \exists y(N(y) \wedge y = s(x))];$

(3) $\forall x \forall y(N(x) \wedge N(y) \wedge x \neq y) \supset s(x) \neq s(y);$

(4) $\forall x[(N(x) \wedge \exists y(N(y) \wedge x = s(y))) \supset x \neq 0];$

(5) $\forall F[[F(0) \wedge \forall x[N(x) \supset (F(x) \supset F(S(x)))]] \supset \forall y(N(y) \supset F(y))]$

(where 'F' is a second-order variable). (1) states that 0 is a number; (2) states that every number has a successor; (3) states that distinct numbers have distinct successors; (4) states that 0 is not a successor of any number; and (5) states that mathematical induction holds for the natural numbers--that any property of 0 that also belongs to the successor of any number that shares that property is a property of all numbers. If we treat 'N', '0', and 'S' not as constants but as variables of the appropriate types ('N' will be a second-order predicate variable, '0' will be a first-order variable, and 'S' will be a second-order function variable), then these five conditions also suffice to characterize the concept of a *progression*. A progression *just is* any system of objects with a unique intial element and function that satisfies these five conditions.

Once we have definitions of number, 0, and successor that satisfy the five conditions characteristic of progressions, we may define 1 as the successor of 0, 2 as the successor of 1, and so on. By (2) every number so defined will have a successor, and by (3) such a number can't be any of the numbers already defined, and by (4) none of the defined numbers can be 0. By (5) all numbers will be in the series of successors, since 0 is part of this series and if a number belongs to this series then so does its successor.

Of course many concepts of pure arithmetic (such as the operation of addition) seem to be "basic" in the sense that we have an understanding of them that doesn't depend on anything outside our understanding of arithmetic and that this understanding suffices for knowledge of the laws governing these concepts. Presumably, then, the laws governing these concepts ought to be consequences of the proposed definitions and whatever axioms govern the candidate objects. Notice, however, that this isn't a *further* constraint. As is well known due to the work of Dedekind and Peano, the arithmetic operations can all be recursively defined in terms of the basic notions of number, 0, and successor; moreover given these definitions, the laws governing the arithmetic operations become deducible from the five conditions that characterize the concept of a progression.

(ii) The notion of a progression is necessary and sufficient to account for our concepts of *pure* arithmetic, but what about *applied* arithmetic--in particular our application of arithmetic in counting? Quine has denied that the cardinality constraint is a necessary condition on the analysis of number:[2]

> The condition upon all acceptable explications of number (that is the natural numbers 0,1,2,...) can be put...as succinctly as...: any *progression*, i.e., any infinite series whose members has finitely many predecessors--will do nicely. Russell once held that a further condition had to be met, to the effect that there be a way of applying one's would-be numbers to the

[2]Quine (1960), pp.262-263. Quine does, however, later endorse the recursiveness constraint cf. Quine (1986), p.403n.

> measurement of multiplicity: a way of saying that (1) There are *n* objects *x* such that *Fx*. This, however, was a mistake. For, (1) can be paraphrased as saying that the numbers less than *n* admit of correlation with the objects *x* such that *Fx*. This requires that our apparatus include enough of the elementary theory of relations for talk of correlation, or one-one relation; but it requires nothing special about numbers except that they form a progression.

Benacerraf sides with Russell against Quine in this matter. You can think of the structural and cardinality constraints (which are stated explicitly as conditions on objects) as codifying those aspects of our arithmetic usage that are genuinely meaning-determining. Now consider the stroke notation:

|, ||, |||, ||||,....

What number does '|' designate? Our natural response would be to say that '|' refers to 1 if only because the usual counting convention is that an n-membered set is represented by the concantenation of n strokes. But of course other counting conventions are possible. The cardinal number n of some set could have been represented by the concatenation of n+1 strokes in which case '|' would refer to 0. Counting conventions, understood as constraints on the application of our number words, thus play a meaning-determining role. In particular they provide a criterion of coreference: Two numerals are coreferential just in case they have the same position in the sequence of number words in virtue of their role in counting. If that's right, then Quine is wrong in not including an account of cardinality in an *analysis* of number,

With the structural and cardinality constraints in place, the skeptical argument can now be recast as a puzzle, a set of inconsistent claims with Benacerraf recommending that we abandon the one identifying numbers with sets:

(6) Numbers are sets;

(7) If numbers are sets, exactly one progression of sets is the natural numbers;

(8) There are indefinitely many progressions satisfying the list of adequacy conditions;

(9) There could be no reason for identifying the natural numbers with any particular progression of sets;

(10) If numbers were sets, we could have reason to believe a particular progression to be the natural numbers.

(6) we have by hypothesis, and (8) enjoys the status of a mathematical result. If numbers are sets, then by (7) there must be some particular progression of sets which the numbers are, and by (10) we could have a reason for identifying the numbers with this particular progression. But by (8) there are indefinitely many progressions satisfying the structural and cardinality constraints; and if the constraints really are individually necessary and jointly sufficient, then by (9) there could be no reason for identifying the numbers with a particular progression. We have a contradiction.

Someone wishing to deny the soundness of the argument will thus have to make at least one of the following objections:

A. Deny the joint sufficiency of the adequacy conditions;
B. Deny (7);
C. Deny (9);
D. Deny (10).

I will discuss these in turn.

A. Benacerraf's skeptical argument turns not on the *necessity* of the stated conditions but on their joint *sufficiency*. Concerning their necessity their has been wide spread non-collusive agreement among mathematicians and philosophers alike.[3] What is perhaps novel and is the linchpin of the skeptical argument is Benacerraf's suggestion that they are jointly *sufficient*. The adequacy conditions codify aspects of our usage that are taken to be exhaustively meaning-determining. That they are meaning-determining guarantees their necessity, that they are exhaustively so guarantees their sufficiency. A natural rejoinder to Benacerraf's argument would be to deny the joint sufficiency of the conditions and claim that there is some further, heretofore overlooked, aspect of our usage which taken together with the stated conditions really are jointly sufficient. In order for such an objection to make good, it would have to be shown that this extra condition provides us with a reason for identifying numbers with particular sets. The problem is that it's difficult to imagine what further aspect of our usage we could appeal to. What about our usage would determine, for instance, that 2 is a member of 3? Benacerraf concludes that nothing would do:[4]

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[3]With the notable exception of Quine, as remarked above.

[4]WNCNB p.285.

> There is no way connected with the reference of the number words that will allow us to choose among them, *for the accounts differ at places where there is no connection whatever between features of the accounts and our uses of the words in question*.

This isn't, of course, an argument, nor does Benacerraf provide one. This is only a minor deficiency, though, since the argument can be readily recast as a skeptical *challenge*: Find a further aspect of our usage which will uniquely settle which sets the numbers are, or give up the idea that numbers are sets.

B. If numbers are sets, they must be particular sets. This apparent truism may be questioned by a determined set-theoretic reductionist. Consider the vagueness associated with names for ordinary material objects. Our concept of mountain is vague in two ways. Familiarly, it admits of borderline cases. There are mountain-like hills, for instance, that are neither determinately mountains nor determinately not mountains. But Quine has observed that 'mountain' is also vague in a distinct sense.[5] Our use of 'Mount Ranier,' for instance, doesn't settle which of the many overlapping mountain-like regions of matter Mount Ranier

[5]Quine (1960), p.126.

really is.[6] Consider two such precise regions which differ only with respect to some very small amount of matter, say, a handful of elementary particles, but otherwise overlap. There is no saying which is the better candidate for being Mount Ranier. This form of vagueness is probably best understood as a species of semantic indecision: we never decided, and probably never could, which of the many overlapping regions uniquely deserves the name 'Mount Ranier.'

Denying this form of vagueness has disastrous consequences. It leads invariably to either a kind of *dualism* or to *eliminativism*. Let me explain. Someone might claim that Mount Ranier is not to be identified with any of the mountain-like regions but is, instead, some other thing. Perhaps Mount Ranier is not any of the original candidates

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[6]The candidate regions of matter we are considering are *determinate* regions (that may be represented by sets of spacetime points if you like). There is a vagueness involved in the notion of a *mountain-like* region of matter in that the notion of being mountain-like admits of borderline cases. Certain regions of matter will be definitely mountain like, others definitely not mountain like, and others still, not definitely mountain-like and not definitely not mountain-like. This doesn't mean that any of the regions fail to be determinate. Even the regions stranded on the penumbra can be determinate regions exhaustively represented by a set of spacetime points--it is just that these regions aren't in the extension or anti-extension of 'mountain-like'. But suppose that Mount Ranier is definitely a mountain, that is, it is well within the extension of our concept of mountain, then the precise candidate regions of matter are only those that are themselves definitely mountain-like, and the vagueness associated with the notion of being mountain-like won't then affect the present discussion.

but is *constituted* by them (where constitution is understood as something other than mereological composition). Or perhaps the vagueness here is not a form of semantic indecision but is located *in re*--Mount Ranier would be conceived to be a vague object that fades away accross the penumbra of candidate regions.[7] Or perhaps Mount Ranier is an immaterial entity that somehow supervenes on the mountain-like regions of matter. The problem with dualism is that any of the proposed entities are either no better than the original candidates or they are worse off. Surely a precise mountain is as good a candidate as a vague mountain, and supervenient immaterial entities are surely worse candidates. Someone apprised of the difficulties facing dualism might heroically deny that Mount Ranier is any of the available candidates--that Mount Ranier, strictly speaking, doesn't exist. Such heroism is misplaced, however. I believe it is more sensible to accept, with Quine, this second form of vagueness.[8]

But notice, if we do, we are committed to Mount Ranier being a mountain-like region of matter without there being a

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[7]There is a natural slide from "Mount Ranier is a *particular* region" to "Mount Ranier is a *determinate* region." If, however, talk of vague objects is consistent, then these notions should be kept apart.

[8]For discussion, see Unger (1980) and Lewis (1991).

particular mountain-like region that Mount Ranier really is. There is no mystery here, as this is simply a consequence of a benign and pervasive form of semantic indeterminacy. But now why accept this principle for numbers when it fails for material objects?[9] The determined set-theoretic reductionist could claim that our concept of number is similarly vague, and hence that while numbers are sets there are no particular sets which the numbers are. This form of set-theoretic reductionism is novel in that eschews the traditional program of explicit definition; but it is still a form of reductionism in that the arithmetic facts are conceived to be nothing over and above facts about sets.[10] Benacerraf's conclusion that numbers aren't sets would be avoided, but not much has been done to forestall the principal skeptical thrust of Benacerraf's dialectic--the uniqueness thesis has also been disavowed by such a theorist.[11]

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[9]This is a version of an objection originally due to Crispin Wright, Wright (1983), pp.125-127. One difference in how I've presented the objection is that Wright appeals to Quine's controversial argument for the indeterminacy of translation thus requiring some nuances in stating the objection in order to avoid a commitment to radical Quinean indeterminacy. By appealing to the vagueness of names for material objects, such difficulties are avoided.

[10]A related view will be discussed in the next chapter under the description "part-time structuralism."

[11]Recall, I am conceiving of vagueness as a form of semantic indecision, a kind of systematic ambiguity. It is not the case that reference is uniquely secured to a *vague*

I'm not sure how stable this position is. Numbers are conceived to be determinately sets without there being particular sets which the numbers determinately are. But what sort of justification can one provide for the claim that numbers are determinately sets and determinately not any other thing? Once one admits this much indeterminacy to our concept of number, I'm not sure that any reason for identifying numbers with sets will be forthcoming. Suppose, for instance, you believe that numbers are sets because in your analysis of number, numbers are identified with a particular sequence of sets. If we give up the idea that numbers are particular sets what reason is there to believe that numbers are sets at all? What in our use of arithmetic vocabulary settles that the candidate progressions are progressions of sets? The necessary and sufficient conditions on an adequate analysis codify those aspects of our usage which are taken to be meaning-determining. But being a progression and suitably coordinating the cardinality relation in no way settles that candidate progressions are progressions of *sets*. Suppose that there are countably many stars, and suppose that they stand in some natural relation to one another which induces a well-ordering on them. If we adopt the appropriate counting conventions, what is there to

progression, where vagueness is understood *in re*.

rule out such a progression as a candidate? If this is right, then numbers aren't determinately sets nor are they determinately not sets. The only way a set-theoretic reductionist can avoid this slide is by locating some *further* aspect of our use of number words which settles that their referents are sets. This amounts to denying the joint sufficiency of the stated adequacy conditions, and the problem is that there just doesn't seem to be any plausible additional condition that could be enjoined to yield joint-sufficiency with the desired result.[12]

C. Another way to resist the argument is to deny (9), i.e., deny that there is no reason to identify the numbers with a particular progression of sets. The plausibility of (9) rests with the joint sufficiency of the structural and cardinality constraints. If they really are individually

[12]The set-theoretic reductionist may take heart in the fact that there seems to be an impressive distinction between our attitude toward the claim that 17 is a certain set, and our attitude toward the claim that 17 is a certain star. Whether we can justify this or not, it is a feature of our ordinary (educated) understanding. Whether this can be made to do any philosophical work is another matter. This view we have does not seem to show up anywhere in the real-life uses of mathematics; and I suppose one could argue (though I would not like to do so) that only those aspects of our pre-theoretic understanding that our manifest in real practice play a meaning-determining role. Once again this is only a minor difficulty for Benacerraf's case, since the argument can be recast as a challenge: Find some *justifiable* feature of our pre-theoretic understanding that settles that numbers are sets, or abandon set-theoretic reductionism.

necessary *and* jointly sufficient conditions, and if by (8) they fail to uniquely pick out a progression of sets as being *the* natural numbers, then there seems to be nothing that would count as a reason for making such an identification. There is a prima facie puzzle, then, about how one can deny (9) without also denying the joint sufficiency of the structural and cardinality constraints.

The proper resolution is not to claim that there is a further constraint to which our arithmetic usage is subject, but rather that the cardinality constraint, despite appearances, really does provide us with a reason to identify the natural numbers with a unique progression. A common reaction to the skepticism of WNCNB is that Benacerraf has failed to fully appreciate the effect of our cardinal employment of the number words--that careful reflection on the concept of cardinality will reveal the uniqueness of the natural numbers. Very often this is expressed quite glibly. Here is Michael Dummett:[13]

> [W]hat is constitutive of the number 3 is not its position in any progression whatever, or even in some particular progression, nor yet the result of adding 3 to another number, or multiplying it by 3, but something more fundamental than any of these: the fact that, if certain objects are counted 'One, two, three', or equally, 'Nought, one, two', then there are 3 of them.

[13]Dummett (1991), p.53.

> The point is so simple that it needs a sophisticated intellect to overlook it.

To be sure, if certain objects are counted 'one, two, three,' then there are three of them--who would deny it? What this *means,* however, is a more controversial matter. I will examine an argument of Russell's that, I believe, articulates the intuition gestured at by Dummett.

In the *Introduction to Mathematical Philosophy*, Russell contends that the structural constraint, though a necessary condition, is not sufficient--our cardinal application of arithmetic must also be taken into account. Russell *further* argues that our counting practices require that there be a *unique* progression to which our number words refer:[14]

> [We] want our numbers to be such as can be used for counting common objects, and this requires that our numbers should have a definite meaning, not merely that they should have certain formal properties.

Russell appeals to the following fact about cardinality, which I'll call the *counting principle*:

(11) Nx(Fx) = n iff there is a 1-1 correspondence between the F's and the natural numbers preceding n.

[14]Russell (1912), p.10.

(recall we are including 0 in the sequence of the natural numbers). Consider the following progression:

100, 101, 102, 103, 104, ...

If the structural constraint *were* sufficient, this progression would would be as good a candidate as any other. Here, 'zero' refers to 100, and the natural numbers will be the smallest set of numbers including 100 and closed under successor. While Russell admits that this progression satisfies the axioms, he argues that it is an inadequate interpretation of the natural numbers since it gives the wrong "allowance of fingers, eyes, and noses."[15] After all, we each have two eyes, not one hundred and two. So given the counting principle, this progression is inadequate for determining the cardinal number of finite sets.

Two lines of resistance are available. One could either attack the model of counting given in the counting principle, or one could question whether Russell had succeeded in specifying the offending progression in a non-question-begging fashion.

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[15]Russell (1912), p,9.

Benacerraf could question the counting principle as it stands, since in making reference to *the* natural numbers it begs the question against someone who denies that numerals are unambiguous names. In its stead consider the following reformulation (call it the *structural counting principle*):

(12) Nx(Fx) = n iff for any progression p, there is a 1-1 function from the F's onto the $\text{numbers}_p <_p n_p$.

A skeptic about uniqueness is well placed to accept such a principle. Roughly speaking, numerals will be correlated with positions in a progression without the numbers being identified with a particular progression. Just because the sequence beginning with 100 satisfies the structural condition, doesn't mean that we are thereby licensed to claim that each of us has 102 eyes. Such a claim is simply not a consequence of the structural counting principle.

Alternatively, one could, instead of questioning the counting principle, question the meaning Russell assigns '102' in his example. Consider the sequence Russell offers as the representation of the natural numbers:

100, 101, 102, 103, 104,...

A skeptic about uniqueness could claim that '102' in this sequence of number words doesn't mean what '102' conventionally means in our standard representation of number. Rather, since it occupies the second position in the progression of number words ('100' occupying the zeroeth), and given the counting principle, '102' refers to what we conventionally refer to by '2' (as understood by the skeptic--'2' doesn't conventionally refer in a determinate fashion to some special platonic entity). So the charge that such a progression isn't adequate for the cardinal number of finite sets turns on an equivocation. Such a theorist would contend that Russell confuses the matter by giving as an example what is, *by Russell's lights*, an unintended model of the natural numbers a sequence specified in terms of the conventional number words omitting the first one hundred. This is the crucial move in the conjuring trick. Had Russell specified a progression without recourse to numerical vocabulary the objection would have initially seemed less compelling. It would have simply become the legitimate further demand that the cardinal employment of our number words must also be accounted for.

It would seem that the prima facie difficulty with denying (9) was genuine. The problem was this: If the structural and cardinality constraints really are individually necessary and jointly sufficient, and if by (8) they fail to uniquely pick out a progression as being the natural numbers, then there could be no reason for making such an identification. Denying (9) thus requires denying the joint sufficiency of the adequacy conditions.[16]

[16]Someone sympathetic to Russell's argument might concede this much but claim that the cardinality constraint doesn't capture all that there is to our concept of cardinality. Indeed some have urged that a further *intensional* constraint must be met if an analysis is to correctly represent our pre-theoretic concept of number (cf. Wright (1983), and Hale (1987)). The idea is this: There is an order of priority that obtains among our ordinary arithmetic concepts--in particular, our concept of cardinality is somehow conceptually prior to any ordinal notions. If the order of our definitions can adequately represent facts about conceptual priority, then the successor function under which a candidate system of objects forms a progression must be defined in terms of the cardinality relation. Call this the *priority constraint*. There is a puzzle about how the priority constraint could help in the present context. To defeat skepticism about uniqueness any proposed further constraints must, in conjunction with the structural and cardinality constraints, determine exactly one progression as being the natural numbers. But notice, *the very same progression* may be definied either by beginning with the successor function or by beginning with the cardinality relation, and both definitions would be, in some suitable sense, mathematically equivalent. What connects this intensional claim with the present ontological concern is that the priority constraint is supposed to be a consequence of the *criterion of identity* for the natural numbers: $Nx(Fx) = Nx(Gx) \equiv Eq(Fx,Gx)$ (where equinumerosity is understood in terms of 1-1 correspondence). Different systems of objects will have different citeria of identity and somehow this will help us sort out which progression really is the natural numbers. This strikes me as little more than a willful conflation of the intensional with the extensional. However since the principle advocates of such an approach deny that the priority constraint will, after all, secure uniqueness (Wright, for instance, claims that any equivalence class under a relation R for which equinumerosity is a congruence will satisfy the criterion of identity for numbers),

D. Someone might object that no realist ought to accept (10)--the premise that if numbers were sets, then we could have a reason for identifying the numbers with a particular progression of sets. Suppose (10) is believed because it is an instance of a general a priori principle connecting reasons with existence (such as the principle of sufficient reason). If mathematical truth is thought to enjoy an objectivity that transcends our practice of fixing mathematical opinion, then belief in this form of mathematical objectivity is inconsistent with the grounds for believing (10). Of course, this conception of mathematical objectivity is not without its critics, especially among constructivists; but the objection retains some point. Its effect is to conditionalize the acceptance of Benacerraf's conclusion on the acceptance of (10)--which itself depends on independent philosophical commitments, thus considerably weakening the skeptical force of the argument.

On reflection I'm not really sure that (10) is inconsistent with a sufficiently robust mathematical realism. In particular, I'm not sure that the plausibility of (10) rests with the fact that it is an instance of some general anti-

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detailing my misgivings about "criteria of identity" will take us too far afield.

realist principle. Consider someone who thought that vagueness was, at bottom, an *epistemic* deficiency. Such a theorist might hold, for instance, that there is a number n such that someone is bald just in case they have fewer than n hairs. The vagueness attaching to our concept of baldness simply consists in the fact that we can never discover what number n is.[17] The meaning of vague expressions is so-conceived that there is an a priori guarantee that we can never discover the full extent of their extensions.

I contend that even a full-blooded realist could find such a conception of vagueness crazy. One need not violate any principle of global realism to deny local matters of fact where there are none. One can be misled by an over-zealous adherence to the realist principle that truth may outstrip assertability and conclude that it must always do so. Thus Paul Horwich writes:[18]

> Thus we are allowing that the predicate 'is a heap' has an extension....True, we could not, even in principle, discover the extension. In particular, we could never know the fact of the matter as to whether our little pile is a heap. Such knowledge is precluded by the very meaning of the word--by its being vague. But why should this be thought odd or implausible? *It is surely only*

[17]This is the position Paul Horwich takes in Horwich (1990), pp.81-87.

[18]Horwich (1990), p.84.

> *the lingering seductiveness of verificationism--an inclination to hold that the existence of fact requires the conceivability of knowing it--that gives rise to discomfort with this situation.* [my emphasis]

To be sure, any direct *inference* from the fact that the conditions of applicability governing the predicate 'bald' aren't determinate for a range of cases to the conclusion that the extension and anti-extension of 'bald' don't exhaustively partition the range of candidates *will* offend realist scruples; but it *is* consistent with realism that 'bald' lacks determinate applicability conditions *because* the predicate lacks an extension and anti-extension which exhaustively partition the range of eligible items. Suppose that associated with a vague predicate is a linguistic rule that determines that expression's extension and anti-extension that doesn't exhaustively partition the range of eligible items. There's nothing mysterious or particularly anti-realist about the conditions of applicability for that predicate not determining whether or not that predicate should apply to an item not in the extension or anti-extension--we are faced with semantic indecision because the linguistic rule governing the predicate is silent on whether the given thing should be in the extension or anti-extension. Given the meaning of the predicate there just is no fact of the matter. The problem with the epistemic conception of vagueness is that there just doesn't seem to be any way for vague terms to come to have the meaning they are supposed to have on such a conception. Consider the predicate 'bald.' According to Horwich, there is a definite number n such that if someone has fewer than n hairs then that person is bald--it is just that knowing what number n is is precluded by the meaning of 'bald'. What about our use of this predicate insures that we don't mean 'shbald', for instance, where the conditions of applicability for 'shbald' are precisely the same as 'bald' and where 'shbald' functions semantically just like 'bald' except that the relevant number of hairs is n+1? We couldn't do it by explicit stipulation: that would require explicitly knowing the full extension of the predicate--something which is supposed to be precluded by the very meaning of the expression. The epistemic conception of vagueness is incredible because the underlying theory of meaning is, and one need not be a verificationist (or Dummettian anti-realist, or internal realist, or what have you) to reject the underlying conception of content.

One can even point to the phenomenon of precisification to argue for this latter perspective. Predicates that admit of borderline cases can also be made more precise--our use of a vague predicate can be extended such that the range of items formally stranded on the penumbra is *narrowed*. Very often in establishing the use of a predicate we never decided whether the predicate should determinately apply or not to a range of cases--perhaps because such cases were well outside our practical concern, or perhaps because the utility and point of such a predicate essentially involves its vagueness. But due to future contingencies our interests can change, or new concerns may arise such that we gain a practical reason for extending our usage. Often there can be competing practical concerns in precisifying a concept that dictate different precisifications. Such cases are patently conventional in character and belie any suggestion that precisification is a matter of *discovery*.

One can be a realist and deny that the phenomena of vagueness is a kind of epistemic deficiency. One need not abandon any realist scruples to find such a view incredible. The point that I have been pursuing is that denying (10) is analogously absurd, and that a commitment to mathematical realism isn't necessarily undermined by its acceptance. To believe that numbers are particular sets even though we could never discover which sets the numbers are, is to be burdened with the mystery of how our number words ever came to refer to these elusive sets; and the residue of this mystery remains even after embracing mathematical realism.[19]

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[19]I don't mean to be claiming that any proposal of the form 'S means that p' must include some account of the fact in virtue of which S means that p; rather, I contend that no

Thus far I have reviewed some main objections to Benacerraf's argument that numbers could not be sets. Though I don't take myself to have decisively refuted any of them, each has emerged, at best, as curiously inconclusive. Though doubtless more could be said in favor of Benacerraf's case, I find the considerations advanced compelling and thus endorse the conclusion that numbers are not sets.

Before moving on to the argument that numbers could not be objects, I want to make a remark about the *generality* of the considerations advanced against set-theoretic reductionism. Notice that Benacerraf's argument can be extended to anything you like simply by substituting an appropriate general term for 'set' in the argument. One can argue, for example, that numbers aren't Roman emperors. Consider the following series:

Julius Caesar, Octavian Augustus, Tiberius, Gaius Caligula, Claudius, Nero,...

Suppose that the Roman empire had never ended, and that it never will, and that the future holds no dead dark stars for

meaning ascription should render unintelligible that our usage could have the ascribed content.

this world, so that we never run out of Roman emperors. The argument could now proceed thus:

(13) Numbers are Roman emperors;

(14) If numbers are Roman emperors, exactly one progression of Roman emperors is the natural numbers;

(15) There are indefinitely many progressions of Roman emperor's satisfying the set of conditions;

(16) There could be no reason for identifying the natural numbers with any particular progression of Roman emperors;

(17) If numbers were Roman emperors, we could have reason to believe a particular progression to be the natural numbers.[20]

[20]That Benacerraf's argument can be so-extended belies any suggestion that his skepticism does anything to further the negative program of nominalism in the philosophy of mathematics. The problem has nothing whatsoever to do with the *abstractness* of mathematical entities. As the above example nicely illustrates, the problem arises even when we restrict our attention to progressions of concreta. While Benacerraf's argument won't *directly* motivate belief in nominalism, it might, however, provide an *indirect* motivation--if, for instance, the best way of working out our post-skeptical intuitions issues in a structuralist analysis that is nominalistically adequate. Even this would be a limited result. Nominalism would be refuted by demonstrating the existence of a single abstract entity. If arithmetic structuralism is nominalistically adequate, this would only establish that no such counterexample would be forthcoming from arithmetic.

Benacerraf's considerations are perfectly general--they appeal to no properties specific to sets. Benacerraf has, in effect, provided a *recipe* for arguing against any reductive identification of the natural numbers.

In order to reach the more radical conclusion that numbers could not be objects at all, Benacerraf would have to do one of two things. He could argue that possible objects must belong to one of a range of types and argue, as above, for each type of thing, that they could not be the numbers--a daunting task. Such an argument would only be as credible as the putative partition of possible objects, and a natural worry would be that one type of object has been left out of account, namely the natural numbers. Perhaps the numbers aren't sets (or Roman emperors, or whatever), but rather, quite simply, themselves. The situation is similar to Kripke's reconstruction of the rule-following considerations.[21] According to Kripke's reconstruction, the proper conclusion of the rule-following considerations is that there are no semantic facts. The argument for content irrealism proceeds by identifying a necessary condition on putative meaning facts: That they ought to determine correctness conditions governing the use of some symbol--the

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[21]Kripke (1982).

so-called "normativity constraint." A range of facts are then proposed as potential meaning facts (the past finite history of the use of a term, phenomenal facts, dispositional facts), and it is argued that each violates the normativity constraint. Once again a doubt can be registered whether the range of candidates is *exhaustive*.[22] Perhaps in addition to phenomenal properties, dispositional properties, etc. there are semantic properties as well; and similarly, the suggestion here is that perhaps in addition to sets, Roman emperors, etc., there are natural numbers as well.

There is another difficulty with extending the anti-reductionist argument in this way. Earlier I argued that Benacerraf's conclusion that numbers could not be sets could be resisted by abandoning the apparent truism that if numbers were sets they must be particular sets. The idea was that our number words are highly ambiguous--our usage doesn't settle which particular sets the numbers are. The difficulty with this line of resistance is that the same considerations that lead one to abandon the claim that numbers must be a particular sets will also militate against the idea that numbers are determinately sets and determinately not any other thing. If the structural and cardinality constraints are really individually necessary and jointly sufficient,

[22]Cf. Boghossian (1989a).

then abandoning uniqueness merely establishes that numbers are, at best, not determinately sets and not determinately not sets.

This objection, while an inadequate defense of set-theoretic reductionism, fares better in the more general setting. Benacerraf's negative ontological conclusion that numbers could not be objects, can be resisted simply by abandoning uniqueness. Once again our number words would be conceived to be highly ambiguous, but the claim that their possible designata are exclusively sets has been abandoned. Notice, however, just as the eliminativist conclusion is unwarranted if numbers are *sui generis*, so too is the claim that there is no determinate identification of the natural numbers. The items "not already known to be the numbers" among which the number words supposedly divide their reference, are all *imperfect* candidates. They each have necessary features that are no part of our concept of number. The nice thing about *sui generis* numbers, if there are any, is that their necessary and sufficient conditions are exhausted by the structural and cardinality constraints and thus would have best claim to being the natural numbers.

The strategy that Benacerraf actually pursues is importantly different. He argues that nothing *could* satisfy the necessary and sufficient conditions governing our concept of number--that *sui generis* numbers are *queer*.

2.3 *Why Numbers Could Not Be Objects*

Benacerraf writes:[23]

> Therefore, numbers are not objects at all, because in giving the properties (that is, necessary and sufficient) of numbers you merely characterize an *abstract structure*--and the distinction lies in the fact that the "elements" of the structure have no properties other than those relating them to other "elements" of the same structure. If we identify an abstract structure with a system of relations (in intension, of course, or else with the set of all relations in extension isomorphic to a given system of relations), we get arithmetic elaborating the "less-than" relation, or of all systems of objects (that is, *concrete* structures) exhibiting that structure. That a system of objects exhibits the structure of the integers implies that the elements of that system have some properties not dependent on structure. It must be possible to individuate those objects independently of the role they play in that structure. But this is precisely what cannot be done with the numbers. To *be* the number 3 is no more and no less than to be preceded by 2, 1, and possibly 0, and to be followed by 4, 5, and so forth. And to *be* the number 4 is no more and no less than to be preceded by 3, 2, 1, and possibly 0, and to be followed by....*Any* object can *play the role of* 3; that is, any object can be the third element of some progression. What is peculiar to 3 is that it defines that role--not by being a paradigm of any object which plays it, but by representing the relation that any third member of a progression bears to the rest of the progression.

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[23]WNCNB.

The first sentence of this passage announces the structure of the argument. The initial clause states the conclusion: "Therefore, numbers are not objects at all..." There are two components of the argument leading to this conclusion: (i) a structuralist component consisting in the claim that the necessary and sufficient conditions of number "merely characterize an abstract structure," and (ii) a metaphysical component consisting of an argument that no system of objects could satisfy the conditions of the structuralist analysis. According to Benacerraf the difficulty with conceiving of numbers as objects "lies in the fact that the "elements" of the structure have no properties other than those relating them to other "elements" of the same structure." The second sentence elaborates the structuralist analysis, and the remainder of the passage is devoted to the metaphysical objection.

Once the structuralist analysis is in place, the metaphysical objection comes into play. The structure of Benacerraf's argument is thus similar to Mackie's argument from queerness.[24] Mackie argues that our concept of the good requires both that moral properties be independent of us and that there be an a priori connection between the good and our will, but how can this be? Moral properties are "queer" (or

[24]Mackie (1977).

would be if there were any) and best not believed in. Similarly Benacerraf is arguing that not only is it a necessary condition for our concept of number that the numbers form a progression and have a suitably coordinated cardinality relation, but it is a sufficient condition as well. The necessary and sufficient conditions of number only suffice to characterize the *structure* of the number sequence, and the difficulty is that it's impossible to maintain this together with the idea that numbers are objects.

Benacerraf's argument is not without its antecedents. In the *Principles of Mathematics* Russell presents a similar objection to Dedekind's conception of the natural numbers:[25]

> [I]t is impossible that the ordinals should be, as Dedekind suggests, nothing but the terms of such relations as constitute a progression. If they are anything at all, they must be intrinsically something; they must differ from other entities as points from instants, or colors from sounds...What Dedekind presents to us is not the numbers, but any progression....

Objects must have intrinsic properties, but the properties of number, on Dedekind's analysis, are purely relational. So it is improper to suppose that Dedekind has given an account of the natural numbers objectually understood, so much as an account of the general properties of progressions.

[25]Russell (1903), p.249.

The ontological anxiety expressed by Benacerraf comes down to this: A number's identity consists in the relations it bears to the other members of the number sequence--each of whose identity in turn consists in the relations they bear to every other number. Just as part of what it is to be 3 is to be succeeded by 4, part of what it is to be 4 is to be preceded by 3. It would be one thing if the defining characteristic of number consisted in relations numbers bore to things whose existence we had an independent grasp of (thus Benacerraf's insistence that if numbers are objects they should have properties not dependent on structure); but does it make sense to think that numbers depend on relations they bear to things whose nature similarly depends on relations to the other members of the number sequence? It is not just the relational character of numbers, but the ungroundeness for the totality, which casts doubt on their being.[26]

Notice this about the argument. Benacerraf isn't claiming that there is a meaning-preserving reduction of talk of the natural numbers to talk of progressions in general and

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[26]Benacerraf's worry is thus more specific than Russell's. While Russell's doubt was directed at the alleged relational character of number he wasn't worried about ungroundedness, per se.

hence that a commitment to the natural numbers is only apparent. The difficulty with an objectual interpretation of arithmetic talk is rather that, so-interpreted, "numbers" would violate the ungroundeness principle.

The argument can be recast as an inconsistent set of premises with Benacerraf recommending that we deny that numbers are objects.

(18) Numbers are objects;

(19) Our concept of number only suffices to characterize a structure--it only determines the formal relations the numbers bear to one another;

(20) The identity of a given number depends on the relations it bears to every other number;

(21) *Ungroundedness*: There could be no system of objects such that the identity of a given element depends on the relations it bears to every other element of the system.

(18) we have by hypothesis. By (19), our concept of number only characterizes a structure. From (18) and (19), (20) follows--that the identity of a given number depends on the

relations it bears to every other number. But by (21) there could be no such system of objects. Once again we have a contradiction.

In order to evaluate the argument we must first get clearer about the structuralist component, (19). This is necessary in order to evaluate whether (20) is really a consequence of (19). Benacerraf briefly elaborates the structuralist component in the second sentence of the passage:

> If we identify an abstract structure with a system of relations (in intension, of course, or else with the set of all relations in extension isomorphic to a given system of relations), we get arithmetic elaborating the "less-than" relation, or of all systems of objects (that is, *concrete* structures) exhibiting that structure.

Two different kinds of accounts are being offered here. The structure of the number sequence is identified with either (a) a particular system of relations taken in intension or with (b) the set of all relations taken in extension isomorphic to a given system of relations. Both accounts quantify over the property of being a progression as opposed to generalizing in some way over progressions. In the absence of a theory of intensions, the first account is hopelessly unspecific. Unfortunately, the second account, while marking an advance over the first in terms of specificity, is also inconsistent with standard set theory.

For any arbitrary rank, there will be some set which is in the field of the isomorphic function--which means that the set of all relations isomorphic to a given system of relations would have to be of unbounded rank. But on the iterative conception *there are no sets* of unbounded rank--such sets are explicitly disavowed in the quest for freedom from the set-theoretic antinomies.

There is another set-theoretic account of structure familiar from model-theory. Structures are identified with n-tuples of sets whose elements include a domain and functions and relations on the domain which satisfy certain conditions. There could be a problem, however, depending upon how broad the structuralist's ambitions are. If structuralism is supposed to provide a *global* account of mathematics, i.e., one that encompasses set-theory itself, then the set-theoretic account of structure is circular. Moreover it seems plausible that the kind of considerations prompting arithmetic structuralism will motivate structuralist analyses elsewhere. What is required by an adequate structuralism is an independently understood conception of structure--independent, that is, of the mathematical theories to be analyzed.

The difficulties with the set-theoretic account of structure provides us with a motivation for taking a closer look at identifying a given structure with a system of relations taken in intension. There are three obvious accounts: set-theoretic constructions from possibilia; Australian style *real* relations; and minimalist "shadows" of polyadic predicates. I will explain these in turn. Doubtless more accounts are possible (indeed, are on the market), but, as I will argue below, the difficulties with deploying these in the course of giving a structuralist analysis are, in the end, perfectly general.

We can pass over the first style of account (where n-adic relations are identified with sets of n-tuples of possibilia) since it shares the circularity problem with set-theoretic account of structure. The second account is more promising in this regard. I have in mind something like D.M. Armstrong's theory of immanent universals in *Universals and Scientific Realism*[27] Relations are understood to be polyadic immanent universals.[28] Universals are multiply located entities wholly present wherever instatantiated. Moreover

[27]Armstrong (1978).

[28]Cf. Armstrong (1978), pp.68-74. I should emphasize, however, that Armstrong is *not* a structuralist even if his metaphysics provides one with the resources for a structuralist analysis.

they are non-spatiotemporal parts of whatever particular instantiates them. Something is a spatiotemporal part of another thing just in case they share spatiotemporal parts in common. Universals, on the other hand, occupy the *whole* of the spatiotemporal region that the particular occupies. They are *immanent*, as opposed to *transcendent*, in the sense that they exist just in case they are instantiated. There is another feature of immanent universals worth mentioning. Our casual talk of properties and relations is comprised of a variety of different conceptions of what properties and relations are. One difference among these conceptions is whether properties and relations are *abundant* or *sparse*. Abundant relations may be external (as opposed to internal), i.e., their instances can be as gruesomely gerrymandered as the extension of any bent predicate. And if similarity is conceived to be the sharing of abundant properties, then the notion of similarity is trivial--in this sense, everything is similar to everything else in indefinitely many ways. Immanent universals, in contrast, are sparse: An inventory of the world's universals would provide a minimal supervenience base for the qualitative character of the world. Their sparseness makes them attractive candidates for certain theoretical tasks such as providing analyses of natural properties, duplication, and cognate notions.

I believe that the theoretical utility of immanent universals provide us with the best reason for believing in them.[29] But notice if the inventory of the world's universals provides us with a minimal supervenience base for the qualitative character of the world, if *that's* what sparseness is, then coming up with such an inventory is an *empirical* endeavor. Universals, so-conceived, have no a priori existence assumptions; and given that mathematical belief is a priori, mathematical structures cannot be comprised of *universals*. Of course, even empirical inventories have a priori constraints--the list of the world's bachelors will contain no husbands for instance. There may be a priori constraints on the *kinds* of empirical things, but there can't be a priori constraints on the *number* of empirical things. And if there are finitely many empirical existences, then there is no *immanent* universal of being a progression. Theorists who believe in universals, such as Armstrong, often believe in a less metaphysically loaded sense of properties that corresponds to the more abundant aspect of our casual talk of properties and relations: "We...can countenance *any* true relational description of an entity as yielding a property of an object, and a state of affairs in which that

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[29]This view is advanced by David Lewis in Lewis (1984). Lewis' official position is that though talk of universals is consistent, judgement should be suspended on their existence.

object figures."[30] "Second class" properties are abundant, but the distribution of second class properties is taken to supervene on the more fundamental distribution of immanent universals. Given the a priori character of mathematical existence claims, the property of being a progression (and mathematical structures generally), must be understood in this more abundant, if less robust, sense of property. Given this observation, the second account of relations reduces, at least in the mathematical case, to the third.

We finally come to the metaphysically unambitious *minimalist* conception of relations. According to the minimalist conception, whenever we have a meaningful polyadic predicate '$R(x_1,...,x_n)$' we may harmlessly speak of the relation R. The transition is supposed to be as unproblematic as our unreflective transition from 'It is true that p' to 'There is a fact that p.' Relations will be individuated by equivalence classes of predicates. Thus two predicates will express the same relation just in case they are synonymous; or perhaps some weaker relation will be invoked. There will be at least as many relations as there are polyadic predicates; but given the inevitable expressive limitations of language, the number of relations must surely

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[30]Armstrong (1991), p.198. This view represents a change in doctrine from *Universals*.

outstrip the available predicates.[31] The usual slogan associated with the minimalist conception is that properties and relations are *shadows* of predicates. The imagery is misleading, however, since it seems to imply either that relations and properties are somehow insubstantial or that they are existentially dependent of the corresponding predicates. Linguistic idealism, however, need not be part of the minimalist conception. It is open to the minimalist, for example, to deny that there was no such thing as distance before the onset of linguistic practice.

Minimalism is not without its problems. Consider the predicate 'non-self-instantiating.' Is the property of being non-self-instantiating itself non-self-instantiating? If the minimalist doesn't suitably restrict the transition from meaningful predicates to quantification over properties and relations, then paradox threatens. The problem is that once

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[31]How the minimalist can account for this is a difficult matter. I suppose he could either appeal to possible predicates after the manner of Chihara or to an ontology of transcendent expression types (*transcendent* types exist whether or not they have any tokens). Thus the minimalist could claim that for every *possible* meaningful polyadic predicate there will be a relation or that for every transcendent expression type of polyadic predicates there will be a corresponding relation. One may wonder if the notion of a possible predicate is sufficiently robust to do the work required. This second option, however, is especially uncongenial since types (whether of expressions or not) are *properties* and are thus precisely what the minimalist seeks to account for.

one begins to codify the appropriate restrictions on the principle governing the existence of properties and relations, the minimalist conception begins to be less deliberately naive and more like some appropriate *mathematical* theory. (Indeed, if type restrictions are imposed, the resulting theory would look a lot like Russell's theory of propositional functions--and whatever reasons people have for doubting that a theory of propositional functions is logic would apply equally to the present case.) And if the minimalist ventures too far down this slide, then the minimalist conception won't have the resources to provide an antecedently understood conception of structure required by a structuralist analysis.[32]

Of course this isn't an argument but a challenge: Provide a consistent account of minimalist existence assumptions

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[32]I take it that Michael Resnik's unpretentious talk of 'patterns' is best understood as a minimalist conception of structure. See, for example, Resnik (1981). Notice, however, his initial statement of what a pattern is is remarkably similar to the informal description of structure you find in abstract algebra. As abstract algebra is usually developed, this informal description is cashed out in terms of the set-theoretic conception of structure described above. And indeed all of the relations between patterns that Resnik describes have natural set-theoretic correlates. Other than Resnik's contention that talk of patterns can be applied to the set-theoretic hierarchy itself, what distinguishes patterns from set-theoretic structures? There is a slide from our casual talk of relations, or in this case patterns, to something that looks more like a well developed mathematical theory.

which can justifiably be described as non-mathematical, or foreswear the explanatory use of such an account in structuralist analysis. This is an instance of a more general worry. If one postulates the existence of an *entity*, a structure, in the course of giving a structuralist *analysis*, the account one gives of these structures can't be formulated in terms of mathematical vocabulary on pain of circularity. Thus we saw that a set-theoretic account of structure is ill-suited to the structuralist's task if structuralism is to provide a general metaphysics for *all* of mathematics--such an account would be viciously circular when applied to set-theory itself. Avoiding this circularity would require that there be a conception of entity, i.e., a structure, describable independently of our mathematical theories but sufficiently rich to be, roughly speaking, the subject matter of mathematics. The difficulty is that a conception of structure adequate for a structuralist analysis must be fairly rich; but the richer such a conception is, the less credible the claim that we have an understanding of structure that is independent of the deliveries of our mathematical theories.

Recall getting clearer on the structuralist component of Benacerraf's argument was required in order to assess the argument that numbers could not be objects. In particular we needed to evaluate whether the structuralist component really has as a consequence that the identity of a given number depends on the relations it bears to every other number. In WNCNB, Benacerraf describes a structuralist account that quantifies directly over the property of being a progression. But given the difficulties with identifying mathematical structures with particular entities, the structuralist analysis should, instead, take the form of some appropriate generalization over progressions. How such an account should be formulated is the subject of the next chapter.

CHAPTER THREE

The Structuralist Conception of Number

3.1 *Introduction*

This chapter is organized as follows: The second section discusses the proper formulation of structuralism and argues that, among the structuralist analyses, some version of modal structuralism has best claim to capturing the content of arithmetic discourse. The third section discusses the nature of the modality involved. The fourth section discusses a pattern of difficulties that arises for the modal structuralist account of cardinality.

3.2 *The Problem of Vacuity*

In the last chapter I discussed Benacerraf's skepticism about the uniqueness thesis. The problem is roughly this: Granted that only progressions are plausible candidates, there seem to be indefinitely many progressions satisfying the structural and cardinality constraints, and no way of deciding which among the candidate progressions really is "*the* natural numbers." Semantic skepticism about uniqueness provides a very natural motivation for structuralism. If the use of our arithmetic vocabulary won't determine unique referents for our number words, this is precisely because they don't *have* unique referents: Arithmetic meaning is preserved by all of a broad class of reference assignments. All that is arithmetically relevant is what the candidate progressions have in common, namely, their structure.

How should we capture the semantic intuitions motivating structuralism? Not only is satisfying the structural and cardinality constraints seen as a necessary condition on referential candidacy, they are seen as a sufficient condition as well. One suggestion of how to cash this out has been to exploit the Peano axioms in identifying the relevant structure. Intuitively, the idea is that arithmetic sentences are implicitly hypothetical and general: They are claims about what would hold of any domain satisfying the Peano axioms. Our first try will thus be a conditional analysis with the Peano axioms as the antecedent. But *which* Peano axioms? The first- or the second-order axioms? The difference lies with the treatment of mathematical induction. In first-order Peano arithmetic, each instance of the schema:

(1) $[F(0) \wedge \forall x[N(x) \supset (F(x) \supset F(S(x)))]] \supset \forall y(N(y) \supset F(y))$

is an induction axiom, where 'x' and 'y' are first-order variables and 'F' is a schematic letter whose instances are monadic predicates definable in the language of first-order arithmetic (+, *, S, N). In second-order Peano arithmetic, induction is the explicit second-order statement:

(2) $\forall F[[F(0) \wedge \forall x[N(x) \supset (F(x) \supset F(S(x)))]] \supset \forall y(N(y) \supset F(y))]$

where 'x' and 'y' are again first-order variables but 'F' is now a monadic second-order variable.

But according to our usual understanding, arithmetic doesn't concern *all* models of the first-order axioms; rather, arithmetic is concerned with what is true in a *standard* model.[1] A standard model is one where every element has finitely many predecessors under 'S.' First-order arithmetic, however, admits of *non-standard* models. Each denumerable non-standard model has the order type $\omega + \eta(\omega^* +$

[1]That we can coherently draw the standard/non-standard distinction and that our arithmetic understanding concerns the standard model has been subject to philosophical skepticism. The philosophical content of Skolem's "paradox" consists in a constructivist worry that we can ever have an unequivocal understanding of infinitary structures. For the purposes of this paper I will assume that we do perfectly well understand what a standard model of arithmetic is despite the philosophical difficulties in stating precisely what this understanding consists in.

ω). Let Q be the structure $\eta(\omega^* + \omega)$ each of whose elements aren't numbers, but *non-standard numbers* tacked on to the end of the standard numbers. Since each element of Q has infinitely many predecessors, models containing Q are non-standard.

One reason to prefer the second-order formulation, then, is that only the second-order axioms are *categorical*--the second-order formulation will characterize the relevant structure up to isomorphism. What this means is that the models of second-order arithmetic are pairwise isomorphic. Let N = <N,0,S> and M = <M,0',R> be any two models of second-order arithmetic, then there exists an isomorphic mapping h from N onto M. Moreover models of second-order arithmetic are *elementarily equivalent*. Again let N = <N,0,S> and M = <M,0',R> be any two models of second-order arithmetic, then for any arithmetic sentence A, N and M will assign the same truth value to A.[2]

[2]But these results hold only with *full* second-order logic. In full second-order logic the second-order variables are represented as ranging over the set D^k of all k-ary relations on the first-order domain D, in contrast to Henkin interpretations where the second-order variables are represented as ranging only over a fixed subset of D^k (cf. Henkin (1950). Second-order logic under a Henkin interpretation is provably equivalent to a two sorted first-order theory which fails to be categorical but does have the usual metalogical properties of first-order logic such as completeness and compactness. Consider a non-standard model of arithmetic. If we allow our second-order variables to

Since arithmetic is concerned with the standard model, the second-order Peano axioms should be used in identifying the relevant structure. Let $PA^2(N,0,S)$ be the conjunction of the second-order Peano axioms where the underlying logic is full second-order logic. On the conditional analysis being considered here, $PA^2(N,0,S)$ will be the antecedent.[3] How will the consequent be formulated?

Consider an arithmetic sentence A expressed in terms of '+,' '*,' etc. Rewrite A in terms of the arithmetical primitives 'N,' '0,' 'S.' Thus all number quantifiers will be relativized to the predicate 'N,' numerical expressions will be replaced by their definitions in terms of 'S' and '0,' and the arithmetic operations will be replaced by their second-order definitions. Call the resulting sentence A(N,0,S). Now treat 'N,' '0,' and 'S' not as constants but

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range over D^k and not just a subset of D^k, then (2) will fail for such a model. Let G be the set consisting of all the standard numbers. 0 belongs to this set, and if n is a number and belongs to G, so does its successor; so the antecedent of (2) is satisfied. However, the consequent of (2) is falsified when evaluated with respect to such a model. In particular no element of the structure Q is in G. But by (2) N should be a subset of G.

[3]The second-order variables in the induction axiom should be relativized thus:
$\forall F A(\ldots F\ldots) \supset \forall F(\forall x_1\ldots x_i(F(x_1\ldots x_i) \supset N(x_1) \wedge\ldots\wedge N(x_i)) \supset A(\ldots F\ldots)$.

as variables of the appropriate types. 'N' will be a second-order monadic predicate variable, 'S' will be a second-order function variable, and '0' will be a first-order variable. The logical form of A is understood as:

(3) $\forall N \forall 0 \forall S(PA^2(N,0,S) \supset A(N,0,S))$.[4]

There is a familiar difficulty with such a view. Call it *the problem of vacuity*. Suppose there are only finitely many things, and hence no infinite progressions; then there will be no function satisfying the conditions set forth in the Peano axioms, thus making the embedded antecedent false and (3) vacuously true.

Why is the possible vacuity of the conditional analysis a problem? Charles Parsons has claimed that if the conditional analysis is correct, and its antecedent vacuous, then arithmetic is inconsistent:[5]

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[4]Analyzing our arithmetic assertions as implicitly hypothetical and general has the advantage of avoiding the apparent circularity of directly quantifying over the structural property of being a progression. Notice, in the analysans, all the mathematical vocabulary has been eliminated in favor of *variables* bound by an intitial quantifier. No mathematical vocabulary is used in the analysans, and the circularity is avoided in precisely the way Ramsification allows one to non-cicularly formulate functionalism, for instance.

[5]Parsons (1990), p.310.

> [I]f there are no simply infinite systems [progressions], then for any N, 0, S the statement...giving the 'canonical form' of an arithmetic statement A is vacuously true. But then both A and ¬A have true canonical forms, which amounts to the inconsistency of arithmetic.

Taken literally, this is to claim too much; since on the conditional analysis being considered, A and ¬A aren't *inconsistent*; there's a model and assignment function relative to which both are true--not much better. It is not hard to share Parsons' discomfort with this situation. Putting worries about the inconsistency of arithmetic to one side, what is the source of this discomfort? You might think that the proper analysis of number, whatever it turns out to be, should not result in an arithmetic sentence and its negation being represented as true or false together. If '¬' in the language of arithmetic functions semantically as classical negation, then an adequate analysis ought to respect the inferential behavior of classical negation. Let f be a mapping from the analysandum to the analysans. Capturing the inferential behavior of negation requires that the analysis of the negation of A, f(¬A), be logically equivalent to the negation of the analysis of A, ¬f(A). The difficulty with the conditional analysis is that f(¬A):

(4) $\forall N \forall 0 \forall S(PA^2(N,0,S) \supset \neg A(N,0,S))$

is not logically equivalent to $\neg f(A)$:

(5) $\neg[\forall N \forall 0 \forall S(PA^2(N,0,S) \supset A(N,0,S))]$

since (4) doesn't imply (5)--although the conjunction of (4) *with the assumption that infinite progressions exist* does. The requirement is not specific to negation but generalizes to other truth-functional connectives. We want truth-functional compounds of the analysandum to be logically equivalent to truth-functional compounds of the analysans. Thus for instance, $f(A \wedge B)$ ought to be logically equivalent to $f(A) \wedge f(B)$.[6]

One response to this difficulty has been to add a categorical existential conjunct:

(6) $\exists N \exists 0 \exists S(PA^2(N,0,S)) \wedge \forall N \forall 0 \forall S(PA^2(N,0,S) \supset A(N,0,S))$.[7]

After all, it's easy to believe that there exist enough things to constitute a progression. What are the commitments

[6]Cf. George Boolos (1990), pp.265-266.

[7]Cf. chapter 2.6 of Lewis (1994) for a similar proposal with respect to structuralist set theory.

of arithmetic according to (6)? The uniqueness thesis has, of course, been abandoned. Arithmetic assertions are represented as carrying commitment to progressions, and progressions may exist in spacetime; but so represented, arithmetic assertions, in conjunction with other beliefs, may still be committed to abstracta (even if there is a nominalistically adequate interpretation of second-order quantification). Whether or not there are any concrete progressions is an empirical matter, so the structuralist may not have unloaded the platonist commitment to necessary beings existing outside of space and time. The only commitment that has been unloaded for sure is a commitment to an allegedly occult feat of reference.[8]

(6) avoids vacuous truth since if the antecedent fails,

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[8]In the last chapter we saw Benacerraf arguing that if some structuralist analysis is correct, then a platonist interpretation of arithmetic is unavailable. And *not* because there is a meaning preserving reduction of arithmetic talk to talk of progressions in general; but rather, because the putative referents of our number words would, on a structuralist understanding, violate the ungroundedness principle. Given the structure of Benacerraf's argument, the analysis should be stated in a way that doesn't pre-judge the issue. Nevertheless, for convenience's sake I will be developing the structuralist analysis as a form of *ontological reduction*--a position that we are not entitled to until *after* the successful completion of Benacerraf's skeptical argument. This is no great difficulty, however, since whether or not the eliminativism urged by Benacerraf is ultimately accepted the structuralist analysis should be *non-circular*--the analysans shouldn't presuppose the existence and uniqueness of the natural numbers.

so will the existential conjunct, thus rendering the entire sentence false. There is a problem, however, in that we want negation to respect falsity just like we want it to respect truth. So just as we don't want A and ¬A both to be true, we don't want them both to be false either. But notice, if the existential conjunct fails, then both f(A) and f(¬A) will be false. As with the previous analysis, f(¬A) fails to be logically equivalent to ¬f(A). Consider ¬f(A):

(7) $\neg[\exists N \exists 0 \exists S(PA^2(N,0,S)) \wedge \forall N \forall 0 \forall S(PA^2(N,0,S) \supset A(N,0,S))]$.

If the existential conjunct is false, so is the embedded formula f(A), thus rendering the negation true.

The problem of vacuity begins with the intuition that an arithmetic sentence A and its negation ¬A can never be true or false together. (6) suffers from the same difficulty as (3)--under certain conditions, they each represent A and ¬A as sharing the same truth-value.[9] Against this the structuralist might reply that these conditions never obtain, thus rendering the problem moot. He might argue: (i) that the envisioned failure of the existential conjunct is impossible, or (ii) that, while the actual world may well be

[9]"The problem of vacuity" is thus a misnomer--the problem has nothing to do with vacuous truth per se.

finite, the existential conjunct only fails on an *optional* reading of the existential quantifier.[10] I will explain these in turn.

Suppose the structuralist claims that there is no problem with his analysis' assigning A and ¬A the same truth-value, since this will only occur if the world is finitely populated, but necessarily there are infinitely many things. Suppose there are pure sets. There are infinitely many of them, and they exist necessarily if they exist at all, so it would be impossible for the existential conjunct to fail.[11] Appealing to the existence of sets to justify the existence assumptions of arithmetic is problematic for the structuralist, however. Evaluating an appeal to sets, whether pure or impure, depends on how broad the structuralist's ambitions are. In particular it will depend upon whether structuralism is supposed to be a local view about specific regions of mathematical discourse or is rather a global view about mathematics. The set-theoretic structuralist can't appeal to the existence of sets to justify the existence assumptions of arithmetic--indeed a

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[10]The first style of response is due to Richard Dedekind. See his disastrous "proof" that there are infinite systems, section 66, Dedekind (1963).

[11]A caveat is in order: That pure sets exist necessarily is metaphysical folklore, and not a consequence of the axioms of set theory.

structuralist account of set theory itself has stronger existence assumptions to justify.

What about part-time structuralism? Consider someone who is a platonist about set theory (he believes that set theory describes a unique hierarchy of mind-independent abstract objects) but is a structuralist when it comes to mathematical theories that can be modeled in set theory. Such a person needs to provide an explanation of how we come to refer unequivocally to sets that won't generalize to cases where he wants to provide local structuralist analyses. If, for instance, his account of what secures reference to sets applies equally well to arithmetic talk, then his motivation for being an arithmetic structuralist is undermined. Suppose that someone thought that singleton was the appropriate primitive notion of set theory.[12] Suppose further that our structuralist contends that the referent of 'singleton' is the most appropriate available meaning such that our usage of the primitive 'singleton' couldn't but mean anything other than what we standardly take it to mean.[13] Given that the

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[12]As does David Lewis. The argument that follows is due to Lewis. See Lewis (1994), pp.111-112.

[13]The general semantic doctrine is sometimes described as 'reference magnetism.' To claim of something that it is a 'reference magnet' is to claim that somehow it is a more eligible candidate for reference than its competitors. David Lewis provides an account of eligibility in terms of natural properties. Cf. Lewis (1984) and Lewis (1983). This semantic position is available on a variety of understandings

Zermelo numbers take a restriction of the member-singleton relation as the successor relation, what's to prevent the meaning of primitive 'singleton' from doing double duty? Just as the member-singleton relation is the most appropriate meaning for his set-theoretic primitive, the restriction of the relation is also the most appropriate meaning for primitive 'S.' Such an explanation of how we refer to sets undermines his arithmetic structuralism. Arithmetic structuralism is motivated by semantic skepticism about uniqueness. If, however, reference to sets is fixed in the manner described above, then such skepticism is unwarranted--the natural numbers would just be the Zermelo numbers. I'm not sure that any part-time structuralism won't be unstable in just this way.

Belief in infinitely many necessary existences isn't forced upon the structuralist, however. One can believe that necessarily there are infinitely many things without believing that anything exists necessarily. Suppose, for instance, you thought that there were no empty worlds, that is:

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of what naturalness consists in--whether naturalness is conceived as a primitive feature of abundant properties, an equivalence class of tropes, or as being picked out by an associated universal or family of universals. I don't mean to be endorsing this view, so much as using it to illustrate how part-time structuralism might be unstable.

(8) $\Box\exists x(x = x)$.

Suppose further you thought that the world was necessarily composed of atomless gunk--an individual each part of which has further proper parts.[14] Then any given thing will be *nested*--it will form a progression under a restriction of the overlap relation. Indeed as long as there must be something rather than nothing, we are guaranteed that there are infinitely many things. Let's change the example. Putting aside worries about part-time structuralism, suppose you believed in sets with urelements but disavowed pure sets. Then as long as each possible circumstance is required to have at least one non-set, there will be infinitely many things--namely, all of the things in the subsequent set-theoretic hierarchy. Notice that neither of these examples relied on there being infinitely many necessary existences, as would be the case with an ontology of pure sets; rather, each example rested on a necessary guarantee that there are infinitely many things.

Even this may be weakened, given the appropriate

[14]While it is an empirical matter whether or not there are any fundamental particles, one may still have a priori reasons for doubting the existence of mereological atoms. Belief in fundamental particles may be retained with a belief in atomless gunk since the notion of a fundamental particle simply requires that it have no *detachable* parts.

assumptions. Suppose you thought that there were unactualized possible things. Suppose further you thought that there were infinitely many of them. Consider the existential conjunct of (6). Notice this about the above argument: It presupposes an *actualist* interpretation of the existential quantifier--that the domain of quantification consists of actual existences. But now we have available an alternative interpretation of the existential quantifier. If we speak with our quantifiers wide open such that they range over absolutely everything, whether possible or actual, then the alleged difficulty disappears.[15] According to this version of the reply, the existential conjunct fails on a reading of the existential quantifier that is strictly *optional*. An appeal to possibilia is weaker since it is compatible with the actual world being finitely populated.

Notice, if these replies constitute a cogent line of resistance, then the motivation to amend (3) is undermined. If there is a necessary guarantee that there are infinitely many things, then it's impossible for the antecedent of the

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[15]There are two sorts of doubts one can have about non-actualist interpretations of existential idiom. You might think that there is an analytic connection between existence and actuality--that 'Everything is actual' is an analytic truth. In contrast, someone may deny the analyticity of 'Everything is actual', yet still believe it to be true. For an argument against the analytic claim see chapter 2.1 of Lewis (1986).

conditional to fail; and if we can unrestrictedly quantify over possibilia, then the antecedent fails on a reading of the universal quantifiers that is only optional. (6) thus says more than is strictly necessary. Either the problem of vacuity is a genuine problem for the analysis, and it should be rejected; or the problem is rendered moot in the ways described above, and the motivation for the analysis is undercut.

It might be objected that since (3) assigns A and ¬A compatible truth-conditions, it represents the "negation" operator as meaning something different from classical negation. According to the semantic principle governing classical negation, a negation is true just in case the embedded formula is false, and it is false just in case the embedded formula is true. If there are no progressions, (3) represents A as being vacuously true; so ¬A should be false by this principle. But f(¬A) is true, so the negation operator in ¬A can't be classical negation.[16] The worry presupposes that surface grammar is reliable to this extent: that it correctly represents negation as receiving wide

[16]This is just Quine's observation that jointly stipulating two previously uninterpreted sentences 'S' and '¬S' to be true, is insufficient to guarantee a contradiction. It *further* must be assumed that '¬' functions semantically as a negation operator--perhaps '¬' isn't a negation operator but an intensional funtor. Cf. Quine (1934), pp.96-97.

scope--that the negation operator is governing the propositional content expressed by A. Notice this implies that f(¬A) is logically equivalent to ¬f(A). The structuralist, however, has abandoned surface grammar as a reliable guide to logical form. Numerals, for instance, aren't treated as constants, but rather as complex variables bound by an invisible quantifier. Similarly, the structuralist might contend that negation as applied to arithmetic sentences is just classical negation, but that surface grammar misrepresents negation as receiving wide scope; negation really only governs the consequent of the conditional. But notice if surface grammar is correct in assigning negation wide scope, then regardless of whether the negation operator in:

(4) $\forall N \forall 0 \forall S(PA^2(N,0,S) \supset \neg A(N,0,S))$

is classical negation, the conditional analysis has misrepresented the content of our arithmetic assertions.

The problem of vacuity is just that, according to the conditional analysis, there is a model and assignment function relative to which an arithmetic sentence A and its negation ¬A are assigned the same truth-value, whereas we have a pre-theoretic belief that A and ¬A must have divergent truth-values.[17] Each of the replies we have considered attempt to deflect this objection by arguing that the conditions under which A and ¬A receive the same truth-value never obtain. The plausibility of this depends on what the source of our initial intuition is, and on what the structuralist takes himself to be doing. Suppose that our belief that A and ¬A must have different truth-values is a *semantic* intuition--a belief solely about the content of our arithmetic assertions. In particular, suppose that we believe this because we believe that negation in arithmetic is just classical negation and that surface grammar correctly represents negation as recieving wide scope. The divergence in truth-value would then be a consequence of their truth-conditions. But according to the conditional analysis, the divergence in truth-value isn't a consequence of the assigned truth-conditions, but rather is explained in part by extra-arithmetic fact. That the problematic conditions never obtain for reasons independent of arithmetic meaning won't then be evidence that the analysis reasonably represents what we meant all along. If, however, the belief rests on an antecedent conviction that the world must be infinite, then the problem of vacuity won't affect the structuralist's claim

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[17]At least given the assumption that arithmetic discourse is bivalent.

to have captured the content of arithmetic discourse.

The discussion of the previous paragraph presupposes that the structuralist is making a hermeneutic claim--a claim about what we meant all along by our arithmetic talk. In contrast, he might be making a revolutionary proposal--he might be recommending that we invest new meaning in our old way of speaking. If the structuralist isn't making a hermeneutic claim, then even if our initial intuition is a semantic belief, the complaint that the analysis doesn't assign A and ¬A incompatible truth-conditions isn't decisive--the semantic belief might be a confused one. Revisionism, however, doesn't sit well with structuralism's motivation. The structuralist is moved by the worry that our usage of arithmetic vocabulary can't determine unequivocal reference for our number words. The structural and cardinality constraints are understood to be exhaustively meaning-determining, and an adequate structuralist analysis should be explicitly formulated to reflect this. Against the platonist, then, the structuralist contends that his analysis has better claim to capturing what we meant all along. The hermeneutic structuralist may also take comfort in our hesitancy when asked whether or not 17 is Julius Caesar. Such hesitancy is explained by the structuralist's contention that talk of numerical reference is only legitimate relative to a background structure. Structuralism thus seems to have reasonably hermeneutic ambitions. There is a clear sense, however, in which structuralism is *epistemically* revisionist. Insofar as we have intuitions in this area, our naive view is that numerals are unambiguous names.[18] Philosophy says this can't be right. So something in our pre-theoretic view about how mathematical language works has to give. Compare structuralism's mixture of hermeneuticism and epistemic revision with the views of Michael Dummett. To be sure, Dummett advocates a reform of our arithmetic speech, but his is not a revolutionary proposal for all that; rather, he is recommending that we restrict our usage to what, in his view, it is *capable* of meaning. Dummett is an epistemic revisionist in that he contends that we have false beliefs about what we mean. Might not our belief that A and ¬A cannot be true or false together similarly rest on a mistake? Perhaps, but I see no way to connect such a doubt to the semantic skepticism motivating structuralism.

Given structuralism's hermeneutic ambitions, the issue comes down to our reason for believing that A and ¬A must have divergent truth values. But is it really plausible that this belief rests on an antecedent conviction that

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[18]This belief is not restricted to laymen--many mathematicians, while displaying structuralist inclinations elsewhere, are platonists about arithmetic.

necessarily there are infinitely many things, or that we can unrestrictedly quantify over possibilia? Is it reasonable to suppose that surface grammar is sufficiently devious such that, despite appearances, we *never* assert wide scope negations of arithmetic propositions? Our initial intuition will only be respected if the fact that A and ¬A never share the same truth-value is a consequence of their truth-conditions, and this is not the case with the analyses we have considered so far.

Taking up a suggestion of Putnam's,[19] the problem of vacuity is easily accommodated, and we arrive at a form of modal structuralism recently championed by Geoffrey Hellman.[20] The basic idea is to replace the commitment to progressions with a commitment to the possible existence of progressions:

(9) $\Diamond\exists N\exists 0\exists S(PA^2(N,0,S)) \wedge \Box\forall N\forall 0\forall S,(PA^2(N,0,S) \supset A(N,0,S))$

where the background logic is second-order S5.[21]

[19]Cf. Putnam (1967). One important difference between Putnam's view and the proposal being considered here is that Putnam uses only first-order modal formulations.

[20]Hellman (1989).

[21]S5 is comprised of the following axioms and a rule of necessitation:

(i) $\Box(A \supset B) \supset (\Box A \supset \Box B)$

(ii) $\Box A \supset A$

(iii) $\Diamond\Box A \supset \Box\Diamond A$

(iv) $\Box A \supset \Box\Box A$

Let A be a true arithmetic sentence. Now suppose there are no infinite progressions. As long as there could have been infinite progressions, the modal existential conjunct will remain true. The conditional conjunct is true as well, if vacuously, when evaluated at the actual world. Notice, however, that the conditional conjunct is governed by a necessity operator. Thus consider the analysis of the negation of A:

(10) $\Diamond\exists N\exists 0\exists S(PA^2(N,0,S)) \wedge \Box\forall N\forall 0\forall S(PA^2(N,0,S) \supset \neg A(N,0,S))$.

In order for the conditional to be true it must be true in all possible circumstances of evaluation. But it fails in worlds where infinite progressions exist, thus rendering (10) false, which is the right result.

The motivation for the modal existence assumption consists in the idea that arithmetic truth shouldn't depend on controversial existence assumptions from other areas. Explicit in the foregoing is an explanation of this intuition. (3) and (6) had to rely on extra-arithmetic existence assumptions to insure that A and ¬A are represented as having divergent truth values. But if surface grammar is correct in representing negation as recieving wide scope, then the fact there is no model relative to which A and ¬A receive the same truth value is a direct consequence of the meaning of classical negation, thus obviating the need to postulate extra-arithmetic entities.

It's not clear that this simple explanation of our intuition is available to the modal structuralist. If negation receives wide scope, then any adequate hermeneutic analysis ought to respect the inferential behavior of classical negation: the analysis of the negation of A, f(¬A), ought to be logically equivalent to the negation of the analysis of A, ¬f(A). But on the usual semantics for modal logic, they are *not* logically equivalent, but would be if the modal existential conjunct were a logical truth (this is just the same problem faced by earlier versions of the analysis). According to the usual Kripkean semantics,[22] a model is a non-empty set of "possible worlds" one of which is designated as the actual world. A possible world is represented by a set of objects that exist in that world plus stipulations as to what predicates apply to in that world in that model and what names denote in that world in that model. The semantic rule for diamond is:

[22] Kripke (1963).

(11) '◊S' is true according to M and assignment function s iff there is a possible world w in M such that 'S' is true in w relative to s.

The difficulty is that if one of the models is finite in the sense that each set of objects associated with a possible world is finite, then the modal existential conjunct isn't a logical truth. If logical truth is correctly represented as truth in all models and there are finite models, then the modal existential conjunct isn't a logical truth: relative to a finite model the modal existential conjunct is false--there is no possible world in such a model with a countable domain. Field, however, has pointed out that matters are different with Carnap's treatment of modality.[23] If we substitute models for state-descriptions, then the semantic rule for diamond is:

(11) '◊S' is true according to M and assignment function s iff there is an M* and assignment function s* such that 'S' is true according to M* and s*.

Given the Carnapian semantics and the usual model-theoretic representation of logical truth as truth in all models, the modal existence assumption will be a logical truth. As long

[23]Cf. Field (1984) and chapter 5 of Carnap (1958).

as there is a model with a countable domain, the modal existential conjunct will be true in all models. If the Carnapian semantics is a correct representation of the modality involved, f(¬A) and ¬f(A) will be logically equivalent as required.

I'm not sure how to adjudicate the choice--I have no reliable intuitions about the logical truth of modal formulas, and there is something to be said for both the Kripkean and Carnapian semantics. (They each capture the intuitive analogy between diamond and the existential quantifier, for instance.) Given the pressure to interpret negation as recieving wide scope, the modal structuralist with hermeneutic ambitions had better argue that the modality involved in our mathematical assertions is best represented by the Carnapian semantics.

According to the platonist, the existence and uniqueness of the natural numbers are justified presuppositions of arithmetic. Some have felt that structuralism, which is motivated by semantic skepticism about the uniqueness thesis, is under conceptual pressure to retain the existence assumption because of the problem of vacuity. But I have argued that if the problem of vacuity is rightly understood, the existence assumption should give way to the logically weaker assumption about possible existence. Rather than requiring that infinite progressions exist, what is needed is the assumption that infinite progressions could exist. According to modal structuralism, arithmetic is a theory about a certain structure, a progression or ω-sequence, whose truth is independent of whether there actually are concrete progressions.

3.3 *Modality and Ontology*

Whether or not the modal presupposition is an *ontologically* weaker assumption, will depend, in part, on whether our understanding of the modal operators involves quantification over "possible worlds" however construed. Assessment of modal structuralism will also hang on the kind of modality involved, e.g., logical, metaphysical, etc.

There are four candidate interpretations of the modality involved: (i) logical, understood in terms of the semantic consequence relation associated with a certain formal system (in the present instance, second-order logic), (ii) metaphysical, in the broader sense of what is "absolutely" possible, (iii) nomological, in the sense of what is possible given our natural laws, and finally (iv) Putnam has suggested that the modality involved is a distinctively mathematical notion of possibility.

Nomological possibility is a *restricted* notion of possibility. Something is nomologically possible if it is possible *given* the natural laws governing the actual world. If nomological possibility is consistency with natural law, then there is a prima facie problem with using this notion in modal structuralist analyses. After all, the laws discovered by the scientific community are stated with mathematical vocabulary, and you can't eliminate reference to mathematical entities with a notion of modality whose analysis itself requires quantification over mathematical entities.[24] Perhaps there is a nominalistically adequate reformulation of our scientific theories--a reformulation which doesn't involve the use of mathematical vocabulary. Notice, this is tantamount to Hartry Field's nominalistic program, and it is a controversial matter whether it can be adequately carried out.[25]

Putnam has claimed that the modal content of our mathematical assertions consists in "a strong and uniquely mathematical sense of "possible" and "impossible"."[26] It's

[24] I owe this point to Paul Benacerraf.
[25] Cf. Field (1980).
[26] Putnam (1975), p.70.

not immediately clear what mathematical possibility amounts to. In particular it's not clear that Putnam didn't have in mind second-order logical possibility as opposed to first-order logical possibility. On this interpretation the mathematical character of second-order logical possibility consists in second-order logic's expressive capacity to characterize infinite structures categorically. Hartry Field has argued that if mathematical possibility isn't understood as second-order logical possibility, then its distinctively mathematical character will involve the existence of mathematical entities.[27] Mathematical possibility sounds like a restricted modality. Something is mathematically possible just in case it is possible *given* certain mathematical truths (presumably set-theoretic principles). Let M be the restricting mathematical theory. '$\Diamond_M$ A' is to be understood as meaning 'A is consistent with M.' Let's assume that M is the conjunction of the axioms of some standard set-theory, NBG for instance. The axioms of NBG include existence postulates, and it's plausible to suppose that so too will any adequate choice of mathematical principles. But if the mathematical truths in terms of which mathematical possibility is to be understood include existential claims, then mathematical possibility won't help eliminate reference

[27]Field (1991), pp.270-271.

to mathematical entities.

Someone might object that in asserting '$\Diamond_M$ A' we don't assert that M is *true*. We simply say that if it *were* true A could be true as well. So, construing mathematical possibility as a restricted modality won't automatically involve the modal structuralist in a commitment to the entities postulated by the restricting mathematical theory.[28] I suspect, however, that something like mathematical truth is covertly involved in Putnam's conception of mathematical possibility. In particular, belief in a kind of objectivity may involve the advocate of mathematical possibility in a commitment to the truth of the restricting theory. Suppose that our modal structuralist believes that it is an objective matter which of our mathematical assertions are true or false. Notice on the modal structuralist analysis, which of our mathematical assertions will count as true will depend on the choice of the restricting theory. Consider two set theories S and S' which are each internally consistent though mutually inconsistent. There will be a mathematical assertion A such that '$\Diamond_S$ A' is true and '$\Diamond_{S'}$ A' is false. So the modal structuralist's belief in mathematical objectivity

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[28]It is probably for this reason that Field doesn't repeat the argument in his discussion of Putnam in "Realism, Mathematics, and Modality".

commits him to there being an objective choice concerning the restricting theory. But in virtue of what is the choice of M *correct*? The modal structuralist can't appeal to the consistency of the restricting mathematical theory--S and S' are each internally consistent, and it's hard to imagine what property of the restricting theory the modal structuralist could appeal to other than its *truth*. Either mathematical possibility presupposes some determinate mathematical truths, or is itself indeterminate.[29]

Worse still, if mathematical possibility is a restricted notion of possibility, explaining mathematical truth in terms of mathematical possibility is *circular*--since the

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[29]Of course this is only a challenge, and it is always open to an advocate of mathematical possibility to claim that such a notion of mathematical objectivity is bogus. (It might plausibly be thought, for instance, that our concept of set doesn't settle how the set-theoretic hierarchy should be extended beyond the first inaccessible cardinal.) The point, however, would remain a good ad hominem objection against Putnam. One of the tasks of Putnam (1975) is to show how one can be a mathematical realist without buying a platonist ontology. (Putnam takes this to be the moral of Kreisel's dictum ("The issue...is not the existence of mathematical objects, but the objectivity of mathematical truths.")--further evidence that Kreisel's dictum functions as a Rorschach test in the philosophy of mathematics.) Putnam writes (Putnam (1975), pp.69-70): "A realist (with respect to a given theory or discourse) holds that (1) the sentences of that theory or discourse are true or false and (2) that what makes them true is something *external*--that is to say, it is not (in general) our sense data, actual or potential, or the structure of our minds, or our language, etc." So understood, mathematical realism implies the notion of mathematical objectivity employed in the above argument.

explanation of mathematical possibility presupposes the content of certain mathematical propositions. If the modal structuralist is giving a *general* account of mathematical truth in terms of mathematical possibility, then the circularity is fatal. He is, after all, proposing that the truth-conditions of *all* mathematical sentences be *analysed* in terms of mathematical possibility which itself is *analysed* in terms of the truth-conditions of a restricted set of mathematical sentences. If the modal structuralist protested that his project is more modest than that--that he only wished to analyse the truth of mathematical propositions other than those required by our understanding of mathematical possibility, then the circularity would be avoided. But at a cost, since how are we then to understand the mathematical truths in terms of which mathematical possibility is understood? Such truths aren't subject to modal structuralist analysis, and so it would seem that our modal structuralist would inherit the conceptual difficulties encountered by the part-time structuralist.

It might be objected that while mathematical possibility can be represented as consistency with M, it needn't be so analysed. Call such a conception *primitive* mathematical possibility. According to the primitivist, the truth-conditions of assertions involving mathematical possibility should not be *explained* in terms of consistency with some mathematical theory. The circularity objection would be avoided, and an explanation of the correct choice of M would be available in terms of the objective truth of M's modal translation.[30] Whether mathematical possibility is best understood as primitive is probably impossible to debate. Nevertheless, I think we should be skeptical of anyone claiming to have a primitive understanding of mathematical possibility. As an autobiographical note, I don't think I have any conception of it whatsoever other than consistency with some restricting mathematical theory. Two sorts of considerations would help alleviate my skepticism. If it could be shown in a non-question begging manner that consistency with a mathematical theory *presupposed* a notion of mathematical possibility, there would then be no question of this notion constituting an *analysis* of mathematical modality.[31] Failing such an argument, the only way that I could find primitive mathematical possibility credible is if someone provided an explanation of how one could acquire this concept without making reference to a rich body of

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[30]Actually, this would be a bit roundabout--the modal structuralist would have a direct explanation of mathematical objectivity that wouldn't proceed through an objective choice of restricting mathematical theory. In Putnam (1975), Putnam provides such an explanation in terms of the objective truth or falsity of the modal translations.

[31]I have in mind an argument analogous to Poincaré's criticism of logicism.

mathematical theory. Of course the explanation in question could not be an analysis, nor should one require it to be; rather what is asked for is some intuitive explanation of how someone could be trained in the use of such a concept in ignorance of the deliverances of mathematics. Note this last requirement is stronger than just requiring that the explanation not involve explicit reference to or quantification over mathematical entities. Suppose that someone were to characterize the content of mathematical possibility axiomatically. Such an explanation would be unacceptable if the only justification for the axioms were that they captured the deliverances of an antecedently understood mathematical theory. It seems unlikely that any adequate explanation will be forthcoming.

Unfortunately, meeting this challenge would only accomplish so much--there is an objection to mathematical possibility which is independent of whether or not it is best understood as primitive. Field has argued that "for purely mathematical statements, mathematical possibility and truth coincide...."[32] Field suggests that part of the content of mathematical possibility might be that a rule of necessitation holds for purely mathematical sentences:

[32]Field (1991), pp.252n.24,270-271.

(12) $A \equiv \Box_M A$

where A is a sentence whose only non-logical vocabulary is mathematical. Notice, the negation of a purely mathematical proposition is itself a purely mathematical proposition. Consider $\neg A$. By the rule of necessitation we have $\Box_M \neg A$ which implies $\neg \Diamond_M A$. So each instance of the schema:

(13) $\neg A \supset \neg \Diamond_M A$

is a valid formula. By contraposition we have:

(14) $\Diamond_M A \supset A$.

Field points out that if A explicitly implies the existence of mathematical entities, then mathematical possibility won't help eliminate this commitment. It doesn't follow, however, that modal structuralism represents arithmetic assertions as being committed to the existence of numbers. First of all, the embedded formula does not express a purely mathematical proposition--no mathmetatical vocabulary occurs in it; rather, mathematical vocabulary has been systematically replaced by variables of the appropriate orders. But even if we extend the rule of necessitation to statements of this form, the modal structuralist still wouldn't be committed to the existence of numbers. Consider the modal existential conjunct:

(15) $\Diamond_M \exists N \exists 0 \exists S(PA^2(N,0,S))$;

by (14) and modus ponens it follows that:

(16) $\exists N \exists 0 \exists S(PA^2(N,0,S))$

but this will only commit one to there being countably many things--not to the existence of the natural numbers. For our purposes, however, this is bad enough. The motivation for the modal formulation was the idea that arithmetic truth shouldn't depend on the existence of extra-arithmetic entities. If this idea is to be sustained, the modal structuralist should disavow mathematical possibility.

There is a more fundamental difficulty with mathematical modality. There's a real sense in which an appeal to mathematical possibility is *superfluous*--the specific mathematical content of our arithmetic assertions is *already* represented by the structural conditions set forth in the modal existential conjunct. According to modal structuralism, our arithmetic propositions are all of the form: (i) there might be progressions, and (ii) if there are progressions each progression would satisfy a given condition. The conception of modality involved can be as broad as you like as long as there might have been progressions on that conception. It adds nothing to the arithmetic content of our assertions to further restrict the operative notion of possibility.

I have emphasized that nomological possibility and mathematical possibility are *restricted* modalities. They can be represented as consistency with natural law and consistency with mathematical theory, respectively. There's a problem with analysing mathematical truth in terms of a restricted modality regardless of its specific content. To do so is to fly in the face of an intuition which motivated logicism. Frege, in presenting his analysis of number, rightly emphasized the generality of content and application of arithmetic:[33]

> The basis of arithmetic lies deeper, it seems, than that of the empirical sciences, and even than that of geometry. *The truths of arithmetic govern all that is numerable. This is the widest domain of all; for to it belongs not only the actual, not only the intuitable,*

[33] Frege (1980), pp.20-21.

but everything thinkable [My emphasis].

If we ignore the epistemologically loaded descriptions of modality characteristic of the philosophical milieu in which he was writing and understand "thinkable" to mean simply possible, Frege can be interpreted as emphasizing the unrestricted character of arithmetic. Analysing mathematical truth in terms of logical or metaphysical possibilty has the advantage of respecting this intuition.

What about logical possibility or, more specifically, second-order logical possibility? The idea is this--the necessity attaching to:

(17) $\Box\forall N\forall 0\forall S(PA^2(N,0,S) \supset A(N,0,S))$

is interpreted as meaning that the embedded formula is a second-order logical truth, and the possibility attaching to:

(18) $\Diamond\exists N\exists 0\exists S(PA^2(N,0,S))$

is explained in terms of the second-order satisfiability of the embedded formula. Once again, there is a circularity problem. A formula is a second-order logical truth just in case it is true in all full models of second-order logic, and it is second-order satisfiable if it is true in some full model. But models are themselves mathematical entities, in particular, sets. A modal structuralist who advocates the use of second-order logical possibility might try to get out of the circle by arguing that the model-theoretic definitions of second-order logical truth and second-order satisfiability aren't *analyses* of these concepts. Consider the following analogy. Turing computability is a formal representation of our intuitive notion of computability. We know a priori that our intuitive notion has certain properties--the class of computable functions is closed under composition, for instance, and we could exploit such an a priori feature to reject a given formal characterization. If, for instance, someone developed a formal account of computability which wasn't closed under composition, we would be justified in rejecting it. We would be justified in so doing since it doesn't correctly represent our *antecedent* conception of computability.[34] Just as Turing computability is a formal characterization of our intuitive notion of computability, someone might argue that we have an antecedent conception of second-order logical truth that the model-theoretic

[34]Indeed, there are actual cases of this sort. The notion of primitive recursiveness was originally introduced as an representation of our intuitive notion of computability, and it was rejected as an adequate representation when Ackerman produced an intuitively computable function that wasn't primitive recursive.

definition attempts to capture.[35] I'm not sure how plausible this analogy is. Moreover a complete defense of this way of breaking out of the circle would involve establishing that second-order quantification isn't ontologically committed to sets.[36]

Metaphysical possibility has the advantage of straightforwardly avoiding the kind of circularity that arises with the other notions of possibility that we have considered. While the criticisms I have advanced against these aren't decisive,[37] I nonetheless advocate interpreting the modality as metaphysical possibility.[38]

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[35]For a defense of this see Etchemendy (1990), especially pp.123-124.

[36]For a defense of the ontological innocence of second-order quantification see Boolos (1984) and Boolos (1985). For criticism of Boolos' defense see Resnik (1988).

[37]Except perhaps with respect to mathematical possibility. I do think that explicating the notion of mathematical truth in terms of mathematical possibility is hopeless.

[38]Field has expressed doubts about the content of "metaphysical" possibility in Field (1991). He points out that it does no good to explain the meaning of this notion in terms of its unrestricted character since this won't distinguish it from logical possibility. What content can we give to something being "absolutely" possible which would distinguish this notion from logical possibility? If second-order logical possibility is ontologically innocent thus avoiding the circularity objection, and if it is adequate for interpreting arithmetic discourse, then Field's skepticism would have to be met. But as we'll see in the next section, second-order logical possibility lacks the logical resources for interpreting cardinal applications of arithmetic whereas metaphysical possibility is, at least in this regard, unproblematic.

As was noted, the truth-conditions of formulas containing modal operators may be explained in terms of quantification over "possible worlds" or such operators may be taken as primitive and characterized axiomatically.[39] According to the primitivist, modal operators aren't understood as quantifiers but as propositional adverbs, and the introduction of modal operators into a language doesn't allow you to quantify over new things so much as it allows you to describe the same old things in new ways. Possibilist analyses are motivated by a strong formal analogy between the inferential patterns governing modal operators and first-order quantifiers. The primitivist, however, may take comfort in the fact that the formal analogy is imperfect: Wide scope quantifiers can bind variables in deeply embedded subformulas--nothing similar can be achieved with the familiar box and diamond. Possibilist analyses divide into those that take such quantification literally--as ranging over possibilia, and those that involve surrogate quantification--quantification, not over possible worlds, but

[39]For a useful discussion of modal primitivism see Kit Fine's postscript to Prior (1977). For the purposes of this essay a moderately realistic attitude towards modality will be assumed. Specifically I'm assuming (i) that modal formulas have truth-conditions and (ii) that some atomic modal formula is true. (i) rules out a non-cognitivist interpretation of modal discourse and (ii) rules out a modal error theory--the view that our modal discourse is subject to systematic and pervasive error.

rather actual entities which represent these. The former analyses are forms of *modal realism*, and the latter are forms of *modal ersatzism*.

If modality is explained in terms of quantification over possible worlds, then the modal existence assumption isn't ontologically weaker than the categorical existence assumption. According to modal ersatzism, there might be progressions just in case there are abstract representations of progressions. But notice, if there are infinitely many ways the world could have been, there will be infinitely many abstract representations, and hence actual progressions of ersatz worlds. And according to modal realism, there might be progressions just in case there are worlds with progressions as parts. Suppose, however, that modality is primitive--that the truth-conditions of modal formulas are not explained in terms of quantification over possible worlds. Suppose further that second-order quantification is ontologically innocent--that second-order quantification commits you to no more than what is in the domain of the first-order quantifiers. Given these conditions, the modal existence assumption would appear to be ontologically weaker than the previous existence assumption, making modal structuralism into a form of eliminative reduction.[40]

3.4 *Modal Structuralism and Cardinality*

The availability of an ontologically innocent interpretation of second-order quantification will depend on how the modal structuralist accounts for mathematical applications. Cardinal applications involve a correlation between the actual things to be counted and a hypothetical progression. Within the present framework such correlations will be represented by a bound second-order function variable. It will emerge that the interpretation of the function variable presents the modal structuralist with three options: The modal structuralist must either (i) make further modal assumptions, (ii) adopt an ontology of possibilia, or (iii) adopt an ontology of suitable second-order entities.

I will begin by considering Hellman's account of applied arithmetic.[41] So far the platonist's commitment to the existence and uniqueness of the natural numbers has been exchanged for an assumption about the possible existence of infinite progressions. In accounting for cardinal

[40]Appearances can be deceiving as chapter four will demonstrate.

[41]Cf. chapter 3 of Hellman (1989).

applications of arithmetic, Hellman further assumes that the actual world could be augmented with enough hypothetical things to represent the natural numbers without interfering with how things actually are. He is thus making three modal assumptions: (i) *Possible Existence*--there might be progressions; (ii) *Compossibility*--the actual things might coexist with progressions; (iii) *Non-interference*--the compossible progressions would not affect the properties of the actual things (other than their Cambridge properties).

Consider the sentence: 'The number of planets is nine.' What kind of truth-conditions should the modal structuralist assign it? To fix our ideas let's first try:

(19) $\Diamond\exists N\exists 0\exists S(PA^2(N,0,S)) \wedge \Box\forall N\forall 0\forall S(PA^2(N,0,S) \supset \exists g(g \text{ is } 1\text{-}1 \wedge \forall x(x \text{ is planet} \equiv \exists y(N(y) \wedge y < 9 \wedge g(y) = x))))$.

where 'g' is a second-order function variable and '<' is an abbreviation for a suitably relativized complex second-order variable. In English this means something like:

There might be progressions, and if there were progressions, then the planets would be 1-1 correlated with the first nine elements of such a progression.

where the definite description 'the planets' gets a narrow scope reading. Don't worry about the occurrence of the expression '9.' It's not a singular term referring to the number, but rather is shorthand for the complex variable expression: SSSSSSSSS0.

The problem with (19) is that 'planet' occurs within the scope of the necessity operator, and we are interested in the number of *actual* planets, not the number of planets there would be if there were progressions. To deal with this, Hellman adds a "non-interference" clause to the antecedent of the conditional:

(20) $\Diamond \exists N \exists 0 \exists S(PA^2(N,0,S)) \wedge \Box \forall N \forall 0 \forall S(PA^2(N,0,S) \supset \exists g(g \text{ is 1-1} \wedge \forall x(x \text{ is planet} \equiv \exists y(N(y) \wedge y \text{ does not interfere with the way things actually are} \wedge y < 9 \wedge g(y) = x))))$.

In English:

There might be progressions, and if there were progressions and if the existence of their elements wouldn't affect how things actually are, then the planets would be 1-1 correlated with the first nine elements of such a progression.

The non-interference clause is supposed to represent both the compossibility and non-interference assumptions.[42]

The explicit assumption of compossibility is forced upon Hellman given the restrictions he imposes on the second-order comprehension scheme:

(21) $\exists R \forall x_1 \ldots x_n [R(x_1 \ldots x_n) \equiv A]$

where 'R' is a k-ary relation variable not free in A and the 'x_i' are first-order variables. Hellman requires that only actualist quantifiers occur in instances of the comprehension schema and that A may not contain any modal vocabulary. Hellman believes that these restrictions are necessary since to abandon them would be to countenance *intensions*--since the extensions of second-order variables could then include things from different worlds.

This is a strange way to put the point given that

[42]I won't address the question of how to formulate the non-interference clause. Michael Resnik has expressed doubts concerning its proper formulation in Resnik (1992). Resnik points out that one controversial feature of this proposal is that "like Hartry Field, [Hellman] needs to fix a mathematical structure *via* a non-mathematical description of the physical facts..." This issue has been much discussed in the literature and is beyond the scope of the present essay.

Hellman officially disavows possibilia. Hellman is claiming that comparing the actual things to be counted with the elements of a hypothetical progression brings with it commitment to intensions. But if, by Hellman's own lights, you can unproblematically make hypothetical-hypothetical comparisons without commitment to possibilia, why can't you make hypothetical-actual comparisons without taking on such a commitment? After all, we unproblematically make such comparisons in natural language without apparent reference to intensional entities. Here is an example due to Burgess:[43]

If he had been in power, those who criticize him, would have praised him.

The example compares the actual behavior of a group of people with their hypothetical behavior, and it does so without explicit quantification over intensions.

I think that the pressure to restrict the comprehension scheme lies elsewhere. Hellman believes in the ontological innocence of second-order quantification--that second-order quantification commits you to no more than whatever is in the

[43]Burgess (1991).

range of your first-order quantifiers.[44] Notice this about nominalist interpretations of second-order idiom--only relations whose relata coexist can be represented. Consider how the modal structuralist with a primitive understanding of modality might represent a function from the actual planets to a hypothetical progression. By hypothesis, infinite progressions don't exist, so the function variable 'g' has no available interpretation. To which the reply is that there might have been infinite progressions, which forces upon him the compossibility assumption.

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[44]Hellman's nominalist semantics for second-order quantification is inadequate, however. He endorses a mereological interpretation of second-order idiom. Interpreting second-order quantification in terms of mereology requires certain assumptions beyond the usual axioms for mereology. Consider the domain of first-order quantification. The elements of the domain are defined as "atoms." The extra assumptions now are these: (i) no "atom" is a proper part of any other "atom," (ii) no proper part of an "atom" is explicitly quantified over in the theory, and (iii) the only objects other than the "atoms" that are quantified over are mereological fusions of these. Given these assumptions, second-order quantification is explained in terms of first-order quantification over mereological fusions. This interpretation of second-order idiom is considered ontologically innocent only insofar as mereology is, i.e., if a commitment to fusions isn't a *further* commitment over and above the commitment to its parts. Unfortunately, this will only work for *monadic* second-order logic, and Hellman needs the resources of *polyadic* second-order logic. To handle this problem, he suggests that in addition to the hypothetical progression we should hypothetically entertain enough things to represent k-tuples. This, however, is clearly inadequate--for what of the relation between an ersatz k-tuple and its elements? For a nominalistically adequate account of how to simulate quantification over relations see the appendix on pairing in *Lewis* (1994).

There are thus two theses motivating the resort to compossibility. The first is the modal actualist thesis that only actual things exist. The second is that second-order quantification can only represent relations whose relata coexist. You don't have to believe in the ontological innocence of second-order quantification in order to accept the second thesis. Suppose you thought that the values of second-order variables were sets. Notice this about sets--their existence depends upon the existence of their elements. There thus is no set representing the function from a collection of actual things to a non-existent progression. Nor will it help to appeal to possible sets since this will only reintroduce compossibility. Retaining modal actualism without compossibility not only requires giving up the ontological innocence of second-order quantification, but also requires making significant assumptions about the nature of the second-order entities. As should be apparent, the existence of suitable second-order entities must be independent of the existence of things falling under them. Such entities don't seem to be spatio-temporally located and are thus presumably abstract and exist necessarily if they exist at all. Fregean concepts are good candidates in this regard.[45]

The problem with Hellman's proposal concerns the justification of the non-interference assumption. In order to evaluate it we must first get clear on the nature of the hypothetical progression. Are its elements supposed to be abstract or concrete? I have no general account of the concrete/abstract distinction, but for our present purposes let's stipulate that the distinction is marked by whether or not something exists in spacetime. Hellman's discussion of applied mathematics assumes that the elements of the hypothetical progression are concrete. There is a prima facie problem, however, with this assumption. If the postulated hypothetical objects are concrete, then they might have causal powers in virtue of their very concreteness. But unless we assume that the hypothetical progression is causally isolated from the actual existences, what confidence can we have in their not interfering with the way things actually are? Notice, the expression 'planet' in the sentence 'The number of planets is nine' means *solar* planet. The planets being counted are the ones orbiting the sun and not some other star. Now suppose the hypothetical progression is a progression of stars. What's to prevent the

[45]Harold Hodes has argued that the intelligibility of second-order idiom presupposes an ontology of Fregean concepts. See Hodes (1984b).

gravitational force of one of these stars from pulling the planets out of their solar orbits in the counterfactual circumstance?

Hellman recognizes this difficulty and suggests that the compossibility need not be *nomological*:[46]

> [W]e are free to entertain the possibility of additional objects--even physical objects--of a given type to serve as components of a mathematical structure. Such objects could be conceived as occupying a region of space-time but as not subject to certain dynamical laws normally stated universally for objects of that type.

Newtonian mechanics is a good approximation for a restricted domain of the actual world. There are nonetheless other relativistic laws which hold generally. Similarly if actual dynamical laws hold within a restricted domain in the counterfactual circumstance, what's to prevent there being alien dynamical laws which hold universally in such a world? If this is the case, then allowing worlds governed by different natural laws won't help.

Hellman adopts non-interference and so is led to entertain a hypothetical restriction of dynamical law. If adequate, his analysis has to track the actual things to be counted in the relevant counterfactual circumstance.

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[46]Hellman (1989) p.97.

Assuming non-interference in addition to compossibility is motivated by the worry that unless the hypothetical progression is causally isolated from the actual things, the wrong collection will be counted. But causal isolation is supposed to be problematic if we assume that the hypothetical progression is concrete. The hypothetical restriction of dynamical law is supposed to be a counterexample to a substantive metaphysical thesis--that concreteness is a sufficient condition for causal agency. Is such a thesis plausible? Suppose there are epiphenomenal qualia. Their instances are spatiotemporally located and thus concrete, but, by definition, they fail to be causal agents even if they are the byproduct of some underlying causal process. Even if concreteness is a sufficient condition for causal agency, it doesn't follow that the hypothetical progression would causally interact with the things to be counted. Suppose there could have been *island universes*--spatiotemporally isolated systems of concreta. If it is a necessary condition that causal relata are spatiotemporally related, then if the actual things and the hypothetical progression exist in distinct spacetime continua, then their concreteness is no obstacle to their causal isolation. If that's right, then compossible concreta may bear no causal relations to one another.

The real problem with assuming non-interference is simply that it is unnecessary. The modal structuralist can reformulate the account just assuming compossibility. All that will be assumed is the possible coexistence of the actual things and the hypothetical progression--the properties of the actual things can vary as you like as long as they "show up" in the appropriate counterfactual circumstance. Thus the actual planets could be pulled out of their solar orbits, or there could be more solar planets than there actually are, etc. Keeping track of the collection of things to be counted will involve the use of an actuality operator, @:

(22) $\Diamond\exists N\exists 0\exists S(PA^2(N,0,S) \wedge \Box\forall N\forall 0\forall S[PA^2(N,0,S) \supset \exists g(g \text{ is 1-1} \wedge \forall x\exists y(g(x) = y \equiv N(y) \wedge y < 9 \wedge @x \text{ is a planet})]$.

Roughly this means:

There might be progressions, and if there were progressions, then the planets that there actually are would be 1-1 correlated with the first nine elements of such a progression.

John Burgess has suggested another account of how modal structuralism should handle cardinal applications which doesn't assume compossibility and thus doesn't require the restrictions Hellman imposes on the second-order comprehension scheme.[47] To motivate Burgess's account, we will begin as before with:

(23) $\Diamond\exists N\exists 0\exists S(PA^2(N,0,S)) \wedge \Box\forall N\forall 0\forall S(PA^2(N,0,S) \supset \exists g(g \text{ is } 1\text{-}1 \wedge \forall x(x \text{ is planet} \equiv \exists y(N(y) \wedge y < 9 \wedge g(y) = x))))$.

Recall, the difficulty with this is that we are interested in the number of actual planets and not the number of planets there would be if there were progressions. A natural emendation to this analysis would involve an actuality operator, @:[48]

(24) $\Diamond\exists N\exists 0\exists S(PA^2(N,0,S)) \wedge \Box\forall N\forall 0\forall S(PA^2(N,0,S) \supset \exists g(g \text{ is } 1\text{-}1 \wedge @\forall x(x \text{ is planet} \equiv \exists y(N(y) \wedge y < 9 \wedge g(y) = x))))$.

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[47]Burgess (1991). Burgess doesn't, however, endorse modal structuralism, he's just trying to be helpful.

[48]Each of the accounts we have considered so far involved the use of an actuality operator. The need for an actuality operator rules out interpreting the modality as second-order logical possibility. Second-order logical possibility lacks the resources to define actuality. What is required is that some full model of second-order logic be distinguished as representing the actual world, but there is no way to pick out such a model exclusively using second-order concepts. Moreover, actuality seems to be an abjectly metaphysical notion. For similar criticisms see Resnik (1992).

In English:

There might be progressions, and if there were progressions, then as things actually stand the planets are 1-1 correlated with the first nine elements of such a progression.

But notice that now the first nine elements of some progression are no longer merely hypothetical--since the values of 'g' are within the scope of the actuality operator. What is needed is some way to restore the hypothetical mood in the subordinate clause. To do this, the modal structuralist must again introduce a new modal operator. Call it the consequently operator, ¢. We now have Burgess' proposal:

(25) $\Diamond\exists N\exists 0\exists S(PA^2(N,0,S)) \wedge \Box\forall N\forall 0\forall S(PA^2(N,0,S) \supset \exists g(g \text{ is } 1\text{-}1 \wedge @\forall x(x \text{ is planet} \equiv ¢\exists y(N(y) \wedge y < 9 \wedge g(y) = x))))$.

which means roughly:

There might be progressions, and if there were progressions, then the planets that there actually are would consequently be 1-1 correlated with the first nine elements of such a progression.

Whereas the actuality operator undoes the effect of the necessity operator, the consequently operator restores it, thus allowing hypothetical-actual comparisons. Consider the following heuristic analogies. The actuality operator functions as if it bestowed the widest possible scope on the embedded formula--much like a carriage return on a typewriter. If the actuality operator functions like a carriage return, then the consequently operator functions like the backspace key--it takes the embedded formula only out of the scope of the preceding modal operator.[49]

[49]As should be readily apparent, the effect of an actuality operator can be had by k-many applications of the consequently operator, where there are k-many preceding modal operators. For more information concerning the consequently or "backspace" operator see Harold Hodes (1984a).

In section 3.3 I pointed out that the formal analogy between box and diamond, on the one hand, and first-order quantifiers, on the other, is *imperfect*. Hodes has shown that enriching S5 with a consequently operator is provably equivalent to a two sorted first-order theory where one style of variable ranges over possible worlds thus rendering the analogy complete. Can a primitive understanding of modality be maintained? It is not at all clear to me that the modal primitivist is in trouble. First of all, there's no immediate inference from the expressive completeness of a modal language to its assertions carrying ontological commitment to possibilia. Let M be a formula of the modal language and PW be its possibilist translation. Why should the equivalence between M and PW be taken as establishing the devious quantificational nature of M as opposed to establishing that the possibilist quantifiers in PW are only apparent? Such an inference could be underwritten by a substantive position in the philosophy of logic--an inferential conception of the meaning of logical operators. According to the inferential conception, the inferential

How are we to interpret the second-order function variable 'g?' Suppose the modality involved is primitive, and the actual world is finite. The hypothetical progression won't then exist. If we now make the assumption that second-order quantification can only represent relations whose relata coexist, 'g' has no available interpretation--it's supposed to represent a function from the actual planets to a non-existent progression. On this analysis, a primitive understanding of modality can only be retained if the values of the second-order variables are taken to be second-order entities whose existence is independent of the existence of things falling under them, as would be the case with an ontology of Fregean concepts. If, however, primitive modality is abandoned in favor of an ontology of possibilia, then an ontology of Fregean concepts may be dispensed with, and the assumption that second-order idiom can only represent

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patterns governing a logical operator exhaustively determine its content. But even granting the inferential conception, a possibilist *analysis* isn't forced upon us. Adopting a primitive understanding of modality is, in part, to disavow a certain explanatory demand--that the truth-conditions of modal formulas be explained in terms of quantification over possible worlds. The inferential conception, if true, requires that we take the posibilist quantifiers seriously and given the equivalence, that assertions of M carry with them commitment to possibilia. It does *not*, however, establish that the meaning of modal formulas should be *explained* in terms of quantification over possible worlds. Indeed a modal primitivist might contend that quantification over possible worlds is itself explained in terms of our antecedent understanding of primitive modal vocabulary.

relations whose relata coexist may be retained.[50]

In this section I have reviewed some of the conceptual difficulties encountered by the modal structuralist in giving a satisfactory analysis of cardinality. Undoubtedly, accounts other than those discussed are possible, but I suspect that the same pattern of difficulties will persist.[51] Three options emerged: The modal structuralist must either (i) assume compossibility, (ii) adopt an ontology of possibilia, or (iii) adopt an ontology of Fregean concepts. As belief in Fregean concepts or possibilia is controversial, and as the controversy is independent of mathematical practice, I recommend the assuming compossibility with a primitive understanding of modality.

3.5 *Modal Structuralism and Ungroundedness*

Benacerraf claims that if our arithmetic assertions are subject to a structuralist analysis, then interpreting them

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[50]Where the notion of existence is non-actualist in the sense that the actual things are only a portion of all that there is. If these options are uncongenial, the analysis could be further ammended by representing the *correlation* as existing hypothetically. But notice that in taking the correlation itself to be hypothetical, the analysis would reintroduce the compossibility assumption.

[51]Anyone care to try the following "primitive" operator: there could have been just as many ____s as there actually are ____s?

at face-value in such a way that our numerals are understood to function as unambiguous names violates an a priori principle, what I've called the ungroundedness principle. The idea is this: If our number words are unambiguous names, what sort of thing must they refer to? The necessary and sufficient conditions of on the analysis of our concept of number merely characterize an abstract structure, and this is supposed to have as a consequence that the identity of a given number depends on the relations it bears to every other number. But there could be no system of objects such that the identity of a given element depends on the relations it bears to every other element of the system. It is not the availability of a meaning-preserving reduction that casts doubt on the existence of the numbers, but rather the ungroundedness for the totality.

Does the identity of a given number depend on the relations it bears to every other number if arithmetic is interpreted at face-value *and* subject to a modal structural analysis? There's a problem with evaluating this question since the relevant notion of dependency remains unspecified. Nonetheless, I think not. Remember, Benacerraf is assuming that our arithmetic talk is interpreted at face-value and is attempting to derive a contradiction from this assumption. Numerals are understood to function as unambiguous names although the arithmetic assertions in which they occur are conceptually equivalent to generalizations about possible progressions. Roughly speaking, on this conception, 1 is understood to encode what is common to every second member of a possible progression (recall, we are including 0 in the sequence of the natural numbers). Thus, for instance, 0 precedes 1 since, necessarily, if something is a second member of a possible progression there is something that is its intial member that precedes it. Moreover, such modal facts don't appear to involve any ungrounded dependencies. Consider again the hypothetical progression of Roman emperors. Julius Caesar is the initial element of this progression and he precedes Octavian Augustus (under the relation of Roman imperial succession). Our understanding of this possibility is perfectly coherent and involves no ungrounded dependencies. It is misleading to describe 17 as depending for its being on the relations it bears to every other number as far as this involves a violation of the ungroundedness principle. Arithmetic is conceptually equivalent to a collection of modal generalizations. Insofar as the modal facts represented by these generalizations don't involve any ungrounded dependencies, neither do the arithmetic facts (in virtue of their conceptual equivalence).

CHAPTER FOUR

What Numbers Could Be

(And, Hence, Necessarily Are)

5.1 *Modal Structuralism and Frege's Task*

The modal structuralist believes that biconditionals of the form:

(1) $A \equiv \Diamond\exists N\exists 0\exists S PA^2(N,0,S) \wedge \Box\forall N\forall 0\forall S(PA^2(N,0,S) \supset A(N,0,S))$

are analytic, a priori, and necessary--indeed, that they constitute a *reductive* analysis of our concept of number.[1] Call

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[1]As their status as analytic will be important shortly, and as many separable theses have been held about analyticity, the operative assumptions about this notion should be made explicit. Quine writes: "A statement is analytic just in case it is true in virtue of meanings and independently of fact." The intention behind such a classification is prima facie clear. In general the truth of a statement depends on its meaning and extra-linguistic fact. If truth is an objective property, and if the meaning of a statement is one of the determinants of its truth-value, then it must at least be a coherent question whether a statement's meaning suffices for its truth. So-understood, analyticity is a purely *semantical* notion. The above characterization has no *explicit* epistemolgical implications: It remains silent on whether analytic statements are a priori, incorrigibly certain, etc. Of course, this is not to say that it has no *implicit* epistemological implications, but what these are depends upon your conception of content. Moreover the bare supposition that some truths could be determined by meaning alone is insufficient to subserve any conventionalist program such as a positivist account of logic or mathematics. A non-factualist understanding of analytic statements is thus not forced upon us. The claim that the structural biconditionals are analytic rests on two further claims: (i) Benacerraf is right in contending that the structural and cardinality constraints are exhaustively meaning determining,

all such biconditionals *structural biconditionals*. The analysis is a reduction since arithmetic facts are conceived to be nothing over and above a range of modal facts that don't seem to involve the existence of actual entities and thus that arithmetic cannot be interpreted at face-value. According to modal structuralism, reference to and quantification over the natural numbers is only *apparent*. Our arithmetic assertions are represented as carrying no commitment to a unique progression of objects. This involves an understanding of our arithmetic talk that is at odds with surface grammatical form. Ordinary numerals aren't represented as the unambiguous names that they appear to be, but as complex variables bound by an invisible quantifier. The modal structuralist also imputes a further deviousness to arithmetic syntax--arithmetic assertions are understood to have a modal content not explicitly represented by their surface grammar. The modal structuralist, in exchanging possibility for uniqueness, contends that surface grammar is systematically *misleading*.

Modal structuralism is thus an instance of a familiar theme in philosophy. Suppose there is a discourse apparently involving a commitment to a range of controversial entities. A theorist who disavows such a commitment may retain the old way of speaking, if only as a *facon de parler*. He may do so if he is able to restate

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and (ii) (as I argued in chapter three) a modal structural analysis is the only way to adequately capture this and is hence a correct explication of our concept of number.

the content of his assertions in a manner that is itself innocent of the troublesome commitment. Thus Berkeley, although holding that the concept of the material is incoherent, doesn't, thereby, reject our everyday talk of corporeal beings. Rather, he contends that there is a meaning-preserving reduction of such talk to talk of our ideas; and the upshot is that the corporeal commitments seemingly undertaken in everyday discourse are *illusory*--to suppose them to be genuine is nothing less than a vulgar (if distinctively philosophical) error.[2] The *ontological reductionist* thus holds:

(2) If a theory or discourse is apparently committed to a putative range of things, F's, but admits of a meaning-preserving reduction to a theory or discourse that apparently involves no such commitment, then the original theory or

[2]My brief remarks gloss over the fact that in certain moods Berkeley appears to think that ordinary talk does not even *seem* to presuppose the philosopher's notion of a material object--and thus doesn't require any special re-construal or interpretation. Rather than casting doubt on the claim that Berkeley is an ontological reductionist about the corporeal, it might be that Berkeley shares with serious minded reductionists a certain ambivalence about whether the reducing theory or discourse actually succeeds in capturing what we meant all along. Thus in certain moods Berkeley appears optimistically to side with the mob, in others to suspect that ordinary discourse may, after all, be infected with material content Berkeley (1965), I, 4: "It is indeed an opinion strangely prevailing amongst men, that houses, mountains, rivers, and in a world all sensible objects, have an existence, natural or real, distinct from their being perceived by the understanding." Perhaps this ambivalence is expressed by his admonition "to think with the learned, and speak with the vulgar" Berkeley (1965), I, 51)

discourse is not really *burdened* with a commitment to F's.

Modal structuralism, thus understood as a species of ontological reduction, is a form of *irrealism* or *eliminativism* about the natural numbers.[3]

If correct, modal structuralism would vindicate Benacerraf's claim that Frege's task was misconceived. Frege believed that a unique reductive identification of the natural numbers is necessary in order to alleviate the unclarity involed in our ordinary conception of the natural numbers--an unclarity revealed by the vague and uninformative answers we are tempted to give to the question what is 1. According to the modal structuralist, the analysis of number won't uncover a reductive identification of the numbers as Frege urged. Indeed, there are no numbers to be identified. Rather, arithmetic fact consists in the mere possibility of instantiating the structure of the number sequence. However, if the reflections of the previous chapter are correct, then no such vindication is forthcoming. The modal structuralist is only entitled to his rejection of the natural numbers *after* the succesful completion of Benacerraf's eliminativist argument. But

[3]For a discussion of reductionism as a form of irrealism see Blackburn (1984), chapter 5, section 3. I should emphasize that the reductionist is not an error-theorist. He is not claiming that the target discourse is subject to widespread and pervasive error. Rather, he is claiming that the content of our assertions, what we meant all along, is at variance with surface grammatical form.

the force of this argument is doubtful at best. I too believe that the form of an analysis of number as Frege envisioned it is problematic, but I don't think that the difficulty lies with Frege's platonism. Instead I believe that what should be disavowed is Frege's insistence that the numbers must be identified with, as Benacerraf puts it, a range of things "not already known to be the numbers." In this chapter I will show how a platonism that eschews reductive identification need not be uninformative as Frege feared.

5.2 *The Problem*

Like the Putnam of "Mathematics Without Foundations,"[4] I believe that we may accept the structural biconditionals as a correct analysis of our concept of number, but without disavowing a commitment to a unique progression of natural numbers. Such a *non-reductive* understanding of the structural biconditionals involves: (i) interpreting surface grammar at face-value (numerals are understood as unambiguous names, apparent quantification over the natural numbers is understood to be genuine, etc.) while (ii) maintaining that arithmetic talk is

[4]Putnam (1967b).

nonetheless susceptible to modal translation.[5] Contrast such a view to the uninformative answer that Frege disdained--that 1 is simply itself and not another thing. Our concept of number would be subject to a non-circular analysis (there would be a meaning-preserving reduction of arithmetic to a theory that doesn't involve any arithmetic vocabulary) and thus would be informative. But, as I shall argue, this provides us with no warrant for abandoning our naive, pre-philosophical belief in *the* natural numbers.

This pair of commitments faces a *prima facie* difficulty, namely, that arithmetic assertions and their modal counterparts apparently differ in their ontological commitments and thus appear to describe distinct domains of fact. Arithmetic assertions, interpreted at face-value, imply the existence of numbers; while their modal counterparts, interpreted at face-value, apparently have no such implication. Given this difference in truth-conditions when interpreted at face-value, they aren't

[5]I should remark that it is *prima facie* unclear what the thesis of the univocality of numerical reference comes to if a statement allegedly making such a reference is to be understood as *conceptually equivalent* with its modal counterpart. Thus Putnam believes that arithmetic assertions and their modal translations describe the very same fact and that each may be interpreted at face-value. Putnam, however, develops this position in terms of Reichenbach's notion of *equivalent descriptions*. This has as a consequence (no doubt congenial to Putnam's later self) that ontological commitment is not fully objective--that a difference in ontology is only *representation relative*. In what follows I articulate a non-reductive understanding that avoids this.

analytically equivalent, let alone synonymous. Indeed if there are no numbers, they aren't even *materially* equivalent--the arithmetic assertions we accept will all be false though associated with true modal "translations." So-understood, our arithmetic assertions and their modal counterparts represent distinct domains of fact. Yet, the structural biconditionals, if true, state necessary connections between the two. But how *could* there be a necessary connection between them? It would seem that this would be just the kind of mysterious necessary connection of which Hume complained.[6] Such a necessary connection would be *innocuous* if the structural biconditionals were analytic on the non-reductive understanding of them. But if our arithmetic assertions and their modal counterparts genuinely differ in their truth-conditions (the former requires, while the latter does not, the existence of a special domain of entities), then how can they be analytically equivalent? The modal structuralist, in denying a face-value interpretation of arithmetic, sidesteps any need to appeal to non-Humean necessary connections--he explains the necessary connection between arithmetic facts and modal structural

[6]Philosophers have not been very successful in providing an account of sameness of fact. And as I have no reason to believe that I could do better, I prefer not to offer an analysis of this notion. But given the importance placed on the identity and difference of fact in the present discussion, the minimal assumptions involved should be made explicit. Fortunately, I need only commit to a fairly uncontroversial sufficient condition: If S and S' are synonymous declarative sentences, they would represent the same fact if true. Any claim about the difference of fact is an objection to the interpretation that I will be defending, so I may remain silent on this issue.

facts by literally *identifying* them. By not interpreting the surface grammar of our arithmetic assertions at face-value, the modal structuralist doesn't impute to them the content that would stand in the way of their being analytically equivalent to the corresponding modal assertions; and given that arithmetic assertions, so-understood, have precisely the same meaning as their modal counterparts, they must describe the same range of fact. Were we to give up on ontological reduction, how would we explain the necessary connection? Appealing to analyticity apparently wouldn't help. (Appearances can be decieiving--or so I will argue.)

More needs to be said to make out this difficulty. Notice, for instance, identification is not the sole province of reductionism. Just as the modal structuralist claims that arithmetic facts are nothing more than modal structural facts, his cousin, the *ontological inflationist*, could, perhaps perversely, claim that the relevant modal facts are nothing more than facts about the natural numbers.[7] It all depends on which side of the

[7]The term is Field's--Cf. Field (1984b). He introduces this position in an attempt to underscore the difficulties of coherently distinguishing a non-reductive understanding from ontological inflationism. The inflationist take on reduction is also noticed by Benacerraf in WNCNB but is used, instead, to cast doubt on the *reductionist* interpretation, p.290: "There is another reason to deny that it would be legitimate to use the reducibility of arithmetic to set theory as a reason to assert that numbers are really sets after all. Gaisi Takeuti has shown that the Gödel-von Neumann-Bernay set theory is in a strong sense reducible to the theory of ordinal numbers less than the least

biconditional you take as having a surface syntax that transparently reveals the nature of the fact in question. The ontological inflationist fails to interpret the relevant modal formulas at face-value--they are understood to carry an implicit commitment to the natural numbers. Although he accepts the philosophical principle underlying ontological reduction, the ontological inflationist disagrees about *which* apparent commitments to accept as genuine. Ontological inflationism, in the present context, can thus be described as a kind of *modal pythagoreanism*--since a range of modal facts are, on analysis, revealed to be nothing over and above facts about the natural numbers.[8]

Despite their obvious differences, the ontological reductionist and the ontological inflationist share an important doctrine. Each holds that since the statements on either side of the biconditional differ in their apparent ontological commitments we can't interpret the surface grammar of *both* at face-value: Either the arithmetic statement or its modal counterpart must be

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inaccessible ordinal. No wonder numbers are sets; sets are really (ordinal) numbers, after all. *But now, which is really which?*"

[8]There are, of course, no modal pythagoreans for the simple reason that the view is incredible. The view is crazy in that arithmetic facts are identified with a *subclass* of the modal facts that can be represented in the language of second-order S5. The operative modal notions have a life beyond the narrow confines of arithmetic.

understood as a mere analytic transform of the other. The obstacle to interpreting both at face-value is that they would represent different ontological commitments, and thus fail to be synonymous.

Suppose, however, one could make out a position according to which the metaphysically significant distinction between (a) primitive modality, and (b) the natural numbers (objectually understood) is genuine, but where *facts* concerning the natural numbers are nonetheless identical with certain modal facts that don't seem to involve any objects at all. There would be a single domain of fact with two ways of representing it--as being composed of certain objects, the natural numbers, or as not involving the actual existence of progressions but, rather, their mere possibility. From this perspective, the natural numbers are *constituted* by the distribution of certain complex modal properties, and facts about the natural numbers *just are* certain modal structural facts. Call such a position *modal platonism*. Modal platonism, if intelligible, would avoid the incredibility of modal pythagoreanism while holding out the promise of a platonism that respects what is right about the intuitions motivating the structuralist conception of number.

Such a conception of fact is not without precedents. In the

Foundations Frege writes:[9]

> The judgement "line a is parallel to line b", or using symbols,
>
> a // b
>
> can be taken as an identity. If we do this, we obtain the concept of a direction, and say: "the direction of line a is identical with the direction of line b". Thus we replace the symbol // by the more general symbol =, through removing what is specific in the content of the former and dividing it between a and b. We carve up the content in a way different from the original way, and thus yields us a new concept.

An assertion about directions is understood to be conceptually equivalent to an assertion about parallel lines. In virtue of this equivalence, if true, they each represent the same fact, despite a difference in ontological commitments *explicitly* represented by surface syntax. What is presently important is Frege's metaphor of "carving up" contents in different ways. If the very same proposition can be subject to multiple analyses, then we have a way of defusing the present difficulty. If a single propositional content lacks a unique analytical structure, then there is no obstacle to claiming that the structural biconditionals are analytic on a non-reductive understanding of them. Consider Frege's example:

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[9]Frege (1980) pp.74-75. For a contemporary development of the leading ideas of this passage see Wright (1983), and Rosen (1993). What follows is heavily indebted to Wright's views, though we do end up with very different conceptions of arithmetic meaning.

(3) $\text{Dir}(a) = \text{Dir}(b) \equiv a \mathbin{//} b$.

The putative problem is that if we understand the biconditional to be analytic, then we cannot legitimately interpret the surface grammar of both the right- and left-hand sides at face-value. Consider an ontological reductionist about directions--talk about directions is conceived to be nothing but highly derived talk about parallel lines. The left-hand side of (3) would be understood to be a mere analytic transform of the right--there would be no genuine identity, and the direction terms would not be open to genuine existential generalization. The ontological inflationist, on the other hand, would claim that the conceptual equivalence reveals facts about parallel lines to be nothing over and above facts about directions. In this case, it is the surface grammar of the *right-hand side* that would no longer be interpreted at face-value. Frege's point is that our initial dilemma is a false one. He suggests that it is possible to describe the very same fact in two very different ways. The right- and left-hand sides can be understood as representing the same fact since they share the same content. The biconditional is analytic, and the idea that the analyticity is genuine can be sustained (on Frege's view) since Frege held that the very same proposition can be subject to multiple analyses. On such a view any *apparent* discrepancy in ontological commitment is no obstacle to synonymy since propositional contents don't admit of unique analyses. Different analyses may highlight different commitments, but the propositional content remains the same. And the result is that the very same fact or state of affairs may be represented as being comprised of different objects. This is precisely what I am proposing with respect to the structural biconditionals.

5.3 *How to Make Sense of Multiple Analyses*

The first step towards modal platonism is to deny that propositional contents have unique analyses. How are we to make sense of propositions being subject to multiple analyses? This doctrine can be motivated, in part, by a *minimalist account of referential candidacy*. An account of referential candidacy is a specification of the conditions under which a term *purports* to refer. It is an account of referential *purport*, as opposed to referential *success*. On *any* credible account of referential candidacy, the following two conditions are necessary:

(4) The expression must be *significant* in the sense that it must have a norm-governed use;

(5) The expression must satisfy the relevant *syntactic* and *inferential* criteria.

A syntactic category can be thought of as an equivalence class of expressions that are intersubstitutable while preserving well-formedness. If an expression functions syntactically as a singular term it must be capable of being meaningfully embedded in the appropriate syntactic contexts. Thus, for instance, Frege held that a name must be capable of occurring on either side of the usual sign for identity, '='.[10] Not only is belonging to the appropriate syntactic category relevant to referential candidacy, but so is an expression's *inferential role*. An expression that functions syntactically as a singular term must also be able to participate in the appropriate inferential transformations. Thus

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[10]There is a prima facie problem with this conception of syntactic category for the case that interests us. Number words have two uses: as nouns and as a special class of adjectives. While we can always replace a proper name or definite description with a number word salva grammaticality, the converse fails:

There are 9 planets

is well-formed, but:

*There are Quine planets

isn't. And the worry is that *prior to syntactic regimentation*, our number words aren't everywhere intersubstitutable with genuine singular terms, and therefore the relation of intersubstitutability salva grammaticality cannot be used to define syntactic categories as equivalence classes. This difficulty is only apparent, however. Number words as they occur in numerically definite quantifiers have only *syncategormatic* occurrences. What we learn is *not* that syntactic categories cannot be defined in terms of the relation of intersubstitutability salva grammaticality prior to syntactic regimentation, but rather they cannot be so-defined prior to *lexical* regimentation. This is not to say that nominal and adjectival occurrences of numerals have at most a typographical resemblance. 'There are 9 planets' and 'The number of planets is 9' are, after all, in *some* suitable sense equivalent.

if 'a' functions syntactically as a singular term, it is a necessary condition on its functioning *semantically* as a singular term that from 'Fa' we may infer '∃x(Fx)'. If an expression didn't exhibit this inferential behavior, it wouldn't be a genuine singular term. While there's room for disagreement concerning the precise account of the syntactic and inferential features necessary for an expression to function semantically as a singular term, everyone should agree that satisfying *some* such criteria is a necessary condition on referential candidacy.[11]

It is important to note that while such conditions must be specified without appealing to the referential content of a given expression, it is not the case that they are specified in purely non-semantic terms. Thus consider Frege's contention that a singular term must be able to flank the usual identity sign. There's nothing special about the morphological properties of '='. The reason that embeddings in this context function as a constraint on referential candidacy is explained, in part, by the content of the identity sign. It is not just the syntactic environment, but also the associated *semantic* context, that explains why certain embeddings are constraints on referential candidacy. Consider why certain inferential roles are necessary for an expression to function semantically as a singular term, in

[11]For further discussion see Wright (1983), Hale (1984), and Brandom (1994).

particular, existential generalization. That an expression occupies a position that is open to first-order existential generalization is a constraint on its contribution to the propositional content of a given sentence in part because of the *meaning* of the quantificational idiom. After all, there is nothing semantically significant about a backwards 'E', although there is *if* it functions as an existential quantifier. And if it does so, it places a constraint on the kind of content that an expression must have if it occupies a position open to existential generalization.

What is distinctive about a minimalist account of referential candidacy is *not* the claim that (4) and (5) are individually necessary, but that they are *jointly sufficient*. A minimalist account of referential candidacy thus amounts to the claim that:

(6) An expression purports to refer just in case it is significant and satisfies the appropriate syntactic and inferential criteria.

Such an account is *minimalist* in the sense that *any* plausible account of referential candidacy will count (4) and (5) as necessary--the minimalist simply makes the further claim that there are no *further* necessary conditions on referential candidacy.[12]

It is partly constitutive of the notion of a singular term that its role in the determination of the truth-conditions of a sentence in which it occurs involves specifying an object. If this is correct, then the following principle must hold:

(7) There is something that a singular term ocurring in a true sentence of the relevant sort designates.

The restriction, "of the relevant sort", is non-trivial. There

[12]It's important to note that a minimalist account of referential candidacy doesn't entail a deflationary conception of reference (though, of course, the converse holds--on deflationary conceptions of reference see Brandom (1984), and C. Hill (1987)). Roughly, the deflationist believes that the content of 'refers to' is exhausted by some appropriate disquotational principle:

If a exists, then 'a' refers to a

Given that the disquotational principle is supposed to exhaust the content of our concept of reference, disquotational reference isn't sufficiently robust to demand of a given expression anything over and above that it be significant and satisfy the appropriate formal features. One can, however, have as robust a conception of reference as one likes and still be a minimalist about referential candidacy. Consider a causal theorist. According to the causal theory, reference is explained in terms of an appropriate pattern of causal relations involving our use of a singular term and the thing denoted. A causal theorist may consistently be a minimalist about referential candidacy since the obtaining (or failure to obtain) of the appropriate causal relations is an account of referential *success*. Indeed such a position is eminently plausible: Intralinguistic relations determine which expressions are apt to refer, while the fact that certain expressions *do* refer is explained in terms of the relevant worldly facts--such as a pattern of causal relations (at least if the designata are *concrete*).

are a whole range of counterexamples to (7) if it is not suitably qualified. Unasserted contexts, negative existentials, opaque contexts, fictional contexts (on some plausible understandings of them), all provide convenient counterexamples. Thus consider:

(8) The Greeks worshiped Zeus.

'Zeus' by our best criteria functions semantically as a singular term. But one can assert (8) without incurring any theological commitments. One can do so since 'worship' is an opacity producing psychological verb. Or consider negative existentials:

(9) There is no Santa Claus.

Asserting (9) doesn't commit one to the existence of Santa Claus, simply because 'Santa Claus' is a bona fide singular term occurring in a true sentence--indeed the assertion explicitly denies the existence of Santa Claus.

What sorts of sentences are in this sense relevant? The principle needs to be restricted to sentences with existential implications. This response is imprecise in two ways: It doesn't specify which sentences have existential implications, and it doesn't specify the relevant notion of implication. Though imprecise, a more specific answer would be implausible. It is a controversial matter which sentences have existential implications since very often, as in the present context, it is a substantive question what the precise truth-conditions of a given sentence are, and is thus implausibly settled by any general a priori principle.[13]

It might be objected that the claim that the relevant sentences are the ones with existential implications is question begging--since the existential implications of our arithmetic

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[13]There is also a genuine puzzle about what could be the appropriate notion of implication (Cf. Jackson (1989)) . Perhaps the relevant notion of implication is *modal implication*: An assertion S carries a commitment to a range of things, F's, just in case it is impossible for S to be true if there are no F's. There's a problem with modal implication. Suppose mathematical entities exist necessarily. If ontological commitment is understood in terms of modal implication, then the nominalist who denies the existence of abstracta would be committed to the existence of mathematical entities despite his most sincere protestations. Surely an unwelcome result. Suppose, however, that the relevant notion of implication is *narrowly logical implication* in the sense associated with a particular formal logic. S carries a commitment to the F's just in case there is no model and assignment function relative to which S is false and 'There are F's' is true. The difficulty with narrowly logical implication is that it doesn't recognize implicit commitments in virtue of *meanings*. To be committed to the existence of wives, for instance, is to be implicitly committed to the existence of husbands in virtue of the meaning of 'wife.' But since narrowly logical implication is implication under the reinterpretation of non-logical vocabulary, the meaning of our non-logical vocabulary can carry no such implicit commitment. I believe that this puzzle is only *apparent*. Though I would not like to defend this here, I believe that the relevant notion of implication is *analytic implication*, i.e., S analytically implies the existence of F's just in case the conditional 'If S then there exists an F' is an analytic truth. But given that the content of philosophically important concepts (such as our concept of number) is always a controversial matter such a suggestion provides no clearcut *criterion* of ontological commitment.

assertions is precisely what is at issue. This problem isn't genuine: While the modal structuralist denies that our arithmetic assertions carry a commitment to *the* natural numbers, it is an uncontroversial matter that, *when interpreted at face-value*, they do bear such a commitment. What's at issue, however, is *not* whether a face-value interpretation of arithmetic involves a commitment to the natural numbers, but whether or not the facts of usage warrant such an interpretation.

Now consider Frege's example of direction talk. Suppose the following biconditional is understood to express a conceptual equivalence:

(3) Dir(a) = Dir(b) ≡ a // b.

The direction terms occurring on the left-hand side are by the best criteria singular terms, and if a and b are parallel, they occur in a true sentence of the relevant sort. Therefore, by (7), there must be something to which they refer. (6) and (7) thus provide a vehicle for a non-reductive understanding of direction talk. The thought is that we can *reconceptualize* facts about parallel lines as being facts about directions. To do so, all we need to do is re-present the content of assertions concerning parallel lines in the language of directions. (6) and (7) thus provide one way of capturing what Frege might have meant by "carving up contents" in different ways.

5.4 *Minimalism and Semantic Indeterminacy*

There is a difficulty with sustaining this position with respect to Frege's example (as Frege himself recognized). The direction terms are supposed to be genuinely referential, and their referents are supposed to be distinct from lines. Now a very natural objection to (3) is that the analysis only specifies conditions under which direction terms are *coreferential.* And this is a fairly weak constraint--one that is compatible with indefinitely many assignments of referents to such singular terms. Cognoscenti will, of course, recognize this as a version of Frege's Julius Caesar problem.[14] The problem is that the analysis doesn't determine truth-conditions for every identity statement of the form:

(10) Dir(a) = q

where 'q' is a singular term that is not a direction term. The analysis will determine *some* such identity statements. For example, let '(ix)' be the definite description operator, then:

[14]Cf. Frege (1980), sections 56 and 66. See also Rosen (1993).

(11) Dir(a) = (ix)(x = Dir(b))

will come out true just in case a and b are parallel. The difficulty with understanding direction terms as genuinely referential *and* as designating items other than lines is that, on the present analysis, their referential content is indeterminate. A tenacious reductionist may claim that such indeterminacy provides him license for semantic legislation. As *he* understands direction talk, the direction terms don't refer to entities distinct from lines, but, say, to some particular representative line.[15]

Fortunately this particular difficulty doesn't affect a modal

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[15]Russell, too, recognized this, but further held that it was a problem for *any* definition by abstraction on an equivalence relation or operation (Russell (1903), p.114):

> The relation of similarity between classes has three properties of being reflexive, symmetrical, and transitive. ... Now these three properties are held by Peano and Common sense to indicate that when the relation holds between two terms, these two terms have a common property, and *vice versa*. This common property we call their number. This is the definition of number by abstraction.
>
> Now this definition by abstraction, *and generally the process employed in such definitions* [my emphasis], suffers from an absolutely fatal formal defect: it does not show that only one object satisfies the definition. Thus instead of obtaining *one* common property of similar classes, which is *the* number of the classes in question, we obtain a class of such properties, with no means of deciding how many terms this class contains.

platonist interpretation. A modal structural analysis is not of the appropriate form--arithmetic identity statements are not analyzed in terms of an equivalence relation or operation. A reductionist might claim that there is, however, a genuine worry about securing unequivocal reference on a non-reductionist understanding of the structural biconditionals. Consider (7). The principle only implies that under certain conditions there is something to which a given numeral refers. While existence may be established, the principle remains silent about uniqueness. And since the reference of numerical singular terms is thus indeterminate, couldn't the reductionist similarly claim that he is free to stipulate that the natural numbers are some progression of things "not already known to be the numbers?"

Against this one might object that the reductionist reinterpretation fails to respect the analytic equivalence, (1). In particular, while the implication from left to right will hold, the implication from right to left fails, i.e., from

(12) $\Diamond\exists N\exists 0\exists S PA^2(N,0,S) \wedge \Box\forall N\forall 0\forall S(PA^2(N,0,S) \supset A(N,0,S))$

it doesn't follow that

(13) A

where A is an arithmetic assertion and where the number words are taken to refer to a range of things not already known to be the numbers. Consider the hypothetical progression of Roman emperors discussed in chapter two:

Julius Caesar, Octavian Augustus, Tiberius, Gaius Caligula, Claudius, Nero....

We are to imagine that the Roman empire had never ended, and that it never will, and that the world is never destroyed, so that we never run out of Roman emperors. If the reductionist rather fancifully chose this progression as the progression of natural numbers and interpreted arithmetic vocabulary accordingly, then the implication would fail. One cannot deduce from the general properties of progressions, for example, that Tiberius is the successor of Octavian Augustus (under the relation of Roman imperial succession). The reductionist, however, has undertaken a deliberate change of meaning. The structural biconditionals, if understood as analytic, in conjunction with (6) and (7), only provide an analytic guarantee that there are at least countably many things, not to the existence of the numbers. And as long as the referential content of our number words remains indeterminate, the reductionist is well within his rights to precisify our concept of number as he sees fit. Insofar as precisification is a deliberate change of meaning, and as a change of meaning is always a conventional act, the reductionist is not required to respect the analytic equivalence in this way.

The objection, however, retains some point if we remember that this sort of semantic indeterminacy only makes sense in a *reductionist* setting. Notice the implication from (12) to (13) only fails on the assumption that the numbers are a range of things not already known to be the numbers, i.e., that an appropriate analysis of number will have as a consequence identity statements of the form:

$$n = q$$

where 'n' is a numeral and 'q' is a singular term that is neither a numeral nor of the form 'the number of so-and-so's.' The difficulty is that the designata of such terms have necessary features that are no part of our concept of number and are thus not implied by the relevant modal facts. But suppose that the reductionist assumption is wrong (as it *must* be, given the conservative nature of arithmetic). Suppose that among the candidate progressions are the numbers themselves (where the numbers are conceived to be sui generis). The nice thing about sui generis numbers, if such there be, is that their necessary conditions are *exhausted* by the structural biconditionals. What other necessary conditions could they have? To suppose that they have some secret nature not revealed by mathematical practice is nothing less than an occult fancy. (In some sense this is the point that Frege is pursuing in the opening passages of the *Foundations*.) And if that's right, when A in (13) is interpreted in terms of the progression of sui generis numbers, then (13) is a genuine consequence of (12). So-interpreting our arithmetic assertions has the advantage of respecting the analytic equivalence--an advantage shared by no sequence of things not already known to be the numbers.

Earlier I emphasized that disavowing uniqueness is a kind of *skepticism*. Such a disavowal is skeptical insofar as some form of unsophisticated platonism is our natural pre-philosophical starting point. This epistemological observation is crucial in evaluating the present dialectic. If the denier of uniqueness is, at bottom, a kind of skeptic, then the burden of proof is on the skeptic to persuade us to abandon, or at least to suspend, this belief. If we may retain belief in uniqueness on the supposition that the natural numbers are sui generis, and if we have no reason to doubt that numbers could be sui generis, then, given a sufficiently strong commitment to uniqueness, the balance of reasons favors belief in sui generis numbers. My point is that *absent some positive reason for disavowing sui generis numbers*, it is *rationally permissible* to retain our belief that numerals function as unambiguous names.[16]

Like all burden of proof considerations, this is rather weak, I concede. But there is more to be said against the insistence that candidate designata of our number words must be something other than the numbers themselves. Notice the reductionist reinterpretation is only available if there are at least countably many things not already known to be the numbers. Suppose that there are actually only finitely many reductive candidates. There won't then be enough candidate referents to constitute a progression. But if (6) and (7) are in order, then the structural biconditionals imply that there are at least countably many things. The countably many whose existence is implied then must be, at least in part, something *other* than our original reductive candidates.[17]

Someone might claim that, in saying this, I am open to the following ad hominem objection. In chapter three I endorsed, with qualification, an objection, due to Hartry Field, to interpreting the modality involved as being distinctively *mathematical*.

[16]Which is not to say that there are no arguments against sui generis numbers. For such an argument see Hodes (1984). Unfortunately full consideration of Hodes' argument is beyond the scope of the present essay.

[17]For a similar use of cardinality considerations to support platonist existence claims see Rosen (1993).

According to Field, it is part of the meaning of mathematical possiblity that a rule of necessitation holds for purely mathematical sentences. We saw that as a consequence each instance of the schema, '$\Diamond_M A \supset A$', is a valid formula. Field claims that if A implies the existence of mathematical entities, then mathematical possibility can't help eliminate this commitment. I pointed out that the existential conjunct governed by diamond doesn't imply the existence of *numbers*, but of *progressions*. Modal structuralism, interpreted in terms of mathematical possibility, implies that there are at least countably many things. This, I claimed, was bad enough, since the *modal* formulation was motivated explictly by the idea that arithmetic truth shouldn't depend on controversial existence assumptions from other areas. But how can I endorse this objection *and* consistently endorse modal platonsim? Modal platonsim, after all, also implies that there are at least countably many things. On pain of inconsistency, it would appear that I must either withdraw the objection to mathematical possibility or abandon modal platonism.

This conclusion is too hastily drawn. It proceeds on the assumption that the countably many must be a range of things not already known to be the numbers. If the countably many are the numbers themselves, then there is no difficulty. Arithmetic truth won't depend on extra-arithmtic existence assumptions but rather, quite sensibly, on the natural numbers alone. (Of course the modal structuralist, if he is an ontological reductionist like Hellman, can't make this reply--he is, after all, an *irrealist* about the numbers.) The point can be made in another way. (1), (6), and (7) imply that there are at least countably many things. If arithmetic is to be *conservative* (in something like Field's sense--as Field insists all mathematical theories must be), we must give up the substantive assumption that the countably many are not simply the numbers themselves.

5.5 *Conceptual Equivalence and the Identification of Fact*

Suppose, then, we are to grant the intelligibility of sui generis numbers. Suppose further we are to grant the minimalist account of referential candidacy and the principle that there is something that a name occurring in a true sentence of the relevant sort designates. What then is there to stand in the way of the claim that arithmetic assertions can be interpreted at face-value while being conceptually equivalent to the appropriate modal generalizations and thus that each represents the same domain of fact? There may be two sorts of residual doubt: The positive reason we have for interpreting each at face-value might cast doubt either on (i) their putative conceptual equivalence or (ii) the claim that they describe the same range of fact. Recall the ontological reductionist and the ontological inflationist share the following doctrine: It is impossible to interpret both the right- and left-hand sides of the biconditional at face-value given the conceptual equivalence and the apparent discrepancy in ontology. The present worry is of a peice: Given that we must interpret each at face-value, the conceptual equivalence (or the identification of fact) fails.

The first thing to notice is that a doubt about the identification of fact is subsumed under the stronger doubt about conceptual equivalence. To see this let's consider the objection that if we interpret our arithmetic assertions and their modal counterparts at face-value, they can't describe an identical range of fact. Perhaps, as is plausible, facts have a structure mirroring the sentences that express them. If facts have a sentence-like structure, then how can arithmetic facts be *identical* to certain modal structural facts? Suppose the arithmetic assertions we accept are interpreted at face-value and, further, are regarded as true. The facts expressed by such assertions, if they are indeed true, involve the existence and special properties of numbers. But the corresponding modal facts don't apparently involve the existence of any entities, so how can they be identical to the arithmetic facts? So far, all that I have been assuming is that our casual talk of facts is in order--that (i) the transition from 'It is true that p' to 'It is a fact that p' is unproblematic, and (ii) any two synonymous, declarative sentences would express the same fact if true. I have not been relying on any more substantive, metaphysically-loaded conception of fact. Both claims have, I contend, the status of conceptual truths and thus cannot be denied without changing the subject. Given that whether or not two sentences express the same fact is a matter of their sharing the same propositional content, a doubt about the identity of fact must involve a doubt about the identity of content. To claim that arithmetic assertions, when interpreted at face-value, represent a domain of fact distinct from that represented by the corresponding modal assertions *just is* to deny their conceptual equivalence. And so, any doubt about the sameness of fact entails a corresponding doubt about the putative conceptual equivalence.

Why might someone deny the putative conceptual equivalence? Someone may accept the minimalist account of referential candidacy and thus grant that the surface commitments of our arithmetic assertions and their modal counterparts are genuine, but deny that they are synonymous *precisely in virtue of* the pressure to regard each at face-value.[18] The first thing to notice, when considering this objection, is that a minimalist account of referential candidacy, while guaranteeing the surface commitments of our

[18]This is roughly the position Field takes with respect to Frege's direction example. Cf. Field (1984b).

assertions to be genuine, doesn't, however, guarantee that the ontological commitments explicitly represented by surface grammar are *exhaustive*. Strictly speaking, minimalism is consistent with the view that a sincere assertion may carry commitment to *more* than what's involved in a face-value interpretation. Minimalism, by itself, can't then cast doubt on the putative conceptual equivalence.

Continuing, however, this skeptical train of thought, it might further be objected that since our arithmetic assertions and their modal counterparts, when interpreted at face-value, apparently differ in their ontological commitments, it is rationally permissible to believe one while denying the other, and thus the synonymy claim fails.[19] While prima facie plausible, I believe that this objection involves an optional, and, in the end, controversial, assumption about meaning--that the content of our thought and talk is *epistemically transparent*. For our present purposes, let the epistemic transparency of meaning be the thesis that:

[19]Field makes a similar objection to Wright's reconstruction of Frege's direction example (Field (1984b), p.166): "I don't see how the existence of objects of any sort can follow logically from the existence of objects of an entirely different sort. To put the point another way, I do not see how it can be maintained that a theory like direction theory that postulates new entities (directions) conditional on old entities (lines) is a theory that *cannot rationally be doubted...*"

(14) If it is rationally permissible to believe (or sincerely assert) the proposition expressed by a sentence S while sincerely denying the proposition expressed by a sentence S', then S and S' fail to be synonymous.

This principle can be extended to subsentential expressions in the obvious way:

(15) If expressions e and e' belong to the same syntactic category, then if it is rationally permissible to believe (or sincerely assert) the proposition expressed by the sentence S[...e...] while sincerely denying the proposition expressed by S[...e'...], then e and e' fail to be synonymous (where the sentence S[...e'...] is arrived at by substituting e' for at least one occurrence of e in the sentence S[...e...]).

Why should we believe that meaning is epistemically transparent? Notice, that the transparency of meaning is incompatible with many contemporary proposals concerning the determinants of meaning. Let's consider just one example. Suppose that the meaning of theoretical terms in science is fixed by their role in the theory, where their theoretical role can be represented by a Ramsey sentence formed by replacing each occurrence of the theoretical term with an appropriate variable bound by an initial existential quantifier of the suitable order.[20] Suppose that in the course of developing some empirical theory we introduce a theoretical term t and, much later, introduce a theoretical term t'. Suppose, moreover, that t and t' turn out to have precisely the same theoretical role--t and t' would then have the same meaning. But if the theory is sufficiently lengthy and complex it is possible that we fail to notice that t and t' mean the same even after their usage is sufficiently entrenched. If that's right, it could turn out that someone may be in a position to assert S[...t...] while denying the proposition expressed by S[...t'...]. It seems to me that that this is a genuine possibility may well be due to the cognitive limitations of creatures like ourselves and need not impugn our rationality. If that's the case, then transparency fails on such an account. Without going into details, the transparency of meaning also seems, at least problematic, if not incompatible with accounts of meaning where contents are individuated in terms of relations borne to a subject's physical or social environment, with popular accounts from the philosophy of mind such as correlational psychosemantics, conceptual role theories, etc.[21]

Pointing out how the transparency of meaning fails for one or

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[20]Cf. Lewis (1983).

[21]For a useful discussion of the epistemology of meaning see Paul Boghossian (1989b).

another account of meaning only accomplishes so much. It would cast doubt on the principle only insofar as we lend credence to one or another of these accounts, and the problem is that *what meaning is* is itself a controversial matter. A more persuasive consideration is that the transparency of meaning runs afoul of what I take to be the most plausible resolution to the *paradox of analysis*--that while there is a sense in which we unproblematically know what we mean, this doesn't thereby guarantee that we are experts at conceptual clarification. And if that's right, then the fact that a competent sincere speaker may assert a modal formula while denying it arithmetic counterpart (perhaps because of the undue influence of contemporary nominalism) is insufficient reason to deny their putative conceptual equivalence.

Let us return to Frege's complaint. Frege believed that the proper analysis of number must take the form of a reductive identification if it is to reveal something about the nature of the natural numbers. To simply say that the numbers are themselves and not another thing does almost nothing to further our understanding. Though I do believe that the numbers are sui generis, I don't believe that rehearsing Butler's dictum that everything is what it is and not another thing exhausts all that we can say about the nature of the natural numbers. Indeed I believe that careful reflection on our concept of number reveals arithmetic facts to be identified with a range of modal facts--that the numbers are *constituted* by the distribution of certain complex modal properties. On the face of it, this is unobvious, I concede. But if correct, haven't we *discovered* something important about the natural numbers even though our analysis doesn't take the form of a reductive identification as Frege urged?

CHAPTER FIVE

Modal Platonism

5.1 *From Minimal Platonism to Modal Platonism*

The arithmetic assertions of the minimal platonist are, so to speak, *guileless*. Good man that he is, he means what he says and believes what he means: Arithmetic is interpreted at face-value, and our practice of fixing arithmetic opinion is understood by him to at least deliver reasonable belief. If minimal platonism correctly represents our naive view of arithmetic, then modal platonism goes a long way towards vindicating commonsense. After all, the modal platonist also insists that the surface commitments of our arithmetic assertions are genuine, and that we have good reason to believe the arithmetic assertions we accept. The difference between the minimal platonist and the modal platonist is a matter of philosophical theory. What makes naive platonism *minimal* is an agnosticism concerning distinctively philosophical theses about the nature of the natural numbers and our relation to them. The minimal platonist suspends judgement about (and probably has never even entertained) philosophical claims such as numbers are classes of equinumerous classes, or that our arithmetic knowledge is mediated by a special faculty of intuition, etc. The modal platonist only parts company with his naive cousin in abandoning such philosophical aloofness. And for good reason. The characteristic commitments of the minimal platonist may only be retained in light of Benacerraf's skeptical challenge by acquiescing to philosophy. Minimal platonism is an entrenched commitment of ours, one that we bring with us to philosophy. If it is indeed a norm of general epistemology that it is rationally permissible to retain our beliefs as long as we have no positive reason to change our minds, then we may retain our naive view of arithmetic unless confronted with some compelling and countervailing reason. Benacerraf's challenge is such an invitation to change our minds, and a distinctively philosophical one. Given certain natural, if philosophical, assumptions, platonism is untenable, or so Benacerraf argues. The conservative nature of belief fixation in conjunction with our naive platonism, provided reason to question these assumptions and thus enter into philosophical debate.[1] And so, with innocence lost, we arrive at modal platonsim--the view that arithmetic should be interpreted at face-value while being conceptually equivalent to a collection of modal generalizations.

In chapter one I described this argumentive strategy as methodological conservativism. Does methodological conservativism provide us conclusive reason to believe modal platonism? Though

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[1]Of course, the minimal platonist's philosophical agnosticism may be retained if, for instance, it is a sufficiently entrenched commitment and if the minimal platonist is sufficiently apprised of philosophy's track record at "establishing" surprising conclusions at variance with common sense.

methodological conservativism provides us *some* reason to believe modal platonism, its truth is far from established. The difficulty is that there are a number of different ways to resist Benacerraf's skeptical conclusion.[2] And each, if pursued far enough, may lead to different conceptions of the natural numbers. Modal platonism is thus a philosophical *hypothesis* about what the numbers could be. In the last chapter I presented something like an a priori argument for the existence of the natural numbers. Isn't modal platonism thus something more a philosophical hypothesis, i.e., a coherent thesis whose denial is rationally permissible? Unfortunately the argument rests on two important claims for which I have given no defense: (i) the structural and cardinality constraints are not only individually necessary, but also jointly sufficient, and (ii) a singular term purports to refer just in case it has the right syntax and inferential role. I am afraid, then, that modal platonism is not a doctrine whose denial cannot be rationally entertained. That being said, I would like to emphasize an explanatory virtue of modal platonism, one that gives it a relative advantage over other available platonist analyses--it can explain why the numbers exist necessarily.

5.2 *Why the Numbers Exist Necessarily*

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[2]I discussed what I take to be the most important ways in chapter two.

The modal platonist has the resources to explain what other platonists cannot: the necessary existence of the natural numbers. That mathematical entities exist necessarily if they exist at all is a persistent intuition in our reflection on mathematical practice. Unfortunately, most platonists offer no explanation of this.[3]

Before presenting the explanation that is available to the modal platonist, I should make a few remarks concerning the nature of the explanatory demand. I don't mean to be claiming that every existence claim requires an attendant explanation in order to be rationally acceptable. After all, explanations have to come to an end somewhere. I doubt, for example, that an explanation for the existence of matter is forthcoming, but I don't think that this should undermine our belief in matter. Similarly, *if* the question, 'Why is there something rather than nothing,' is a demand for an explanation, then I believe the question should be rejected--since I doubt that anything *could* count as an

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[3]This might not be a bad thing. It is perhaps open to someone to claim that the necessary existence of mathematical entities is a bit of metaphysical folk-lore that may be dispensed with without impugning our mathematical practice. Perhaps, but such a theorist must be able to explain the persistence of the modal intuition in some other way than as a reflection of mathematical reality. For a "hygenic" explanation of this sort see Field's "Realism, Mathematics, and Modality." Field suggests that the alleged modal intuition is actually an inarticulate recognition of a special property of mathematics--its *conservativeness*. While necessary truth implies conservativeness, conservativeness doesn't imply necessary truth (or even truth for that matter).

explanation.[4] I contend that, everything else being equal, we would prefer an explanation for an existence claim if it were available. I don't think that the situation is very different when it comes to *modal* existence claims except in this regard: Suppose that someone claims that a range of controversial entities, the F's, exist and furthermore exist necessarily. That a commitment to the F's is *controversial* is testimony to the fact that the non-existence of the F's is at least *conceivable*. If a certain state of affairs is conceivable, this provides us with at least a defeasible presumption in favor of its possibility; and if the F's might not exist, then they don't exist necessarily. The theorist who would defend the necessary existence of the F's must present countervailing evidence against the conceivability claim. If no countervailing evidence is forthcoming, the debate evidently stalls. (Although the theorist's commitment to the necessary existence of the F's isn't *thereby* rationally undermined--it is, perhaps, all too easy to erase in thought a given type of object.) Everything else being equal, an account which had the resources to explain why the F's exist necessarily would be preferable to an account which either offered no such explanation or precluded the very possibility of such an explanation.

Consider, then, modal platonism. If arithmetic may be

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[4]In fairness to Heidegger, whatever the true intent of his question, it is doubtless *not* a demand for an explanation in the requisite sense.

interpreted at face-value while being conceptually equivalent to a collection of modal generalizations, then an explanation of the necessary existence of the natural numbers is forthcoming. By the axioms of second-order S5, the modal generalizations, if true, are necessarily true. Given the equivalence expressed by the structural biconditionals, it follows that our arithmetic assertions are themselves necessarily true, if true. Given minimalism about referential candidacy, our arithmetic assertions must be interpreted at face-value. If, when so-interpreted, they imply the existence of numbers and are, moreover, true, then the natural numbers exist necessarily. The explanation proceeds in two stages: the necessary existence of the numbers is explained by the fact that the modal translations of our arithmetic talk are, if true, necessarily true which is in turn explained by the meaning of the relevant modal idiom. (Suppose, as is plausible, that the axioms governing second-order S5 codify rules of use for the modal operators that are at least partially constitutive of their meaning, then the necessitation is ultimately explainable in terms of the content of the modal operators.) That such an explanation is available to the modal platonist is an explanatory virtue of the account and gives modal platonism a relative advantage over other platonist analyses.

5.3 *Numerical Reference*

Not only can modal platonism explain why the natural numbers exist necessarily, it can also explain why our number words refer to the natural numbers. The explanation is broadly naturalistic in the sense that has come to function as a regulative ideal of contemporary philosophical commonsense. Of course such naturalism isn't easy to state, easy formulations being doomed either to triviality or falsehood.[5] Despite its resistance to explicit formulation, the operation of such naturalism is relatively clear, and I hope to at least show that the modal platonist explanation is adequate by whatever standards govern this notion.

The linchpin of Benacerraf's skeptical argument is the claim that the following constraints are individually necessary *and* jointly sufficient conditions on an adequate analysis of number:

(i) The structural constraint: The candidate objects ought to form a progression.

(ii) *The cardinality constraint:* The cardinality relation ought to be suitably coordinated with the candidate progression--something like the following:

Nx(Fx) = n iff there is a 1-1 correspondence between the F's and the numbers preceding n

[5]Yet, pehaps like jazz, if you got to ask, you'll never know.

ought to be a provable consequence of the axioms governing the candidate objects and the proposed definitions;

Recall, the structural and cardinality constraints, though explicitly stated as conditions on *objects*, are understood by Benacerraf to encode meaning determining constraints on our use of arithmetic vocabulary. Roughly speaking, Benacerraf agrees with Wittgenstein that the use of our ordinary numerals is entirely given by reciting them in order and using them to count transitively.[6] Not only, then, are these constraints understood to be meaning determining, but exhaustively so. Benacerraf claims that the structural and cardinality constraints are individaully necessary and jointly sufficient *because* the constraints on usage they represent are conceived to be exhaustively meaning determing. In chapter three I argued that, if the structural and cardinality constraints really are individually necessary and jointly sufficient, then a modal structuralist analysis is the best representation of the content of our arithmetic assertions. If this argument is cogent, and if Benacerraf is right about the joint sufficiency of the structural and cardinality constraints, then there is no mystery about how our arithmetic assertions came to have the truth-conditions assigned them by a modal structural analysis.

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[6]Cf. section 10 of the *Philosophical Investigations*.

But that is only part of the account. What secures numerical reference is minimalism about referential candidacy and the doctrine (platitude?) that there is something that a singular term occurring in a true sentence of the relevant sort designates. It is because our ordinary numerals have the syntax and inferential roles that they do, that they can be legitimately seen as function semantically as singular terms. And since, moreover, they sometimes occur in true sentences of the relevant sort (given the modal structuralist truth-conditions), there are some things to which they refer--the natural numbers.

There is a feature of this account that I find attractive independent of its (alleged) truth. Any adequate account of the determinants of numerical reference must go some way towards dispelling what I take to be a bad picture of reference to the natural numbers (and to mathematical entities generally). It is, perhaps, an insurmountable obstacle to understanding the semantics of arithmetic to conceive of our number words as "tags" or "labels". Roughly the picture is that one must specify in a non-circular fashion the referents of our number words *prior to* legislating their use. It is this picture that invites the familiar nominalist objection that, given a causal theory of reference, reference to abstracta is impossible. But why think of our number words as functioning in this way? Reference to the natural numbers isn't fixed by any kind of initital "baptism." It's not as if we first encounter the natural numbers and then attach labels to them (as Peter Strawson once grumbled about facts--its not as if we can trip over them or spill our coffee on them). Rather it is our use of the numerals in a calculus that determines their meaning.[7] David Kaplan gives admirable expression to this:[8]

> There is a sense in which the finite ordinals...find their essence in their ordering. Thus names which reflect this ordering in an *a priori* way, as by making true statements of order analytic, capture all that is essential to these numbers. And our careless attitude towards any intrinsic features of these numbers (e.g., whether zero is a set, and if so, whether it has any members) suggests that these names have captured all that there is to numbers.

This alternative picture, though vague, is a useful

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[7]In his paper "Nominalism," Dummett's anti-nominalist strategy revolves around deploying Frege's context principle to flesh out just this intuition: "The mistake which makes Frege's view difficult to accept, which makes one feel that '28' does not really stand for anything as 'Eisenhower' does, is the idea that proper names are the simplest parts of language, hardly parts of language at all. This rests on imagining that learning the sense of a proper name consists in learning to attach a label to an object *already picked out as such*..."[my emphasis]; in *Truth and Other Enigmas*, Harvard University Press (1978). The idea is also implicit in the opening passages of Wittgenstein's *Philosophical Investigations*.

[8]"Quantifying In" in *Reference and Modality*, ed. by Leonard Linsky, Oxford University Press (1971). Kaplan also points out that this conception of numerical reference is anticipated by Carnap's notion of an 'expression of standard form.' Cf. section 18 of *Meaning and Necessity*, University of Chicago Press (1958).

corrective in thinking about the semantics of arithmetic. It is, though, just a picture. The nice thing about the modal platonist account of numerical reference is that it is a reasonably tractable (if debatable) philosophical *theory*, that conforms to the alternative picture. That the structural and cardinality constraints determine modal structuralist truth-conditions for our arithmetic assertions is one possible precisification of the claim that our use of numerals in a calculus determine their meaning. Moreover, minimalism about referential candidacy, if true, would explain how our number words are nonetheless referential. Formalism is right to this extent: In order to legislate a use for our number words we don't have to antecedently attend to a domain of abstracta, and the content of our number words is determined by their use in a calculus. But for all that, numerical singular terms aren't irreferential. Regardless of the truth of modal platonism, I am convinced that picturing our ordinary numerals as labels is to badly misconceived their semantic function. It is thus a virtue of modal platonism that it presents a viable alternative to this picture.

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