NUMBERS WITHOUT SCIENCE

by

Russell Marcus

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Abstract

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Numbers without Science opposes the Quine-Putnam indispensability argument, seeking to undermine the argument and reduce its profound influence. Philosophers rely on indispensability to justify mathematical knowledge using only empiricist epistemology. I argue that we need an independent account of our knowledge of mathematics.

The indispensability argument, in broad form, consists of two premises. The major premise alleges that we are committed to mathematical objects if science requires them. The minor premise alleges that science in fact requires mathematical objects.

The most common rejection of the argument denies its minor premise by introducing scientific theories which do not refer to mathematical objects. Hartry Field has shown how we can reformulate some physical theories without mathematical commitments. I argue that Field's preference for intrinsic explanation, which underlies his reformulation, is ill-motivated, and that his resultant fictionalism suffers unacceptable consequences.

I attack the major premise instead. I argue that Quine provides a mistaken

criterion for ontic commitment. Our uses of mathematics in scientific theory are instrumental and do not commit us to mathematical objects. Furthermore, even if we accept Quine's criterion for ontic commitment, the indispensability argument justifies only an anemic version of mathematics, and does not yield traditional mathematical objects.

The first two chapters of the dissertation develop these results for Quine's indispensability argument. In the third chapter, I apply my findings to other contemporary indispensabilists, specifically the structuralists Michael Resnik and Stewart Shapiro. In the fourth chapter, I show that indispensability arguments which do not rely on Quine's holism, like that of Putnam, are even less successful. Also in Chapter 4, I show how Putnam's work in the philosophy of mathematics is unified around the indispensability argument.

In the last chapter of the dissertation, I conclude that we need an account of mathematical knowledge which does not appeal to empirical science and which does not succumb to mysticism and speculation. Briefly, my strategy is to argue that any defensible solution to the demarcation problem of separating good scientific theories from bad ones will find mathematics to be good, if not empirical, science.

Preface

The indispensability argument alleges that we have knowledge of the abstract objects of mathematics, and that this knowledge is justified by our uses of mathematics in empirical science. The evidence for mathematics is thus supposed to be empirical. In this dissertation, I defend mathematical knowledge while denying its empirical justification. Mathematics requires an epistemology independent of that for empirical science.

There are at least three ways of arguing for empirical justification of mathematics. The first is to argue, as Mill did, that mathematical knowledge is knowledge of empirical objects. I will not pursue criticisms of this dead end; I refer the reader to Frege (1953).

The second is to argue that while mathematical knowledge is knowledge of abstract objects, we have sensory access to such objects. I will not pursue this Aristotelian line, either. I take the fallout from Penelope Maddy's attempt, including her own rejection of that position, to suffice.[1]

The currently most popular way to justify mathematics empirically is to argue:

A) Mathematical knowledge is of abstract objects;
B) We have experiences only with concrete objects; and yet
C) Our experiences with concrete objects justify our mathematical knowledge.

This is the Quine-Putnam Indispensability Argument, in its broadest form. In Chapter 1 of the dissertation I present and defend my interpretation of Quine's version of

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[1] See Maddy (1980) for the position, and Balaguer (1994) for an excellent summary of problems with the position.

the indispensability argument, which depends on his method for determining ontic commitment. I also show how the most popular and promising response to Quine's argument, Field's dispensabilist project, fails to defeat it.

In Chapter 2, I provide three criticisms of Quine's argument. In Part 1, I deny Quine's method for determining ontic commitment. In Part 2, I defend instrumentalism as an alternative to Quine's method. In Part 3, I show how even if we accept Quine's method, the indispensability argument does not yield mathematical objects.

In Chapters 3 and 4, I consider other versions of the indispensability argument. In Chapter 3, I present essential characteristics of indispensability arguments, and the unfortunate consequences of these arguments. I apply these general results to structuralists who rely on the indispensability argument. Part 1 of Chapter 4 is an aside on the central role that the indispensability argument plays in Hilary Putnam's work. In Part 2, I show how non-holistic indispensability arguments, Michael Resnik's pragmatic argument and Putnam's success argument, fare no better than holistic versions.

The concluding Chapter 5 has four parts. In Part 1, I argue for the legitimacy of mathematics as a science in its own right. In Part 2, I argue that we should pursue an epistemology for mathematics independent of that for empirical science. I characterize several elements of this approach, which I call autonomy realism, including its dependence on mathematical intuition. In Part 3, I revisit Field's work, as he also seeks an alternative to the indispensability argument. I show that autonomy realism is a preferable alternative to both indispensabilism and Field's fictionalism. I conclude by indicating a few areas for further research.

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But most of all, Emily, whose confidence in me, puzzlingly, never wavered.

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Some Abbreviations Used in the Dissertation

QI, Quine's Indispensability Argument:

QI.1: We should believe the theory which best accounts for our empirical experience.
QI.2: If we believe a theory, we must believe in its ontic commitments.
QI.3: The ontic commitments of any theory are the objects over which that theory first-order quantifies.
QI.4: The theory which best accounts for our empirical experience quantifies over mathematical objects.
QI.C: We should believe that mathematical objects exist.

QP, Quine's procedure for determining our ontic commitments:

QP.1: Choose a theory.
QP.2: Regiment that theory in first-order logic with identity.
QP.3: Examine the domain of quantification of the theory to see what objects the theory needs to come out as true.

Ground rules for dispensabilist reformulations:

GR.1: Adequacy: A reformulation must not omit empirical results of the standard theory.
GR.2: Logical Neutrality: A reformulation must not reduce ontology merely by extending logic, or ideology.
GR.3: Conservativeness: The addition of mathematics to the reformulated theory should license no additional nominalist conclusions.
GR.4: Attractiveness: The dispensabilist must show, "[T]hat one can always reaxiomatize scientific theories so that there is no reference to or quantification over mathematical entities in the reaxiomatization (*and one can do this in such a way that the resulting axiomatization is fairly simple and attractive*)." (Field (1980) p viii, emphasis added)

The Essential Characteristics of indispensability arguments:

EC.1: Naturalism: The job of the philosopher, as of the scientist, is exclusively to understand our sensible experience of the physical world.

EC.2: Theory Construction: In order to explain our sensible experience we construct a theory of the physical world. We find our commitments exclusively in our best theory.

EC.3: Mathematization: We are committed to some mathematical objects and/or the truth of some mathematical statements, since they are ineliminable from that best theory.

EC.4: Subordination of Practice: Mathematical practice depends for its legitimacy on empirical scientific practice.

The Unfortunate Consequences which arise from reliance on an indispensability argument:

UC.1: Restriction: Our commitments are to only those mathematical objects required by empirical science. Mathematical results which are not applied in scientific theory are illegitimate.

UC.2: Ontic Blur: Mathematical objects are concrete.

UC.3: Modal Uniformity: Mathematical objects do not exist necessarily.

UC.4: Temporality: Mathematical objects exist in time.

UC.5: Aposteriority: Mathematical objects are known a posteriori.

UC.6: Uniqueness: Any debate over the existence of a mathematical object will be resolved by the unique answer generated by empirical theory.

Chapter 1: Quine's Indispensability Argument and Field's Response

Part 1: Quine's Indispensability Argument

§1.1: Quine's Argument

Quine nowhere presents a detailed indispensability argument, though he alludes to one in many places.[2] In Part 1 of this chapter, I first present a concise version of the argument, and proceed to discuss Quine's defenses of each premise. I indicate how the argument relies on various aspects of Quine's methodology, including his procedure for determining ontic commitment, physicalism, and confirmation holism. Lastly, I defend the argument against modal reinterpretations of mathematics. In Part 2 of the chapter, I examine Hartry Field's response to Quine.

Quine's indispensability argument can be stated rather simply:

(QI) QI.1: We should believe the theory which best accounts for our empirical experience.
QI.2: If we believe a theory, we must believe in its ontic commitments.
QI.3: The ontic commitments of any theory are the objects over which that theory first-order quantifies.
QI.4: The theory which best accounts for our empirical experience quantifies over mathematical objects.
QI.C: We should believe that mathematical objects exist.

The conclusion of Quine's indispensability argument is thus that as far as we know, mathematical objects exist. Our knowledge of these objects is justified by the empirical science at the core of our best theory.

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[2] Among them, Quines (1939a), (1939b), (1948), (1951), (1953a), (1954), (1955), (1958), (1960a), (1964), (1969b), (1976b), (1978a), (1981b), (1986b), and (1992).

§1.2: A Best Theory

The first step of Quine's argument is to settle on a single empirical theory to which we can look for our commitments. I examine two questions regarding QI.1. First, why should we find our commitments exclusively in an empirical theory? Second, why should we look to a single theory, which, for Quine, is a reductive physical theory?

Quine's belief that we should defer all questions about what exists to empirical science is really an expression of his naturalism, which he describes as, "[A]bandonment of the goal of a first philosophy. It sees natural science as an inquiry into reality, fallible and corrigible but not answerable to any supra-scientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method." (Quine (1981c) p 72)

Quine contrasts his naturalism, or relative empiricism, to the phenomenalist's radical empiricism which requires that all knowledge be reducible to claims about sense data.[3] Instead of starting with sense data and reconstructing a world of trees and persons, Quine assumes that ordinary objects exist. Further, Quine starts with an understanding of empirical science as our best account of the sense experience which gives us these ordinary objects. The job of the naturalist epistemologist, at least in part, is to describe the path from stimulus to science, rather than justify knowledge of either ordinary objects or scientific theory.[4]

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[3] In early work, Quine was agnostic between physicalism and phenomenalism. See, for example, the end of Quine (1948).

[4] We may find that our best theory rejects the existence of ordinary objects in favor of an ontology of just space-time, fields and their values at different points. In this

Quine allows for mathematical objects by rejecting the phenomenalist's requirement for individual reductions of scientific claims to sense data. He only requires that the justifications of our scientific theory, taken as a whole, be empirical. In part, he rejects the phenomenalist's project on its own demerits, the impossibility of actually tracing the course from what appears to us in raw experience to the general and abstract claims of empirical science. More importantly, Quine rejects phenomenalism on the basis of his insight that we can adjust any theory to accommodate any evidence. The phenomenalist describes a system of piecemeal theory construction, where our individual experiences are each independently assessed. In contrast, Quine defends confirmation holism, that there are no justifications for particular claims independent of the justification of our entire best theory. Confirmation holism arises from an uncontroversial logical claim, that any sentence s can be assimilated without contradiction to any theory T, as long as we readjust truth values of any sentences of T that conflict with s. These adjustments may entail further adjustments, and the new theory may in the end look quite different. But we can, as a matter of logic, hold on to any sentence come what may.[5]

Since Quine relegates all questions about what exists to holistic empirical science,

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case, we can accept only space-time points into our ontology, accounting for trees and persons on that basis. See Quine (1976b) for just such a position.

[5] Confirmation holism is distinct from semantic holism, the claim that meaning is a property of entire theories rather than individual terms or sentences. Semantic holism is one of the few Quinean doctrines generally irrelevant to his indispensability argument, which does not rely on a particular interpretation of the meanings of terms or sentences. For the remainder of the dissertation, 'holism' refers to confirmation holism.

he rules out independent justifications for formal sciences like mathematics while allowing that mathematical knowledge can be justified as a part of our best theory.

Quine's naturalism may best be seen as a working hypothesis in the spirit of Ockham's razor. We look to our most reliable endeavor, empirical science, to tell us what there is. We bring to science a preference that it account for our entrenched esteem for ordinary experience. And we posit no more than is necessary for our best scientific theory.

Quine's naturalism, though, does not settle any questions regarding the nature or structure of empirical theory. In particular, it does not determine whether empirical science is really just physics, or whether it has various, autonomous branches. Call scientific pluralism the position which accepts various branches of science (biology, neuroscience, semantics, economics, etc.) as independent and non-reducible. The pluralist sees our best theory as some sort of amalgam of various areas of science.

In places, Quine seems to adopt scientific pluralism. He repeatedly presents the example, from Frege, of a set-theoretic definition of ancestor, as one of various,

> [O]ccasions which call quite directly for discourse about classes. One such occasion arises when we define ancestor in terms of parent, by Frege's method: x is ancestor of y if x belongs to *every class* which contains y and all parents of its own members. There is this serious motive for quantification over classes; and, to an equal degree, there is a place for singular terms which name classes - such singular terms as 'dogkind' and 'the class of Napoleon's ancestors'. (Quine (1953a) p 115; see also Quine (1960a), §48 and §55; and Quine (1981b) p 14)

Quine's references to statistical generalities also make him appear pluralistic. He countenances groups of people rather than the collections of elementary particles which would concern the reductive physicalist. "Classes [belong in our ontology] too, for whenever we count things we measure a class. If a statistical generality about populations quantifies over numbers of people, it has to quantify also over the classes whose numbers those are." (Quine (1981b) p 14)

Even in the discussions of space and time, which do seem relevant to a physicalist theory, Quine's examples reflect mundane applications of mathematics, rather than ones that might be used in a complete physics. "When we say, e.g. that four villages are so related to one another as to form the vertices of a square, we are talking of the arithmetical relation of the distance measurements of these villages." (Quine (1974) p 133)

But Quine seems really to intend such talk of common uses of mathematics as merely precedental of the kind of uses that one would have in a mature physical theory. Quine is most accurately seen as a physicalist about our best scientific theory, one who believes that this theory will consist of the axioms of a completed physics.[6] This is the position presupposed by Field's response to the indispensability argument, Field (1980). Also, regarding Goodman's pluralism of world versions, Quine writes, "I take Goodman's defense of it to be that there is no reasonable intermediate point at which to end it. I would end it after the first step: physical theory." (Quine (1978b) p 98)

Putnam also ascribes physicalism to Quine. "Quine proposes to reduce logic, mathematics, and philosophy itself to physics. The price he pays for his futurism is an

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[6] In addition to the supporting citations in this section, see Quine (1992), p 72; Quine (1981b) p 23; Quine (1990b); and Stroud (1990).

enormous implausibility: virtually no one has followed Quine into the belief that the axioms of number theory, for example, are justified by their (indirect) utility in physics and natural science." (Putnam (1981d) p 183)

The question of whether to be a physicalist or a pluralist is most relevant to QI.4. The physicalist needs to find out whether uses of mathematics in physical science are eliminable. The pluralist must wonder whether one could eliminate mathematics from a broad range of scientific theories.

I proceed by taking Quine as a physicalist. But neither Quine's physicalism nor his naturalism determine the way in which we discover the commitments of our theories, which is the concern of further premises.

§1.3: Believing Our Best Theory

The second premise of Quine's argument states that our belief in a theory extends to the objects which that theory posits. I sketch Quine's argument for this premise, and examine his response to the criticism that any theory we currently believe is likely to be substantially false.

Quine's argument that there is no wedge between our belief in a theory and our beliefs in its objects is that any such distinction is double-talk. One can not arbitrarily commit only to certain elements of a theory which one accepts. If we believe a theory which says that there are ghosts, for example, then we are committed to ghosts. If we believe a theory which says that there are subvisible particles, then we are committed to subvisible particles. If our best theory posits centers of mass, or mathematical objects, then we must believe that these, too, exist.

Quine's response to Carnap's internal/external distinction relies on the double-talk criticism. The claim that there is a prime number between four and six seems to entail that a number exists. Carnap proposed that we can accept that five is prime, since that is an internal result within mathematics, without making the further step of accepting that numbers exist, which is properly speaking an external question about whether to adopt number language. Quine responds that if we accept that five is prime, then we are committed to its existence. If we reject number language, we can no longer claim that five is prime, since there are no numbers to be prime.

Putnam makes the double-talk criticism explicitly. "It is silly to agree that a reason for believing that p warrants accepting p in all scientific circumstances, and then to add 'but even so it is not *good enough*'." (Putnam, p 356)

Field also makes the double-talk criticism, specifically regarding mathematics. "If one *just* advocates fictionalism about a portion of mathematics, without showing how that part of mathematics is dispensable in applications, then one is engaging in intellectual doublethink..." (Field (1980) p 2)

QI.1 and QI.2 together entail that we should believe in the objects that our currently best theory says exist. These posits are made together, in the same way. Any evidence applies to the whole theory, which produces its yield uniformly. Quine thus makes no distinction between justifications of observable and unobservable objects, or between mathematical and concrete objects. All objects, trees and electrons and sets, are equally posits of our best theory, to be taken equally seriously. Call this aspect of

Quinean metaphysics, that all posits are made together with equal seriousness, homogeny. Homogeny is a manifestation of Quine's insistence that there is only a single way of knowing of anything. We receive sensory stimulus and construct a single theory to account for it. What exists, all objects, are the posits of that theory. "To call a posit a posit is not to patronize it." (Quine (1960a) p 22)

Homogeny permits Quine to avoid some criticism, as Charles Parsons notes. "In the case of abstract entities, certain protests against Platonism become irrelevant. There is no mysterious 'realm' of, say, sets in the sense that they need to have anything akin to location, and our knowledge of them is not based on any mysterious kind of 'seeing' into such a realm. This 'demythologizing' of the existence of abstract entities is one of Quine's important contributions to philosophy..." (Parsons (1986) pp 377-8)

But, there will be conflict between our currently best theory and ideal theories future science will produce. Ideal theories are, of course, not now available. What exists does not vary with our best theory. Thus, any current expression of our commitments is at best speculative. How can we hope to determine what exists on the basis of a continually changing science?

Facing the problem of the progress of science, one might mis-interpret the indispensability argument too strongly:

(SQI) SQI.1: What exists is what our ideal theory will quantify over.
SQI.2: Our ideal theory will quantify over mathematical objects.
SQI.C: Mathematical objects exist.

This stronger indispensability argument has a more compelling conclusion, but can not truly be ascribed to Quine. First, and most importantly, we do not know what our ideal theory will be, so we lack support for SQI.2. Additionally, Quine has several reasons for denying that we have any categorical knowledge of what exists, as SQI.C claims we do. Some of these reasons emerge in his later work on ontological relativity. Empirical theories are generally underdetermined by evidence, so we have to choose a best theory from among empirically equivalent options. Also, the objects to which a theory commits are found by examining models of our theories and our best theory will have multiple conflicting models. Thus, we can only determine a theory's commitments relative to a given model. The models themselves are subject to interpretation, as well.

We must have some skepticism toward our currently best theory, if only due to an inductive awareness of the transience of such theories. Applying this skepticism, one who denies Quine's indispensability argument might say that our best theory commits to mathematical objects, but we are not really committed to our best theory. Such skepticism, though, must be strictly speculative, and unavailable to Quine. For Quine, we are adrift on Neurath's boat, with no external, meta-scientific perspective from which to judge our best theory.

QI.1 and QI.2 say that we should believe that the posits of our best theory exist. They do not tell us how to determine what those posits are, which is the job of the next premise.

§1.4: Quine's Procedure for Determining Ontic Commitments

The third step of Quine's argument is an appeal to his general procedure for determining the ontic commitments of any theory. Any one who wishes to know what to believe exists, and in particular whether to believe that mathematical objects exist, needs a general method for determining ontic commitment. There are many possible criteria. Most casually, we might rely on our brute observations. But our senses are limited, and the content of experience is ambiguous.

Another method would involve looking at our ordinary language. Perhaps the referents of our common singular terms are what exist. But ordinary language is also misleading and incomplete.

Quine provides a simple and broadly applicable procedure for determining the ontic commitments of any theory.

(QP) QP.1: Choose a theory.
QP.2: Regiment that theory in first-order logic with identity.
QP.3: Examine the domain of quantification of the theory to see what objects the theory needs to come out as true.

I have discussed the application of QP.1 to the indispensability argument in the previous two sections. But, QP applies to any theory. Theories which refer to ghosts, caloric, and God are equally amenable of Quine's general procedure. In the next two subsections, I discuss Quine's arguments for QP.2 and QP.3.

§1.4.1: First-Order Logic

In this section, I sketch Quine's defense of first-order logic as his canonical language. Quine credits first-order logic with extensionality, efficiency, and elegance, convenience, simplicity, and beauty. More concretely, Quine stresses first-order logic's unification of the referential apparatus of ordinary, and scientific, language. The existential quantifier is a natural cognate of 'there is', and Quine proposes that all existence claims can and should be made by existential sentences of first-order logic, which also has various technical virtues which make it an attractive language. He also writes of his canonical language, "*The* reason for taking the regimented notation as touchstone is that it is explicit referentially, whereas other notations, having other aims, may be vague on the point." (Quine (1986c) p 534, emphasis added)

First-order logic with identity is a formal language of predicates and variables, logical connectives, some optional punctuation, quantifiers, and an identity predicate.[7] Quine argues that we can use this language as canonical since we can use it to express anything we need to say. "The doctrine is that all traits of reality worthy of the name can be set down in an idiom of this austere form if in any idiom." (Quine (1960a) p 228)

We should take first-order logic as canonical only if: A) We need a single canonical language; B) It really is adequate; and C) There is no other adequate language. In Chapter 2, I will deny each of these clauses. Here, I sketch Quine's reasons for holding them.

Condition A arises almost without argument from QI.1 and QI.2. One of Quine's most striking and important innovations was his linking of our concerns when constructing formal theory, when regimenting, with general existence questions. When we regiment our correct scientific theory correctly, we will know what exists. "The quest of a simplest, clearest overall pattern of canonical notation is not to be distinguished from

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[7] Henceforth, I will call Quine's canonical language merely 'first-order logic'.

a quest of ultimate categories, a limning of the most general traits of reality." (Quine (1960a) p 161)

Quine's arguments for condition B consist in showing how first-order logic is useful as a tool for semantic ascent. It provides a framework for settling disagreements over ontic commitments, for with it we can deny the existence of objects without seeming to commit to them. Thus, $\sim\exists xPx$, where 'P' is a predicate standing for the property of being Pegasus, carries with it no implication that Pegasus exists. On the contrary, some one who holds that the meaning of a name is its referent must confront the puzzle of how 'Pegasus does not exist' can have meaning. Still, the burden of showing that first-order logic is adequate is greater than showing its utility in some contexts, a topic to which I return in Chapter 2. I proceed to condition C, Quine's argument that no other language is adequate for canonical purposes. I focus on Quine's arguments against languages with names, and against higher-order languages.

Quine contrasts the use of the first-order quantifiers to express reference with the use of languages with names. Names may be non-referential, like 'Pegasus'. There are not enough names for distant stars and real numbers. Reference may also be found in pronouns, which diffuses the matter. Unifying reference in the first-order quantifiers, rather than using names, simplifies the task. Instead of looking for real names among the various general and singular terms, pronouns, and proper nouns, we can look only to the quantifiers of the theory.

We are forced to choose between names and quantifiers, since we can not just include names in a language with quantifiers. Consider the following derivation, in a language which includes both, which entails that anything named exists:

| 1. ~(∃x)x=a	Assumption, for indirect proof
| 2. (∀x)x=x	Principle of identity
| 3. (∀x)~x=a	1, Change of quantifier rule
| 4. a=a	2, UI
| 5. ~a=a	3, UI
6. (∃x) x=a	1-5, Indirect proof[8]

The derivation should be taken as a reductio on the adequacy of any language which includes both names and quantifiers. If we accept Quine's reasons for preferring a canonical language which includes quantifiers, then we are left to choose among first-order and higher-order logics.[9] First-order logic has a variety of characteristics which higher-order logics lack. In first-order logic, a variety of definitions of logical truth concur: in terms of logical structure, substitution of sentences or of terms, satisfaction by models, and proof.[10] First-order logic is complete, in the sense that any valid formula is

[8] I owe the derivation to David Rosenthal.

[9] I do not accept Quine's reasons for preferring quantifiers over names. See Chapter 2, Part 1.

[10] See Quine (1986a) p 79, and p 87. The importance of logical truth is emphasized by Quine's hesitance to include identity as a logical particle. The class of logical truths shifts when we substitute other predicates for the identity predicate. Quine includes identity, despite the loss of logical truth via substitution, since identity applies generally, and there are complete proof procedures for first-order logic with identity; see Quine (1960a) §24. The identity predicate facilitates working with names, e.g. to characterize a uniqueness clause essential to Russell's treatment of empty names. We can also perform elementary arithmetic tasks without appeal to mathematical objects. Lastly, Quine takes identity as a basis for ontology: "No entity without identity." (Quine (1958) p 23) Strictly speaking, Quine's use of identity for dividing reference need not entail its inclusion as an element of the canonical language. In fact, Quine urges us to think of identity as definable in terms of the extensions of predicates. This assumes, of course, a finite stock of predicates. See Quine (1986a) p 64.

provable. Every consistent first-order theory has a model. First-order logic is compact, which means that any set of first-order axioms will be consistent if every finite subset of that set is consistent. It admits of both upward and downward Löwenheim-Skolem features, which mean that every theory which has an infinite model will have a model of every infinite cardinality (upward) and that every theory which has an infinite model of any cardinality will have a denumerable model (downward). All of these properties fail in second-order logic.[11] Furthermore, second-order logic quantifies over properties. If we take its quantifiers as indicating existence, then it yields too many objects even for the needs of mathematics. As with Quine's arguments for preferring quantifiers to languages with names, I return critically to this matter in Chapter 2.

§1.4.2: The Domain of Quantification

QP.1 and QP.2 give us a first-order theory. Now, we must determine its commitments. Reading existential claims seems prima facie quite straightforward. Consider the null set axiom, $(\exists x)(\forall y){\sim}(y \in x)$, which, taken at face value, states that the null set exists. We can not conclude that objects exist directly from existential claims. Instead, we must figure out what objects the sentences of the theory require for their truth. We ascend to a metalanguage to construct a domain of quantification in which we find values for all variables of the object theory. "To be is to be a value of a variable." (Quine (1939a) p 50, among others)

The move to a metalanguage means that we do not directly interpret first-order

[11] See Mendelson (1997) p 377.

theories to find ontic commitments. We look to their models. Quine's reasons for examining models, rather than the theorems directly, is simply formal. We find our commitments in examining existential quantifications, but quantifications bind variables which are not themselves the things we think exist. Nor are their substituends what exist; these may be taken as names of the things that exist. Variables take as values the things that exist, and these values are located in the domain of the theory.

One reason to favor Quine's procedure is because it can prevent prejudging what exists. Call this the neutrality of Quine's method. On his view, we construct scientific theory without prior determination of what exists. Scientists take the evidence and the theory wherever it leads them. They balance formal considerations, like the elegance of the mathematics involved, with an attempt to account for the broadest empirical evidence. The more comprehensive and elegant the theory, the more we are compelled to believe it, even if it tells us that the world is not the way we thought it is. If the theory yields a heliocentric model of the solar system, or the bending of rays of light, then we are committed to heliocentrism or bent light rays. Our ontic commitments are the byproducts of this neutral process.

The method I am ascribing to Quine, especially its neutrality, may seem a bit like a caricature. For, it makes the determination of our commitments the result of blind construction of scientific theory.[12] This criticism is fundamentally correct, but it is a criticism of Quine's method itself, and not of my interpretation. I argue this against

[12] Michael Levin, in particular, has suggested that I am misrepresenting Quine, who never intended his method to turn us into metaphysics automata. Michael Devitt has made a similar point.

Quine in Chapter 2.

The indispensability argument, which Quine clearly holds, depends on the method I have ascribed to Quine. Without QP, we are free to interpret quantifications over mathematical objects instrumentally, as not indicating commitments to them.[13] This fact supports my interpretation. Further, Quine's naturalism and his presumption of parsimony makes him skeptical of abstract objects.[14] Unless we ascribe to Quine the neutral method I have presented, there is no reason for him to yield his skepticism. If Quine abandoned neutrality, he could easily see the indispensability argument as a reductio on his procedure for determining commitment. His strong pre-theoretic commitment to nominalism's austere landscapes should take over. The neutrality of Quine's method is what ensures that he does not stack the deck against mathematical objects.

Field's response to the indispensability argument, as I show in the next part of this chapter, is a response to the method I described. So, even if the argument I have presented is not in fact Quine's, it is an important one, and worth consideration.

§1.5: Mathematization

The final step of Quine's indispensability argument involves simply looking at the domain of the theory we have constructed and regimented. We discover that the

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[13] I defend an instrumentalist interpretation of mathematical quantifications in Chapter 2,

[14] See Quine and Goodman (1947) for his attempt to avoid mathematical objects.

theory includes, in the casting of physical laws, functions, sets, and numbers. For example, consider Coulomb's Law: $F = k\,|q_1 q_2| / r^2$. This law states that the electromagnetic force between two charged particles is proportional to the charges on the particles and, inversely, to the distance between them.

Regimenting Coulomb's Law, or any sentence of physics, all the way down into first-order logic would make it quite complicated. Here is a first step, using 'Px' for 'x is a charged particle'.

(CL) $\forall x \forall y \{(Px \wedge Py) \supset (\exists f)[<q(x), q(y), d(x,y), k, F> \mid F = (k \cdot |q(x) \cdot q(y)|) / d(x,y)^2\}$

This regimentation is incomplete, but it suffices to give the idea of the commitments of the law. Besides the charged particles over which the universal quantifiers in front range, there is an existential quantification over a function, f. Furthermore, this function maps numbers (the Coulomb's Law constant, and measurements of charge and distance) to other numbers (measurements of force between the particles).

In order to ensure that there are enough sets to construct these numbers and functions, and in order to round out the theory, which may be justified by considerations of simplicity, our ideal theory will include set-theoretic axioms, perhaps those of Zermelo-Fraenkel set theory, ZF. Quine, unsatisfied with ZF for its piecemeal, type-theoretic avoidance of paradox, formulated alternative systems NF and ML, which extended NF by adding classes. We can derive from the axioms of any of these set theories a vast universe of sets. So, CL contains or entails several existential claims.

CL, with its mathematical commitments, is representative of the kind of physical law that motivates Quine's indispensability argument. In Chapter 2, I will present considerations against the version of the argument I have presented here. First, in the last section of Part 1 of this chapter, I show that attempts to avoid commitments to mathematical objects by reinterpreting the mathematical claims of science modally do not succeed as responses to QI. Then, in Part 2 of this chapter, I consider the most well-known and fecund response to the argument presented here, Field's project.

§1.6: Modal Reinterpretations

Typically, criticisms of indispensability arguments deny QI.4 and attempt to show that science can be recast to avoid commitments to mathematical objects. These are dispensabilist projects. In this section, I show how QI withstands modal dispensabilist projects.

The dispensabilist is likely to be motivated by mathematical nominalism, which takes diverse forms, many of them interesting independently of the indispensability argument.[15] The exhaustive Burgess and Rosen (1997) elegantly unifies many significant dispensabilist strategies within a flexible technical framework. Most of these strategies include significant appeals to modality, or other extensions of first-order logic, rejecting Quine's canonical language. If a reformulation of scientific theory is to eliminate commitments to abstract objects, our real commitments must be found in the

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[15] I use 'nominalism' for the denial that mathematical objects exist, and 'dispensabilism' for granting QI.1 - QI.3 while denying QI.4.

reformulated theory. But theories which appeal to modality or extended logic can not rely on Quine's arguments that we find our commitments by looking at our regimented theory. Such arguments only apply to Quine's canonical language, not to any kind of regimentation. Thus, reformulations which reject the indispensabilist's method for determining commitment do not succeed as dispensabilist strategies.

In particular, almost all of the reformulations considered by Burgess and Rosen swap mathematical objects for possibilities. The complexity and sophistication of these attempts does not mask an underlying modal profligacy. The addition of modal logic mitigates the significance of the elimination of quantification over mathematical objects. "Avoidance of modalities is as strong a reason for an abstract ontology as I can well imagine." (Quine (1986b) p 397)

The problem of reducing the ontic commitments of a theory by extending one's logic may be demonstrated quite clearly. One method involves predicate functors.[16] One can construct a first-order language including predicate functors and predicates, but no variables. The first-order quantificational sentence '∃xPx', for 'There are prime numbers', has a predicate functor correlate, say, 'ζP'. Given a first-order theory, one can introduce functors to replace logical connectives, and then to replace quantifiers and variables. Burgess and Rosen present the following transformation of, 'Whatever lives, changes'.

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[16] Quine first explored predicate functors, in Quine (1960c), as a way of explicating quantification in sententialist terms, replacing variables (pronouns) with sentential operators. See also Quine (1960b), Quine (1970), and Quine (1982), §45; and Bacon (1985) for deduction rules and completeness results. A summary is provided in Burgess and Rosen (1997) pp 186-7.

$\sim\exists x(Fx \bullet \sim Gx)$	Translation into first-order logic
$\sim\exists x(Fx \bullet (\nu G)x)$	Introducing a functor for negation
$\sim\exists x(\kappa F(\nu G))xx$	Introducing a functor for conjunction
$\sim\exists x(\rho(\kappa F(\nu G)))x$	Eliminating one variable using a functor for 'the same thing'
$\sim\zeta(\rho(\kappa F(\nu G)))$	Eliminating the quantifier and bound variable for a functor
$\nu(\zeta(\rho(\kappa F(\nu G))))$	Negation, again.

Predicate functors allow us to eliminate quantification completely. A scientific theory reformulated in the language of predicate functors has no quantifications over mathematical objects. "If it is permitted to help oneself to whatever logical apparatus one wants, while ignoring the usual definitions and giving no other explanations, then not only a nominalistic but a monistic and even a nihilistic reformulation can very easily be given..." (Burgess and Rosen (1997) p 186)

No one should consider a translation of standard science into the language of predicate functors a demonstration that mathematical objects do not exist. Adopting predicate functors, one changes the way in which commitments are to be found within a theory. The example shows that we must concern ourselves with the logic used in dispensabilist projects.

The question at hand is whether modal reformulations of science can succeed as dispensabilist projects.[17] Modality is typically construed in terms of possible worlds. Philosophers have become increasingly comfortable with possible worlds in the wake of developments in model theory for modal logic, especially Kripke models. In a Kripke model, '$\Diamond\mathscr{F}$' is true in a world just in case there is an accessible possible world in which

[17] More precisely, we are considering modal reinterpretations of the mathematical axioms used in scientific theories.

$\mathscr{T}$ is true.

Different modal logics reflect varying degrees of modal commitment. Putnam's modalism is among the most profligate. For Putnam, a statement is possible just in case it is consistent with any true mathematical theory. He thus does not eliminate mathematics. "Something very akin to mathematical truth (and therefore to mathematical existence) is being sneaked into Putnam's possibility operator." (Field (1984) p 85)

Burgess and Rosen's pure modal strategy, based on Charles Chihara's constructabilism,[18] relies on a novel version of modal logic. Possible inscriptions capable of coding real numbers replace mathematical objects. This reformulation requires what Burgess and Rosen call metaphysical modality as opposed to metalogical modality. Metaphysical modal logics are rigid, which means that iterated modal operators are redundant; rigid modal logics admit no double subjunctive moods. Most modal logics of interest are metalogical, in which iterations of modal operators create different formulas with different implications. Metalogical modality makes weak modal commitments since it interprets possibility as non-contradiction, and may be palatable to those wary of modal logics.[19] In contrast, metaphysical modality commits to what might have been the case if the world had been other than it is: what kinds of objects may exist together, and how a thing would be if other things had existed. The pure modalist must claim that some inscription, 'a', if it were to exist, would have marked more than another inscription 'b',

[18] See Chihara (1990).

[19] Field uses the weaker modality with his modal operator. See §2.6

were it to exist.

Metaphysical modalities are just as epistemically intractable as abstract objects. In fact, they may be taken to be abstract since they are isolated from us outside of space and time. The use of modal logic undermines the claim that we should look to science to determine our commitments. In fact, any appeal to possible worlds is problematic for the dispensabilist. Modal claims conflict with the indispensabilist's parsimony.

If a mature science required substantial modality for purposes other than the elimination of mathematical objects (to formulate laws which apply to counterfactuals, say) then modal reformulations of science might be worth adopting. This is an open possibility, despite Quine's skepticism, but not one that I am prepared to examine in this dissertation. I will thus ignore modal reformulations and other dispensabilist strategies which rely on significantly extending logic.[20]

As we deviate from the canonical language, the claim that we find our commitments in the quantifications of a theory fades.

> [I]t can be seen that there is something dubious about the practice of just helping oneself to whatever logical apparatus one pleases for purposes of nominalistic reconstruction while ignoring any customary definitions that would make the apparatus nominalistically unpalatable: for by doing so, one can make the task of nominalistic reconstruction absolutely trivial – and so absolutely uninteresting. (Burgess and Rosen (1997) p 175)

Quine chose first-order logic as his canonical language for its neutrality.

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[20] Specifically, I ignore those of Chihara, both the early predicativism and the later constructibilism, Bostock's Russellian project, Hodes's Fregean project, and those of Bigelow, Putnam, and Hellman, among others. See Burgess and Rosen (1997), §IIIB.

Dispensabilist reformulations which rely on extensions of logic which violate that neutrality can not be seen as legitimate responses to the indispensability argument. QI thus withstands this kind of response. In the next part of this chapter, I show how Field's *Science Without Numbers* project, and related ones, present a more significant challenge to Quine's argument.

Part 2: Field's Dispensabilist Response

§2.1: Introduction

In this second part of Chapter 1, I consider how Field's dispensabilist project fails to establish mathematical nominalism in response to QI. Field, like the modalists considered in §1.6, unacceptably extends his logic, though he attempts to maintain as much as he can of QI.3. He accepts QI.1 and QI2, denying only QI.4, that mathematical objects are required in our scientific theories. Of course, if Field is right that QI.4 is false, Quine's argument fails.

At the heart of Field's dispensabilism is the reformulation, in Field (1980), of Newtonian Gravitational Theory (NGT). He shows how to replace quantification over mathematical objects, specifically the bridge functions used to represent measurements of quantities like mass and velocity, with quantification over space-time points. Field thus eliminates mathematical objects from NGT.

Field says that his construction is not directly an argument for nominalism. "I would like to make clear at the outset that nothing in this monograph purports to be a positive argument for nominalism. My goal rather is to try to counter the most compelling arguments that have been offered against the nominalist position." (Field (1980) p 4)

Still, Field's goal is clearly to establish nominalism. He aims at the indispensability argument, because he sees it as the most serious challenge to nominalism, and he accepts its major premise. "The only non-question-begging arguments that I have ever heard for the view that mathematics is a body of truths all rest ultimately on the applicability of mathematics to the physical world." (Field (1980) p viii)

In addition to developing a nominalist counterpart to a standard scientific theory, Field tries to show that mathematics applies conservatively to nominalist theories, to assure that nominalist counterparts are adequate substitutes.

In lieu of 'nominalism' as a label for his position, Field prefers 'fictionalism', the view that mathematical terms are empty names and mathematical sentences are either false, for existence claims, or vacuously true, for purely mathematical entailments.[21] Field also calls his position deflationism in connection with his denial of mathematical knowledge. I show:

FC.1: Field's project, as presented, does not succeed as a response to QI. It extends logic unacceptably, and there are no similar constructions for current and future scientific theories.
FC.2: Field misapplies an attractiveness criterion for the acceptability of dispensabilist reformulations.

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[21] Field perhaps follows a suggestion in (Putnam 1971) that earlier versions of fictionalism were wrongly rejected. Burgess (2004) p 19 urges that Field uses the term too broadly; we should restrict 'fictionalism' only to views I will call 'instrumentalist', which do not offer reformulations of scientific theory. I follow Field's usage.

FC.3: Conservativeness, as Field originally imagines it, does not hold, though a restricted version does hold.

FC.4: The representation theorems Field originally attempts to construct to defend his reformulation as an account of the application of mathematics to science are not available, though other representation theorems to the same effect do hold.

Much of the work in the remainder of this chapter is a summary of technical work done by others. My analysis of Field's project, especially the material in §2.2 and §2.4 is original. The summary of the status of the project to date is intended to be newly comprehensive, but I present no new technical results. The reader familiar with Field's project and its critics could easily skip the long §2.5, §2.6, and §2.7. I present most of my original concerns about Field's project in Chapter 5, Part 3.

Field is right that we can excise mathematics from regimented science, though not as he originally imagined, and not in a way which defeats QI or which establishes nominalism.

§2.2: Attractiveness, and Other Ground Rules

Before proceeding to evaluate Field's project, I review some ground rules, general criteria for the acceptability of dispensabilist reformulations used as responses to QI. I argue in this section that Field misapplies an attractiveness criterion.

Three ground rules are fairly uncontroversial.

GR.1: Adequacy: A reformulation must not omit empirical results of the standard theory.

For an inadequate theory, consider all the theorems of standard science which make no use of numerical constants.

> GR.2: Logical Neutrality: A reformulation must not reduce ontology merely by extending logic, or ideology.

The modal nominalists of §1.4, violate GR.2. Any reformulation must accept the logical neutrality that Quine finds in first-order logic. Otherwise, unintended or unwarranted commitments may be made.

> GR.3: Conservativeness: The addition of mathematics to the reformulated theory should license no additional nominalist conclusions.

Field provides a formal definition of conservativeness. Let A be any nominalistically statable assertion, N any body of such assertions, and S any mathematical theory. Take 'Mx' to mean <u>x is a mathematical object</u>. Let A* and N* be restatements of A and N with a restriction of the quantifiers to non-mathematical objects.[22] This restriction yields an agnostic version of the nominalist theory; it does not rule out the existence of mathematical objects. S is conservative over N* if A* is not a consequence of N*+S+'∃x~Mx' unless A is a consequence of N.[23]

Conservativeness may be either deductive, which means that mathematics does not allow new theorems to be derived, or semantic, which means that no additional

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[22] Restrict the quantifiers by inserting into every universal quantification of N, just after the quantifiers, '$\sim Mx_i \supset$', for all x_i in the formula; and into every existential quantification of N, just after the quantifiers, '$\sim Mx_i \bullet$'. This ensures that all statements will be neutral about the existence of mathematical objects.

[23] '∃x~Mx', that there is at least one non-mathematical object, is a technical convenience. See Field (1980), pp 10-16.

statements come out true in any model of the theory which includes mathematics. In a complete theory, deductive and semantic completeness are coextensive. In an incomplete theory, they diverge.

Conservativeness is essential to the dispensabilist argument for nominalism for two reasons. First, it serves as a check on the adequacy of the nominalist reformulation. If mathematics does not apply conservatively to NGT*, then the standard theory will yield more consequences. The nominalist theory omits theorems.

Second, conservativeness provides an account of the applicability of mathematics to science. Without this account, we must remain suspicious of the practical utility of mathematics, and its ubiquity in actual science.

The possibility of fulfilling this second goal makes Field's project especially alluring. He does not merely eliminate mathematics from scientific theory. He attempts to show that our ordinary uses of mathematics are consistent with nominalist principles.

Lastly, Field claims that the dispensabilist reformulation must be attractive.

> GR.4: Attractiveness: The dispensabilist must show, "[T]hat one can always reaxiomatize scientific theories so that there is no reference to or quantification over mathematical entities in the reaxiomatization (*and one can do this in such a way that the resulting axiomatization is fairly simple and attractive*)." (Field (1980) p viii, emphasis added)

Field's interpretation of GR.4 is widely accepted, though he does not explicitly cash out what he means by 'attractive'. I think he misapplies this criterion, as I show in the remainder of this section.

Attractiveness is a widely discussed criterion. Mark Colyvan defends the indispensability argument based on formal elegance. "[T]here is good reason to believe that the mathematised version of a theory is more 'virtuous' than the unmathematised theory, and so there is good reason to believe mathematics is indispensable to our best theories." (Colyvan (2001) p 80) Further, "[I]f these theories were stripped of their mathematical content it seems that they would lose much of their appeal." (Colyvan (2001) p 80)

Colyvan's claim makes the attractiveness criterion appear subjective. Field seems to have specific constraints for GR.4 in mind, though. Consider a theory which he thinks violates it, which I will call N_1. "[T]here is a quite uninteresting way to get a first-order subtheory of N [the second-order nominalist scientific theory] with precisely the same first-order sentences... as consequences that N has as consequences: simply take as axioms all the first-order sentences that follow from N." (Field (1980) p 98)[24]

Notice that this theory satisfies GR.2, though it will not satisfy GR.1, since it omits all second-order sentences. Field's concern here, though, is not the adequacy of the reformulation. He objects to cleaning up a controversial theory by merely taking as axioms its uncontroversial theorems.

Field's reasons for objecting to N_1 require examination. For, if we reject Field's attractiveness criterion, adequate dispensabilist constructions are quite easy to design.[25] Consider another example, I'll call N_2. We can eliminate mathematical objects from science by replacing the standard theory with the theory which consists of all of its

[24] For another example, see Field (1985a) p 129.

[25] Burgess provides several examples, in Burgess (1991a), p 123.

nominalistically acceptable consequences.

Both N_1 and N_2 may be made more attractive by a Craigian reaxiomatization. For N_2, we can eliminate mathematical vocabulary from standard science by adding new predicates and formulas. For every formula of the original theory, one adds a new primitive and an axiom which defines that primitive to hold of only the non-mathematical elements of the theory. Then we delete the formulas which refer to mathematical objects. The new axioms take the place of any formulas which used mathematical vocabulary, replacing one-by-one each of the physical consequences of the original theory with nominalistically acceptable formulas. Craig's Theorem insures that though the new theory has infinitely many primitives and axioms, and so the set of axioms may not be recursive, there is another axiomatization, which yields the same theorems and is effectively decidable. We can determine, for every formula of the language of the theory, whether it is an axiom.

A Craigian theory provides all the consequences of the original theory without quantifying over mathematical entities. It does not reduce diverse experiences to a few, simple axioms. Still, the resulting theory adequately yields all the desired consequences.

Field's main criticism against both N_1 and N_2 is that they are unattractive. "If no attractiveness requirement is imposed, nominalization is trivial... Obviously, such ways of obtaining nominalistic theories are of no interest." (Field (1980) p 41)[26]

This claim is, on the contrary, not obvious. Why should an adequate, logically

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[26] Similarly on p 8. Field makes this point about a Craigian theory specifically, at Field (1985a) p 129.

neutral, and conservative theory be ruled out because it is, in some sense, unattractive?

In order to determine if GR.4 is a legitimate criterion, we must clarify the purpose of the reformulation. If it is intended to produce a useful theory for the practice of science, N_1 and N_2 are certainly unacceptable, even if they satisfy GR.1, GR.2 and GR.3. But, the standard theory regimented in first-order logic is similarly unacceptable. What working scientist could have any use for a fully regimented physical theory? GR.4 is too strong a requirement, since the original theory violates it, too.

Penelope Maddy mistakenly interprets attractiveness as a condition for the working scientist to evaluate. She criticizes one version of Field's reformulation for adding theoretical steps to the scientist's use of the theory. "Even after science is rewritten, the scientist must...perform extra checks of a sort that no practicing scientist would take to be needed." (Maddy (1990b) p 195)

The reformulation, like the original regimented standard theory, is written to reveal commitments, not to be useful. Field himself makes this point. "As I once heard Hilary Putnam remark, you need to introduce an awful lot of definitions and an awful lot of background theory before you can formulate the laws of electromagnetism on a T-shirt. Imagine what they'd look like formulated in set theoretic terms, using 'ϵ' as the only mathematical primitive!" (Field (1990) p 210)

Burgess and Rosen make the same error. They urge that practicing scientists will not accept Field's construction. "For ultimately, the judgment on the scientific merits of a theory must be made by the scientific community: the true test would be to send in the nominalistic reconstruction to a mathematics or physics journal, and see whether it is published, and if so how it is received." (Burgess and Rosen (1997) p 206)

Field puzzlingly agrees that his reformulation must not be awkward to use. "My formulations turn on some measurement-theoretic apparatus that is less familiar and has doubtless not yet been optimally formulated, but I doubt that it is intrinsically very much more awkward." (Field (1990) p 210)

The ease of use of either the regimented standard theory or the dispensabilist reformulation is irrelevant. Neither Quine nor Field should be read as suggesting any kind of reforms for the practicing scientist. The purpose of regimentation and reformulation is only to reveal and clarify the commitments of a given theory.

For Field, this should be especially obvious, given his attempts to establish conservativeness for mathematics. He does so in order to establish that it is acceptable to continue using standard science. Field's goal is not to replace the working scientist's methods and tools, but to legitimate their use. So, there is no justification for taking GR.4 as a requirement that reformulations be useful for scientists. Can anything more be made of it?

We might take GR.4 to be a demand that the reformulation is as explanatory as the standard theory. We can see that Field wants his reformulation to be explanatory in the way he defends his reformulations on the basis of a principle of intrinsic explanation.[27]

Explanatory merit again places too great a burden on a reformulation, though.

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[27] See Chapter 5, §3.2 - §3.6 for my explication and criticism of this principle. I argue that it is in principle impossible for a dispensabilist reformulation of a physical theory to be better than the original.

The standard theory it is meant to replace, when written to reveal commitments, can not be explanatory either. We can not demand more of the reformulation that we do of the original. No regimented theory can be explanatory, since it can not be perspicuous.

When we regiment either standard science or the dispensabilist reformulation, we show that in principle a theory may be written in terms of the basic logical vocabulary and as few primitives of the theory at hand as possible. We can imagine such regimentations and their domains of quantification, and use these to clarify our commitments. Given a phenomenon and a covering law, we might say that in principle we could regiment the law and apply it to some initial conditions which yield, through syntactic transformations, the phenomenon. But the original unregimented theory is the one which does all the explanatory work.

While the regimented theory provides no explanation, it does form part of an account. Some elements of an account need not be perspicuous, or otherwise useful. But the question at hand is whether GR.4 can be taken as a demand for an explanatory theory. The dispensabilist construction may reveal our commitments, but at the cost of explanation. Like practical utility, explanatory merit can not serve to cash out Field's attractiveness requirement.

Under any interpretation of GR.4, Burgess and Rosen worry that the dispensabilist's primitives are unavoidably unattractive. Their strategy for removing references to numbers in their construction involves a nominalistic counterpart to the order primitive on numbers, and counterpart axioms for basic algebraic and generalized coordinate axioms.

> Except for the counterpart of the continuity scheme for numbers, which amounts to something very like the continuity scheme for points, these axioms have an artificial look from a geometric viewpoint, though they are counterparts of axioms that had a natural look from an algebraic viewpoint. Thus the ontological benefit...of eliminating numbers is accompanied by the ideological costs of artificial new primitives and artificial new axioms. (Burgess and Rosen (1997) 109)

Dispensabilist reformulations, even if they were adequate, logically neutral, and conservative, must include new primitives and axioms which are unavoidably unattractive. Field's construction can not succeed on its own terms.

GR.4 can not be a requirement for either practical utility or explanatory merit of the reformulated theory. Reformulations are merely in-principle accounts like the original Quinean theory and need not be useful or explanatory.

Ruling out GR.4 means that reformulations like N_2, if adequate, logically neutral, and conservative, show how to eliminate mathematics from empirical science. They do not provide what Burgess and Rosen call a Tarskian reduction, a sentence by sentence translation of the original theory which leaves the basic structure intact.[28] N_2 provides a recursively enumerable list of nominalist theorems as axioms, but does not reduce the laws to a neat and tidy few axioms. It does not show how to translate axioms of standard science directly into nominalist language. A Tarskian reformulation would do this, despite still being unusable and non-explanatory.

Given either N_2 or a Tarskian reformulation, we must appeal to the original theory for understanding, prediction, unification, and elegance. Still, there is a difference

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[28] I discuss the details of Burgess's Tarskian reformulation in §2.5.4.

between being unable to use a theory because it is regimented into first-order logic and being unable to use it because all you have is a long list of consequences of another, perspicuous theory. However Field might cash out and justify GR.4, the result will be a demand for a Tarskian reformulation.

Despite my rejection of GR.4, I proceed with Field, taking that requirement as a demand for a Tarskian reformulation in order to better apprehend Field's project. I will return to my criticisms of GR.4 in Chapter 2, Part 2.

In this section, in preparation for evaluating Field's project on its own terms, I delimited four ground rules for dispensabilist reformulations, ones which Field takes either implicitly or explicitly: GR.1: Adequacy; GR.2: Logical Neutrality; GR.3: Conservativeness; and GR.4: Attractiveness. I expressed my concerns about GR.4. In the ensuing sections, I show how Field's reformulation violates each of the other Ground Rules. First, I show how his project does not succumb to a common criticism, that space-time points are nominalistically illegitimate.

§2.3: Space-Time Points

In this section, I defend Field's claim that space-time points are nominalistically acceptable, though I dispute his claim that we have sensory access to them. Quantification over space-time points is a natural place for concern, which Putnam made explicit. "Hartry Field has claimed...that one can do physics without reference to abstract entities. But his construction requires that we accept *absolute space-time points* and arbitrary sets of space-time points as 'concrete'; most philosophers (including myself) would regard this as 'cheating'." (Putnam (1981d) p 175).

Field defends space-time points on the basis of independent considerations in science.[29] In field theories, we may reify space-time in order to ascribe the field properties to something. In general relativity, for example, distortions created by the gravitational pull of massive bodies may be seen as curving space-time itself. "Space-time regions are now known to be genuine causal agents: that is what field theories like classical electromagnetism or general relativity or presumably quantum field theory tell us." (Field (1989a) p 47)

Field also argues that we have sensory access to space-time points. "For there are quite unproblematic physical relations, viz., spatial relations, between ourselves and space-time regions, and this gives us epistemological access to space-time regions. For instance, because of their spatial relations to us, certain space-time regions can fall within our field of vision." (Field (1982a) p 68)

Sensory access to empty regions is implausible. While a space-time region can fall within our field of vision, we can not actually see an unoccupied region of space. We can not even see one that is filled with nitrogen and oxygen. If we could see empty space, Leibnizian arguments for relationalism could not get started.[30]

Worries about access to space-time points are moot because we can explain our

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[29] He defends them in Field (1980); responds to criticisms of that project, especially Malament (1982), in Field (1982a); presents the extended Field (1985b), in which the topic is central; and again forcefully supports the thesis in Field (1989a).

[30] Field's claim that we can see space-time regions led Maddy to propose perception of sets. See Maddy (1988) §5. Maddy's failure may rebound on Field.

beliefs in them in other ways. We learn about physical theory through textbooks and lectures and such. We justify these beliefs based on how the posit of space-time points helps account for our best physical theory.

Field's argument that space-time points are empirical objects is more plausible, since we may take space-time points and regions as concrete objects. Consider Jerrold Katz's clear distinction between abstract and concrete objects. "An object is abstract just in case it lacks both spatial and temporal location and is homogeneous in this respect. An object is concrete just in case it has spatial or temporal location and is homogeneous in this respect." (Katz (1998) p 124)

For Katz, objects, like the equator, may also be composite, with both abstract and concrete properties. Mathematical objects are abstract, and distinctly so, and empirical objects, like chairs and electrons, are concrete objects, and distinctly so. On Katz's criterion, space-time regions are concrete, which coheres with Field's description. Given Field's adoption of QI.1 and QI.2, including their homogeny, he need not claim sensory access to an empirical object to have knowledge of it.

David Malament argues that we can dispense with space-time points by reifying fields.[31] We would ascribe to fields the same structure that the adoption of space-time points provides. "Small wonder that after giving field theories a strained interpretation according to which fields are entities that have the structure of space-time built into them, a separate space-time is then dispensable." (Field (1985b) p 183)

Malament's preference for reifying fields is based in part on concerns that space-

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[31] Malament (1982) p 532, fn 11.

time points seem illegitimate since they straddle the abstract/concrete boundary. Since space-time points are concrete, reifying fields has no advantage.

Quantification over space-time points constitutes no violation of the Ground Rules. They are empirical objects, like trees, electrons, and, as I argue in Chapter 2, Part 3, the indispensabilist's so-called mathematical objects.

§2.4: Two Arguments for Conservativeness

In this section, I proceed to the details of Field's project. I show that Field can not generate the general conservativeness result he claimed, though limited versions are available. I then show that Field's reformulation violates GR.3, though not egregiously.

Field provides two kinds of arguments for conservativeness, which I will call top-down and bottom-up. The broader top-down argument alleges that set theory is conservative over any nominalistically acceptable body of assertions. Field's argument consists of showing, both set-theoretically and proof-theoretically, the proximity of conservativeness and consistency. He calls conservativeness, "A generalized form of consistency," (Field (1982a) p 75)

To establish top-down conservativeness, Field relies on a proof that adding mathematics to a consistent nominalist theory yields a consistent theory. "What we want to prove is that for any theory T, $ZFU_{V(T)}$ applies conservatively to T.[32] That is, we want

[32] $ZFU_{V(T)}$ is Zermelo-Fraenkel set theory with choice, allowing for urelements, but not for any non-set-theoretic vocabulary in the comprehension axioms, except for instances of the comprehension schemas which include vocabulary of the nominalist theory T.

to prove (C_0): If T is any consistent body of assertions, then $ZFU_{V(T)}$ + T* is also consistent." (Field (1980) p 17)

Field's proof of (C_0) starts with a model for T, and shows how to construct a model for the theory when it is supplemented with mathematical axioms. Field's top-down argument thus shows that adding mathematics to T will not get generate a contradiction unless T is already inconsistent.

Field's top-down argument is in principle insufficient. His proof only shows that the addition of mathematics does not make a consistent theory inconsistent. Conservativeness entails that adding mathematics to a nominalist theory should not license new conclusions. Field does not show that $ZFU_{V(T)}$ + T* won't license new conclusions.[33]

Field's bottom-up argument for conservativeness relies on his representation theorems. Representation theorems are standard functions which establish a homomorphism between two sets, or structures. The bottom-up argument demonstrates the conservativeness of particular mathematical theories over particular scientific theories, and is more successful.

Field bases his reformulation of NGT on Hilbert's axiomatization for Euclidean geometry. Hilbert constructed representation theorems to map geometric points onto the real numbers, demonstrating that geometry used analysis inessentially. He showed how statements about the lengths of line segments could be reformulated as statements about

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[33] The argument for conservativeness is successful in the first-order version of Field's reformulation, where the logic of the theory is deductively complete. But he can not formulate his representation theorems in the first-order theory. See §2.5.3.

ratios of line segments, introducing predicates of betweenness and congruence for line segments to formulate these ratios.

If we take points in space to be represented by real numbers, and ultimately by sets, we call the theory analytic. Euclidean, or 'synthetic', theories, identified real numbers with ratios of lengths, areas, or volumes. Hilbert thus replaced analytic geometry with synthetic geometry.

Standard, analytic, NGT relies on continuum mathematics. The shift to analytic physical theories came in response to nineteenth century worries about non-Euclidean geometries. A set-theoretic foundation seemed stronger. Field's project casts NGT as a synthetic physical theory, using only as much geometry as one already needs in physical theory in the guise of structured space-time. "In so far as it seeks to restore geometry as a foundation for mathematics, geometric nominalism is not revolutionary, but counter-revolutionary." (Burgess and Rosen (1997) p 99)

In order to write NGT in terms of space-time points, Field extends Hilbert's representation theorems from two to four dimensions, and adds predicates for dynamic notions of mass density and gravitational potential.[34] Field's representation theorems map space-time points onto real numbers, showing how to translate nominalist statements into statements about the real numbers. They are constructed within a background theory which includes mathematics, since we need mathematics in the theorems, and for the mapping functions. The representation theorems may not,

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[34] See Field (1980), Chapter 8, or Burgess and Rosen (1997), II.A.

therefore, be available to the nominalist.[35] Their role is to convince the platonist that the nominalist theory is acceptable, since the platonist can see that any sentence of physical theory deducible using mathematics can be derived without mathematics.

Field's representation theorems explain how mathematics can be useful, given a nominalist theory, by showing that statements which use mathematics are convenient shorthand for nominalistically acceptable sentences. The representation theorems are the source of much of the acclaim for Field's project as an account of the applicability of mathematics.[36] Any mathematics in the range of the representation theorems is conservative over a given nominalist theory. If Field's representation theorems are available, the conservativeness of the theory of real numbers over NGT follows. In the next section, I show that Field does not generate his representation theorems, though Burgess neatly produces ones to serve Field's needs.

I argued that the top-down argument for conservativeness fails. The bottom-up argument, through the representation theorems, may work in some specific cases, as I will show, but it also fails, generally. Mathematics can be shown conservative over limited theories, but not more broadly, as Field originally claimed. Since conservativeness is not generally established, Field does not provide a comprehensive response to Quine's indispensability argument. Still, he provides a limited response worth examining.

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[35] If the top-down argument for conservativeness were to hold, then the nominalist could use the platonist mathematics knowing that these results could in principle be generated with only nominalist resources.

[36] See Shapiro (1983a) p 529; Resnik (1983) p 514; and Friedman (1982).

§2.5: Representation Theorems

In this section and the next, I discuss Resnik's claim that, "Field's version of science employs modalities, infinite conjunctions, the 'complete logic of Goodmanian sums', and extensions of standard mathematics...Each of these notions seems as problematic as abstract entities." (Resnik (1985a) pp 178-9) I present no new technical results in these two sections. In this section, I show that Field can not construct the representation theorems he originally attempted, though he defends a restricted representation theorem, and Burgess develops other limited dispensabilist constructions. The Quinean indispensabilist need not admit defeat, but the case for mathematical objects on the basis of the indispensability argument is weakened.

In part, the problem for Field arises from his use of second-order logic. Field originally presented two versions of his reformulation.[37] He outlined a first-order version, and, in greater detail, a second-order version relying on mereology, and a finite cardinality quantifier.[38] QI rests on the sufficiency of regimented scientific theory to reveal our commitments. The theory in which we find our commitments must be complete, for this entails the equivalence of semantic consequence and derivability. There will be no underivable truths. The incompleteness of the second-order theory renders it unfit to do the work that QI requires.

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[37] For details of Field's development of the representation theorems, see Field (1980), Chapter 8.

[38] Burgess and Rosen (1997), pp 109-11, avoid the finite cardinality quantifier in their version of Field's project by using a different definition of proportionality.

§2.5.1: Continuity

Field introduces second-order quantification to treat continuity. Since many physical functions, like mass, temperature, and velocity, are assumed to be continuous, real numbers are used to represent their measurements. In standard physics, the continuity of the reals is assumed via the Dedekind axiom of continuity. A function f is continuous at a point c if for every neighborhood V of f(c) there exists a neighborhood U of c such that f(x) ∊ V whenever x ∊ U.[39] Notice that the converse domain is an open set. When the continuity axiom is assumed in standard scientific theory, the converse domain is an open set of reals.

For Field's project, continuity must be defined without real numbers. His continuity axiom second-order quantifies over collections of space-time points.

> A region R is scalar-basic iff there are distinct points x and y such that either
> a) R contains precisely those points z such that z Scal-Bet xy and not ($z \approx_{scal} x$) and not ($z \approx_{scal} y$); or
> b) R contains precisely those points z such that y Scal-Bet xz and not ($z \approx_{scal} y$).[40]
>
> A function is continuous at c if, for any scalar-basic region that contains c, there is a spatio-temporally basic subregion that contains c.[41]

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[39] A set is a neighborhood of a point (within a topological space) if it has a subset in the topology which contains the point.

[40] Scal-Bet, defined over the space-time points, is a primitive predicate in Field's nominalist theory, used in his representation theorem. 'x Scal-Bet yz' says that x displays the value of a scalar quantity between those which y and z display. '$x \approx_{scal} y$' is short for 'x Scal-Bet yy,' and means that x and y display the same value. In the actual theory, predicates like Temp-Bet or Mass-Bet or Veloc-Bet would be used. See Field (1980) pp 55-64, especially p 62.

[41] Spatio-temporally basic subregions of space-time are those which can be mapped onto basic open sets of $\mathbb{R}^4$. See Field (1980) p 63.

The replacement of sets of reals with space-time points is what makes Field's version nominalistic. In order to generate sufficient regions of space-time, Field needs mereological machinery. Full mereology asserts that given any predicate, there is a new object which is the sum of all the objects of which that predicate holds.

Field's assumption of mereology yields a space-time thick enough to construct physical analogues of statements like the continuum hypothesis and the axiom of choice. These statements will be settled on the basis of physical geometry, despite being open questions mathematically. "[W]ithin pure second order logic, a straightforward equivalent of the axiom of choice or its denial can be stated; so that if we take second order logic to be 'physically meaningful,' the axiom is already settled at the physical level." (Field (1990) p 212) Settling the size of the continuum would be counterintuitive, given Cohen's independence results.[42]

Field de-emphasized his appeal to mereology, following George Boolos who attempted to deflate the commitments of second-order logic. "We need not think of full second order logic as involving quantification over special 'second order entities' like properties or classes...they are plural quantifiers over ordinary objects." (Field (1990) p 211)

The de-emphasis does not solve the difficulties arising from Field's treatment of continuity. Field suggested switching to a first-order version, by trading in the second-order axiom schema for a first-order version, which quantifies over regions themselves. I

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[42] This does not settle the matter against Field. One could argue for new set-theoretic axioms which do settle the size of the continuum.

examine each version, in turn, in the next two sub-sections.

Burgess and Rosen take the first-order version as official. In print, Field continues to hedge his bets. He mentions that he, "Came down more solidly in favor of the first order formulations," in Field (1985a). He continues to take the second-order version seriously in Field (1990), which Burgess and Rosen cite in support of the first-order view. I examine the second-order version first.

§2.5.2: The Second-Order Version

To defend his second-order reformulation, Field focuses on ensuring that the values of the second-order variables are nominalistically acceptable. The quantifiers range only over regions of space. "[I]f we write Hilbert's formulation of the Euclidean theory of space in this way, it has a purely nominalistic ontology." (Field (1980) p 38)

There are several problems with the second-order theory independent of the acceptability of regions. All relate to the loss of deductive completeness, which makes the language inadequate for a dispensabilist construction.

Field recognizes that the dispensabilist's commitments must be found exclusively in its quantifications, if he is to respond to QI.[43] Field's logic, then, demands a defense. The stronger logic does not permit you, in principle, to write down an exhaustive list of theorems. "In practice, how is the nominalist to know the second-order logical truths? No method suggests itself other than using the apparatus of transfinite set theory to gain

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[43] For example, Field criticizes Hodes (1984) for using "an ontologically loaded logic." (Field (1989a) p 7, fn 6)

insight into the structure of sets of points in a finite dimensional real space." (Urquhart (1998) p 151) Appeal to transfinite set theory is not a live option.

Field presents operationalist worries about incomplete logics.

> A number of readers of *Science without Numbers* have been puzzled as to how the appeal to the relation of semantic consequence in a logic without a complete proof procedure can possibly be of use. This does seem puzzling at first, since surely it seems plausible that the means by which we can know that one thing follows from another are codifiable into a proof procedure, and that seems to imply that no appeal to anything stronger than the proof procedure can be of practical utility. (Field (1985a) p 138)

But, he argues, the user of an incomplete logic may commit only to a recursively enumerable subtheory for knowledge of consequences. The preference for a recursively enumerable, compact theory can be offset by ontic gains.

Field implies that the problem with the theory which uses the extended logic is its unattractiveness. "[A]lthough there are certainly advantages to using only a compact and recursively axiomatized fragment of logic in developing physics, there are also advantages to keeping one's ontological commitments to a minimum... It seems to me that the methodology to employ in making such decisions is a holist one: we should be guided by considerations of simplicity and attractiveness of overall theory." (Field (1980) p 97)

Attractiveness is not the problem, since the second-order theory is still a Tarskian reformulation. Field focuses on establishing GR.2, logical neutrality, for his reformulation, but the real problem is GR.3. The incomplete logic makes Field's conservativeness claim implausible.

> [N]ominalists are not *entitled* to the logical resources of [the second order language]... The claim...was that the detour derivation of S_L [a nominalist sentence] from T [a nominalist theory] making use of auxiliary mathematical hypotheses is justified insofar as those hypotheses are conservative. For then, according to Field, one can always prove S_L from T alone. But how, in the absence of a formal derivation system is a nominalist supposed to do *that* (in general)? (Malament (1982) p 530)

The problem of an incomplete logic has the feel of an existence theorem in mathematics with no correlative construction, like the Axiom of Choice, which asserts the existence of a choice set for any set without providing a method for constructing one. Fortunately, there is a true statement of the language of the second-order theory, underivable within that theory. This statement is essentially a Gödel sentence for the theory. Field mentions it in his original monograph, crediting Burgess and Yiannis Moschovakis. Stewart Shapiro worked it out in Shapiro (1983a). Field responded, Field (1985a), by ceding (mostly) the second-order theory.

Shapiro develops Gödel sentences for both the second-order and first-order versions of the theory. The Gödel sentence for the second-order version is derivable in the theory which combines the nominalist theory and the mathematical theory, but is not derivable from the nominalist theory alone. Its variables range over only space-time points and regions. Since the natural number structure is representable in the domain of space-time, the Gödel incompleteness theorems apply. There is a formula of the nominalist theory which asserts the consistency of the nominalist theory in terms of points. This sentence will not be derivable from the nominalist theory alone.

If we add set theory, which Field alleges is conservative over the nominalist theory, we can derive the consistency of the nominalist theory. The loss of completeness entails the loss of conservativeness.[44] We have found a counter-example to the top-down conservativeness claim.

Field responds that the problem with incompleteness is just that we can not know the truths of the theory. "If plural quantification or some other version of second order logic makes clear sense, it shows that logic itself is more epistemologically problematic than one might hope." (Field (1990) p 220)

The problem is deeper. We have assumed that our commitments are to be found by examining our regimented theory. The existence of a true sentence of that theory that can not be asserted within the theory is troublesome. Even if the domain of quantification of the theory contains all existing objects, the theory will not be able to yield all the truths.

Field tries to minimize the importance of the omissions. "I suspect that the extra strength that [the platonist theory] has over [the nominalist theory] is confined to such recherché consequences..." (Field (1980) p 104)

The class of sentences provable by adding set theory to the nominalist theory is larger than this one case.[45] Urquhart describes a version of the Banach-Tarski paradox which may be constructed in Field's theory. A region consisting of a solid ball of unit radius can be decomposed into finitely many parts and rearranged to form a solid ball of twice the radius. As a theorem of pure mathematics, this is unobjectionable. As a

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[44] See Shapiro (1983a) pp 526-527. Burgess and Rosen, and Maddy, discuss another example of how Field's theory settles mathematical questions empirically, concerning Borel determinacy. See Burgess and Rosen (1998) §II.A.5.b.

[45] See Burgess and Rosen (1997), pp 120-123.

theorem about physical space, it is repugnant. "It would be much more reasonable to postulate that all physically relevant regions are measurable. In a case like this, it would *not* be reasonable to expect mathematics to be conservative with respect to physical theory." (Urquhart (1988) p 152)

Even if there are no cases where adding mathematics makes a difference to the physics, strictly construed, there are cases which make a difference to the geometry. Since Field takes geometry as a physical theory, these are physical differences.

Urquhart concludes that the problem is the breadth of Field's conservativeness claim. Mathematics is not generally conservative, as the top-down argument claimed, but that required by physical theory is. He suggests that the dispensabilist should look only for bottom-up arguments for conservativeness. "Abandon any hope of a general conservative extension result with respect to mathematical theories, but only...develop such results for the mathematics actually needed in physical theory." (Urquhart (1988) pp 153-154)

Field argues, in his defense, that standard mathematics is also best formulated as a second-order theory. This is not an argument for his reformulation against QI.4. It is an argument against QI.3, which Field assumes. If he is correct, it shows that the Quinean indispensabilist has a further problem defending his commitments to mathematical objects.

> I myself have substantial doubts about the philosophical legitimacy of regarding 'the logic of the part-of relation' (or any other form of second order logic) as genuinely a logic... Because of this doubt (and some others) I think that the extended representation theorem [from Field (1980)] and the conservative sub-theory result that follows from it probably do not ultimately yield a satisfactory

> reply to the Quine-Putnam argument, and that attention to the case of first order theories is required. (Field (1985a) p 141)

In this section, I showed how Field's second-order reformulation violates GR.3. I demonstrate in the next section the problems with his first-order reformulation.

§2.5.3: The First-Order Version

While the first-order reformulation avoids the incompleteness of mereology, it does not provide an adequate framework for Field's project. I discuss results from Shapiro, who shows that the representation theorems are not constructible, and Maddy, who argues that the resulting theory is inadequate for science and thus violates GR.1.

In Field's first-order reformulation, variables range over space-time regions, with a subregion relation included in the theory. Empty regions, regions with no subregions, are taken as points. This reformulation is logically neutral, since the logic is deductively complete. Still, Field can not construct his representation theorems. A Gödel sentence again demonstrates the difficulty.

In the first-order language, we can still construct a sentence, Con_N, asserting the consistency of the first-order nominalist theory, N. Con_N is unprovable within N, but it is provable within a stronger set theory, S. If there were a representation theorem which mapped the empty regions into R^4, then Con_N would be derivable from S+N. Since S is conservative over N, ex hypothesi, there must not be first-order representation theorems.[46] Without representation theorems, Field has not shown that his reformulation is adequate.

Maddy demonstrates that the first-order version violates GR.1 by being too weak for science in three ways. First, the first-order version of the continuity axiom, which entails that a line divided in two will have exactly one point of division, guarantees the division, but not the point of division. Whether that point is available depends on other elements of the vocabulary. "Before the conclusion [that a region is continuous] can be properly drawn, the scientist must verify that the division in question is definable in the formal vocabulary of the appropriate fictionalist theory." (Maddy (1990b) p 195)

Maddy's second criticism regards the lack of representation theorems. "Second-order fictionalism allows for the non-literal but instrumental use of the claim that 'the structure of space-time is that of a four dimensional Galilean space of the field of real numbers,' but the first-order version only allows 'that the structure is that of a four-dimensional Galilean space over some "nice" real-closed field which might or might not be the field of real numbers.'" (Maddy (1990b) p 196)

Field made this point himself in trying to generate a weaker, extended representation theorem for the first-order version of the theory.[47] "[I]n practice, it doesn't matter: the fields in question are so much like the reals that we can get away using the reals in practice even if one of the other fields is in fact the correct one." (Field (1990) p

46 See Shapiro (1983a), pp 529-530, where a semantic analog to this deductive argument is provided.

47 In Field (1985a) pp 133-135.

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Maddy's third complaint also arises out of the failure of the representation theorems. "The first order fictionalist adopts an anemic view of the scientific theory of space, one that pays closer attention to narrow evidential relations between particular sentences than to broader explanatory goals. This last may be its greatest weakness." (Maddy (1990b) p 203)

The reference to narrow evidential relations concerns, again, continuity. Only instances of the continuity schema are available in the first-order version. Maddy argues that the best explanation for the individual instances comes by reference to the full axiom. The standard theory includes the full version, and the second-order nominalist can appeal to it, too, but the first-order theory takes each instance as brute. Field denies that this is a problem, but provides no argument.[48]

Field agrees that his reformulated theory is weaker than the standard one, but insists that it is not thus inadequate. Instead of violating GR.1, he argues, the weakness of the first-order version is an asset, since a weaker physical theory will be better tested. An account of the applicability of mathematics to science restricted to the mathematics applied in science seems desirable. "[F]rom a fictionalist perspective...it is not at all anemic to withhold belief in those arcane 'excess consequences' that the Dedekind continuity axiom has in a platonist setting." (Field (1990) p 216)

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[48] See Field (1990) p 214. Maddy ascribes to Field's first-order theory a schema for continuity, but Field insists that he takes a single axiom. The confusion arises because Field takes a schema for the existence of regions. Maddy can reformulate her objection in terms of regions. His response takes the problem in terms of regions.

Even if Field is right that a weaker theory may be adequate, Maddy's claim buttresses Shapiro's argument against his first-order reformulation. Without the representation theorems, Field has not demonstrated that he has provided the synthetic formulation he promised.[49]

In this section, I argued that Field's first-order reformulation violates GR.1. In the previous section, I showed that the second-order version violates GR.3. Fortunately for Field's project, Burgess generates elegant first-order representation theorems for physical theories like NGT which violate neither.

§2.5.4: The Burgess Construction

Burgess argues for the adequacy of some dispensabilist reformulations in a series of papers.[50] Burgess (1984) demonstrates a general method for replacing analytic formulations of theories like NGT with synthetic ones. He refined and extended his work in Burgess (1991a) and (1991b). The presentation in Burgess and Rosen (1997), Chapter II.A, summarizes his approach. He presents a standard, first-order theory which generates a restricted representation theorem. I sketch his construction, in order to give

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[49] Field later argues that we do not need them. "In the weaker logics, the usual representation theorems would have to be given up; but I argued that even without them one could in a certain sense argue for the adequacy of the nominalistic theories." (Field (1990) p 212) In Field (1985a), he provides alternative, non-standard representation theorems. Considerations from the next section renders these moot.

[50] Burgess's interest is in the clarification of technical matters, and not the defense of nominalism. "[Field (1980)] is also concerned with the development of synthetic alternatives to the usual analytic presentations of physical theories, or as he *somewhat tendentiously* terms them, 'nominalistic' alternatives to the usual 'platonistic' presentations." (Burgess (1984) p 394, emphasis added)

the reader a feel for a successful reformulation in the spirit of Field's original work.

Burgess uses a two-sorted theory in a two-sorted first-order language. Primary variables range over physical entities. Secondary variables range over mathematical entities. Primary predicates have no places for secondary variables, secondary have no places for primary variables, and mixed predicates have places for both kinds of variables. Formulas are classified according to their predicates. Pure mathematics is the study of the secondary formulas and primitives. To generate the appropriate representation theorem, Burgess shows how to eliminate all secondary and mixed formulas from the scientific theory.

Burgess relies on a consensus that scientific theory needs only the axioms of analysis, and so only as much set theory as is needed to construct the real numbers. "Field's own informal descriptions of his method...generally down-play the dependency of his strategy for its success on the presumed fact or expert opinion that all the mathematics required for physical applications to date can be developed on a much more restrictive basis than that of the standard axioms of set theory." (Burgess and Rosen (1997) pp 191-192)

Burgess proceeds without any mixed theorems. This may be surprising, for these are the bridge functions used for correlating physical objects with measurements expressible by real numbers. "In order to be able to apply any postulated abstract entities to the physical world, we need *impure* abstract entities, e.g. functions that map physical objects into pure abstract entities. Such impure abstract entities serve as a bridge between the pure abstract entities and the physical objects; without the bridge, the pure objects would be idle." (Field (1980) p 9)

Burgess eliminates impure mathematical entities in two steps. First, in lieu of terms which refer to impure numbers, like '20 grams', he introduces, as Quine suggested, measurement predicates, like 'mass-in-grams' which require only pure numbers. Second, he assumes a plausible exclusion principle, that no two objects have the same measurements. This allows construction of a measurement profile for each object. The –tuple of real numbers required to measure *n* properties can be seen as a single measurement, since we can code it with a single real number, by well-known methods. A similar coding of set-theoretic notions by real numbers suffices to eliminate the need for impure mathematical objects in order to account for counting.

> [I]f physical objects can be represented by real numbers, then countable sets, countable sets of countable sets, countable sets of countable sets of countable sets, and so forth, of physical objects or of physical objects and real numbers, can be represented by countable sets and so forth of real numbers, and hence by single real numbers. So it seems plausible that no impure mathematical entities, and no mathematical entities other than real numbers, will be needed. (Burgess and Rosen (1997) p 79)

Burgess starts with a general, two-dimensional, affine coordinate geometry, written in the usual analytic style, with variables ranging over real numbers. Since there are multiple distinct admissible coordinate systems, he generalizes and invariantizes the theory. He demonstrates how to transform any arbitrarily chosen coordinate system to a generalized coordinate system, eliminating the arbitrary choice. Then, he replaces references to numbers with references to points. Lastly, for the geometry, he "beautifies" the theory, introducing a geometric definition of proportionality.

Field would call the resulting geometric theory nominalistically acceptable. It ranges over points, which one may take as space-time points. Burgess's construction suffices as a framework for NGT and a limited number of other physical theories.

To apply this theory to classical physics, Burgess extends the method to analysis, from algebra, adding a primitive for integrity, and appropriate axioms. He extends from two to three, and higher, dimensions, and extends the geometry from affine to Euclidean. Each addition provides a conservative extension of the original theory.

Lastly, kinematic axioms, patched together from the affine and Euclidean cases, are added to the theory, as well as dynamic primitives for mass density and gravitational potential. The result is a mapping from the traditional two-sorted scientific theory to a one-sorted theory which contains only the primary primitives and axioms of the original theory, and counterparts of each of the secondary and mixed primitives and axioms. This mapping provides the Tarskian reformulation Field sought for NGT.

Burgess notes that his methods are easily adaptable to special-relativity, where the kinematics are essentially just variations of the pre-relativistic ones. A reverse of the construction demonstrates how the analytic axioms can be derived. Thus, Burgess shows how to derive the mathematics needed for NGT, and how to extend this method to other physical theories based on a flat space-time.

Burgess shows that geometric dispensabilism requires only standard logic. He does not generate conservativeness for set theory generally, the top-down argument. He provides a bottom-up argument via his representation theorem, for the conservativeness of analysis over NGT and related theories.

The absence of the top-down argument for conservativeness weakens the appeal of Burgess's approach as a general response to QI. It points to the problems of extending the strategy to other physical theories. With no general conservativeness result, entirely new constructions must be generated for each extension.

Adequate Tarskian reformulations for some physical theories which need not violate GR.1, GR.2, or GR.3 are thus available. Field has not shown that mathematics is generally conservative over physical science. Also, Field has not established that these reformulations are attractive, though I disputed this Ground Rule in §2.2.

Even though the logic of Burgess's construction is neutral, Field's argument for mathematical nominalism on the basis of dispensabilist constructions is not complete. A more contentious logic will be required to finish the project.

§2.6: Metalogic and Modality

To support his claim that we can do without mathematical objects, Field not only provides a reformulation of standard science, but also an account of what we ordinarily take to be our mathematical knowledge, including knowledge of mathematical inferences. Knowledge of inferences is ordinarily taken to be metatheoretic knowledge, and so Field has paid special attention to metalogic.

One way to account for metalogic would use a construction like the one he provides for physics. It is easy to see why Field would avoid such an empirical account. If metalogical claims were known empirically, then they would be factual. Given his close connection between mathematical knowledge and logical knowledge, this could lead to truth values for mathematical existence claims.[51]

Field could deny the need for an account. In fact, he takes an instrumentalist approach to model theory: it is useful but its claims are just not true. But Field's hostility to instrumentalism generally makes this option, as a complete account of metalogic, unsatisfying. Instead, he provides an account which relies on modality.

In this extended section, I show that Field's use of modality violates GR.2. First, in §2.6.1, I show in greater detail why Field introduces modality. In §2.6.2, I show how he uses his modal operator. In §2.6.3, I show how the modal operator violates GR.2.

§2.6.1: Mathematical Knowledge as Logical Knowledge

In this section, I connect Field's motivations for introducing object-level modal operators with his dispensabilist project. The modal operators have been misunderstood to be independent of the project in *Science Without Numbers*.

Field's account of metalogic is sometimes taken as a response to a new indispensability argument: mathematics is indispensable to metalogic. Field encourages this reading by discussing an indispensability argument for metalogic.[52] On this reading, Field's introduction of object-level modality is a dispensability argument parallel to the one for the mathematics in physics.

Burgess and Rosen interpret Field as arguing for nominalist metalogic on its own

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[51] An empirical account would also conflict with Field's Principle of Intrinsic Explanation, which I discuss in Chapter 5, Part 3.

[52] Field (1988), §4. Also, Putnam includes logic as a field to which mathematics is indispensable in Putnam (1971).

terms. "While Field has not explicitly said that the development of a nominalistic metalogic is required for the sake of nominalistic physics, he has said that the development of a nominalistic metalogic is required for its own sake." (Burgess and Rosen (1997) p 193)

In contrast, the role of Field's metalogical account is intimately tied to his dispensabilist project. Field's conservativeness claim uses mathematics. Model theory is really a branch of set theory. Field needs an account of how the dispensabilist can apprehend these mathematical inferences. If the top-down argument for conservativeness held, there would be no problem here, since any use of mathematics would be justified. Since Field can only establish the bottom-up argument for conservativeness of a particular mathematical theory over a particular nominalist theory, he requires a new argument for the legitimacy of mathematics in each new theory.

Field argues that much of what we take to be mathematical knowledge is really logical knowledge. His use of modality distances his position from two others which explain mathematical knowledge in terms of logical knowledge: the logicist and the formalist. In the remainder of this section, I explain how Field distances his position from either of these.

Field accounts for what is ordinarily seen as mathematical knowledge in three ways. First, he argues that much of what we take to be mathematical knowledge is really empirical knowledge, of space-time for example. Field argues that the intuitive basis for set theory is the part-of relation.[53] In addition, some empirical knowledge about which

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[53] See Field (1985a) p 137, fn 12.

axioms are generally accepted by the mathematical community, and which problems are seen as important, might be taken for mathematical knowledge.

Second, though only a surprisingly small part of the account, Field claims that, for limited results, there is no knowledge to be had. Some debatable axioms, like the axiom of measurable cardinals, are not derivative of other commonly held axioms, so they can not be taken as known on the basis of inference. Neither can they be taken to be known empirically, since they correspond to no physical facts.

Lastly and mostly, Field argues that much of what we take to be mathematical knowledge is really logical knowledge of which theorems follow from which axioms.[54] Field thus defends some degree of mathematical objectivity, without committing to mathematical objects, on the basis of logical objectivity. "[Y]ou don't need to make mathematics actually be about anything for it to be possible to objectively assess the logical relations between mathematical premises and mathematical conclusions." (Field (1998a) p 317)

Field's replacement of mathematical knowledge with logical knowledge has received less attention than his replacement of mathematical objects with space-time points. Yet even without a successful reformulation of mathematics, Field may have accounted for much of what we take to be mathematical knowledge without commitment to mathematical objects.

Field is not a classical logicist, of course. Frege and Russell failed in their

[54] Colyvan, defending indispensability, similarly accounts for the obviousness of mathematics by the obviousness of mathematical entailments. See Colyvan (2001) pp 116-122.

broader attempt to interpret mathematics as logic. Field criticizes the logicists for violating Kant's principle that the denial of an existence claim should never lead to a contradiction. "[C]ounting existence assertions as part of logic...would tend to mask the fact that there is a substantive epistemological question as to how it is possible to have knowledge of the entities in question (God, numbers, etc.)" (Field (1984) p 80)

Kant's principle is designed for empirical questions, rather than mathematical ones. The existence of trees or electrons or God should not follow merely from logical principles. But some inferences are facilitated only with a logic strong enough to entail existence claims and there is no harm in using a logic which entails the existence of objects which exist in all possible worlds. Nonetheless, in the context of the Quinean indispensability argument, one must accept the Kantian principle of logical neutrality.

So, Field is not a logicist, since he is careful to avoid a logic which makes existence claims. He is also not a formalist. Field uses a modal operator to represent metalogical assertions, like consistency, while formalism does not license the assertion of the consistency of a set of axioms. The formalist can only claim that the consistency of one set of theorems follows from another set. "The implausibility of the crude form of deflationism lies in its being forced to try to explain apparent knowledge of *what doesn't follow* in terms of knowledge of *what does follow*. But there is no point in trying to do this if both have equal claims to count as logical knowledge." (Field (1984) p 83)

Field introduces modality for three reasons. First, he needs to account for difficulties arising from the incompleteness of the second-order version of his reformulation of physics. If we drop that version, the motivation for object-level modality is weakened. Second, Field needs object-level modality to account for the nominalist's apprehension of conservativeness. Lastly, it distinguishes his account of mathematics as logic from the logicist and formalist projects.

§2.6.2: Metalogic and the Modal Operator

My discussion of Field's project has strayed a bit from the indispensability argument, so it is worth a moment to regain focus. Field's response to QI primarily consists of the reformulation of NGT. If successful, Field not only denies that science needs numbers, but also that we have any mathematical knowledge. But we seem to have lots of mathematical knowledge. We seem to know mathematical axioms, for example, and that certain theorems follow from them. Field claims that the axioms are false, and knowledge of entailments is just logical knowledge. We also seem to know of the consistency of sets of (especially mathematical) sentences.

To account for our purported knowledge of consistency, Field introduces an object-language operator '◊', to be read 'it is logically consistent that'. We may take '~◊~(p ⊃ q)' as saying that p is a consequence of q. The claim that p is consistent is expressed '◊p' in lieu of the metatheoretic assertion that p has a model. Field's goal for his modal operator is to count all consistent sets of axioms as logically possible. Thus, '$\Diamond AX_{ZF}$', is the claim that the axioms of Zermelo-Fraenkel set theory are consistent.

Logical consistency is not ordinarily understood as an object-level notion. We generally understand consistency metalinguistically, as a property of sets of sentences of an object language. Field argues that we should see consistency the way we see conjunction, or quantification, as an element of our logic.[55] "Possibility seems intimately concerned with logic in a way that it is not intimately connected with physics: logic is the science of the possible." (Field (1982a) p 76)

Use of the operator forces an adjustment to ordinary semantics. Field takes the rules of S5 to govern the operator, and adds truths of the form '◊A' for non-modal A. Replacing the standard Kripkean model theory, Field considers various models in lieu of possible worlds. The new semantics still commits to sets.

Field urges us to think of his modal operator as representing a strictly logical notion of possibility. Sentences like '◊∃x (x is a bachelor • x is married)' count as logically true, since it is possible to substitute different predicates to make the sentence true. This narrow notion of possibility allows few logical truths.

Field does not introduce an additional modal operator for proof theory, in part due to concerns about modal commitments. Instead, Field uses the one operator to construct surrogates for traditional proof- and model-theoretic reasoning. The platonist uses, he claims, four schemas in reasoning meta-theoretically about mathematics: model-theoretic possibility (MTP); model existence (ME); modal soundness (MS); and modal completeness (MC).

MTP: If there is a model for 'A', then ◊A
ME: If there is no model for 'A', then ~◊A
MS: If there is a proof of '~A',[56] then ~◊A.

[55] Discussion with Jared Blank helped me understand and state this issue more clearly.

[56] In some appropriate formal system. Similarly for MC, MS#, and MC#.

MC: If there is no proof of '~A', then ◊A.

The nominalist can offer the following object-level surrogates, using non-modal sentences as substituends, and even, in ME#, for modal substitutions.

MTP#: If □[57](NBG[58] ⊃ there is a model for 'A') then ◊A
ME#: If □(NBG ⊃ there is no model for 'A') then ~A
MS#: If □(NBG ⊃ there is a proof of '~A') then ~◊A
MC#: If □(NBG ⊃ there is no proof of '~A') then ◊A

The purpose of these surrogates is to support Field's claim that the nominalist can help himself to standard model and proof theory, since he can see the entailments from NBG to models or proofs. He uses the object-level modal operator to represent his findings most austerely. Again, Field replaces mathematical knowledge with logical knowledge.

§2.6.3: Modal Metalogic: Criticisms

In this section, I argue that Field's modal operator violates GR.2, Logical Neutrality, and that, in the absence of general conservativeness results, the nominalist can not help himself to the metatheoretic tools he needs to apprehend inferences using the metalogical surrogates.

Model-theoretic results are ordinarily understood in terms of relations among sets

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[57] '□' is '~◊~', and represents logical necessity.

[58] 'NBG' is the axioms of von Neumann-Bernays-Gödel set theory. You may substitute your favorite set theory.

of sentences, or propositions. Field's modal operator forces him to take consistency, and its related notions, as brute. While this approach may work for logical connectives, it is less plausible in model theory, where the concepts seem fundamentally tied to groups, or sets, of sentences.[59]

More problematically, we will not be able to understand the semantics of model theory without invoking mathematics. Field insists that we need not believe the results of model theory since it is useful for discovering what are ordinarily considered metalogical facts, but it is not necessarily truth-conducive. Field thus interprets claims involving models instrumentally. He urges that theories we use for explanation reveal our commitments, but theories we use for discovery are not to be believed.

If mathematics were generally conservative, Field's instrumentalism would be less tendentious. All mathematical claims would be in principle eliminable from model theory. But Field is allowing the nominalist to help himself to model-theoretic results without justification. If the instrumentalist approach is acceptable here, why not in physics, where mathematics is also used for discovery?

Field's account of metalogic has no epistemic advantage over the traditional one, since a minimal condition on the adequacy of his metalogical devices is their provable equivalence with mathematical results. Shapiro argues, "It is unfair to reject set theory,

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[59] Mark Balaguer also defends a primitive object-level consistency. He says that we can reflect on people's intuitive grasp of what sets of sentences are consistent, from a first logic class, say, and yet have no idea about metalogical notions of semantic or proof-theoretic consistency. Reliance on an intuitive notion for Balaguer is acceptable, since he does not claim that all our commitments are to be located within a single best formal theory. Balaguer is free to extend his logic as long as it facilitates inference.

as our modalists do, and then claim that we have a pre-theoretic grasp of the modal notions that, when applied to mathematics, exactly matches the results of the model-theoretic explication." (Shapiro (1993) p 457)

More importantly, the modal operator upsets the canonical language of QI. For example, Quine's defense of first-order logic on the basis of the concurrence of various definitions of logical truth is lost. Moreover, the operator violates GR.2, Logical Neutrality. "The promise of fictionalism is that an epistemology of the concrete may be more tractable than an epistemology of the concrete and abstract. But the fictionalist now seems to require an epistemology of the actual and possible, and it is not clear that this is a gain." (Shapiro (1993) p 462)[60]

Field argues that his modal commitments are quite weak, and needed anyway. "[T]here is no serious prospect of doing without [logical possibility] in one's philosophy: surely there is a difference between on the one hand characteristics (such as being red-and-not-red) which nothing logically *could* have, and on the other hand characteristics which nothing happens to have." (Field (1988) p 252)

Field's casual appeal is strained, given that he restricts the operator to logical possibility, and allows as logically possible such concepts as married bachelors. His use of possibility will not capture our intuitive notion.

Quine's proscriptions against modality may implausibly restrict his idea of a best theory. Still, these are the terms of the debate. Field's modal operator entails commitments to possibilia which violate GR.2. The dispensabilist must not take modal

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[60] Resnik argues similarly. See Resnik (1985b) p 207.

notions unexplicated, as logic. The dispensabilist lacks the resources to admit modality, even if he takes it at the object level. The utility of model-theoretic reasoning remains unexplained, for Field.

In §2.2, I outlined four ground rules for Field's reformulation. There I showed that Field is unlikely to satisfy GR.4: Attractiveness. In §2.4 and §2.5, I showed that Field's project does not generate conservativeness generally, violating GR.3. In this section, I showed that Field's project, when it includes his modal operator, violates GR.2: Logical Neutrality. The second-order version violates GR.2. doubly, as I showed in §2.5.2. In §2.5.3, I showed that the first-order version violates GR.1: Adequacy. In the next section, I adduce further worries about Field's ability to satisfy GR.1.

§2.7: Extending the Strategy

To defend nominalism in response to QI, Field must argue that mathematics applies conservatively to available, adequate, nominalistically acceptable formulations of all scientific theories. Field must show that any good mathematical theory is conservative over any good physical theory, even a false one, since mathematical theories are supposed to be compatible with any state of the world. Field himself calls conservativeness, "Necessary truth without the truth." (Field (1982a) p 59)

While some see promise in Field's strategy, and related ones, the consensus opinion, which I review in this section, is that these extensions of his project are unavailable, i.e. that GR.1 is unsatisfiable.

Malament was at the head of the line of critics who claimed that Field's project is not extendable. He doubted the likelihood of developing reformulations of classical Hamiltonian mechanics, a phase-space theory, and of quantum mechanics (QM). The problem with phase-space theories is that the quantifiers would have to range over mathematical models, which are not nominalistically acceptable. A reformulation of QM would have to quantify over points and regions of Hilbert space, again repugnant. Propositions, sets of quantum events, would play the role that space-time points played in Field's reformulation of NGT.

Mark Balaguer, beginning a dispensabilist project for QM, takes these sets of quantum events as physically real propensity properties of quantum systems. The properties can either be taken as nominalistically acceptable, or one may nominalize them in the way that Field nominalized geometric properties, like length.[61] Prima facie, a propensity property seems like the kind of abstract object a nominalist would abhor. Still, even if Balaguer's technique is successful, there are other physical theories which we do not know how to nominalize, and future physical theories will bring their own difficulties.

For general relativity, as with QM, there is no available synthetic version of the geometry. Field was originally optimistic about the availability of a synthetic geometry for space-times which are not flat. "I believe that the ideas here are extendible to curved space-time... It is not however a trivial task to work out the details of this, for the whole construction would have to be based on a representation theorem of a more complicated kind than any I have seen." (Field (1980) p 123)

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[61] For more details, see Balaguer (1998) pp 117-127.

Burgess and Rosen cite the curvature as a problem.[62] "But the most obvious obstacle to developing an elegant, synthetic, pure, natural-looking, invariant, straightforward version of such a theory at present is the circumstance that so far no one has developed even an inelegant, analytic, coordinate, artificial-looking arbitrary-choice-dependent, devious version of such a theory." (Burgess and Rosen (1997) p 118)

On another front, Resnik argues that the dispensabilist has no account of statistical inference.

> Consider, for example, the claims and methods of cardiology or botany, which are about as non-mathematical as they come. These sciences use statistical significance testing to determine whether data are more than coincidental, and statistical estimation theory to determine the values to associate with sets of measurements... Neither Field, nor Krantz et al. address the question of how we are to deal with this kind of applied mathematics. (Resnik (1997) pp 56-57)

Field has not given us a way to account for scientific inferences involving probabilities. Physics may be easier to nominalize, just because it is so mathematized. The close connection between physical geometry and pure geometry make a synthetic formulation more likely. Still, Balaguer's construction for QM might go some distance toward answering Resnik's complaint, for one relevant difference between NGT and QM is the use of probabilities.

Pronouncements on the likelihood of extending Field's strategy are doubly speculative. We do not know the nature of our ideal physical theory. And we do not know what kinds of dispensabilist techniques may be developed. "Ultimately, of course,

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[62] Urquhart (1988) also expresses worries about the availability of a geometry for curved space-time.

a discussion of the Quine-Putnam argument should be carried out not for Newtonian gravitational theory, but for physical theories that we regard as true. But such a discussion is not possible at present (since it must await advances in physics, and very likely in nominalization techniques as well." (Field (1985a) p 141)

Burgess and Rosen, who are not sympathetic to nominalist goals, urge that the concerns I have discussed in the section about satisfying GR.1 are ill-founded. "As a consequence of nominalism's being mainly a philosopher's concern, this open research problem is...one that has so far been investigated only by amateurs - philosophers and logicians - not professionals - geometers and physicists; and the failure of amateurs to surmount the obstacles is no strong grounds for pessimism about what could be achieved by professionals." (Burgess and Rosen (1997) p 118)

Worries about whether dispensabilist constructions like Field's are or will be available for future science are thus indecisive.

§2.8: Does the Mathematics Remain?

Some philosophers do not see a dispensabilist reformulation as eliminating mathematics, even if it abides by the Ground Rules. They maintain that Field's theory includes mathematics, despite lacking quantification over mathematical objects. I argue in this section against this 'persistence criticism' of Field's project. But, the idea behind the persistence criticism, that eliminating quantification over mathematical objects physical theories does not suffice to eliminate mathematics from our ontology, is right.

Resnik claims that persistence is in line with Quinean principles. "Owing to the richness of Field's physical ontology, philosophers in Quine's tradition might object that Field has just hidden his mathematical objects in physical disguises." (Resnik (1985b) p 192)

Maddy also formulates this objection, which amounts to saying that mathematics is essentially concerned with structure, whether instantiated by space-time points or by real numbers. In space-time, we have, "Everything essential to the real numbers." (Maddy (1990b) p 201)

Shapiro does not exactly claim persistence, but does argue that Field's reformulation does not reduce our commitments. "The intended structure - and the ontology - of each theory is the same as that of the corresponding realist theory, and is not to be preferred on ontological grounds." (Shapiro (1993) p 479)

Field provides two responses to the persistence criticism. First, anticipating the criticism, he argues that it confuses mathematical and physical structure. "It is hardly surprising that mathematical theories developed in order to apply to space and time should postulate mathematical structures with some strong structural similarities to the physical structures of space and time. It is a clear case of putting the cart before the horse to conclude from this that what I've called the physical structure of space and time is really mathematical structure in disguise." (Field (1980) pp 33-34)

Field also describes the desirability of ridding physics of space-time points. "Getting rid of points would also remove any temptation (already inappropriate) to view regions as sets of points. In general, it would help reduce the (already inappropriate) tendency to view appeal to space-time as appeal to mathematical entities in disguise."

(Field (1989a) p 48)

Burgess (1991a) demonstrates a method for dispensing with space-time points in synthetic theories. We can refer to any space-time point while only committing to occupied points, as long as we make two assumptions, that matter is dispersed, and some region of space is fully occupied. Consider the Kepler point, the unoccupied focus of the ellipse defined by Earth's orbit. We can describe it relative to a point on Earth, directly above Kuala Lumpur, say. We describe the distance to that point in terms of the ratio of the distance between, say, a plum's center and a point on its surface and between the plum and the Khyber Pass. If Field's construction shows us how to do science without numbers, then this construction may show us how to dispense with space-time points, depending on the plausibility of the assumptions.[63]

Field is right that space-time points and regions are not abstract entities. The geometric constructions based on space-time points are applied versions of pure, abstract geometry. If we accept QP, Field's response suffices. Ex hypothesi, all mathematical objects are eliminated from the reformulation, which refers only to concrete dopplegangers. If we reject QP, as I suggest we do in the next chapter, I think the persistence criticism has merit. There is something geometrical, ineliminably mathematical, about space-time structure. Especially if we adopt Field's interpretation of GR.4, that the new theory be structured like the original, it should be implausible that we can eliminate our commitments with this quick, if skillful, stroke.

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[63] Given that matter seems not to be densely packed, the second assumption appears false. But all Burgess needs is one fully occupied, dense region to serve as the source of all measurements.

§2.9: Field's Dispensabilism: Conclusions

Field's project violates the Ground Rules. Against Adequacy, there are no reformulations for many current and future scientific theories. Against Logical Neutrality, Field includes a modal operator and a second-order quantifier. Against Conservativeness, only the bottom-up conservativeness results are available. And against Attractiveness, Field's reformulation includes unattractive primitives.

While the main problem with Field's project may be his extension of Quine's canonical language, he introduces a further extension.[64] His account of consistency leaves us unable to know that non-finitely axiomatizable mathematical theories, like Peano arithmetic, are consistent, since we can not know infinitely many instances of an axiom schema. To solve the problem, he introduces a substitutional quantifier to conjoin the infinitely many theorems. Still, Field's nominalism limits the number of conjuncts he can produce. "As a result, the nominalist's language is not closed under the operation of forming infinite conjunctions...This once again places outside his ken infinitely many theories treatable by the platonist." (Resnik (1985b) p 206)

Field invokes simplicity in defense. "It seems to me that the methodology to employ in making such decisions is a holist one: we should be guided by simplicity and attractiveness of overall theory... It is the simplicity of the overall conceptual scheme that

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[64] This is another example of Field's habit of extending logic to avoid difficulties. In response to a different set of problems, he introduces a 'determinately' operator. See Field (2003). See Resnik (1985b) p 207, for a similar criticism. Field defends his methodology: "My attitude has been that we ought not to regard first-order logic as having been laid down from on high... Nor, of course, is my attitude that whenever we run into trouble with the nominalization programme, we can feel free to invent some fancy new operator that solves the problem by fiat." (Field (1989a) pp 51-52)

ought to count." (Field (1980) p 97)

Given the incompleteness of the resultant logic, Field's claim to simplicity founders. Field's logical extensions also undermine his claim to semantic uniformity. He urges that we not take mathematical propositions as true claims containing oblique references to concrete objects because doing so makes explaining the applicability of mathematics to science more difficult. If we adjust the logic, we are no longer taking the sentences at face value.

Field argues that it does not matter what we take as logic.

> What is important is the use of various operators...taken as not needing definition, but governed by a body of primitive assumptions, and primitive rules of inference. By taking the operators as primitive, we avoid having to employ the set-theoretic definitions that engender ontological commitment. I think it is natural, if one has the primitive operators, to call them logical, but this lexicographical decision is not what is playing the substantive role. (Field (1989a) p 49)

True, classification is not at issue. The problem is that a construction which takes the operators as primitive can not be a successful response to QI. "One reason for [its failure] is that Field is forced to make such strong extensions of standard first-order logic that it is no longer possible to assess the nominalist nature of his theory solely by the range of its bound variables." (Resnik (1985a) p 163)

On the other hand, Field has made significant steps toward undermining QI. Burgess's neat first-order representation theorem provides a limited bottom-up conservativeness. Field's interpretation of GR.4 makes the elimination of mathematics too difficult. Easier reformulations, like N_2 are available. If we can get by without a substantial metalogical account, it may be possible to generate a strong response to QI. Given the potential advances in nominalization techniques, a cautious optimism, on the part of Field and other dispensabilists, may be justified.

More importantly, Field's project mistakenly accepts the Quinean terms of debate, i.e. Quine's method for determining ontic commitment. In the next chapter, I discuss problems with QI.1 - QI.3.

Chapter 2: Against Quinean Indispensability

Chapter 2 consists of three parts. In Part 1, I raise concerns about the first three premises of QI. In Part 2, I defend an instrumentalist response to QP. In Part 3, I show that even if we grant Quine QI, it does not generate the mathematical objects it advertises.

Part 1: Problems for Quine's Argument

§1.1: Introduction

Once again, consider QI.

(QI) QI.1: We should believe the theory which best accounts for our empirical experience.
QI.2: If we believe a theory, we must believe in its ontic commitments.
QI.3: The ontic commitments of any theory are the objects over which that theory first-order quantifies.
QI.4: The theory which best accounts for our empirical experience quantifies over mathematical objects.
QI.C: We should believe that mathematical objects exist.

Field, like the proponents of modal reinterpretations, denied QI.4. In the first part of this chapter, I show that the problems with QI arise earlier in the argument. While I discuss problems with QI.1, and QI.2, I focus mainly on QI.3, which encapsulates QP, Quine's procedure for determining ontic commitments.

Quine seeks a single formal language for revealing our commitments because of the homogeny that underlies QI.1and QI.2. In §1.2, I account for and criticize Quine's homogeny, and mention a few difficulties with the physicalism of QI.1. In §1.3, I deny the confirmation holism which underlies Quine's homogeny.

My main concern, though, is with QI.3. QP purportedly allows us to reveal our commitments without ontic prejudice: we regiment our ideal theory and the commitments fall out of it. This method misrepresents the way in which we determine our commitments. Instead, we regiment our preconsidered commitments. If, like Quine, we are predisposed to nominalism, regimentation should not commit us to mathematical objects.

Against QP, I argue in §1.4 - §1.7 that we should not look to first-order versions of scientific theory for our commitments. There are many useful logics, some of higher order, some which include names. None of them are the unique language for expressing our ontic commitments. The method Quine suggests at QP.3 is faulty. My criticisms will thus apply both to the use of first-order logic as canonical language, and to the way in which Quine reads the commitments from a regimented theory. Without QP, Quine fails to generate ontic commitments to mathematical objects.

Quine notes a trap into which he falls, that of favoring a formal tool for the wrong reasons. He writes that anthropologists, carried away with the virtues of symbolism and first-order logic, overemphasize the notion of kinship.[65] I argue that Quine's reliance on first-order logic arises similarly, from overemphasizing its structural virtues which do not justify choosing that language to reveal our commitments exclusively.

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[65] See Quine (1978a) p 154.

§1.2: Homogeny

QI.1 reflects Quine's physicalism. Combined with QI.2 it reflects Quine's homogeny.[66] In this section, I mention two difficulties with physicalism and argue that homogeny is ill-motivated.

First, on a literal reading, Quine's physicalism seems to eliminate the existence of ordinary objects. There are no people or trees or brick houses on Elm Street. We may use first-order logic to regiment sentences about ordinary objects, but such work is irrelevant to questions about what exists. Our real commitments are only to be found in our physics. There are only particles, or strings, or whatever objects are yielded by a mature scientific theory.

Second, the physicalist assumes not only that all objects are reducible to the elements of complete physics, but also that explanations of all events, including intensional ones, are in principle available at that level. Putnam, in later work, voiced objection to this assumption of Quine's.

> The fact is that most of science and meta-science cannot even be expressed in a perfectly precise notation (and all the more so if one includes philosophy under the rubric 'meta-science' as Quine does). Words such as 'normally', 'typically', etc, are indispensable in biology and economics, not to mention law, history, sociology, etc.; while 'broad spectrum' notions such as 'cause' and 'factor' are indispensable for the introduction of new theoretical notions, even if they do not appear in 'finished science', if there is such a thing. Philosophy cannot be limited to commentary upon a supposed 'first-class conceptual system' which scarcely exists and whose expressive resources cover only a tiny fragment of what we care about. (Putnam (1979) p 132)

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[66] See Chapter 1, §1.2 (physicalism) and §1.3 (homogeny).

A scientific theory (e.g. a version of Newton's laws of motion) may quantify over centers of mass or only space-time points. Still, the objects of our ordinary experience are physically real, even if our best theory does not quantify over them, and may not even be able to define them adequately. Jody Azzouni argues that a theory which described my bat hitting a ball in terms of elementary particles would not even describe that phenomenon. "Even a fundamental physical theory (on this view) could be *true* without what it is true of appearing among its predicates; for the things that *are* physically real could be too gross in nature (baseball bats, laboratory apparatus) to be even *definable* in terms of that theory's predicates." (Azzouni (1997b) p 203)

I will not pursue my concerns about reductive physicalism here. Even if we jettison Quine's physicalism, we can formulate QI.1.

Quine's homogeny rests on his insistence that there is a single way of knowing anything, that all evidence is sensory evidence. Call this his demand for a uniform epistemology.[67] Quine's main positive argument for uniform epistemology comes from the web of belief metaphor. He cashes out this metaphor as a thesis about meaning, and the distribution of content. The resulting semantic holism is stronger, and more contentious than he needs for the indispensability thesis, which depends merely on confirmation holism. I raise concerns about confirmation holism in the next section.

Another part of Quine's argument for his uniform epistemology involves an account of the origins of the referential terms of our language, an account

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[67] One can see how QI relies on Quine's uniform epistemology by noting that if we drop the requirement for uniformity, then mathematics may be justified independently of science, and the indispensability argument may become superfluous.

which Jaegwon Kim has criticized for lacking normativity.[68] In the remainder of this section, I show that Kim is wrong that Quine's epistemology lacks a normative element. But, Quine supports homogeny with a descriptive account which is faulty in the way Kim argues.

There are two sorts of stories which might serve as epistemology, for Quine. First, there is a purely descriptive account of how we acquire our referential vocabulary. In *Roots of Reference*, Quine traces how we learn referential terms of natural language, including mathematical terms, from observation sentences, through the distinction of singular terms from general terms, to the positing of abstracta and adoption of idioms like 'there are'. "Our general objective was a better understanding of how scientific theory can be achieved." (Quine (1974) p 81)

As a naturalist science project, Quine's account is objectionable only on scientific grounds. But Quine has another goal. He relies on these genetic musings, his descriptive epistemology, to support his uniform empiricist, broadly behaviorist, epistemology. He traces the roots of reference to show that empirical evidence suffices to include mathematical terms in our best theory.

Quine might be right about the origins of our referential terms, but this is no argument for the way in which we justify our knowledge of their referents. The origins of our beliefs are independent of their justifications. I may learn that 7+5=12 by counting apples and chairs, but my knowledge that 7+5=12 transcends experience with simple physical collections. As Kim argues, justification is normative, not descriptive.

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[68] See Kim (1988).

"For epistemology to go out of the business of justification is for it to go out of business." (Kim (1988) p 43)

If one were attempting to formulate reductions of the objects of knowledge to sense-data, say, then a genetic account would be most illuminating. If one starts, as Quine does, with objects, then one needs merely a good theory to account for them, however we learn the language in which our knowledge is couched.

The divergence of origins and justification is especially important for Quine, who believes that abstracta are causally isolated from human sensory organs. For, whatever justificatory story we tell about our knowledge of mathematical objects, it will have to be independent of the experiential account of our acquisition of language.

Quine does provide a justificatory story about mathematics, of course. The indispensability argument, in fact the whole project of constructing and interpreting our best theory, is Quine's second type of epistemological story. This project is normative: we should believe in all, and only, the objects in the domain of our best theory.

Kim's claim that Quine has neglected the normative aspect of epistemology is wrong. Still, Quine does lean on the account of the origins of our beliefs in defending a uniform epistemology which leads directly to homogeny. Quine favors first-order logic in part because it unifies the referential apparatus we have acquired. That part of the account does fail to be normative. Consequently, the uniform epistemology which underlies the indispensability argument is ill-motivated.

In the next section, I continue to argue against homogeny by providing considerations against Quine's confirmation holism.

§1.3: Confirmation Holism and Disciplinary Boundaries

Homogeny consists of two claims. The first is Quine's confirmation holism, that we can construct a single best theory and any evidence we have for any part of that theory is actually evidence for all parts of the theory. The second is that our ontic commitments are found by examining the posits of that best theory. In the previous section, I argued that the uniform epistemology which underlies Quine's homogeny is wrongly motivated in part by a confusion between the origins of our beliefs and their justifications. In this section, I attempt to undermine holism.

Quine argues for holism, his allegation that our beliefs face the tribunal of experience only when taken together, from a quick, uncontroversial logical point. Any sentence can be held without contradiction and come what may as long as consequent adjustments are made to the background theory. I argue, following Elliot Sober, that Quine's holism ignores the important differences between posits of mathematical objects and posits of empirical objects.[69] In practice, we shield mathematics from empirical refutation, even if the logical point is correct.

Holism is suspect precisely when it comes to mathematics. Prima facie, no empirical evidence would force us to give up our beliefs about the numbers, even though we could, with but a little effort, imagine evidence that would force us to give up beliefs in the existence of electrons, or other subvisible particles.

Quine argues that this commonsense distinction between mathematics and empirical science is illusory because all objects are posits, including ordinary ones.

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[69] Sober calls the doctrine epistemological holism, but it is the same holism.

"Physical objects, small and large, are not the only posits. Forces are another example; and indeed we are told nowadays that the boundary between energy and matter is obsolete. Moreover, the abstract entities which are the substance of mathematics - ultimately classes and classes of classes and so on up - are another posit *in the same spirit*." (Quine (1951) p 45, emphasis added)

In lieu of the commonsense distinction, Quine presents a continuum of commitments from our most firm and central, to our most tenuous and peripheral. We are unlikely to give up our beliefs in ordinary empirical objects either, but any posit may be questioned. Starting with ordinary objects, as Quine does, is no guarantee of ending with them. If on a scientific basis, say, Berkeleyan idealism turned out to be a better theory (e.g. more useful, simpler) then we should in fact abandon beliefs in physical objects. We can cede any belief, including our basic mathematical ones.

On the other hand, if no empirical, scientific reasons would sway us to abandon our mathematical beliefs, then we seem to have the basis for a distinction which will undermine holism. Alan Musgrave argues that we would not give up our mathematical beliefs because they exist necessarily. "If natural numbers do exist, they exist of necessity, in all possible worlds. If so, no empirical evidence concerning the nature of the actual world can tell against them. If so, no empirical evidence can tell in favour of them either." (Musgrave (1986) p 91)

Appeals to necessity, and possible worlds, are notoriously tendentious, though Musgrave's use of modality is not particularly problematic. Musgrave's necessity is essentially the one which underlies Field's claim of conservativeness for mathematics.

Mathematical theories are supposed to be compatible with any empirical theory.

By itself, Musgrave's contention is insufficient. For, Quine has a familiar response which refers to the centrality of beliefs we never cede. Like logical principles, mathematical beliefs are interconnected with our other beliefs in such an integral way that abandoning them would always force impractical redistributions of truth values among the remaining components. As a practical matter, we never give them up, even though we could, in principle. The appearance of necessity remains a decision, because we can always choose to give up something other than the mathematical elements of our theory. "If asked why he spares mathematics [in revising his theory in the face of recalcitrant experience] the scientist will perhaps say that its laws are necessarily true; but I think we have here an explanation, rather, of mathematical necessity itself. It resides in our unstated policy of shielding mathematics by exercising our freedom to reject other beliefs instead." (Quine (1992) p 15)[70]

Sober calls holism into question with a different explanation of why we never cede mathematical beliefs on the basis of empirical experiment. We subject mathematical claims to completely different kinds of tests, and do not hold them open to refutation on the basis of empirical evidence.

Sober calls the problems which confront science discrimination problems. We evaluate a scientific hypothesis against other hypotheses. We are only able to do this

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[70] Resnik, supporting Quine, argues that the holist can account for the apparent apriority of mathematics pragmatically, too. "'Good sense', in the form of pragmatic rationality, underwrites the special role mathematics has come to play in science and bids us to treat it *as if* it were known a priori." (Resnik (1997) p 120)

when other hypotheses are available. Sober calls this description of scientific methodology contrastive empiricism. Experiments solve discrimination problems among competing hypotheses by providing evidence in favor of one or another. For example, Sober considers these three competing hypotheses:

(Y_1) Space-time is curved.
(Y_2) Space-time is flat.
(Y_3) Space time is not curved, although all evidence will make it appear that it is.

Empirical evidence will discriminate between Y_1 and Y_2, but no evidence will discriminate between Y_1 and Y_3. Similarly, no discrimination problem can help us to confirm the truth of mathematical statements, or the existence of mathematical objects. "If the mathematical statements *M* are part of every competing hypothesis, then, no matter which hypothesis comes out best in the light of the observations, *M* will be part of that best hypothesis. *M* is not tested by this exercise, but is simply a background assumption common to the hypotheses under test." (Sober (1993) p 45)

Sober thus shows that Quine's allegation that it is always in principle possible to cede any beliefs in light of recalcitrant experience, is in fact contradicted by the ways we test our hypotheses. Our tests ensure that mathematical beliefs are never called into question. Sober provides examples of everyday failures of additivity: two gallons of salt and two gallons of water do not yield four gallons of salt water; two foxes and two chickens yield only two fat foxes and a pile of feathers. If Quine's holism were right, there should in principle be an option of giving up our mathematical beliefs in such cases. If all examples where we would cede mathematical beliefs are unavoidably abstruse, Quine's doctrine appears suspect.[71]

Quine notes that his holism is not a practical matter. Regarding "Two Dogmas of Empiricism," he writes, "All we really need in the way of holism... is to appreciate that empirical content is shared by the statements of science in clusters and cannot for the most part be sorted out among them. Practically the relevant cluster is indeed never the whole of science; there is a grading off..." (Quine (1980a) p viii) While Quine refers to the stronger semantic holism, his point is that there is a factual element in the content of any sentence, and thus an ineliminable component open to confirmation or refutation in every sentence, including those of mathematics. Quine is ceding that these elements may be undetectably subtle. Sober's contention is that they are not there.

Sober's argument relies on practical differences in testing. Resnik denies that differences in practice refute holism. "Sober is right that in practice we rarely, if ever, put mathematical laws to the sorts of specific tests that we apply to some scientific hypotheses. But this does not imply that purely logical considerations show that mathematics is immune to such testing." (Resnik (1997) p 124)

Sober need not establish a difference between mathematical and empirical posits on a logical basis in order to establish the distinction. Quine's naturalism is a commitment to the methods of science. Scientific methodology holds mathematical principles immune from revision.

Azzouni also recognizes a difference in kinds of posits despite accepting Quine's

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[71] Sober is agnostic about whether empirical evidence can ever influence our beliefs about number theory. See Sober (1993) pp 36-37, fn 5.

point about the logic of confirmation. "[D]espite the fact that every posit is treated in the same way, logically speaking, by quantifiers in a theory, nevertheless, mathematical posits get into scientific theories *the wrong way*." (Azzouni (1997a) p 481)

If Sober's criticism of holism is correct, then different elements of our best theory are separate and isolatable. Resnik challenges Sober to provide non-arbitrary lines between logic, science, and mathematics, admitting that holism would be refuted if one could establish, "[A]n epistemically principled division between the empirical and formal sciences. But I do not see much hope of success here." (Resnik (1997) p 135)

One way to distinguish among logic, mathematics, and science is by the ontology they require. Physical science likely entails no more than denumerably infinitely many objects, while mathematics demands more. Also, the types of space with which mathematicians are concerned exceed even the most tutored intuitions. The oddity of a space does not count against the existence of a topological surface. Odd physical spaces, such as regions near large dense masses, require significant upheaval of physical theory.

Putnam suggests that we can distinguish mathematics from science by the fact that scientific theories have viable competitors, whereas mathematical theories lack them.[72] This methodological difference echoes Sober's point.

There are two independent points here. The first is that we can make a principled distinction between mathematical objects and empirical objects, and between

[72] Putnam (1967b). Putnam argues that mathematics and science require the same epistemology, but this depends on his argument that mathematics is quasi-empirical. In the absence of such a successful argument (see below, Chapter 4, especially §1.5 - §1.6) the distinguishing characteristic stands.

mathematics and empirical science, which Resnik concedes would refute holism. I return to the general problem of disciplinary and ontic boundaries in §3.2, §3.3, and §3.6 of this chapter. I return to Resnik's attempts to blur these boundaries in Chapter 3, §6.

The second point is Azzouni's claim that mathematical objects get into our empirical theories in a different way than empirical objects do. There is a difference between positing an element into an already-existing framework, as we do with electrons, and positing an entire abstract realm.[73] Consider how mathematical objects get added to a theory on the holist's picture. We do not start with them, as we do with trees. We do not set out to describe the behavior of mathematical systems. We construct a theory without reference to mathematical objects until we find that our theory requires them for the account of other phenomena. Then, we add mathematical axioms only as far as the theory requires them.

The case is different with subvisible particles. If we add axioms which commit to new particles, like those for quantum physics, for example, we do so on the basis of the inadequacy of prior axioms to account for the empirical phenomena. When we introduce electrons, we include a story about the physical relation between the electrons and the bodies which actually concern us. Trees are made of subatomic particles, they are not made of sets. When we adopt mathematical entities, there is no effect which they are postulated to cause. We never construct experiments to observe them, or seek, in

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[73] The claim that mathematical posits are different kinds of posits from posits of subvisible objects is one that both realists and nominalists can agree upon. See, for example, Cornwell (1992), which argues for nominalism, against indispensability, on the basis of a counterfactual interpretation of the mathematical posits of a theory.

Azzouni's terms, thick epistemic access to them.[74] We are just forced to quantify over mathematical objects by the desire for greater facility in manipulating descriptions of physical situations.

Quine claims that all posits are made in the same way, but theoretical posits receive justification from within the domain that posits that object. An indispensability claim bridges two independent disciplines, presuming disciplinary blur.

Parsons raises another objection to Quine's assimilation of theoretical posits and indispensability claims. High-level theoretical posits tend to be made tentatively. Propositions involving such posits are speculative, and hotly debated, in contrast to the obviousness of mathematics. In mathematics, we have, "The existence of very general principles that are universally regarded as obvious, where on [a Quinean] empiricist view one would expect them to be bold hypotheses, about which a prudent scientist would maintain reserve, keeping in mind that experience might not bear them out..." (Parsons (1980) p 152)

Within empirical science, Quine's confirmation holism may hold. Justification may well be spread throughout empirical theory and the web of belief, restricted to our empirical beliefs, may remain a useful metaphor. The extension of this picture to mathematical objects is unjustified.[75]

Quine insists that all philosophical questions are to be answered using scientific

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[74] See Azzouni (2004) p 383.

[75] Benacerraf (1973) assimilates the posits of numbers and electrons based on grammatical roles. But even if we grant that a uniform semantics shows that both numbers and electrons exist, they may be justified differently.

methods. To hold that science must test mathematical statements as it tests empirical ones is to favor a methodology based not on science but on prior philosophical prejudice. Mathematical theories are tested differently from empirical ones, undermining homogeny.

The first two steps of QI involve settling on a single physical theory in which to express all of our commitments. In the past two sections, I have argued that our commitments are not all made in the same way by the same theory. Still, even if we grant QI.1 and QI.2, QI depends on Quine's general procedure for determining ontic commitments, which I deny in the next four sections.

§1.4: Names

In this section and the following, I argue that Quine's preference for first-order logic over other formal languages is unjustified. First, I show that Quine's argument that the first-order existential quantifier is the best tool for indicating existence since it is a natural cognate of 'there is' is undermined by his elimination of names.

Against looking to names to find the commitments of a theory, Quine points to four problems. Some names do not refer. We often find reference in terms which do not look like names on the surface, in pronouns for example. There are not enough names. And, there is a profound conflict between names and quantifiers.

The problem of non-referring names concerns 'Pegasus' and nouns like 'sake' which look, grammatically, as if they refer. That there are such terms does not decide the matter in favor of quantifiers, though, since we can easily form an existentially quantified statement which also seems to commit us to the existence of Pegasus. Both names and quantifiers may be used to reflect real and errant commitments. We can be clear about when we intend to use an empty name. Similarly, the problem of diffusion of reference seems less like an argument for eliminating names, and more an argument to be careful when approaching questions of reference.

To the problem of not having enough names for all objects, we may respond by adopting an infinite language with enough names, or by dropping the requirement that every object have a name.

Given that we must choose between names and quantifiers, Quine favors eliminating names. But, eliminating names makes Quine's formal language less natural, since names are naturally taken as indicating reference. Quine's worries about names are avoidable without eliminating them. Quine appropriately requires that theories be rigorously constructed if used to express commitments. We must beware of the sloppiness of ordinary language. This is merely counsel to be careful with whichever language we choose. We have to ensure that the language expresses the real commitments of the theory. If we are clear about our commitments, the choice to include names or not is arbitrary. Independently of Quine's preference for first-order logic, it is hard to see why names should not refer.

Quine's argument for taking the existential quantifier as indicating commitment was based on its natural equivalence with 'there is', but Quine uses this criterion to suit his independent purposes, and not as a principled guide. Languages with names are more natural in that they are more perspicuous. Using names facilitates inference. The naturalness of using the existential quantifier for 'there is' is counterbalanced by the ease of using a perspicuous language with names.

We can adopt, for the purpose of revealing the ontic commitments of a theory, a language less formal than first-order logic, a language with names and no quantifiers. We may use a cleaned-up version of our ordinary language. In the next section, I argue that if we must use a formal language, ones other than Quine's may also be useful.

§1.5: Higher-Order Logics and the Existential Quantifier

Just as he rejects names unjustifiably, Quine rejects higher-order logics for insufficient reasons. Quine presents three concerns about higher-order logics. First, he worries that they make too many commitments. Second, Quine points to a constellation of technical results: the concurrence of a variety of definitions of logical truth, the elimination of names, completeness, that every consistent first-order theory has a model, compactness, and its admission of both upward and downward Löwenheim-Skolem features. Third, he argues that first-order logic avoids referential vagueness. I argue that these concerns are insufficient grounds for choosing first-order logic as the exclusive language for revealing ontic commitments.

The quantifiers of any first-order or higher-order logic have two distinct roles: a purely formal and syntactic inferential role, and a translational role. In their inferential role, they bind variables. They may be used or removed in deductions.

The translational role of the quantifier involves the uses we might make of the formal theory and the meaning we give to the quantifier. It is common to take, with

Quine, the existential quantifiers as indicating existence. But we need not do so. We can interpret them as substitutional quantifiers, focusing only on their inferential role.

Azzouni suggests separating the two roles. First-order and higher-order logics force the existential quantifier into an independent role, indicating existence, for which it is not fit. "Even if one accepts the idea that scientific theories must be regimented in first-order languages, nothing requires the first-order existential quantifier...to carry the burden of *ontological commitment*." (Azzouni (1998) p 3)

Abandoning the translational role of the quantifiers, and first-order logic as the language of commitment, leaves the inferential role of the quantifier alone. The same holds for higher-order logics, which Quine rejected for their ontic extravagance. In fact, quantification over properties in higher-order logics may be a virtue. It provides all the predicates we might need for science or mathematics. We need not take quantifications of variables attached to those predicates as indicating commitment.

The technical virtues of first-order logic do not decide the matter, either. For example, the completeness which Quine thinks favors first-order logic only means that every valid formula is derivable. It does not mean that every intuitively valid inference is representable in first-order logic. There are intuitively valid formulas and inferences which are not valid in first-order logic. For example, first-order logic with identity can not comfortably accommodate inferences to common properties of two individuals, Frege's definitions of numbers, and Leibniz's identity of indiscernibles. The completeness of first-order logic is a technical virtue which can make first-order logic useful. Higher-order logics which may accommodate such inferences may also be useful.

Consider the claim EM1: $(\forall x)(Px \vee {\sim}Px)$. A similar sentence can be written substituting any predicate for 'P'. This is a higher-order fact, which we can summarize as EM2: $(\forall \wp)(\forall x)(\wp x \vee {\sim}\wp x)$. Quine rejects EM2, though he accepts every instance of it along the lines of EM1. He prefers to take the 'P' in EM1 as a schematic letter. But EM2 provides a uniform representation of the underlying fact which is not present in any sentence of the form EM1.

Quine complains that logics other than first-order logic may be vague. If we focus on a language which is not Quine's canonical notation, but with the same goal, to explicate existence, there is no reason why that language need be vague. In all cases, we must be clear, antecedently, about our commitments.

Quine's objections to names and higher-order logics arise from his desire to formulate a single canonical language in which to represent all commitments. If we abandon that method, we may welcome names. We may regiment into first-order logic to clarify our meanings or to reveal deductive relations, or we may use other formal languages when they suit our purposes.

I have argued against QP.2, Quine's insistence that we look to first-order logic to find our commitments. In the next two sections, I show that the way in which Quine reads commitments from first-order logic is misleading.

§1.6: Wilson's Double Standard

If we ignore the criticisms of recent sections and adopt, with Quine, QP.1 and QP.2, we have two ways in which we might proceed to determine the commitments of a regimented theory. We can look at its theorems or we can look to the range of its bound variables. QP.3 indicates that Quine takes the latter route. In this section, I present Mark Wilson's argument that Quine's choice misreads theoretic commitments. The arguments of this section and the next, which also concern Quine's way of reading commitments from regimented theories, apply only to Quine's idiosyncratic approach, which is separable from his more central claims, QP.1 and QP.2.

Wilson demonstrates a difficulty which arises from the translational role of the quantifier. Theories with different existential claims may not differ in ontology. Consider two theories which agree in all elements except that one claims that there is a beagle in Baltimore, and the other denies this claim, putting that beagle in Philadelphia. The two theories have existential claims with different contents, but quantify over the same objects.

In choosing among theories with conflicting existential claims, we first determine if one makes false assertions. The beagle may not be in Baltimore. Second, there may be an equivocation such that the two theories really say the same things. In this case, we will have to rely on some interdefinability to determine whether two theories have the same commitments. Once we generate the appropriate translation, the choice between the two theories is arbitrary. Lastly, we might have to admit that the ontologies of the two theories really differ.

Wilson argues that claims of the former two types, which he calls type A), override claims of the last type, which he calls type B). "[I]f a plausible claim of type A) can be found, it will always *overrule* conclusions reached by B)." (Wilson (1981) p 413)

Wilson's point is that the ontology of a theory is best found in the intended interpretation of its theorems. When we apply this lesson in science, we find that QP miscounts physical entities as mathematical. Consider items such as vectors in Hilbert space, which are used in quantum mechanics. These objects of applied mathematics do double-duty, once as mathematical in the mathematical theory used by the physical theory, and once as physical, when applied to the physical theory. When applied in the physical theory, the intended interpretation is physical, and not mathematical. Wilson correctly points out that QP counts objects only once, even if they are the subjects of purely mathematical statements, and also the subjects of applied mathematical statements. He calls this single-counting of what should be distinguished and thus counted twice univocalism, and it is a short step from Quine's homogeny. "Univocalism's stress upon amalgamation of one's 'total theoretical corpus' tends to make one miscount entities; instead of separate physical and mathematical entities, it only sees the latter. This moral I call 'the double standard in ontology'." (Wilson (1981) p 417)

Wilson's argument indicates a rift between the ontology of a formal theory, determined by the ranges of the bound variables, and the ontic commitments of the theory, determined by the theorems. He argues that we determine the commitments of a theory by looking at theorems and their intended interpretations, not at the range of variables. "Our *prior* knowledge that two theories, e.g. ZF and [ZF + the axiom of measurability], share the same 'intended structure' may overrule the 'evidence' that they differ ontically displayed in their differing existential theorems." (Wilson (1981) p 412)

Wilson argues that consideration of the double standard supports a structuralist criterion for ontic commitment.[76] Shapiro presents such a criterion, in opposition to QP. On the Quinean method, ordinary set theory and set theory with numbers as ur-elements have different ontologies. "I take this as nearly a reductio against the Quinean criterion." (Shapiro (1993) p 477) On the structuralist criterion, their ontologies are the same

Wilson encourages us, pace Quine and QP.3, to look for our commitments in the theorems of a theory and their intended interpretation. Quine looks instead to the range of the variables. In the next section, I argue that this is an error.

§1.7: Appealing to the Metalanguage

While QP.3 is excisable from QP, and inessential to QI, it is not a minor element of Quine's work. It arises directly from Quine's slogan that to be is to be the value of a variable.[77] It also leads directly to Quine's doctrine of ontological relativity. In this section, I argue that Quine is wrong to push us to the values of variables in lieu of the intended interpretations of the theorems.

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[76] Wilson argues that the mathematician's grasp of this difference leads to structuralism. The theory which claims that numbers are von Neumann sets has different existential claims from that which claims that the numbers are Zermelo sets. Benacerraf argued that the option of choosing one reduction over the other is closed. To avoid choosing, the structuralist argues that the two reductions are in some sense equivalent. The mathematician, whose interest stops at the theorems, chooses structuralism because he thinks that the differences between the von Neumann and Zermelo reductions are not worth debating. I return to the indispensabilism inherent in some forms of structuralism in Chapter 3.

[77] See Quine (1939a), Quine (1939b), and Quine (1948)

Quine defended taking first-order logic as canonical because while it is refined and precise, it is also easily interpreted. We can readily see the quantifiers as close relatives of the ordinary 'there is'. But, Quine denies that we should interpret ordinary language at surface value. For example, Quine argues that languages in which quantifiers may be translated away, or which do not contain quantifiers, are unable to generate an ontology. A finite theory which contains names may eliminate quantifiers in favor of truth-functional connectives. This type of theory, Quine claims, will leave no ontic footprint.

> Ontology thus is emphatically meaningless for a finite theory of named objects, considered in and of itself... What the objects of the finite theory are, makes sense only as a statement of the background theory in its own referential idiom. The answer to the question depends on the background theory, the finite foreground theory, and, of course, the particular manner in which we choose to translate or embed the one in the other. (Quine (1968) p 63)

The problem Quine is raising here applies to all languages. A theory can not prescribe its own interpretation. We can not know the references of the names of a language with no quantifiers. In a first-order theory, one must look to a domain of quantification to find values of its variables. Domains of quantification are located within a model for the theory. Theories do not determine their own models, which are written in a metalanguage. Thus, Quine's dictum forces us to construct a model in a metalanguage in order to discover the commitments of a theory.

Appeal to a metalanguage generates an infinite regress of formalism. If we want to know what the names in the metalanguage refer to, we have to construct a model for the metalanguage, and so on. But if we want to know what the commitments of a theory are, we have to stop somewhere. Quine's resolution of this matter is ontological relativity, that we have no absolute answers to ontic questions. "What makes sense is to say not what the objects of a theory are, absolutely speaking, but how one theory of objects is interpretable or re-interpretable in another," (Quine (1968) p 50)

Ontological relativity means that QI is even weaker than it seems, since it yields no commitments to abstract objects. It only yields a theory which may be interpreted as making such commitments. The theory will have other interpretations, given that it will be strong enough to ensure the impossibility of generating a unique intended model.

To avoid ontological relativity, we could take the metalanguage of our first-order theory at homophonic face value. But this pragmatic response will not resolve the problem, as Michael Devitt notes. "We do not need to move into a metalanguage discussion of our object-language claims to establish ontic commitment. Indeed, if commitment could never be established at the level of the object language, it could never be established at all." (Devitt (1984) p 50)

We have lots of ways to express commitments. We can also make statements whose commitments we deny. We can clarify matters at the level of the object language when pressed. We need not be pushed into a metalanguage. Further, Quine's argument that we should take the existential quantifier as indicating existence because of its proximity to the ordinary language 'there is' is undermined by the claim that we can not interpret ordinary languages as making ontological commitments.

In §1.2 and §1.3, I argued that the homogeny of QI.1 and QI.2 is both poorly motivated and false, and I mentioned some concerns about the physicalism of QI.1. §1.4

and §1.5 concerned problems with QP.2. §1.6 and §1.7 concerned problems with QP.3's requirement that we examine metalanguages. While QP.3 is easily excised, these worries about the individual steps in QP should make us extremely wary of any argument, like QI, which depends on it.

The major flaw in QP is its reliance on formal theory to reveal our commitments. In the next three sections, I put aside the specific worries about the steps of QP, and present general concerns about this methodology and its application, at QI.3, to the indispensability argument.

§1.8: The Regimentation of Ontic Prejudice

One reason to favor QP is because the clarity of regimented language can help reveal the presuppositions of a theory. This clarity can help us avoid making errant claims. We can regiment scientific theory without consideration of its commitments. We focus on generating a simple and elegant axiomatization. Then, we look to the regimented theory to reveal its existence claims, which are byproducts of a neutral process.

The Quinean picture I just described is misleading, and in this section, I show how. When we regiment, with Quine, to clarify the commitments of a theory, we permit existential generalization only where we desire that the theory express commitment. A nominalist with respect to any kind of entity will cast his theory in a way which avoids commitments which a realist will make. Quine recognizes this. "The resort to canonical notation as an aid to clarifying ontic commitments is of limited polemical power... But it does help us who are agreeable to the canonical forms to judge what we care to consider there to be. We can face the question squarely as a question what to admit to the universe of values of our variables of quantification." (Quine (1960a) p 243)

For example, consider Quine's rejection of propositional attitudes as "creatures of darkness." (Quine (1956) p 188) We do not construct a semantic theory, and then notice whether it quantifies over propositional attitudes. We consider the world, and our minds, and make that decision.

The picture which I called misleading is closely related to the idea that formal theories are uninterpreted, or disinterpreted. Quine (1978a) argues against the disinterpretive stance, which was held by formalists who tried to eschew metaphysical controversy by emphasizing the syntactic properties of mathematical theories. Quine rightly saw that mathematical theories are useless if taken as disinterpreted. They are about mathematical objects, and we can not pretend otherwise.

This is a fairly obvious point: translating ordinary language into regimented form can aid clarity, but the regimented language is not magically protected from errant commitments. Determining our commitments is a task prior to regimentation. We can regiment the existence of unicorns as easily as that of horses.

QI makes exactly the mistake against which I am cautioning. Quine's indispensability argument alleges that we must admit mathematical objects into our ontology since they are required for the regimentation of formal science. Quine's implication that we are forced to quantify over mathematical objects is misleading. We have already added mathematical theorems to our best theory prior to regimentation. We do not merely examine the domain of quantification of the regimented theory and discover them there. Quine violates his own strictures against disinterpretation, by emphasizing the needs of regimentation over the content of the theory. "Structure is what matters to a theory, and not the choice of its objects." (Quine (1981b) p 20)

We must disconnect theory, and its structure, from ontology. Formal theory is inappropriate for revealing commitments just because disinterpretation is not possible. We construct formal theory knowing the references of the terms of the theory, in order to get it to say what we want it to say. Our ontology is a constraint on regimentation, not a result of it.

§1.9: Incompleteness, and the Limits of Formal Theories

In the previous section, I argued generally against using regimented theories to discover our commitments. In this section, I discuss specific problems with the application of QP to the indispensability argument. I argue that the incompleteness of any formal theory sufficiently strong to encapsulate scientific theory makes that theory insufficient for revealing ontic commitments. It will omit relevant information. I first sketch a bit of the history of formal theories which led to Quine's adoption of QP. Quine's linking of regimentation and ontic commitment was an innovative move, independent from the motivations of those who initially developed those systems.

A regimented scientific theory will consist of a set of axioms within a deductive apparatus which guides inference syntactically. Regimentation makes as much as possible of a field of inquiry (e.g. logic, mathematics, or physics) syntactic. The earliest formalisms, like Euclid's axioms for geometry, were more casual about their language and deductive apparatus than are present-day formal systems. Using a formal system, one can assure the validity of an inference, and avoid worries about being misled by its semantic properties, by examining only its structure.

Aristotle's syllogisms are the prototype for separating syntactic questions from semantic ones, but he was concerned with clarity, not ontic commitments. The work on formal theories with the most historical relevance to QP started in the nineteenth century, when several problems in mathematics impelled mathematicians to seek greater clarity and foundations for their work. In geometry, the questions which had been percolating about Euclid's parallel postulate reached a head around mid-century with the work of Lobachevsky and Riemann.[78] Cantor's controversial work in set theory soon followed. While Cantor looked to foundations to defend the rigor of his work with transfinites, his set theory itself, which entailed the Burali-Forti paradox and relied on the faulty axiom of comprehension, impelled increased precision. Worries about foundational questions in mathematics had reached a tipping point, and formal systems came to be seen as essential within mathematics proper. For about fifty years, from, say, Frege's *Begriffsschrift* (1879) to Gödel's Incompleteness Theorems (1931), formal systems were explored with

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[78] Euclid's parallel postulate states that if a line intersects two other lines and makes the interior angles on the same side less than two right angles, then the two lines meet on that side. The parallel postulate is equivalent to Playfair's Postulate, which states that given a line and a point not on that line, exactly one line can be drawn through the given point parallel to the given line. There are two ways to deny Playfair's postulate, or the parallel postulate, both of which are consistent with the other axioms of geometry. If one can draw no parallel lines, the geometry defines the surface of a sphere. If one can draw more than one parallel line, one defines a surface called a hyperbolic spheroid, or a pseudo-sphere.

the hope that foundational questions in mathematics could be answered.

Mathematicians were encouraged by the clarity of formal theories in mathematics, especially Peano's postulates for arithmetic (1889) and Hilbert's subsequent axiomatization of geometry (1902), if not their fruitfulness. The key work in non-Euclidean geometry was done prior to axiomatization. Similarly, set theory was not axiomatized until 1908, when Zermelo presented the first rigorous system, after Cantor's success with transfinites. (Dedekind had published a fragmentary development in 1888.)

Of course, there were existence questions on the minds of those who developed these formal systems, questions about the existence and plenitude of transfinites, for example. But the main worry was antinomy. Despite resistance due perhaps to worries about specific formulations of set theory, Cantor's achievements were compelling. Hilbert, for example, refused exile from Cantor's paradise, despite profound concerns to establish finitistic foundations for mathematics.

Though mathematical proofs had long existed, once the formal theories of the late nineteenth and early twentieth centuries were developed, the notion of proof became grounded. A proof in any discipline is a sequence of statements each of which is either an axiom, or follows from axioms using prescribed rules of inference. Other notions of proof were either reducible to this kind of proof, or dismissed as unacceptably informal.[79]

The main philosophical goal of formalizing mathematics was to explicate

[79] As an example of the latter, consider Wittgenstein's picture of commutativity (Wittgenstein (1991) p 233). The rotation of a grid of inscribed dots does not strike one as a proof, since it does not conform to this formal notion. See Brown (1999) for a defense of picture proofs in mathematics.

mathematical truth in terms of provability: Mathematical theorems are true just in case they are provable in a formal system with accepted axioms. In one direction, deriving truth from provability would ground mathematics with assurance that theorems are derived from accepted postulates. We would know that our theorems are clean. In the other direction, the equation would delimit clear boundaries on the possible theorems of mathematics.

Frege, hoping to return to the "Old Euclidean standards of rigor," (Frege (1953) p 1) looked to formalize all deduction. Formal languages like Frege's were easily adaptable to include physical axioms. All of human knowledge, it could easily have been hoped, could be derived within a formal theory. Truth and provability could be aligned in all disciplines.

Russell's paradox for Frege's nascent set theory was the first sign of a problem for formalism. Frege did not abandon his formalist projects, though one might see the paradox as a reductio on the sufficiency of axiomatizations of set theory to capture our notion of set. After Gödel's incompleteness theorems, hopes for identifying truth with provability for sufficiently complex formal theories were dashed. Mathematical truth turned out to be provably distinct from mathematical proof within a single formal system.

The divergence of mathematical truth and proof is a remarkable philosophical achievement, and it extends beyond mathematics. In any discipline whose formalization is sufficiently strong to be of interest, we must sever truth from proof within a single formal system. Any formal theory which might serve as our best scientific theory is strong enough to be shown incomplete. The commitments of scientific theory can not be found in a formal theory for the same kinds of reasons which applied to Field's incomplete reformulation.[80] In particular, the indispensability argument, which relies on the construction of formal scientific theories, is invalid.

One might think that the inference from Gödel's incompleteness theorems to the invalidity of QI is too quick. For, QI needs only the sufficiency of proof for truth, and Gödel only showed that formal theories omit commitments, not that they generate false ones. But further problems arise from relying on formal theories to reveal ontic commitments. Even a complete theory, like the first-order theory of the reals, may not be categorical. A theory is categorical if all its models are isomorphic. Failure of categoricity entails that there will be non-standard models.

Just as Gödel's theorem cleaves truth from provability in a single formal system, the Löwenheim-Skolem theorem shows that formal models of a sufficiently strong theory can be deviant and unintended and thus do not represent our true commitments. The availability of deviant models is commonly taken to demonstrate the indeterminacy of our commitments. This indeterminacy is merely a defect in the formal representation of our independently clear commitments. Once we release our hold on that dogma, indeterminacy becomes merely a defect in the formal representation of our commitments, which may be clear, independently. Regimentations will be useful only if we have a prior conception of what we want to say and of how to make the formal theory say it.

Even in mathematics proper, formal theories have limited appeal. Consider Paul Benacerraf's argument that we can not choose between various adequate set-theoretic

[80] For examples of omissions, see Burgess and Rosen (1998) §II.A.5.b.

reductions of the numbers. Katz responds that we do have tools to select determinately the objects which appropriately model our number-theoretic axioms. Calling numbers communal property, among different fields which share interests in their diverse properties, he argues that no formal system can capture all we know about numbers.

> It is in the nature of formalization and theory construction to select those properties of the objects that have a role in the structure chosen for study. Moreover, selectiveness is essential in the formal sciences because numbers and the other objects they study are not the private property of any one discipline... The mathematician's special interest in numbers is with their arithmetic structure; the philosopher's is with their ontology and epistemology. From the standpoint of the inherent selectiveness of formalization and theory construction, the assumption of Benacerraf's argument that we know nothing about the numbers except what is in number theory seems truly bizarre. (Katz (1998) p 111)

Typical axiomatizations of number theory provide no information about the abstractness of numbers, or how we come to know about them. We can formalize the notion of circle as the locus of all points equidistant from a given point, but unless the domain is strictly larger than the rationals, we do not even really get circles. Geometric axiomatizations provide no insight into the epistemology of points, or surfaces. Set theoretic axiomatizations give us no insight into the modality of sets.

The problems I have so far discussed may give the impression that the phenomenon at issue, difficulties in determining one's existence claims on the basis of regimented theories, is isolatable within the philosophy of mathematics. The problem is broader.

Skolemite puzzles about models arise within formal systems. We can generate such questions by appeal to indeterminacy of translation, but support for that doctrine seems strongest on appeal to a metaphor from the problems which arise within formal systems. Hillary Putnam (1980) argues for a broad anti-realism by appealing to problems constructing formal models of any theory. Saul Kripke's Plus/Quus example (Kripke (1982)) demonstrates difficulties for formalizing even clear and simple mathematical concepts. He, too, develops broader conclusions for our ability to know and follow rules in all areas. Without the problems from formal model theory, Kripke's puzzle is merely skeptical. In general, the problems of unintended models are either skeptical or arise from unjustifiably artificial limitations on our abilities to determine those models. We can not even successfully formalize '7+5=12'. We should not seek answers to general metaphysical questions this way.

Field remarks that, "To say that one accepts an informal inductive argument that cannot be formalized in one's theory is to say in effect that one accepts a stronger theory." (Field (1984) p 110) My claim is that whatever theory one accepts for locating one's commitments, it is not a formal theory at all. This leaves me in muddy waters which Quine's procedure was thought to have cleaned up. Metaphysics is dirty work.

Mathematicians and philosophers of mathematics came to rely on formal theories because they insured the healthy deductions that motivated Aristotle and Euclid and Frege. If we know that our axioms are true, a difficult task, then we can be sure that the theorems which follow from them will also be true. We may maintain formal theories for generating results within any field in which they might be useful. We also must acknowledge that the purposes of formal theoretic construction do not include answering all metaphysical questions. Formal theories are generally indifferent to philosophically interesting properties, like abstractness. And, "There is no mathematical substitute for philosophy." (Kripke (1976) p 416)

§1.10: Devitt and the Metaphysical Horse

In this section, I draw an analogy between my concerns about reliance on formal theories to reveal our commitments and Devitt's caution against allowing one's semantics to lead one's metaphysics. Our reliance on formal theories violates the spirit of Devitt's counsel.

The most objectionable way of emphasizing semantics over metaphysics is to make one's existential commitments subservient to a controversial semantic theory. This is Devitt's concern, and he argues that it puts the semantic cart before the metaphysical horse. A different way to emphasize semantics over metaphysics is to turn one's metaphysics into a semantic project. This is the error implicit in QP which forces us to construct a formal theory, and then model that theory, in order to discover our commitments.

Devitt presents several maxims which guide his metaphysical realism, including:

> Maxim 2: Distinguish the metaphysical (ontological) issue of realism from any semantic issue.
> Maxim 3: Settle the realism issue before any epistemic or semantic issue. (Devitt (1991) pp 3-4)

Devitt argues for the primacy of our metaphysical conclusions, since we are clearer and more confident about these. Semantics, on which we are much less clear, should conform to our established metaphysical picture. By making metaphysics into a project of theory construction, QP sullies metaphysics with considerations of formal theory building and modeling.

Devitt's worry about leading with one's semantic theory arises in the context of his defense of metaphysical realism about common-sense and scientific physical objects. The point is more broadly applicable. We always construct formal theory, in mathematics and in science in general, according to prior concerns about ontic commitment, and according to concerns about the form and structure of the theory. The latter concerns may or may not have ontic import.

Devitt's maxims themselves leave open the question of how one is supposed to determine one's commitments. There is no simple answer to this question. We look to scientific theory, and to common sense. We try to resolve tensions among various intuitions and balance formal science with casual observation. We recognize that the construction of scientific theory is ongoing, and that our currently best theory may be radically wrong or incomplete. Our currently best neuroscience and our currently best psychology radically under-explain mental phenomena. It would be silly to think that what exists is what these best theories say exists.

One might wonder why we should accept Devitt's maxims. Consider that there's an open question in the philosophy of mathematics about whether the debate over realism in mathematics should be taken as concerning the existence of objects or as concerning the objectivity of mathematical statements.[81] If the debate concerns objects, then it is clearly metaphysical. If the debate concerns the objectivity of mathematical claims, we

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[81] See Field (1998a) for the latter, and Field (1988) for the former.

focus on how to interpret the truth of mathematical claims, a semantic project. We may best account for objectivity by reference to a broader semantic theory. If we prioritize ontic issues over semantic ones, we resolve this debate by fiat. The objectivist position deflects our interpretation of mathematics to our theories of truth.[82] If we have to settle the ontological issues before the semantic ones, then the objects/objectivity question is a non-starter; the debate must be over whether there are mathematical objects.

There may be cases in which semantics has some role in determining ontic commitments. Some dispensabilists argue that reinterpreting mathematical existence sentences as referring to mental objects or inscriptions, or as sentences of modal logic which do not refer to mathematical objects avoids commitments to mathematical objects.[83] Those who favor reinterpreting mathematics say that the semantic is tied to the ontological, that realism is exactly a doctrine about truths of statements. For example, a structuralist who thought that his position helped solve the problems about access to abstract objects would be obviously wrong. Benacerraf (1965) seems to defend this position. If we extend Devitt's claim into an insistence that we settle all metaphysical claims before semantic ones, we can rule out these reinterpretations. Similarly, the best way to deal with posits like fields and space-time points may be via the semantics of

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[82] Devitt wonders, in communication, whether we can see the debate over objectivity as metaphysical, involving what he calls the independence claims of metaphysics, rather than existence claims. But those who favor objectivity over objects may do so precisely because of the benefits of assimilating mathematics into a broader semantic theory. They hope to solve the epistemic problems for mathematics by making the debate concern formal theory.

[83] For example, the modal dispensabilists I discussed in Chapter 1, §1.6.

sentences which refer to them rather than via the existence of them as objects.

Devitt holds Maxim 2 and Maxim 3 because he thinks we are clearer about metaphysics than we are about semantics. "The metaphysical issue of realism is the fundamental one in our theory of the largely impersonal world. Semantic issues arise only in our theory of people (and their like) in their relations to that world." (Devitt (1984) p 3) This may be true about the physical world, though if all of science is reducible to strings on Planckian scales, or some other fundamental particles, then I am committed to them, and I am not clear about them at all.

It is worthwhile to consider a broader explanation of why metaphysics should not generally follow semantics. This is where I think the analogy between Devitt's maxims and my argument against Quine's reliance on formal theory for revealing ontic commitments is particularly edifying. Formal methods for determining ontic commitment like QP distract our gaze from reality, where it belongs, to the linguistic shadows on the walls of the cave.

§1.11: Conclusions Opposing QI

In §1.2 - §1.3, I argued that Quine's homogeny, which arises from QI.1 and QI.2, is unfounded. It relies on his insistence on a uniform epistemology and confirmation holism. Holism misrepresents the actual workings of science and its relation to mathematics.

In §1.4 - §1.7, I presented concerns about Quine's procedure for determining the ontic commitments of a theory. He defends the simplicity of first-order logic, but it is hard to see exactly how first order logic is simple, beyond unifying reference. In some ways, natural language is much simpler. Even if one could establish that first-order logic were the simplest language available, the connection between simplicity and truth needs to be made. "It is hard enough to believe that the natural world is so nicely arranged that what is simplest, etc. by *our* lights is always the same as what is *true*...; why should one believe that the universe of sets... is so nicely arranged that there is a preestablished harmony between *our* feelings of simplicity, etc., and *truth*?" (Benacerraf and Putnam (1983) p 35)

Quine's choice of first-order language is insufficient to establish that this language is the only one in which we can express ontic commitment. Since there are other languages in which we can easily express ontic commitment, the quick inference to the existence of mathematical objects from the need to quantify over them in our best, first-order-regimented theory does not follow.

The considerations of §1.8 - §1.10 undermine Quine's application of QP to QI. QI can not work, for its metaphysical conclusion arises from the construction of formal theory. We are free to construct and interpret our formal theories as we wish. We can adopt semantic ascent for clarification without also turning to regimentation as the source of commitment. Ontology need not recapitulate philology.

Quine does notice that we regiment only when useful. "A maxim of shallow analysis prevails: expose no more logical structure than seems useful for the deduction or other inquiry at hand... [W]here it doesn't itch don't scratch." (Quine (1960a) p 160) But he uses this maxim as merely a practical guide. We eschew full regimentation only because we can envision what it would look like, and what its yield would be. If we have ontic questions, for Quine, we have to look at the fully formal framework.

Despite the independence of philosophical issues and formal systems, we do construct formal systems with an eye to our commitments. We investigate those things we believe to exist and we do not, generally, regiment fiction. Our commitments arise prior to regimentation, just as Devitt argued that metaphysics is prior to semantics. Reasoning within a formal system can, theoretically, affect our independent beliefs about what exists. It may turn out that mathematicians discover new theorems by working within a formal theory, though mathematical reasoning does not generally work this way. Regimentations are instead used as a check on fallible, informal reasoning. The benefits of mathematical regimentation may translate to the mathematized portions of science, but it is unlikely that writing science in a formal, canonical language would lead to any scientific advances.

Quine made metaphysics acceptable, in the aftermath of logical positivism, but at the cost of reducing it to a byproduct of formal theoretic construction. He resurrected the discipline, despite the problems with his methods. It is as if philosophy went back to its infancy with the positivist program, and had to start all over again. Rejecting QP brings us back to philosophy. We must return to the good old days of the worst kind of speculation.

In Chapter 1, I defended my interpretation of Quine's method against a charge that it misrepresents the way in which we make our commitments. The arguments of this section are in concord with that criticism. In the next two chapters, I examine indispensability arguments which do not presume the method I have ascribed to Quine. Perhaps, a critic of my interpretation may find in those chapters an argument closer to the one they think is Quine's.

To those familiar with Quine's method, the contention that we are not committed to our existential quantifications may seem striking. In the next part of this chapter, I defend an instrumentalist interpretation of those quantifications.

Part 2: Instrumentalism

§2.1: Introduction

I have argued that Quine's indispensability argument, QI, is unsuccessful, especially since it depends, at QI.3, on his procedure for determining ontic commitment, QP. In Part 1 of this chapter, I presented and rejected some reasons to hold QP. In this part of the chapter, I argue directly that a theory's commitments should not be found exclusively in its quantifications by presenting and alternative. We may adopt instrumentalism, on which quantification over objects does not entail commitment to their existence. I present a puzzle about the status of our beliefs in mathematical objects within a Quinean framework, and show that instrumentalism provides a satisfactory resolution of it. A principled instrumentalism can withstand its major Quinean criticism.

§2.2: A Puzzle About Mathematical Commitments

I have called Quine's allegation that our commitments all arise together as the posits of our best holistic theory homogeny. We do not, though, make all our posits with equal enthusiasm. For example, we are more committed to ordinary objects like trees than to, say, quarks, belief in which arose only tentatively from recent scientific theory.

The Quinean accounts for these differences in enthusiasm casually, and meta-theoretically. We know, on reflection, that our best theory is ever-changing. Some elements of the theory are central to the web of belief and so our commitments to them are less liable to shift; other elements are peripheral, more apt to be abandoned. But if the metaphysics police knock at our door and demand our list of commitments, we hand them one list, with a single column.

Homogeny and the web of belief metaphor which supports it lead to a puzzle about our beliefs in mathematical sentences. On the one hand, sentences which refer to mathematical objects are ubiquitous, and our beliefs in them are central. The notion that mathematical elements are core beliefs transcends Quine's position. Our commitments to broadly applied elements, like mathematical objects, should be most enthusiastic.

In contrast, the existence of mathematical objects is highly contested. Since the question of whether the dispensabilist succeeds in excising mathematics from empirical science, and thus our ontology, is open, our mathematical beliefs should be peripheral to our web of belief.

Rejecting homogeny could dissolve the puzzle of whether mathematical beliefs are central or peripheral, but it entails differentiating among the posits of our best theory, taking some of them as merely instrumental. I consider three grades of instrumentalism, rejecting the first, sensory instrumentalism, and accepting the second, mathematical instrumentalism. The third version, reflective instrumentalism, is also defensible.

§2.3: Sensory Instrumentalism and the Double-Talk Criticism

A crude way to reject homogeny is to limit one's commitments to those objects we can perceive. Call this sensory instrumentalism, or SI. Early proponents were Duhem, Vaihinger, and Mach, who denied the existence of atoms, at least for a while, despite affirming the utility of atomic theory.[84] More recently, van Fraassen's constructive empiricism denies that the utility of our scientific theories establishes knowledge about theoretical posits like atoms. Nancy Cartwright defends a skeptical view about the truth of sentences which posit subvisibles.

The sensory instrumentalist owes us an acceptable account of direct perception, but we can accept a rough distinction on the basis of paradigms. Dubious commitments are exemplified by electrons, and legitimate commitments are exemplified by trees. SI affirms the existence of trees, and denies, or remains agnostic about, the existence of electrons.

QP is a response to SI. The increasing importance of subvisible elements of scientific theory in the late nineteenth and early twentieth centuries had demanded a philosophical account, especially from empiricists, who had two options. They could withhold commitments to these elements, believing only in elements which can be directly sensed; this is SI. Or, they could account for our knowledge of objects which we can not directly perceive, justifying the use of microscopes, for example. The positivist sense-data reductionism of Carnap's *Aufbau* takes this latter approach by attempting to show how all talk of physical objects, including those observed using microscopes, was

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[84] See, for more detail, Burgess and Rosen (1997) p 61.

translatable into a sensory language. Quine favored reductionism, in principle, and QP is a response to the impossibility of a satisfactory reductionist account.

The Quinean argument against SI claims that affirming the existence of some elements of one's theory while denying others is double-talk. One can not arbitrarily commit only to certain elements of a theory which one accepts.[85] We can use a theory we do not believe for practical purposes, as we do when we calculate moderate velocities of medium-sized objects using classical mechanics, without falling into double-talk. But, we must appeal to a different theory for our real commitments.

There are various good reasons to reject SI, independent of the double-talk criticism. We learn of subvisible elements of scientific theory by using instruments as reliable as those of our senses, if not more so.[86] Atomic theory simplified ontology, since diverse physical objects could all be seen as constructed out of the same kinds of atoms. It unified our explanations of sensory experience. Put these criticisms of SI aside.

The real problem for SI's refusal to accept subvisible particles is that its fundamental distinction between accepted objects and rejected ones is capricious. The world may not be cut at human sensory joints. By relying on our arbitrary abilities to sense objects as the source of the distinction between real commitments and merely instrumental posits, the sensory instrumentalist differentiates arbitrarily. While SI may be motivated by empirical concerns about what we can discover, it ends up relativizing our commitments to our sensory apparatus. We want our commitments to be

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[85] See Chapter 1, §1.3 for a discussion of Quine's double-talk criticism.

[86] See Azzouni (1997) and Azzouni (2004) for a defense of this assertion.

independent of us.

This complaint suffices to reject SI, and demonstrates a lesson. A principled distinction between our real commitments and our instrumental ones can deflect the double-talk criticism.

§2.4: Mathematical Instrumentalism and the Eleatic Principle

Call the denial that mathematical objects exist, even if they appear as indispensable elements of our best theory mathematical instrumentalism, or MI. MI, if established, would reduce the importance of dispensabilist projects. Adopting MI entails rejecting QP, and thus QI.3, and QI.

In order to avoid the double-talk criticism which inflicted SI, MI must make a principled distinction between real and instrumental posits. MI is thus a family of views, one for each such distinction. The version I consider makes its distinction on the basis of our causal isolation from mathematical objects.

Balaguer calls the fact that we are unable to interact with mathematical objects the principle of causal isolation, or PCI. He uses PCI to reject the indispensability argument, in essence defending MI. "Even if our mathematical theories are indispensable to the descriptions and inferences of empirical science, this gives us no reason whatsoever to believe that these mathematical theories are literally true or that there are any such things as abstract mathematical objects. The reason, in a nutshell, is that abstract objects are supposed to be causally inert." (Balaguer (1999) p 113)

Balaguer's PCI is an instance of what Colyvan calls the eleatic principle. Besides

Balaguer, there are naturalists, like Azzouni and David Armstrong, who reject the existence of mathematical objects despite their presence in scientific theory. Armstrong asserts that science can accept objects that, "[H]elp to explain the behavior of physical things," but then denies that mathematical objects can do this, since they lack causal efficacy. They are merely heuristic devices. "If any entities outside the [spatio-temporal] system are postulated, but have no effect on the system, there is no compelling reason to postulate them." (Armstrong (1980) p 154)

To avoid the double-talk criticism, the eleatic denies QP. QP is unwelcome for the eleatic, anyway. The eleatic rejects non-physical objects fundamentally, and rules out any procedure for determining ontic commitments, like QP, which leaves open the question of the existence of mathematical objects.

In order to consistently deny QP, the eleatic must be able to separate claims about mathematical and physical objects. Armstrong, Azzouni, and Balaguer agree that we can distinguish between mathematical and non-mathematical content. In explaining a physical phenomenon, we only commit to the non-mathematical, physical content, even if we refer to mathematical objects along the way. We know going into our theoretic construction the kinds of things to which we are committed. Our explanation of why my hand can not pass through a wall may refer to mathematical objects, but the subjects of the explanation are hands and walls, and not mathematical objects.

Colyvan argues that attempts to refine Eleatic principles suffer serious difficulties. While Colyvan may be right that these principles are difficult to specify exactly, the difficulty in specifying a distinction is no real argument against it. Important distinctions are often difficult to get exactly right. Still, Colyvan defends indispensabilism against the eleatic, and the instrumentalist, by arguing that we are committed by physical theory to non-mathematical, non-causal entities. If Colyvan is right, then the principled distinction which supports the version of MI I have been discussing is wrong and MI may be rendered untenable. In the next section, I show that Colyvan's allegation is false.

§2.5: The Eleatic and the Indispensabilist

Colyvan defends QI.[87] He argues that attempts to avoid commitment to mathematical objects on the basis of independent methodological principles like PCI fail, and that we must look for commitment, with Quine, in the construction of scientific theory.[88] Colyvan argues that the eleatic principle is wrong because non-causal, non-mathematical entities play indispensable explanatory roles. If we admit these, then we should also admit mathematical objects. And, Colyvan argues, there are good reasons to admit these objects. I show that there are not.

Colyvan presents three examples. The first concerns the bending of light. He argues that the best explanation of light bending around large objects, like the sun, is

[87] Or, anyway, he defends his interpretation of Quine's indispensability argument.

[88] Indispensability arguments must present some goal for which commitment is indispensable. For Quine, this goal was the construction of scientific theory. Colyvan, recognizing that there is no unified goal of science, focuses on scientific explanation, as Armstrong did. It does not matter that Colyvan focuses, with Armstrong, on the goal of explanation, in whatever form it may come, rather than on theoretic construction, with Quine. The examples play the same role in both domains.

mathematical. Light moves along space-time geodesics. The large mass covaries with the curvature in space-time, but it is not clear, on a causal picture, which causes which. “Simple covariance doesn’t guarantee that one of the factors causes the other.” (Colyvan (2001) p 48) Furthermore, mass can not be the only cause of curvature since, according to the non-Minkowski vacuum solutions to the Einstein equation, there are empty space-times in which the curvature of space is not zero. On the causal picture, these curvatures are uncaused, and thus unexplained.

Colyvan’s second example concerns antipodal weather patterns. The causal explanation of the existence of two antipodes with exactly the same pressure and temperature at the same time, which refer to atmospheric conditions, does not suffice. The existence of antipodes is guaranteed by a topological theorem. The proof of this theorem provides the remainder of the explanation. We need both causal and non-causal elements in the explanation.

Lastly, Colyvan asks us to consider the Fitzgerald-Lorentz contraction. A body in motion contracts, relative to an inertial reference frame, in the direction of motion. Minkowski’s explanation of this contraction relies on equations in four dimensions, representing the space-time manifold. Colyvan calls this, “A purely geometric explanation of the contraction, featuring such non-causal entities as the Minkowski metric and other geometric properties of Minkowski space.” (Colyvan (2001) p 51)

The first example either begs the question, or is insufficient. Colyvan must show that non-causal entities other than mathematical objects play an explanatory role. If we take geodesics as pure mathematical objects, Colyvan begs the question by presenting a geometric object as explanatory. If we take the geodesic to be a physical entity, then we should see the geodesic as a property of space-time, and we can see the mass as causing that curvature. Colyvan rejects this interpretation. "[A]ny account that permits mass to *cause* the curvature of space-time is unintuitive to say the least." (Colyvan (2001) p 48) The unintuitiveness, for Colyvan, may arise from thinking of space-time as abstract. If we think of it substantivally, the causal explanation is not problematic.

In the case of the antipodes, we must again make a pure/applied distinction regarding the topological theorem. The pure mathematical theorem does not guarantee that these antipodes have the same temperature and pressure. We need bridge principles which apply this theorem to the Earth and its weather patterns. Once we add these bridge principles, it is no longer clear that the proof which guarantees the antipodes should be regarded as a non-causal explanation. For, surely the bridge principles will refer to causal structures within the Earth's atmosphere, and it is these which explain the existence of the antipodes. This explanation will, as Colyvan notes, refer to non-causal entities such as continuous functions and spheres, but these are mathematical objects. We are looking for non-mathematical, yet non-causal, elements.

A similar response applies to the contraction example. The equations which explain the contraction are supposed to make indispensable reference to non-causal entities. But the equations apply to the physical world, and thus explain the contraction of a physical body in motion, only if coupled with bridge principles which explain their applicability. The physical objects provide the explanation.

In no case has Colyvan shown that a non-causal entity, other than a mathematical object, plays an essential role in scientific explanation. The eleatic need not show that mathematical entities can be removed from explanations in the physical world. Thus, Colyvan provides no reason to abandon the eleatic principle, or to undermine MI.

Even if we establish that there are objects which are not causally efficacious, but which figure in scientific explanations, we may not need to cede the eleatic principle. For, we could, as Colyvan considers, claim that such objects are causally relevant even if they are not causally efficacious. An abstract property, like squareness, can program for the efficacious property, like having overlapping portions so that a square peg can not fit into a round hole, without itself being causally relevant. Colyvan responds that this interpretation only works when a fully causal explanation is available, and so will not work for the Lorentz contraction. The relevance/efficacy distinction allows non-causal elements to count as causal, but it blurs the notion of causality. “I am more inclined to admit that causally idle entities can have explanatory power than to fiddle with the definition of ‘causal’ in this way.” (Colyvan (2001) p 52)

Colyvan’s preference is unjustified, especially since these examples are intended to reject the eleatic principle. We need not fiddle with the notion of causality if we adjust this principle. The eleatic committed to causality as the defining characteristic of the physical since it seemed to work. But there may be good reasons to adjust it. As Colyvan notes, the eleatic principle may classify items outside our light cones as non-physical, since they are causally isolated from us.

The fundamental commitments of scientific theory, for the eleatic like Armstrong, or for the mathematical instrumentalist, is to physical objects. The eleatic principle was merely an attempt to explicate that commitment. We know when an object is or is not physical. We are committed to quantum particles, because they are the constituents of ordinary physical objects. We struggle with the question about access to mathematical objects precisely because they are non-physical.

Azzouni adjusts his methodological principle to include elements outside our space-time cone.[89] Colyvan accuses Azzouni of abandoning the eleatic principle, in this case, but this criticism is misguided. Since items outside our space-time cone are causally isolated from us and yet physically real, this principle needs to be adjusted.

The mathematical instrumentalist need not rely on a strict eleatic principle, but whatever refined principle she adopts should entail Balaguer's PCI. We should expect, given PCI, to be able to write scientific theory without reference to mathematical objects. The unavailability of nominalist theories might imply that mathematical objects play some kind of role not merely in our explanations of physical phenomena, but in the phenomena themselves. If our concern is to describe the empirical world, then we should be able to do so without referring to things that play no role in the world. "For if PCI is true, that is, if there are no mathematical objects that are causally relevant to the physical world, then it seems that there should be an attractive way of describing the physical world that makes no reference to such objects." (Balaguer (1998) p 130)

Consider, for example, the Craigian theory N_2 outlined in Chapter 1, §2.2. It dispensed with mathematical objects by adopting the theory which consists of all nominalistically acceptable consequences of standard scientific theory. The Craigian

[89] See Azzouni (1997b) §III.

method originally interested philosophers who defended SI, and looked to eliminate theoretical terms from a theory, leaving only observational vocabulary. The theoretical/observational distinction is just another way to voice the capricious SI. The Craigian theory yields all the theorems of empirical science without referring to mathematical objects. Its unattractiveness is irrelevant, since instrumentalists and dispensabilists agree that the working theory will be the standard one.

MI renders dispensabilist reformulations moot. We need not see ourselves as committed to mathematical objects on the basis of scientific theory. It remains an open question whether we should believe in mathematical axioms and theorems as well as those of empirical science.

§2.6: Reflective Instrumentalism

I have argued that the double-talk criticism fails against an instrumentalism, like MI, which relies on a principled distinction between real and instrumental posits. MI thus provides a reason to reject QP. Other versions of instrumentalism distinguish between legitimate and instrumental physical posits. A reflective instrumentalism, or RI, may base the distinction on activity within the causal nexus, or on spatio-temporal location. Like MI, RI is a family of views. In this section, I sketch some defenses of RI others have made. If any RI is acceptable, MI is likely to be acceptable as well.

Azzouni defends RI by arguing that QP, applied in QI, commits us to objects we do not really believe exist. He describes instances in which existential quantifications within science proper should be seen as merely instrumental. The users of scientific theories are not committed to objects such as quasi-particles, centers of mass, and mathematical objects. Consider a system of two masses connected by a spring, moving in a gravitational field. The separate motions of the masses are too complicated to calculate, but if we consider the system in terms of its center of mass, which is not located on the springs, and its reduced mass, we can describe the system.

Azzouni refers in the same spirit to quasi-particles, which are posits used to replace one intractable many-body problem in condensed matter physics with many one-body problems, using Fermi Liquid theory. Scientists introduce these particles in order to simplify impossible calculations, aware that a fictionalization is involved. "No physicist stops to ask if a longer route eschewing such fictional items is in principle possible. But in failing to stop to ask this, it's not that physicists are failing to ask whether or not they're committed to the entities introduced in this way. They already take themselves not to be so committed. That's why, for example, such 'particles' are called quasi-particles." (Azzouni, (1997b) p 195)

If we grant the availability of a single best theory, we might want to determine its commitments. Azzouni suggests introducing a predicate which applies to all and only those objects which are physically real to avoid errant commitments to instrumental elements. The predicate could apply to all and only physical objects, and not to mathematical ones. The mathematical nominalist can treat only objects in the extension of that predicate as real, regardless of the quantifications of his theory.

An existence predicate can be crafted to suit our needs, and it may take only a little work to construct it. "The unregimented discourse of science *already has* one or more phrases which are taken to carry the burden of ontological commitment, be it ‘causally efficacious’, ‘observational’, or whatever. When regimenting, therefore, one mints a predicate that, more or less, *replicates* this role.” (Azzouni (1998) p 3, latter italics added)

On this picture, pre-formalized considerations establish our ontology and the regimented version of our ideal theory makes it explicit. Another option Azzouni suggests for revealing commitment within a regimented theory is to introduce a subscripted quantifier for this purpose. We could also introduce a distinct new quantifier.

While Azzouni intends his suggestions as ways to avoid commitments to mathematical objects, existence claims for mathematical objects may be made independently. We may introduce a further predicate, ‘is mathematically real’, which applies to all and only existing mathematical objects: to three, but not to the rational square root of two.

Maddy defends a similar position when she argues that scientists do not think that all objects used indispensably in their best-confirmed theories are equally legitimate. She cites the case of skepticism surrounding atoms in the early stages of atomic theory. Until the experiments which yielded more direct evidence of the existence of atoms were performed, scientists hedged their bets about these elements, even when they accepted chemical theory.

> [T]hough atomic theory was well-confirmed by almost any philosopher’s standard as early as 1860, some scientists remained skeptical until the turn of the century - when certain ingenious experiments provided so-called “direct verification” - and even the supporters of atoms felt this early skepticism to be scientifically justified. This is not to say that the skeptics necessarily

> recommended the removal of atoms from, say, chemical theory; they did however, hold that only the directly verifiable consequences of atomic theory should be believed, whatever the explanatory power or the fruitfulness or the systemic advantages of thinking in terms of atoms. In other words, the confirmation provided by experimental success extended only so far into the atomic-based chemical theory *T*, not to the point of confirming its statements about the existence of atoms. (Maddy (1992) p 280-1)

According to Maddy, it is accepted scientific practice to separate our actual commitments from those made by our best theories. This allows scientists to withhold assent to elements of a theory, even if the theory is the best available at the time. Atomic theory was accepted, it expressed commitment to atoms, but scientists did not really believe that the atoms existed. They took an instrumentalist attitude toward them. "If we remain true to our naturalistic principles, we must allow a distinction to be drawn between parts of a theory that are true and parts that are merely useful. We must even allow that the merely useful parts might in fact be indispensable." (Maddy (1992) p 281)

Maddy also cites examples of false assumptions within science, specifically in applied mathematics: taking water waves to be infinitely deep, and treating matter as continuous in fluid dynamics. These examples, like Azzouni's center of mass example, are idealizations which make a scientific theory practically useful, but which should not be taken literally.

One might respond, as I assume Quine would, that scientists, or philosophers of science, should have accepted the existence of atoms, based on the posits of atomic theory. This is the point of QI.2 and the double-talk criticism. For Maddy, QI.2 violates the naturalist emphasis on scientific practice.

A more serious criticism of RI arises from recognition that indispensability is always for some purpose. It may be that instrumental posits, such as those of centers of mass and the infinite depth of an ocean, are only indispensable for the practical purposes of solving applied scientific problems, and that they are not really indispensable to the ideal theory to which QI.1 refers. This amounts to a claim that all posits that are practically indispensable are really dispensable in a more fundamental theory.

This response is not available in the case of pre-Einsteinian atomic skeptics, Maddy argues, since they had no more fundamental theory to which to refer and in which atoms were dispensable. Scientists consider certain referents of their theories as merely instrumental. A theory is not merely easier when we talk about centers of mass, but is formulated in those terms. To see centers of mass as merely a simplifying calculating device is to import prior considerations about what is real into the theory.

Devitt accommodates instrumentalism by urging us to suspend judgment on some elements of our theory, to take a range of attitudes toward the different elements of the different theories which comprise our monolithic best theory. "The Realist does not recommend to scientists that they should believe strongly in the entities of all theories, only in those of the established theories that we have the very best grounds to accept. Toward the entities of other theories, which we in some sense accept, he may recommend a variety of other attitudes, ranging from mild belief to outright disbelief." (Devitt (1984) p 131)

Devitt is recommending abandoning Quine's homogeny. If Quine were to adopt Devitt's attitude, he would not admit mathematical objects on the basis of indispensability.

In a similar vein, one might emphasize the distinction between the commitments of the theory and the commitments of those who construct the theory. In light of the failure of formal theories to provide a categorical criterion for ontic commitment, the indispensabilist might then claim that the uses of scientific theory are indispensably committed to mathematical objects, even if the theory is not. But this is a dead end for the indispensabilist, since those who construct scientific theory need have no further commitments to mathematical objects on the basis of their role in science, once their place in the theory is interpreted instrumentally.

Other versions of RI are possible, based on space-time properties, or on the commitments that scientists see their theories as making. Azzouni relies on what he calls thick epistemic access. If any RI which treats some physical posits instrumentally is correct, then MI is likely to be, as well.

§2.7: Resolving the Puzzle

The considerations which favored taking mathematical statements to be central to the web of belief relied on the ubiquity and utility of mathematics. Since MI allows us to reject homogeny, ubiquity and utility are no arguments for centrality. Even if we use mathematics always and everywhere, we can maintain a fictionalist attitude toward it, as far as scientific theory is concerned. We should take our beliefs in mathematical objects to be peripheral.

Taking our beliefs in mathematical objects to be peripheral better reflects our actual attitudes. It also accounts for the debates between indispensabilists and dispensabilists, which seem hopelessly mired in the details of scientific theory. The debates over the viability of nominalism predate indispensabilism, and they remain, independently of it. Instrumentalism trumps dispensabilism. Still, the nominalist can appeal to the brute fact of reformulations of science. The mathematical realist can appeal to the brute fact of the obviousness of mathematical truth. These philosophical arguments are at the periphery of the web of belief, if the metaphor really holds, which instrumentalism predicts.

In this part of the chapter, I granted QI.1 and QI.2, and argued against QI.3. In the next part, I grant QI.1, QI.2 and QI.3, and show that QI still fails to generate mathematical objects.

Part 3: No Mathematical Objects Here

§3.1: Introduction to Part 3

Quine's indispensability argument concludes that we are committed to mathematical objects.

(QI) QI.1: We should believe the theory which best accounts for our empirical experience.
QI.2: If we believe a theory, we must believe in its ontic commitments.
QI.3: The ontic commitments of any theory are the objects over which that theory first-order quantifies.
QI.4: The theory which best accounts for our empirical experience quantifies over mathematical objects.
QI.C: We should believe that mathematical objects exist.

So far, I focused my arguments mainly against QI.3, which depends on Quine's procedure for determining ontic commitment, QP. In this part of the chapter, I grant QI.1

- QI.3, and show that QI.C still does not follow. The dispensabilist, like Field, rejects QI.4 because he thinks that a reformulation of standard science which contains no quantifications over mathematical objects is a better theory. I reject QI.4, but not in the way that the dispensabilist does. I argue that the objects over which we quantify in standard science are not really mathematical objects.

My claim that the indispensabilist does not generate mathematical objects requires at least a characterization of mathematical objects. Traditionally, mathematical objects are taken to be abstract, homogeneously located outside of space and time. They are mind and language independent. They exist necessarily, in all possible worlds. Mathematical objects and their properties are also traditionally taken to be known a priori. We need not take any of these characteristics to be essential.

In the next four sections, I show how the objects generated by QI fail to have too many of these properties to be considered mathematical. The indispensabilist is saddled with disciplinary blur, ontic blur, a restricted ontology, and modal uniformity, among other difficulties. These problems show that the things which satisfy the mathematical axioms in standard science are not really mathematical objects.

§3.2: Disciplinary Blur

As I noted in §1.3 of this chapter, Quine's holism entails disciplinary blur.

> Boundaries between disciplines are useful for deans and librarians, but let us not overstate them - the boundaries. When we abstract from them, we see all of science - physics, biology, economics, mathematics, logic, and the rest - as a single sprawling system, loosely connected in some portions but disconnected nowhere. Parts of it - logic, arithmetic, game theory, theoretical parts of physics -

> are farther from the observational or experimental edge than other parts. But the overall system, with all its parts, derives its aggregate empirical content from that edge; and the theoretical parts are good only as they contribute in their varying degrees of indirectness to the systematizing of that content. (Quine (1963) p 76)

Disciplinary blur is not an essential element of Quine's canonical system. We can draw a line between mathematical statements, those which contain only logical and set-theoretic particles, and the rest of the system, which contains also non-logico-mathematical predicates.[90] Such a line places claims of pure mathematics apart from those of science. Some existential quantifications over mathematical objects may remain on the empirical side of the line, depending on whether nominalist reformulations of science are possible.

Not only can we draw a distinction between the sentences of science and those of mathematics, but we can also distinguish their methodologies. Disciplinary blur misrepresents mathematical methodology. Consider the distinction between Babylonian mathematicians, who focused on practical problems and justified their results on the basis of practical utility, and Greeks and later mathematicians who did not appeal to empirical considerations for justification. We can characterize this difference on the basis of an a priori/a posteriori distinction, or proofs and axiom systems present in the latter but absent in the former. We might rely on modality, since practical work yields only contingent results, or on whether we allow errors. The indispensabilist can describe only a difference in degree.

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[90] Parsons makes this point (Parsons (1986) p 381), and solves a difficulty which arises from the delineation. The example presumes a full reduction of mathematics to set theory, but can easily be amended to dispense with this presumption.

If the disciplines of mathematics and science were really indistinct, it would be difficult to see quantifications in ZF, say, as ranging over mathematical objects. If there is only one discipline, there should be only one kind of object. This claim will be made clearer in the next section, on ontic blur.

§3.3: Ontic Blur

QI results in ontic blur, an inability to distinguish abstract from concrete objects. The indispensabilist alleges that mathematical objects are abstract, and that the abstract/concrete distinction remains clear, only by fiat. Such fiats are common in Quine's work. For example, "Classes, therefore, are abstract entities; we may call them aggregates or collections if we like, but they are universals." (Quine (1953a) p 115)

To explain the difference between concrete and abstract objects, Quine uses paradigms. "It will suffice for now to cite classes, attributes, propositions, numbers, relations, and functions as typical abstract objects, and physical objects as concrete objects *par excellence* and to consider the ontological issue as it touches such typical cases." (Quine (1960a) p 233)

The distinction between abstract and concrete objects must be an entailment of Quine's system, especially since he defers philosophical questions to science. But the system does not support the distinction. As Parsons notes,

> Although Quine makes some use of very general divisions among objects, such as between 'abstract' and 'concrete', these divisions do not amount to any division of *senses* either of the quantifier or the word 'object'; the latter sort of division would indeed call for a many-sorted quantificational logic rather than the standard one. Moreover, Quine does not distinguish between objects and any more general

> or different category of 'entities' (such as Frege's *functions*). (Parsons (1986) p 377)
>
> Quine's informal distinction has no representation in our best theory.[91]

Furthermore, he wonders if the distinction is sustainable.

> [O]dd findings [in quantum mechanics] suggest that the notion of a particle was only a rough conceptual aid, and that nature is better conceived as a distribution of local states over space-time. The points of space-time may be taken as quadruples of numbers, relative to some system of coordinates... We are down to an ontology of pure sets. The state functors remain as irreducibly physical vocabulary, but their arguments and values are pure sets. The ontological contrast between mathematics and nature lapses. (Quine (1986b) p 402)[92]

The indispensabilist's theory is constructed to represent phenomena involving ordinary objects. "Bodies are assumed, yes; they are the things, first and foremost. Beyond them there is a succession of dwindling analogies." (Quine (1981b) p 9)

As these analogies dwindle, the abstract/concrete distinction becomes less clear. The indispensabilist struggles to classify some objects, like fields. "I am concerned to urge the empirical character of logic and mathematics no more than the unempirical character of theoretic physics; it is rather their kinship that I am urging, and a doctrine of gradualism." (Quine (1986a) p 100)

Quine's gradualism forces him to reject PCI. Balaguer thinks that the assimilation of abstract objects to the rest of the posits of science is definitive of QI.

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[91] Burgess and Rosen call Quine's commitment to abstract objects a "loose" and "unofficial" extension of the official sense of ontological commitment. See Burgess and Rosen (1997), p 226.

[92] See also Quine (1978); Quine (1960a) p 234; Quine (1974) p 88; and Quine (1969b) p 98.

"The Quine-Putnam argument should be construed as an argument not for platonism or the truth of mathematics but, rather, for the falsity of PCI." (Balaguer (1998) p 110)

The problem of determining boundaries between mathematical and empirical objects may look like a vagueness problem. Vagueness problems may be seen as technical difficulties which do not undermine the legitimacy of our ordinary concepts. The problem of ontic blur, though, undermines the implausible claim that the indispensabilist can provide an abstract ontology with an empiricist epistemology.

In Chapter 1, §2.3, I noted that Katz marks a clear distinction between abstract and concrete objects, based on spatio-temporal location. Given any such clear characterization of the abstract/concrete distinction, Quine's claim that the mathematical objects yielded by QI are distinctly abstract seems implausible. The only mathematical objects Quine countenances are sets. The indispensabilist commits to sets only for their use in systematizing scientific theory. Sets are only sets of objects in the scientist's ken: chairs, electrons, space-time points, etc. All of these objects are located in space and time, with the possible exception of space-time points themselves.[93]

Colyvan, who follows Quine and welcomes ontic blur, mentions difficulties in disentangling empirical from mathematical vocabulary to supplement his argument for blur. "For our purposes, it will suffice to note that there is no obvious way of disentangling the purely mathematical propositions from the main body of science." (Colyvan (2001) p 36)

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[93] See Chapter 3, §6, especially §6.3, for a discussion of ontic blur and space-time points.

Here is an easy way to disentangle the purely mathematical propositions: those which are theorems of pure ZFC, or another preferred set theory, are purely mathematical. The rest are not. We can include theorems of number theory and topology, among other areas, as mathematical, if we reject set-theoretic reductionism. Only the controversial claim of disciplinary blur makes disentangling appear difficult.

QI thus does not generate abstract objects. In the next section, I show how the indispensabilist is further restricted by his rejection of pure mathematics.

§3.4: Restriction: The Rejection of Pure Mathematics

Mathematicians discuss many different mathematical objects. The scientist finds some of these useful, and applies them in her work. QI alleges that we are committed to mathematical objects because of these applications. Thus, the indispensabilist is not committed to any objects which are not used in scientific theory. "I recognize indenumerable infinities only because they are forced on me by the simplest known systematizations of more welcome matters. Magnitudes in excess of such demands, e.g., $\beth_\omega$ or inaccessible numbers, I look upon only as mathematical recreation and without ontological rights." (Quine (1986b) p 400)

Quine's denigration of pure mathematics conflicts with mathematical practice and any substantial conception of mathematical truth. In this section, I extend this criticism, which has been made by others.

An indispensabilist is entitled only to set theories much weaker than those commonly used in mathematics. Parsons criticizes Quine for proceeding as if nearly all of mathematics were justified. “If ability to explain observational data in the ordinary sense were the reason for accepting set theories, then weaker theories (perhaps only some form of second-order number theory) would be preferred to full set-theory on grounds of simplicity and perhaps lesser risk of inconsistency. Quine’s actual accounts of set theory proceed in a much more abstract fashion.” (Parsons (1986) p 382)

Maddy criticizes the indispensabilist’s restrictions, and defends pure mathematics on the basis of mathematical practice.

> Now mathematicians are not apt to think that the justification for their claims waits on the activities in the physics labs. Rather, mathematicians have a whole range of justificatory practices of their own, ranging from proofs and intuitive evidence, to plausibility arguments and defences in terms of consequences. From the perspective of a pure indispensability defence, this is all just so much talk; what matters is the application. (Maddy (1990a) p 31)[94]

Chihara also criticizes the restrictions. “It is suggested [by Quine] that which mathematical theory we should take to be true should be determined empirically by assessing the relative scientific benefits that would accrue to science from incorporating the mathematical theories in question into scientific theory. It is as if the mathematician should ask the physicist which set theory is the true one!” (Chihara (1990) p 15)

The restrictions do not merely apply to the outer regions of set theory. Justifications of mathematical claims vary with shifts in our best scientific theory. As science progresses, and uses new mathematical tools, the mathematics which is justified changes, though no mathematical progress need be made.

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[94] Maddy is here subject to a bootstrapping criticism. I show how to avoid this in Chapter 5, §1.2.2.

The adoption of mathematics with no further mathematical justification is not the worst problem arising from the restrictions. We could see a profound upheaval of the justifications of mathematics. Maddy suggests that all of science could, in principle, become quantized.[95] We would lose continuum mathematics, the calculus and analysis. Not only is the line between justified and unjustified results drawn in the wrong place, but it can move.

Quine's attempts to adopt the full mathematical panoply fall short. He notes that while we may no longer need certain mathematical results in one area of science, we may continue to require them in others.

> The actual formula 'f is a group', with variable 'f', does belong to pure mathematics; grouphood is a mathematical property of various mathematical and non-mathematical functions. Various mathematical truths about groups are derivable from the definition, and they remain in force even when some particular function in natural science that was thought to be a group turns out to be otherwise. (Quine (1986b) p 398)

Quine only keeps results which have been shown not to apply when other uses for these objects remain. His restrictions remain for results that lose all connection with science, or never gain any.

Quine can round out his theory to make it a bit simpler. A theory which does not awkwardly restrict results on a case-by-case basis is more natural than one which does. We may fill in a theory which, in bulk, is justified by application to empirical theory. "Further sentences such as the continuum hypothesis and the axiom of choice, which are

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[95] See Maddy (1992), p 285-6. Relatedly, Christopher Pincock (forthcoming) wonders whether physical science really has continuum many commitments.

independent of those axioms, can still be submitted to the considerations of simplicity, economy, and naturalness that contribute to the molding of scientific theories generally." (Quine (1992) p 95)

There are some finite numbers which do not usefully apply to any actual objects; there are no scientifically acceptable predicates whose extensions have those cardinalities. Still, the indispensabilist welcomes all finite numbers. Quine indicates approval of the axioms of constructibility, *V*=*L*, on the basis of streamlining results. Still, the existence of large cardinals not derivable from ZFC + *V*=*L* are not justified by this limited policy.

Quine's restrictions have roots in his understanding of set theory which, Parsons argues, is Fregean, founded in predication. The legitimate predicates of our best theory are the empirical predicates of science, not those of number theory. "Set theory for Quine has always been a theory of extensions of predicates, in that the paradigm of a set is the extension of a predicate, and the axioms of set theory are taken as attributing extensions to certain predicates." (Parsons (1986) p 383)

Set theory is not typically conceived this way. Sets, as understood by the mathematician, diverge from extensions. As Parsons points out, both 'x is a woodchuck' and 'x is not a woodchuck' have extensions, the latter including itself within its extension. On any standard set theory, though, one or the other must not be a set. If sets are extensions of predicates, then both woodchuckhood and non-woodchuckhood define sets. {x: x is not a woodchuck} would be the universal set, less only a finite set the size of the number of woodchucks. The existence of a universal set leads famously to contradiction, as would the existence of this set. ‘x is not a woodchuck’ has an extension, but does not define a set.

There are a variety of ways around the paradoxes, of course, for both the traditional, pure set-theorist, and the indispensabilist. Most commonly, one rejects an unrestricted axiom of comprehension, which allows any property to define a set, replacing it with specific axioms, including a limited comprehension schema, to ensure the existence of enough sets for mathematics. Differences among axiomatizations yield different set-theoretic universes. Mathematicians debate the correct axiomatization, and whether there is one. By taking sets as extensions of predicates, the indispensabilist unnecessarily constrains his choice of set theory.

Quine’s awkward restrictions are not the first that a philosopher has tried to impose on the mathematician. Intuitionists restrict mathematics to theorems which can be proved constructively, but they make no claim to realism. The mathematical realist can appreciate intuitionism, which delimits an interesting subset of classical results, but he chafes at the indispensabilist’s restrictions.

I have argued that there are two unacceptable results of Quine’s arbitrary distinction between accepted mathematics and mathematical recreation. The accepted results are arbitrarily limited. And the line between what is accepted and what is not can shift. Relatedly, the indispensabilist also loses mathematical necessity, which is the subject of the next section.

§3.5: Modal Uniformity, Aposteriority, and the Fallibilist Apriori

If the physical world were different than it is, if it required different mathematics, then, for the indispensabilist, different mathematical objects would exist. Thus, for the indispensabilist, mathematical objects do not exist necessarily.[96] Call this property of the commitments of a proponent of QI modal uniformity.

Quine rejects modalities, so he would see the inability of QI to yield mathematical necessity as a virtue.[97] "Avoidance of modalities is as strong a reason for an abstract ontology as I can well imagine." (Quine (1986b) p 397)

Still, it is hard to shake the feeling that mathematics is not contingent in the way that empirical claims are. Quine attempts to account for the feeling of mathematical necessity, while rejecting modalities, by arguing that while we may always revise mathematics in response to recalcitrant experience, in practice we always choose to revise empirical statements (or the *more* empirical statements). Revising obviously empirical claims requires less upheaval of the entire system.

Quine's account of the necessity of mathematics, while counterintuitive, has an insidious resilience. He claims that mathematical statements are contingent, but also

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[96] There may be necessary facts about mathematical objects, in the same way that there may be necessities involving other contingently existing objects. The positive square root of four may be two, necessarily, despite its contingent existence, for the indispensabilist.

[97] Quine responded unfavorably to Parsons's attempts to generate some modality for him. "Parsons argues for the utility of...mathematical possibility. One example: he would like to be able to say that the construction of natural numbers 'can' be continued without limit. I have no desire to say this." (Quine (1986b) p 397) Parsons's example is poor. Quine can avoid modality in this case since he can express finite quantities using logic, even if he can not say that he can do so, within his theory.

admits that we never do give them up. This looks like an irrefutable doctrine, in Popper's sense. Whether or not we include modality as an element of the canonical framework, we need an account on which mathematical existence statements are distinct from statements about the existence of Bengal tigers. Quine has given us only a doctrine of gradualism.

Colyvan welcomes modal uniformity, as he embraced blur. He argues that mathematical objects exist contingently,[98] and are known a posteriori. "[M]athematical propositions are known a posteriori, because the existence of mathematical objects can be established only by empirical methods - by their indispensable role in our best scientific theories." (Colyvan (2001) p 116)

Aposteriority obviously afflicts the mathematical objects generated by QI as well, though the connection between aposteriority and modal uniformity is not as close as Quine would have it. Quine's well-known rejection of a priori knowledge does not directly yield a rejection of necessity for mathematics, since mathematical truths could be among any necessary a posteriori truths. For example, Kripke argued that water is H_2O necessarily, though that discovery was empirical. But mathematical truths are not like the Kripke cases. That water is H_2O is discovered a posteriori, whereas mathematical methodology seems to be a priori, if anything is. Kripke's insight was that empirical truths may be necessary. Only a broader skepticism about a priori knowledge would justify classifying mathematical statements as known a posteriori.

Quine's rejection of a priori knowledge is based on his belief that every statement

[98] This is Field's position, too. See Chapter 5, Part 3.

is empirically revisable. He elides an important difference between apriority, which is an epistemic property, independent of revisability, and necessity, which does entail unrevisability. A sentence is believed a priori, approximately, if it is acquired independently of experience. A sentence is necessary if it could not be false. We could wrongly take a sentence to be necessary and then discover that it is not, and never was. Kant's ascription of necessity to the Euclidean structure of physical space is an obvious example.

In contrast, when we hold a belief on the basis of a priori considerations, and then discover that it is false, our discovery of its falsity does not change how we justified the original belief. For example, belief in the axiom of comprehension, that every property defines a set, may be held a priori. After considering, non-empirically, Russell's paradox, one may give up the earlier belief. Consideration of Russell's paradox does not change the way in which we came to believe the axiom of comprehension; it merely shows that unrestricted comprehension is false. We held a false belief a priori.

Quine links apriority with necessity, and concludes that the falsity of a belief entails that we never believed it a priori. Since any belief, he claims, is open to rejection, no belief can be a priori. Quine's position might be called an infallibilist a priori.

In contrast, any defensible notion of a priori knowledge must be fallibilist, accounting for the revisability of mathematical statements while preserving their a priori status. Mark McEvoy's reliabilist account of mathematical knowledge (McEvoy (2002)), based on a fallibilist a priori, is such an account. On a fallibilist a priori, our a priori mathematical beliefs are defeasible. This claim is stronger than the claim that we could be wrong when we think we know something a priori. It is the claim that better a priori considerations may defeat weaker ones.

The fallibilist assumes that belief-forming processes may be classified as either empirical or a priori. Examples of a priori processes may include intuition, rational reflection, and deduction. The fallibilist need not commit to the legitimacy of any particular a priori processes, just that some belief-forming processes are a priori. I will return to this notion of a fallibilist a priori, especially in my discussion of Putnam, who makes the same error as Quine.

In this section, I have described how Quine's putative mathematical objects do not exist necessarily, and are not known a priori. Combined with both disciplinary and ontic blur, and the rejection of pure mathematics, these characteristics undermine the claim that the indispensability argument generates mathematical objects. In the next section, I extend my allegation that QI does not yield mathematical objects by considering Putnam's work, as it coheres with Quine's.

§3.6: Putnam and Blur

In Chapter 4, I examine Putnam's non-Quinean indispensability argument. He also, at some time, held QI. In this section, I show how Putnam's version of QI fares no better than Quine's.

Putnam especially evinces blur in the way he uses 'mathematics' to refer to non-mathematical, or applied mathematical, results. "[I]t is not difficult to find mathematically true statements which quantify only over material objects, or over sensations, or over days of the week, or over whatever 'objects' you like..." (Putnam (1964) p 2)

As an example of a mathematically true statement quantifying over material objects, Putnam describes a Turing machine and a halting problem for that Turing machine. In this example, a mathematical result is applied, but the mathematically true statement applies directly only to the mathematical objects. It requires bridge laws to associate the mathematical objects with physical objects. The assertion is subject to a slew of background physical conditions. Perhaps the machine falters in humid conditions, or no one oils the moving parts and they get stuck. These kinds of conditions are all subject to physical laws and empirical description. They undermine the certainty of the given statement, its mathematical quality. Putnam's halting result is an applied mathematical statement.

As another example, Putnam offers that there are mathematically true statements which quantify only over inscriptions. The theorems of a formal theory are mere inscriptions, and yet the metatheoretical results which apply to these inscriptions are also mathematically true. Again, there are issues of bridge principles: burned or ripped paper, smudges, mistakes in inscription. Only the abstract case is certain. Once we apply it to the physical world, we lose that mathematical character.

Putnam even calls theorems of a theory of inscriptions contingent, because some of them have not been inscribed. We could re-gain mathematical character for a contingent theory of inscriptions if we appealed to the possibility of applying the mathematical results. What is certain, or mathematical, about these inscriptions is that if we perform certain acts with a certain precision, following physical rules, we will arrive at a particular physical result. We can apply mathematical inferences to concrete phenomena, deducing mathematically what will happen if all the background physical hypotheses are satisfied. The theory of inscriptions is not mathematical, even if we reduce the physical elements of that theory as far as possible.

Quine does not make the obvious error of calling empirical beliefs, like those about Turing machines, mathematical. But, any indispensabilist is concerned primarily with mathematics as it applies to the physical world. Putnam's elision of pure and applied mathematics is characteristic of the indispensabilist, who has little real interest in mathematics proper. We must be careful not to allow the indispensabilist to coopt the term 'mathematical,' applying it to the objects to which we are committed by QI despite their non-mathematical character.

The considerations of this chapter so far apply to any proponent of QI. In the next section, I show how an element of Quine's work which is not directly entailed by QI, his set-theoretic reductionism, also undermines his claims to mathematical objects.

§3.7: Reductionism and Mathematical Diversity

Quine interprets QI as committing us only to sets, because all other mathematical objects are reducible to sets. "[W]hen we use some progression of sets for counting or for other purposes commonly served by numbers, we are apt to call these sets numbers and refer to them by numerals, but this is a mere notational convenience, conveniently dropped when philosophical questions arise." (Quine (1986b) p 401)

There is nothing in QI itself which demands Quine's mathematical reductionism, though it is consistent with the austerity which underlies the indispensability argument. It is more accurately, for Quine, an indication of his general predilection for extensional objects. Call the anti-reductionist position that mathematical objects with different properties (numbers, sets, topological spaces, etc.) all exist, mathematical diversity.

In this section, I argue that diversity is preferable to reductionism. This argument is an aside, since QI does not require reductionism, but it is a point to which I will return in Chapter 5. I mention it here because Quine's reductionism, like the other characteristics I discuss in this chapter, should make us wary of QI.

Putnam also accepts Quine's reductionism. After reviewing the technical machinery required to reduce real numbers, functions, and relations to sets, he concludes, "Instead of saying, therefore, that physics essentially requires reference to functions and real numbers...we could simply have said that physics requires some such notion as *set*, for the notions of number and function can be built up in terms of that notion." (Putnam (1971) p 343)[99]

We need not see the translations between numbers and sets as reductive. One can view such translations as arguments for the acceptance of non-sets. If we are convinced of the consistency of set theory, and we note that the objects of some other mathematical structure are translatable to sets, then that is good argument for the consistency of the non-set-theoretic structure, and thus good reason to believe in the additional elements,

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[99]Putnam later rejects Quine's reductionism. He nods approvingly toward a non-reductionist position in Putnam (1975a) p 65. Also, he distances himself in his discussion of holism in Putnam (1994), p 504.

too. Translations work in two directions.

Diversity avoids certain difficulties of reductionism. Primarily, it avoids the problems of multiple reductions of Benacerraf (1965). Without the latent logicist reductionism which impels one to think of numbers as sets, the problem of determining which sets are the numbers vanishes.

Quine's parsimony proceeds from a principle applicable in empirical science: do not multiply entities without good reason. When building empirical theories, it is important not to posit more than that which accounts for the phenomena. We reject superfluous elements of a theory, like phlogiston.

Parsimony does not apply in mathematics in the same way. The mathematician explores his universe with a desire to multiply entities. The mathematician does not assert that the rational number two is a different entity than the positive square root of four. Despite the different constructions of these numbers, rationals from ratios of integers and reals from Dedekind cuts, for example, the mathematician identifies them. If the square roots were shown to have a different structure than the numbers, we would admit these in addition, though. This is what we do when we admit the real numbers, in addition to the natural numbers.

Nevertheless, the limitations on multiplying entities which restrict the empirical scientist do not apply to the mathematician. Once we have admitted abstracta into our ontology, we do not run out of room. The discovery of new mathematical objects is an achievement for the mathematician. Worries about the introduction of new mathematical entities, as with transfinites, or complex numbers, focus on their consistency, or the rigor with which they are introduced. "Issues about rigor, we suggest, cloud virtually every case of apparent 'ontological' debate in the mathematical sciences." (Burgess and Rosen (1997) p 225)

For example, Kripke models for modal logic have ameliorated mathematical worries about modality, even though they commit to the same kinds of possible worlds about which the philosopher worries. The background set-theoretic, or category-theoretic, reduction serves to allay worries about consistency, not ontology. The intuition that mathematical theories should not be parsimonious is a basis for Balaguer's plenitudinous platonism (FBP), on which every consistent set of mathematical axioms truly describes a mathematical universe.[100]

We start our mathematical reasoning with numbers. Sets are a logical reconstruction of the mathematical universe. In empirical science, we may eventually have to choose between elementary particles and observables: either there are trees or there are the component elements which combine into a tree, and 'tree' is just a convenient manner of speech. Or we may admit both trees and their component particles. In mathematics, we can have sets, and numbers, and spaces.

Quine could accept mathematical diversity. Consider his comments on the failure of logicism:

> If... the concepts of mathematics were all reducible to the clear terms of logic, then all the truths of mathematics would go over into truths of logic; and surely the truths of logic are all obvious or at least potentially obvious... This particular

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[100] See Balaguer (1995) and Balaguer (1998), Chapters 3-4. Balaguer originally called his position full blooded platonism, hence the acronym.

> outcome is in fact denied us, however, since mathematics reduces only to set theory and not to logic proper. Such reduction still enhances clarity, but only because of the interrelations that emerge and not because the end terms of the analysis are clearer than others. As for the end truths, the axioms of set theory, these have less obviousness and certainty to recommend them than do most of the mathematical theorems that we would derive from them. (Quine (1971) p 70)

Quine's point is that we can not consider the reduction of mathematics to set theory as establishing certainty for mathematics. Nothing could, for Quine. But Quine indicates a more salient insight about translations between set theory and other mathematical theories, that such "reductions" enhance clarity by indicating interrelations, but do not reduce, or eliminate, our commitments to abstract objects.

Similarly Quine's rejection of formalist disinterpretation of mathematical theory demonstrates his acceptance of mathematical properties in their own rights. On the disinterpretation view, which he attributes to Russell,

> Pure number, pure addition, and the rest would be viewed as uninterpreted; and their application, then, say to apples, would consist perhaps in interpreting the numbers five and twelve as piles of apples, and addition as piling them together. I find this attitude perverse. The words 'five' and 'twelve' are at no point uninterpreted; they are as integral to our interpreted language as the word 'apple' itself. They name two intangible objects, numbers, which are *sizes of* sets of apples and the like. (Quine (1978a) p 149)

While we may thus excise Quine's reductionism, indispensabilism is awkward paired with mathematical diversity. The indispensability argument is based on the notion that we do not admit objects into our ontology unless they are required. All methodology is empirical scientific methodology. The indispensabilist takes austerity as a central principle of theoretical construction, and thus is unfriendly to diversity.

§3.8: Conclusion

None of the problems I have discussed in this part of the chapter are decisive against QI. This is clear from the fact that both Putnam and Colyvan welcome certain characteristics of the objects to which the proponent of QI is committed. Still, the characteristics I described show that QI does not yield traditional mathematical objects.

We look to the mathematician to determine the extent of the mathematical universe. Mathematical objects are presumed to be abstract, to exist necessarily, outside of space and time. We ascribe an a priori epistemology to mathematics, at least in part. The objects to which the indispensabilist is committed are not really mathematical objects, even if they are the posits of set-theories like ZF. Just as we can model at least portions of the axioms of number theory with obviously concrete objects, like grains of sand, we can model the axioms of set theory with the indispensabilist's concrete sets. Quine urges a doctrine of gradualism from observables like trees, through subvisible objects like electrons, to space-time points and sets. Just as space-time points are not mathematical objects, neither are the indispensabilist's sets. They are empirical posits.

Since the arguments of this part of the chapter grant the false QI.3, they are really moot. But, they will arise again in Chapters 3 and 4, where I discuss problems with other, non-Quinean, versions of the indispensability argument.

Chapter 3: Other Holistic Indispensability Arguments

§1: Introduction

In Chapters 1 and 2, I focused on what I consider to be the strongest indispensability argument, based on Quine's procedure for determining ontic commitment. In this chapter, I consider more casual appeals to the uses of mathematics in science, ones which do not depend on the construction of formal theory, to account for mathematical knowledge. I look especially at the structuralism of Michael Resnik and Stewart Shapiro, which relies on Quine's holism. In the next chapter, I examine Putnam's non-holistic indispensability argument and Resnik's pragmatic indispensability argument, which also rejects Quine's holism.

In this chapter, I first distill the indispensability argument to its essential characteristics, and note the unfortunate consequences that result. Then, I argue that a common form of structuralism embodies an indispensability argument, and suffers from its failings.

§2: The Essential Characteristics of Indispensability Arguments

The following are Essential Characteristics of any indispensability argument for mathematics.

EC.1: Naturalism: The job of the philosopher, as of the scientist, is exclusively to understand our sensible experience of the physical world.

EC.2: Theory Construction: In order to explain our sensible experience we construct a theory of the physical world. We find our commitments exclusively in our best theory.

EC.3: Mathematization: We are committed to some mathematical objects and/or the truth of some mathematical statements, since they are ineliminable from that best theory.
EC.4: Subordination of Practice: Mathematical practice depends for its legitimacy on empirical scientific practice.

It follows from Naturalism that we never need to explain mathematical phenomena, like the existence of unexpectedly many twin primes, for their own sake. Ultimately, the justification for any mathematical knowledge must appeal to an account of our sense experience.

Theory Construction indicates a general source of ontic commitment, but does not settle a particular procedure for determining commitments. If we drop QP, we need another method. Some indispensabilists leave their methods obscure, or implicitly rely on Quine's procedure. Theory Construction rules out independent appeal to an autonomous mathematical theory for justification of mathematical claims.[101]

Mathematization is an empirical claim about the needs of theory construction. I call this claim empirical since it is an open, and it seems to me empirical, question whether we can formulate nominalist alternatives to all good scientific theories, including ones yet to be formulated. The nature of these alternative theories depends on our procedure for determining ontic commitment. The indispensabilist relies on speculation that acceptable nominalist alternatives are impossible.

[101] One might think that an indispensabilist may also admit an autonomous epistemology for mathematics. He would seem to have two routes to mathematical knowledge, one independent of empirical science, and the other relying on it. But, the former route would do all the real work. Science might explain our beliefs, but it would not justify our knowledge.

Subordination of Practice is implicit in the other characteristics, but it nicely emphasizes the relationship between mathematics and empirical science for the indispensabilist. Dropping EC.4 from an indispensability argument would entail either adopting an alternate justification for mathematical knowledge or denying that mathematical practice yields any commitments.

§3: The Unfortunate Consequences, and their Links to the Essential Characteristics

The following are Unfortunate Consequences of any indispensability argument:

UC.1: Restriction: Our commitments are to only those mathematical objects required by empirical science. Mathematical results which are not applied in scientific theory are illegitimate.
UC.2: Ontic Blur: Mathematical objects are concrete.
UC.3: Modal Uniformity: Mathematical objects do not exist necessarily.
UC.4: Temporality: Mathematical objects exist in time.
UC.5: Aposteriority: Mathematical objects are known a posteriori.
UC.6: Uniqueness: Any debate over the existence of a mathematical object will be resolved by the unique answer generated by empirical theory.

In the previous chapter, I showed how these Unfortunate Consequences arose from QI. In this section, I discuss them as results of any indispensability argument.

Restriction follows from Mathematization and Naturalism, which implies that there is no non-empirical way to justify mathematics. Supplementing the indispensability argument to justify unapplied results, e.g. by appeal to a priori intuition or logicism, renders the original argument superfluous. It is difficult to say precisely which mathematical objects the indispensability argument would justify, i.e. how much mathematics science actually needs. The point at which the indispensabilist draws the line is unimportant. What is relevant is the existence of a gap. Burgess and Rosen offer the following: "It has been the received view and expert opinion among competent logicians since the 1920s that the mathematics needed for applications can be developed in a theory known as mathematical analysis, in which the only entities mentioned are real numbers." (Burgess and Rosen (1997) p 76)

Ontic Blur follows from Theory Construction. No indispensabilist can differentiate between abstract and concrete objects. He may call some objects abstract and others concrete, but these are empty labels, for the indispensabilist. All commitments are made in the same way, for the same purpose, to account for sensory experience. We should classify the indispensabilist's purported abstract objects with the concrete objects they are used to explain or describe.

Independently of the indispensability argument, we can establish a criterion for abstractness by distinguishing the disciplines of mathematics and empirical science. Since the epistemology for mathematics is separate from that of empirical science, and the ontologies are distinct, the claim that mathematical objects are abstract is plausible.

Modal Uniformity follows similarly. Mathematical objects are posited to account for our experience of a world which exists contingently. If the world were different, it would require different objects. Suppose, for example, that charge really is a continuous property of real particles in this world. The indispensabilist alleges that the world thus contains continuous functions. Further, suppose that in a different possible world, there are no continuous properties. In that world, says the indispensabilist, there are no continuous functions.

Modal Uniformity is ironic since the indispensabilist's claim that mathematics can not be excised from science includes an appeal to modality. All hope for modality is not lost for the indispensabilist, though. For, there are several notions of necessity. When one asserts that the world is possibly Newtonian, even if relativistic, one relies on a weak, physical necessity, on which phenomena in accord with scientific laws follow from them necessarily. A statement may also be logically necessary, which may be construed as entailing a contradiction when negated, or as being either a logical law or following from one. Any Quinean will be wary of any modality besides logical necessity. Even more strongly, a statement may be metaphysically necessary, or true in all possible worlds. Saul Kripke alleges that some identity statements, ones flanked by rigid designators, like the identity of water and H_20, are metaphysically necessary.

Some naturalists claim insight into metaphysical necessities. Perhaps the indispensabilist could similarly claim some necessity among mathematical claims. But, firstly, naturalism would seem to debar such claims. Explanations of Kripkean metaphysical necessities tend to rely on a non-naturalist a priori intuition. Furthermore, even if some mathematical identity statements are metaphysical necessity, this does not establish that mathematical objects exist necessarily.

By linking mathematics to the physical world, the indispensabilist may retain the weaker notion. Unfortunately, the weaker notion is not the one traditionally imputed to mathematics, and is unsatisfactory. It would follow that under a different set of physical laws, two and two might not equal the square root of sixteen.[102] While this idea may be

[102] I assume that '2+2' and 'the square root of sixteen' do not rigidly designate.

alluring to some, it seems absurd. Only a stronger necessity will appease intuitions that mathematical truths are broader than physical ones.

Temporality follows quickly from Modal Uniformity. For, if mathematical objects exist contingently, then there can be a time when they do not exist. For example, if the existence of continuous functions depends on the existence of continuous physical quantities, as the indispensabilist claims, then if the physical quantities were to be extinguished, the mathematical functions used to represent those quantities would have to be removed from our list of commitments. Again, mathematical objects are traditionally atemporal. "It would betray a confusion to ask, 'When did (or when will) these primes exist? At what time may they be found?'" (Burgess and Rosen (1997) p 21)

While it is traditional to ascribe to mathematics an a priori methodology, the indispensabilist only provides an epistemology for empirical science. This single epistemology also entails that the indispensabilist's mathematical objects are, like concrete objects, known a posteriori. Indeed, many indispensabilists, like Quine, are motivated by a desire to avoid a priori epistemology.

Lastly, Uniqueness arises from Subordination of Practice. In Resnik's terms, the indispensabilist's appeal to Euclidean rescues is limited. When relativity supplanted classical mechanics, the structure of space-time was discovered to be non-Euclidean. A Euclidean rescue defends both the new mathematical theory and the old one despite the change in physical theory. All three possibilities concerning the parallel postulate are consistent with unfalsified geometric theories.

We can perform a Euclidean rescue any time a mathematical theory fails to apply in science. In such cases, the indispensabilist generally rejects the mathematics, or, like Quine, demotes it to recreation due to a lack of empirical evidence. The traditional response is the Euclidean rescue, unless the mathematics is shown inconsistent.

Consider two conflicting mathematical theories, like ZF + CH and ZF + not-CH, or Peano arithmetic with and without its Gödel sentence. It is possible, in these cases and others like them, that each of the conflicting pairs will find some physical application. This application need not be to any fundamental situation; any application will do. In such cases, no Euclidean rescue is necessary. This is also what happens when Euclidean, Riemannian, and Lobachevskian geometries all find application to the physical world.

Failing application, the indispensabilist must demote the mathematical theory to secondary status. If there were no scientific uses for spherical geometry, the indispensabilist could see the pure mathematical theory as describing a possible world in which there were spheres. The indispensabilist's Naturalism, though, insists that such pure theories are, as Quine put it, "Without ontological rights." In such cases, the traditional mathematician may multiply universes. Perhaps there are multiple set-theoretic hierarchies; in some the continuum hypothesis holds while in others it fails, and in multiple different ways. On the contrary, the indispensabilist, who is committed to austerity in abstracta, can not easily accommodate conflicting mathematical theories.

As a corollary, the indispensabilist should also insist that the adoption of classical mechanics justified the Euclidean geometry on which it is based. Euclid's geometry might have had some plausibility before Newton, but the lack of a comprehensive physical theory which employed it as its theoretical basis meant that it could have been pure speculation, or platonic idealism. This claim is implausible. The status of Euclidean geometry remained unaffected by the formulation of Newtonian mechanics.

As a last illustration of the problem with Uniqueness, consider the introduction of complex numbers, as solutions to quadratic equations with missing real roots. So-called imaginary, or impossible, numbers were derided, despite their mathematical uses. They simplified mathematics, since ad hoc explanations about why certain quadratic equations had two roots, others just one, and others none, were avoided. A fruitful field of study was born with geometric, graphical representations. The theory of complex numbers was not found to contain any inconsistency, aside from the conflict with a presupposition that all numbers were real numbers. Physical applications were later discovered, for example in representing inductance and capacitance as the real and imaginary parts of one complex number, instead of as two distinct reals.

For the mathematician, the legitimacy of complex numbers came early. The indispensabilist, prior to the discovery of their applicability, could make no room for them. Even the analogy with negative numbers, which arose from similar disgrace, serves as no argument for the indispensabilist. Lacking application, work with complex numbers was just mathematical recreation.

We can be sure that mathematicians working today in the farthest reaches of pure mathematics do so without knowing that their work has any physical application. One may arise, or their work may lie fallow. If the only justification for mathematics is in its application to scientific theory, then unapplied results are unjustified, even if they may eventually be useful. The indispensabilist is saddled with Subordination of Practice, which makes the mathematician depend on the scientist for the justification of his work.

We saw these general claims about indispensability arguments applied to QI in the third part of Chapter 2, where I said that they are not decisive against the indispensabilist. In this chapter and the next, I show how these Unfortunate Consequences apply to other indispensabilists. Again, I find independent reasons to reject their arguments.

§4: Semantic Indispensability

Mark Colyvan refers to an indispensability argument based on the needs of semantic theory. We might believe in abstract objects because we need them for semantics. Specifically, we might want a theory on which sentences which are structurally equivalent, but which differ in that some refer to abstract objects in places where others refer to abstract objects, are treated uniformly.[103] If reference to some class of entities is indispensable to our best semantic theories of natural and scientific languages, then we ought to believe that those entities exist. Abstracta are indispensable to semantic theory, so we ought to believe in them. In this section, I show that such an indispensability argument is unlikely to be successful. Semantic theory is generally a quagmire, and I have avoided talk of semantics, but so much is made of the relationship between commitments to abstract objects and uniform semantics, that Colyvan's argument warrants a moment.

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[103] Colyvan cites Crispin Wright, Bob Hale, Steven Wagner, Bernard Linsky and Ed Zalta as holding versions of this argument, at Colyvan (2001), p 15. See also Katz (1998), Chapter 5.

The semantic indispensability argument could yield more objects than the scientific one, if there are abstract objects for every predicate. It thus avoids the problem of Restriction. Colyvan argues that it yields too many objects, including the round squares needed to make sense of 'The round square is round'. The argument eschews the robust sense of reality which Russell urged us to maintain in these cases. Colyvan also devalues this argument because, echoing Devitt's maxims, semantics is less credentialed than science.

The weakness of our commitments to any particular semantic theory, as well as our general lack of certainty that we will demand a uniform semantics, does weaken this argument. The acknowledgment that a uniform semantics demands abstract objects only yields a disjunction: we can either have uniform semantics or no abstract objects.

More importantly, the demand for a uniform semantics is no reason to adopt an indispensability argument. Rather, it is only an argument to believe in semantic objects like properties and propositions. Any defense of abstract intensions allows a uniform semantics. A traditional, i.e. non-indispensabilist, account may be a better route than the indispensability argument, because it does not suffer from the Unfortunate Consequences.

In the remainder of this chapter, I show that the structuralist, if he is to account for mathematical knowledge, relies on an indispensability argument.

§5: Sentence Realism, Formalism, and Structuralism

In this section, I characterize a form of structuralism, and show how it relies on an indispensability argument. I use 'structuralism' to refer to a limited number of positions which might be called structuralist. I use it to refer to a philosophy of mathematics which commits to structures, or patterns, defined by mathematical theorems.[104] Some mathematical realists are object realists, since they focus on the existence of mathematical objects. The structuralist is a sentence realist, focusing on the truth of axioms and theorems of mathematics. The structuralist can either eschew mathematical objects, or derive them from structures.

The structuralist adopts his position to gain advantage in describing how we can have knowledge of abstract objects or of truths which refer to abstract objects.[105] The indispensabilist solves this problem by referring all justification to empirical theory. The structuralist adopts an indispensability argument when he refers to empirical evidence to justify knowledge of structures.

Structuralism, as a philosophy of mathematics, became popular in the wake of Benacerraf (1965) as an attempt to dissolve the problem of choosing which sets are the numbers. Benacerraf's problem was that different sets of sets can model the Peano axioms, and nothing can tell us which one of these sets is the right one. It seemed, to Benacerraf and others, that object realism commits us to indeterminacy. In an attempt to avoid the Benacerraf problem, the structuralist claims that all that matters is the structure

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[104] Anti-realist versions of structuralism are possible, as well as ontically neutral ones. See Hellman (1989). These are not my concern, here.

[105] The contemporary formulation of this problem appears in Benacerraf (1973). Katz argues that mathematical anti-realists ignore the corresponding epistemic problem for empirical science, though Benacerraf made it clear that both sides have a problem. See Katz (1998), Chapter 2.

defined by the axioms. We need not extend our commitments to any particular set of objects for modeling the theory. Since the structuralist is committed only to the truths of the axioms and theorems, the problem of indeterminacy among objects is dissolved.

There are other ways around Benacerraf's problem. A sui generis solution avoids the problem of multiple set-theoretic models of the Peano axioms by denying that we should expect any set-theoretic model to be uniquely correct. Number-theoretic axioms are modeled by the numbers, and there are various set-theoretic dopplegangers.

Additionally, structuralism generates problems of its own. A structure is merely a more complex abstract object, just like the set of numbers. We need an account of our access to these structures. Furthermore, within structures, there are nodes, or points. Within each member of the class of structures defined by the Peano Axioms, for example, are positions which correspond to each of the natural numbers. If we have knowledge of the structure, then we would seem to have knowledge of the positions in the structure, and thus have knowledge of individual mathematical objects after all. The structuralist, in order to resolve these difficulties, may appeal to our empirical knowledge. When he does so, he relies on an indispensability argument.

Before I get to the details of structuralism's reliance on indispensability, I put the program in a bit more historical context. When Quine embraces Restriction, he distinguishes between mathematics proper and mathematical recreation. Quine opposes the formalist, or if-then-ist, who presents a pure/applied distinction to accommodate non-Euclidean geometry. Quine's formalist interlocutor has abandoned the claim that any of the three independent geometries are the true geometry. Instead, the formalist merely notes the consistency of each of the incompatible axiomatizations, and claims that those systems, characterized by their axioms, should be regarded as un-interpreted.

The original formalist project was shown unsustainable, mainly by the Gödel results. The structuralist inherits the formalist's focus on sentences rather than objects. Both claim that our commitments to the theorems of mathematics do not necessarily entail commitments to the objects to which the singular terms in those statements apparently refer. The formalist argues that mathematical theories can not be true, and thus can not refer, and the structuralist denies that we should take the mathematical terms to indicate reference to particular objects.

Quine rightly urges, against the formalist, that mathematical theories must be interpreted. But the only interpretation an indispensabilist can provide must at root depend on the physical world. Quine invokes the sharpened tools metaphor to explain the origins of mathematical systems. The physical world provides certain approximate regularities: the ratio of the circumference of a circular inscription to its diameter, for example, or the additivity of many groups of objects. Formal mathematical systems regiment idealizations of these physical facts. Different interpretations of these sharpened tools leads to the deviant urge to conceive of those systems as un-interpreted; but they are interpreted, even if variously.

Resnik and Shapiro, separately defending versions of structuralism which rely on indispensability, essentially elaborate Quine's sharpened tools metaphor. They return to the formalist's sentence realism, but adopt Quine's commitment to using only the physical world as a model of the axioms.[106] Their versions of structuralism are, in relevant ways, variants of Quinean indispensability and so suffer from the same Unfortunate Consequences.

The epistemic problem facing mathematics arises whether the objects in question are numbers or structures. If the structuralist commits only to concrete structures, he has not generated mathematical objects. If he purports to generate mathematical objects, he must account for our access to abstract structures. Shifting the focus of the debate does not remove the challenge.

So, the problem facing the structuralist is how to account for our knowledge of structures. Resnik tries to solve the problem, in part, by denying that structures are objects.[107] He provides no criteria for identity among structures, for example.[108] Resnik argues that we should expect that structures are not objects in the same way that we expect numbers not to be sets. There is no fact of the matter about whether two structures are identical, or whether a position in one structure is identical to a position in another. He maintains, though, that positions in patterns are mathematical objects, like numbers, and that these exist.

Denying that structures are objects comes at the cost of any explanation of how

[106] Some structuralists, e.g. Shapiro, accept positions in structures as objects which may model the mathematical axioms. Insofar as they do so, they abandon structuralism as a solution to the epistemic problem at hand.

[107] Denying that structures are objects also helps Resnik avoid a problem of pattern self-inclusion. See Resnik (1997) p 256.

[108] Quine demanded no entity without identity. See Jubien (2004) for concerns about this criterion.

we know about mathematical objects. For, if we can not know about structures as objects, then we need a separate account of how we know about their positions. Resnik can not make any headway in accounting for mathematical knowledge by denying that structures are objects.

By claiming that we derive our knowledge of mathematical objects from structures, Resnik opens himself up to two additional problems. He can not differentiate among isomorphic patterns, as those defined by various Peano systems, by appeal to the natures of the individual objects, as they have none except those they get from their positions in structures. And he is committed to the equal status of all models of a theory, including nonstandard ones. This makes the previous result even more counter-intuitive.

Resnik buttresses his account of our mathematical beliefs with an historical description of our experience with concrete structures. He speculates that our ancestors had a basic understanding of concrete patterns, like a template for drawing or a chessboard. They generalized, or idealized, to the notion of an abstract, underlying structure. Their ideas about structures and their relations applied both to concrete and abstract instances, as do our own. "*Pattern congruence* is an equivalence relation whose field I take to include both abstract mathematical structures and arrangements of more concrete objects." (Resnik (1997) p 204) The abstract structure forms the basis of classical mathematics, but we need not posit any access to this structure beyond our physical, causal connection to token patterns.[109]

Maddy, finding the indispensabilist account of our knowledge of mathematics

[109] MacBride (2004) calls Resnik's account "historically perverse" (p 315).

unsatisfactory, proposed actual perception of abstracta.[110] Resnik avoids committing to an implausible Maddy-esque perception. He portrays the introduction of abstracta as an implicit or explicit posit, on the road from actual to possible concreta.

Resnik justifies the leap to abstract objects by its fruitfulness. "I hypothesize that using concretely written diagrams to represent and design patterned objects, such as temples, bounded fields, and carts, eventually led our mathematical ancestors to posit geometric objects as sui generis. With this giant step behind them it was and has been relatively easy for subsequent mathematicians to enlarge and enrich the structures they knew, and to postulate entirely new ones." (Resnik (1997) p 5)

Resnik's quasi-historical account of mathematical beliefs shows how the structuralist attempts to erode the abstract/concrete distinction to establish ontic blur. If the line between abstract and concrete objects is eroded, then an account of those elements of our ontology ordinarily deemed abstract may be continuous with our account of those we call concrete.

Resnik admits that there is a difference between mathematical and scientific positing, and notes that scientists posit to explain previously observed phenomena, and devise experiments to detect new posits. In contrast, we have a local conception of mathematical evidence, which relies on proof, computation, and logic. "In practice, when justifying a mathematical claim, we hardly ever invoke such global considerations as the benefits to natural science. We ordinarily argue for pieces of mathematics locally by appealing to purely mathematical considerations." (Resnik (1997) p 6)

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[110] See Maddy (1990).

But, Resnik argues, observation is also relevant to mathematics.

> It would be wrong to conclude from its possessing a local conception of evidence that mathematics is an apriori science, disconnected evidentially from both natural science and observation. ...[W]hen supplemented with auxiliary hypotheses, mathematical claims yield results about concretely instantiated structures, such as computers, paper and pencil computations, or drawn geometric figures, that can be tested observationally in the same way that we test other scientific claims. (Resnik (1997) p 6)

For Resnik, even simple acts like counting become confirmation for mathematical theories. "Practice with counting, measuring, surveying, and carpentry suggested and confirmed the elementary rules of practical arithmetic and geometry long before they were elevated to the status of inviolable laws and codified into mathematical systems." (Resnik (1997) p 48)

Quine, and Field, pushed the question of whether mathematics was necessary for scientific theory to the outer regions of science: do we need mathematics for quantum physics, or for special relativity? The structuralist suggests that even more mundane experiences, as of the pattern of a chessboard, can justify mathematical knowledge. Resnik appeals to all sorts of scientific uses for mathematics, from physics and chemistry to economics, using techniques from measurement to statistical inference. His abstract mathematical structures are defined by theories which arise from consideration of empirical phenomena.

Resnik alleges that we test mathematical theories as directly as we test scientific theories. We reduce their empirical content. We can construct Turing machines. While we can not test all mathematical theories empirically, we can not test all scientific ones, either, like the hypothesis that space is continuous.

Shapiro also tries to blur the line between mathematics and science in order to support structuralism. He (1983b, 1997) presents structuralism as primarily an account of the application of mathematics to physical theory. He argues that the traditional philosophies of mathematics (i.e. formalism, logicism, traditional platonism, and intuitionism) provide either no explanation of the relationship between mathematics and physical reality or an unsatisfactory one. According to Shapiro, structuralism solves the problem, in part by providing a more holistic view of the relationship between of mathematics and science. We have experience with concrete instantiations of structures, and this provides insight into the abstract natures of structures. “My view is that, extensionally speaking, there is no difference, or at any rate no philosophically illuminating difference [between mathematical structures and other kinds of structures].” (Shapiro (1983b) p 542)

If there is no difference between these two types of structures, we can infer knowledge of mathematical structures on the basis of our knowledge of physically instantiated structures. The inference to mathematical knowledge from knowledge of the physical world is the fundamental claim of the indispensability thesis.

Both Shapiro and Resnik move from the instantiation of mathematical structure in the physical world to our knowledge of abstract mathematics. The structuralist says that abstract mathematics applies to physical science because mathematical structures are exemplified in the physical world.

The structuralism I am examining posits no epistemology for mathematics other than that which we already need for our knowledge of the empirical world. It satisfies all the Essential Characteristics EC.1 - EC.4, though it may restrict Mathematization to truth for sentences, and thus is saddled with all the Unfortunate Consequences. Though the structuralist avoids Uniqueness for some questions, like the identification of numbers with sets, he can not avoid committing to a fact of the matter about others, like that of the status of non-Euclidean geometries. For any indispensabilist, only the geometry used in our best physical science is justified.

Attempting to make lemonade out of lemons, an indispensabilist may embrace some of the Unfortunate Consequences. In the next section, I show how Resnik tries to argue for Ontic Blur. He also denies that the structuralist's limited ontology, its Restriction, is a problem. "[Science] falls short of affirming the existence of many of the entities studied by the far reaches of contemporary mathematics. I do not find this a drawback at this point..." (Resnik (1993) p 58)

It is a drawback, and Resnik later recognizes it. "Axioms limiting the size of the set-theoretic universe would discourage the development of mathematics through limiting the structures it recognizes. Furthermore, while limiting the variety of structures would probably not hinder contemporary science, it might hinder future science. So the good of neither mathematics nor science as a whole calls for adding limitative axioms to set theory." (Resnik (1997) p 147)

While Quine embraced Restriction, distinguishing mathematical recreation from legitimate mathematics, Resnik attempts to justify unapplied results. "I would advocate a liberal version of holism, one which would allow the development of a local conception of mathematical evidence that could countenance mathematical truths that have no foreseeable empirical use." (Resnik (1997) p 131)[111]

Resnik slides too quickly from a limited claim to mathematical objects to a more complete mathematical universe. "[I]f we do take the plunge and countenance limit entities, then we could also countenance numbers, linguistic types, and a host of other abstract entities our templates represent more adequately than they represent limit entities." (Resnik (1993) p 56)

An indispensabilist can claim only those objects necessary to scientific theory, in this case to our understanding of concrete structures. Resnik argues that restrictions might limit future science. If future science demands an extension, the restriction in this case will be eased. For now, we do not know which results might be needed. Despite his efforts to avoid it, Resnik's structuralist suffers from Restriction.

In this section, I have described a structuralist position which is defended by Resnik and Shapiro, and showed how it relies on an indispensability argument to account for mathematical knowledge. Resnik recognizes some of the drawbacks of reliance on indispensabilism, and responds in two ways. In some cases, he accepts the consequence, but minimizes its importance. We saw this approach to Restriction. He takes a similar approach with Aposteriority, and argues that pragmatic rationality encourages us to treat mathematics as if it were a priori even if it is not.[112]

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[111] Maddy (1992) defends a view similar to Resnik's liberal holism. I discuss the problems with this approach in Chapter 5, §1.2.2.

[112] Colyvan, too, argues that we may mistake mathematical truths for logical ones and thus mistake a posteriori truths for a priori ones. He attempts to explain the

Resnik's other approach, which he shares with Shapiro, is to embrace Ontic Blur. Their strategy is to show that blur is to be expected and so while it is a consequence of structuralism, it is not an unfortunate one. In the next, extended, section, I deny that we should expect ontic blur.

§6: Structuralism's Supplement: Ontic Blur

At first glance, the structuralist's account of mathematics based on sensory experience of concrete patterns seems as unlikely to be fruitful as approaches which start with apprehension of discrete physical objects, or inscriptions of rough shapes. All of these approaches stumble on the leap from knowledge of concreta to knowledge of abstracta.

Resnik adds an argument for ontic blur to bridge the gap. If concrete and abstract objects are no different in kind, then there is no leap to be made. Our knowledge of abstract objects would be like that of concrete objects. If the structuralist can establish Ontic Blur, concomitantly no objects exist necessarily, and an argument for UC.3, Modal Uniformity, is also established. Similar arguments hold for UC.4, Temporality, and UC.5, Aposteriority.

Central to Resnik's argument for blur is his connection between physical explanation and mathematical objects. "Mathematical facts and properties of mathematical objects play essential roles in physical explanations themselves." (Resnik

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obviousness of mathematics in terms of the obviousness of logic.

(1993) pp 42-3) Consider a ball thrown straight up in the air. The explanation of why it stops and turns around at a particular point, when the vector sum of the upward and downward velocities equals zero, relies on mathematical properties of the ball's velocity. "Moreover, this velocity itself, being a function, is a mathematical object. So, the explanation uses mathematical objects and their properties to explain the behavior of a physical thing." (ibid, p 43)[113]

Resnik uses this example to argue that mathematical objects not only play a role in a physical explanation, but they have efficacy in the spatio-temporal world. Physical objects and mathematical ones are linked in the world, and Balaguer's PCI is false.

The question of whether mathematical objects can participate in physical events raises the question of what the mathematical objects are. We can agree on some core objects: numbers, sets, and geometric points. Resnik extends the list, though his claims conflict with our ordinary conception of mathematical objects. He claims that velocity, quantum particles, and fields qualify as mathematical.[114] "It can be unclear whether a given explanatory object (for example, a field) is physical or mathematical, or even whether something counts as physical behavior (for example, the collapse of a field)." (Resnik (1993) p 45)

I examine Resnik's arguments that traditional attempts to distinguish between mathematical and physical objects are unsatisfactory. He argues that mathematical objects may change their properties, participate in events, and be located in space and

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[113] Shapiro makes the same argument in Shapiro (1983b).

[114] Compare with Putnam's use of 'mathematical', discussed in Chapter 2, §3.6.

time, just like physical objects. I show how mathematical objects are unlike physical objects, thus refuting the purported blur.

§6.1: Mathematical Objects and Property Change

In addition to existing necessarily, mathematical objects are traditionally distinguished from physical objects by having their properties necessarily. Resnik argues for blur by arguing that mathematical objects, like physical ones, can change properties. I argue that while some properties of mathematical objects may change, this does not block the distinction between mathematical and physical objects.

Resnik presents an example of how a number can change its properties. "Just as Smith may be thin as a child, and not as an adult, the number 60 may register Smith's height in inches at age 12 and not at a later age." (Resnik (1997) p 108)

Even if we accept such Cambridge properties as real, we can still distinguish between mathematical and physical objects on the basis of property change. Mathematical objects have many properties that do not change: seven is unalterably the square root of forty-nine, the sum of four and three, and prime. The mathematical properties of seven do not change, even if it ceases being my daughter's favorite number. Physical objects do not have eternal, mathematical properties.

We should not consider at least some of the properties in question real properties, anyway. Resnik argues that attempts to refine the notion of property in order to rule out changeable properties of mathematical objects are doomed to failure. For example, one might denigrate all relational properties. Resnik alleges that this would define electrons, which have all their properties relationally, out of existence. "[E]lectrons... have properties only by virtue of their relations to other particles." (Resnik (1997) p 108)

Electrons do have many relational properties, like charge and spin, which are calculated relative to other electrons. They also have calculable mass and velocity, which are more plausibly considered as independent of other electrons. Even if all our knowledge about electrons comes from their relation to other objects, at least some of their properties may still be non-relational. One might be led to believing that all properties of electrons are relational since we have no direct perception of them. We only know of isolated electrons through their relations to observational devices. Still, properties of electrons may be independent of our observations.

The properties of mathematical objects which change are extrinsic, unlike their eternal mathematical properties. Katz introduced the notions of intrinsic and extrinsic relations of numbers. "[T]here is no intrinsic change in the number seventeen when someone stops thinking of it and starts thinking of the number eighteen, but only an extrinsic relation between the person and the realm of numbers..." (Katz (1998) pp 136-7)

Some properties, like this epistemic one, are extrinsic. The discovery of Euler's constant does not change it, even though on the most broad notion of properties we can ascribe a property change from being unknown to humans to being known. Similarly, the property of marking Smith's height-in-inches at age 12 is extrinsic to 60. A proper theory of properties might focus only on intrinsic properties, and rule out extrinsic ones.

Lacking a complete theory of properties, I remain agnostic. One might develop a successful theory of properties on which mathematical objects really do have some changeable properties. Still, we can distinguish mathematical and physical objects by the wealth of properties mathematical objects have which do not change. A successful argument for ontic blur must be found elsewhere.

§6.2: Mathematical Objects and Participation in Events

Resnik argues that mathematical objects participate in the events which we describe with their indispensable help. But the objects he considers are not the pure mathematical objects at issue. If we consider paradigmatically mathematical objects, such as numbers and circles, Resnik's claim that mathematical objects participate in events is clearly false. Resnik considers other objects, of three kinds: 1) Physical functions, such as velocity; 2) Subatomic particles; 3) Fields.

Taking the velocity function first, Resnik concedes that a nominalizing project for functions might show that they do not participate in events. There would be no need to posit them. Still, he argues, even if we can dispense with mathematical objects in our explanations of notions like velocity, "We have no reason to think that [such a nominalizing project] can be done with events involving subatomic particles, whose basic features, such as charge, spin and energy level, correspond to no commonsense ideas." (Resnik (1993) p 45)

Nominalizing projects, Resnik implies, may only work when the concepts to be nominalized correspond to commonsense ideas. This suggestion is implausible. If the general public were better educated about subatomic physics, these features would be commonsensical.

Setting aside the question of dispensabilist reformulations of science, thinking about functions such as velocity as mathematical objects is odd in two ways. First, just as we distinguish between pure set theory, and set theory with ur-elements, we can distinguish between purely mathematical objects and applied ones. The velocity function is an applied mathematical object. Pure mathematical objects do not participate in events, and it is our knowledge of pure mathematical objects which requires an account.

Second, even if we were to take the velocity function as a mathematical object, we should distinguish between velocity itself and the velocity function. Consider the ball thrown directly upward. Resnik claims that the explanation of the ball stopping its ascent and returning downward requires reference to mathematical objects, including the velocity of the ball, and the vector sum of the upward and downward velocities. Resnik is right that our description of the event refers to the velocity function. But there is no object named 'velocity' which participates in the event. The ball participates in the event. The air and the Earth participate in the event. The hand which tosses the ball participates in the event. At most velocity is a causally relevent property of an object, not an object itself.

In formulating descriptions of events, we often refer to mathematical objects as a result of reifying properties like velocity in order to simplify our descriptions. If the indispensability argument is right, then we should conclude that mathematical objects exist. Still, this would not show that a mathematical object velocity participates in any events.

Resnik admits that this first case, physical functions, is the weakest of the three.

The second and third examples of mathematical objects participating in events collapse, since quantum particles are, for Resnik, like mathematical objects due to their relations to fields. An electron might be better understood as a manifestation of a field at a point, and the field may be plausibly construed as a distribution of probabilities. "This suggests to me that quantum fields straddle the border between mathematics and physics. Under certain conditions they have 'observable' physical properties, under others they are little different from functions from space-time to probabilities." (Resnik (1997) p 104)

If we construe space-time substantivally, then even if we take a field to be a function from space-time to probabilities, it is no more a mathematical object than the velocity of a moving train. Even if we do not take space-time substantivally, the field is located in space and time.

Balaguer agrees that Resnik's position conflates the mathematical properties of an object with the object itself. "I do not see why a full-blown realist about quantum fields and superposition states cannot maintain that while fields can be represented by probability functions, they are not functions themselves." (Balaguer (1999) p 115)

Resnik adduces another example from quantum mechanics as evidence of the mathematical nature of quantum objects. He cites David Bohm's interpretation of a particle's wave function as physically significant, as a force field which guides the particle's trajectory. This wave function splits into parts, with only one part following the particle. "The remaining parts of the wave function/force field are completely undetectable, are causally inert, and have no effects on other particles... Bohm's proposal blurs the distinction between mathematical and physical objects because the vacant parts of the wave function are undetectable and causally inert." (Resnik (1997) p 105)

Mathematical objects share the properties Resnik describes, but this does not make the vacant parts of the wave mathematical objects themselves. They are still parts of the field of a particular particle, and located in space and time, or at least in time, if in superposition. Resnik admits, "But presumably they are located in space-time and thus not fully abstract." (ibid) There is no reason to think they are abstract at all, as long as they are located in space-time.

Resnik is clearly right that quantum particles and fields participate in events. But they are not mathematical objects. He correctly insists that mathematics is relevant in describing fields. But even the indispensable use of mathematics in describing an object is no indication that the object is mathematical. Our best scientific explanation of the behavior of the table in front of me will appeal to its mathematical properties. It is one table, with four legs, of dimensions we can describe geometrically and arithmetically. Even its color properties are describable, at least in part, by wavelengths of light reflected by its surface. The possession of mathematical properties is thus no indication that an object is mathematical, and is no indication of ontic blur. For an object to be mathematical, it must lack physical properties altogether.

Many properties of fields and quantum particles are mathematically describable. Resnik concludes that, "Fields seem to be hybrid entities hovering between the paradigmatically mathematical and the paradigmatically physical." (Resnik (1993) p 45) This conclusion is unwarranted, even if all objects are describable in all the ways important to scientific theory using mathematics. In order to show that an object is a mathematical object, one must show that it has not merely mathematical properties, but that it is mathematically constituted. The spatio-temporal location of subatomic particles is enough to distinguish them from purely mathematical objects.

Resnik argues that black holes and virtual processes like photon-electron-positron-photon transformations also are like mathematical objects, in being undetectable. These may share undetectability with mathematical objects, but that also does not make them mathematical objects. Resnik says, "It's not clear that the interiors of black holes or the vacant parts of Bohm's wave fronts are supposed to be physical in any ordinary sense." (Resnik (1997) p 107) Still, they are physical, even if extraordinary.

Mathematical objects are empirically undetectable. Some physical objects are, too. If being undetectable by humans were sufficient to make an object mathematical, Resnik's argument might work. But this criterion is implausible.

In part, Resnik calls quantum particles and fields mathematical objects due to his conflation of space-time and mathematical points. If space-time points are mathematical, then fields, which are distributions of intensities over regions of space-time points, could also be construed as mathematical. But the difference between space-time and mathematical points is easily seen in the divergence of Euclidean space and our actual Riemannian space. We could not even understand alternate geometries if there were not an important difference between points in space and geometric points, between lines which extend infinitely in both directions and lines which curve with the shape of the universe. Parallel lines never meet in Euclidean space, maintaining a constant distance between them; lines of space-time can diverge or converge, with the curvature of the universe.

Resnik argues that there are objects which exist in space and time yet are mathematical. They can, therefore, participate in events and there is no good way to draw the line between mathematical and physical objects. This argument relies on the possibility of mathematical objects being located in space and time. If we can show independently that it is impossible for mathematical objects to exist in space-time, then it follows, a fortiori, that they can not participate in events, even if they participate in our descriptions of those events. Thus we have no reason to blur the line between mathematical and physical objects. In the next section, I deny Resnik's contention that mathematics objects exist in space and time.

§6.3: Mathematical Objects and Space-time Location

Resnik poses a series of challenges for the view that mathematical objects are not in space-time, though physical objects are.

> But what is it to be in space-time? To be located in it? To be part of it? To be either? Are space-time points in space-time? Is all of space-time in itself? These are not idle questions. The ontic status of the universal gravitational and electromagnetic fields, prima facie physical entities, as well as that of space-time points, prima facie mathematical entities, turns on how we answer them...Even quantum particles, such as electrons, widely regarded as paradigm physical objects, pose difficulties for a locationally grounded division between the mathematical and the physical. Where are these particles when they are not interacting with each other? On one interpretation of quantum theory, under some circumstances, these particles are not even located within a finite region of space-time. Then, are they everywhere or nowhere? (Resnik (1993) p 44)

We can extract one major objection to the thesis that mathematical objects can not be located in space-time: the criteria for spatio-temporal location are not clearly defined for a variety of objects, including

a) Space-time points;
b) Space-time itself;
c) Gravitational and electromagnetic fields; and
d) Quantum particles.

Elsewhere, Resnik argues for ontic blur on the basis of the unclear status of

e) Undetectable objects, such as the interiors of black holes.[115]

Resnik implies that without a criterion to determine whether a - e are located in space-time, we can not rule out mathematical objects being included in space-time. The challenge he poses is to find a criterion for spatio-temporal location that will unambiguously place paradigmatically mathematical objects outside of space-time, leave paradigmatically physical objects inside of space-time, and provide reasonable determinations for the hard cases a - e.

Katz's abstract/concrete distinction again serves. On Katz's criterion, numbers, pure sets, and geometric figures are abstract, homogeneously lacking spatio-temporal location, while chairs and donkeys and persons are concrete, since they all have at least temporal location. The criterion distinguishes physical objects from mathematical objects. But it is based on the spatio-temporal properties of objects. We first have to know whether an object is located in space and/or time to determine whether the object is abstract or concrete. Still, Katz's criterion may help here, since we can use it in reverse,

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[115] See Resnik (1997), p 106.

starting with our basic intuitions about whether an object is abstract. The challenge is to apply it to the hard cases.

Fields, quantum particles, and undetectable objects, cases c, d, and e, are concrete objects. Even if they are not located at particular positions in space, they have temporal location. Resnik's complaint that they may not have spatial location is moot, since an object need only possess spatial or temporal location, not both, to be concrete.

Resnik argues that some particles are undetectable in principle, and so are causally inert. Since mathematical objects are also causally inert, the undetectable objects are mathematical. Fallacy aside, whatever principle Resnik has in mind links detectability to human perceptual processes and instruments. This is a different principle from the one which grounds the undetectability of mathematical objects.

Resnik calls space-time points, case a, prima facie mathematical objects, repeating the failure to distinguish between pure and applied mathematical objects. Resnik points out that Field and Geoffrey Hellman, in constructing nominalist systems for mathematics, rely on space-time points, and thus think them nominalistically admissible. But, he argues, the nominalist has to show that space-time points are more epistemically accessible than mathematical objects. Field gives three reasons for thinking so.

> 1) We can observationally test theories about space-time, as we have tested relativity;
> 2) Space-time points are a part of space, and thus a part of our physical world - we can even see some;
> 3) We can identify fields with properties of space-time, making space-time points and regions causal agents. (Field (1982a) pp 68-70; cited in Resnik (1997) p 109)

Resnik argues, in opposition to 1), that we can only apply indirect tests to these theories, like those which would work for mathematical objects. Against 2), he argues that we can only see space-time points if we see the matter within, and that means we would have to accept 3). And 3) only yields space-time regions. We can not attribute the same property to points.

In response to Resnik's last argument, the nominalist could appeal to the same kind of rounding-out thesis that the indispensabilist uses to ensure simplicity. From the empirical nature of space-time regions, it is easy enough to argue for points on the basis of mereological methodology. Points could be posited as constituents of regions.

The spatial status of space-time points is puzzling, but Resnik's response to Field does not show that they are not more epistemically accessible than mathematical objects. He merely shows that they are not as tractable as ordinary physical objects. This is no objection to the concrete nature of space-time points, and no argument for ontic blur.

Resnik's appeal to the geometric properties of space-time to establish blur again confounds an object with its properties. "The tendency for physicists to seek structural explanations of the fundamental features of physical reality also undermines the idea that a fundamental ontic division obtains between the physical and mathematical... [P]hysicists have proposed that all of physical reality is an eleven-dimensional space, whose geometric properties give rise to all of the known physical forces." (Resnik (1993) p 46) If reality is an eleven-dimensional space, say, it follows that the parts of the physical world are in space, and thus concrete, according to Katz's criterion. Resnik's blur is not established. We might, truly, give up the notion that the elements of our physical space are most correctly described in the language of bodies. Resnik's further step, to argue that this makes the parts of space mathematical objects, is unwarranted.

The remaining case b is whether space-time itself is located within space-time, if there is any object to which Resnik refers by 'space-time itself'. This question is no more puzzling than whether any object contains itself, and is an intriguing one for theorists of material constitution. It is no argument for blur.

§6.4: The End of Blur

Resnik attempts to establish Ontic Blur in order to support the claim that there is no evidentiary distinction between mathematical and physical objects, and support the structuralist's contention that our perception of concrete arrangements leads to knowledge of abstract objects. Resnik's arguments assimilating mathematical and physical objects fail. Mathematical objects do not change their properties, they do not participate in events, and they are not located in space-time. While some physical objects have properties that mathematical objects have, like not being directly detectable by humans, they are not mathematical objects.

Resnik does not extend his characterization of mathematical objects so far as to include ordinary physical objects. "While subatomic particles occupy the vague region between mathematics and physics, tables and chairs are unquestionably not mathematical objects." (Resnik (1997) p 265) This clarity about the concreteness of ordinary physical objects clashes with his more general holism. Really, for the holist, terms like 'mathematical object' and 'physical object' do not pick out different kinds of objects.

Balaguer noticed the conflict. "What I find puzzling in Resnik's view is not so much his blurriism as the fact that he embraces blurriism together with the thesis that abstract objects exist 'outside space and time.' For it seems to me that the thesis of non-spatio-temporality brings with it a very clear abstract-concrete distinction." (Balaguer (1999) p 114)

The abstract/concrete distinction distinguishes mathematical from physical objects. A good explication of this distinction is in terms of spatio-temporal characteristics of concrete objects. The lack of spatio-temporal characteristics for mathematical objects may explain, in part, why they lack causal powers, but we must not characterize everything which lacks causal powers as mathematical.

The distinction between mathematical objects and physical ones leads directly to a distinction between the disciplines of mathematics and physics. Mathematics is concerned with mathematical objects; empirical science is concerned with physical ones. The disciplines may share techniques. Applied mathematics can bridge them. The clear separation forms another bulwark against holism, rejecting disciplinary blur, and allowing us to differentiate clearly between theoretical positing and an indispensability argument.

§7: The End of Structuralism

Any one who believes that abstract objects exist must provide an account of human access to those abstracta. Insofar as the structuralism I have considered provides any such account, it appeals to the applications of structures to the physical world. This is the indispensability argument.

Aside from its inability to account for mathematical knowledge, Balaguer has argued that structuralism fails to solve the problem of multiple reductions for the natural numbers to sets, its motivating problem. The structuralist offers unique structures which can be instantiated by various sequences of objects, the von Neumann sets and the Zermelo sets, say. But the structures themselves may suffer from a similar indeterminacy. "[T]here may be multiple structures that satisfy all the desiderata for being the natural number sequence and differ from one another only in ways that no human being has ever imagined..." (Balaguer (1999) p 117; see also Balaguer (1998), Chapter 2.)

Resnik argues that taking mathematical objects as positions in patterns is not intended as an ontological reduction. Since most formulations of number theory do not have individual variables for sets, they can not assert that there is a number sequence. They treat numbers but not the sequence or structure. In general, a theory can not require its universe of discourse to contain itself, though we can extend a theory to include its (sub-)self. Just as numbers can be both nouns and adjectives, and when we explicate the numbers, we paraphrase one of the uses in terms of the other, so, Resnik argues, we can see patterns both as individuals and not, and paraphrase away the individual uses. This response amounts to a denial of indeterminacy among structures, since there are no facts concerning which sets are the numbers, or which structures are the number-theoretic structures, about which to be indeterminate.

We can deny that there is a fact of the matter for some question by excluding it from the field of our truth predicate, or by weakening our logic. But we can also, more strongly, just bar the question. Resnik denies that sentences such as ‘numbers are sets’ have truth value. But we can not banish ‘set’ from our language. Resnik prefers to restrict logic so that excluded middle does not apply generally. In his defense, he argues that our commitment to many disjunctions, like those involving fictional or vague terms, is weak. In the cases Resnik cites, though, we do want to banish the terms, rather than restrict excluded middle. We do not adjust excluded middle because we are puzzled about ‘Nemo the clownfish is cute’. ‘Numbers are sets’, on the other hand, is false.

Balaguer agrees. “If we look at mathematical practice *as a whole*, it is, I think, apparent that there are very definite facts of the matter about these questions: mathematicians think that numbers are *not* identical with sets and that 2 is not identical with {{0}}” (Balaguer (1999) p 116)

Resnik argues that his structuralism is independent of the indispensability argument, but it depends on indispensability if it is to account for mathematical knowledge.[116] Structuralism suffers the Unfortunate Consequences, and the structuralist fails to establish ontic blur in order to use our perceptions of concrete patterns as evidence for abstract mathematics. Structuralism does not even solve its motivating problem. We may still hold this much of structuralism, that all that matters to mathematicians are positions in structures, as the theorems define them, without adopting structuralism in the philosophy of mathematics.

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[116] Resnik additionally provides a pragmatic indispensability argument, which is independent of structuralism. I examine this argument in Chapter 4, §2.2.

Chapter 4: Non-Holistic Indispensability Arguments

Part 1: E Pluribus Putnams Unum: Putnam's Indispensabilism

§1.1: Introduction

This first part of Chapter 4 is mainly an exegetical aside.[117] Neither Quine nor Putnam formulate a direct, detailed indispensability argument, though Putnam comes closer. Analyzing Putnam's work, I noticed two interesting things. First, while he did in places support Quine's argument, he also formulated his own version which depends on neither holism nor QP, and which I discuss in Part 2 of this chapter. Second, while Putnam has a deserved reputation for shifting his positions, especially in the philosophy of mathematics, there is an underlying theme through his work in this area, and it is none of the themes he suggests unify his work. Throughout his oeuvre, Putnam appeals to empirical evidence for mathematics, the hallmark of indispensabilism. In Part 1 of this chapter, I show how indispensabilism unifies Putnam's work in four areas of the philosophy of mathematics. Call my allegation the Unification Thesis.

There is an easy way to tell if some one is an indispensabilist: If he argues that the theory of relativity shows that Euclidean geometry is false, he links mathematical truth with empirical evidence. That is the core of the indispensability argument. The rejection of Euclidean geometry on the basis of considerations from relativity is sufficient for indispensabilism, though not necessary, since the indispensabilist can make

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[117] In §1.5, I distinguish Putnam's indispensability argument, which I examine in Part 2 of the chapter, from Quine's, which may be seen as central to my argument.

ad hoc Euclidean rescues.

Putnam consistently takes this telltale position regarding Euclidean geometry despite exploring various positions in the philosophy of mathematics: Deductivism (1967a), Modalism (1967b, 1975a), Realism (1971, and 1975a again), Anti-realism (1980, 1981e, 1994).

Given that the indispensabilist attempts to establish mathematical realism, there is a tension between my ascription of indispensability and the three non-realist positions. In the case of anti-realism, this tension is merely apparent. Putnam's anti-realism, which he calls internal realism, denies that we can establish transcendent realism, or truth, or reference, for any domain, including mathematics and physical science. He relies on indispensability to generate as much realism for mathematics as he generates for anything else. The empirical and mathematical justificatory structures remain linked.

Putnam's modalist claims are subordinate, in his presentation, to the claim that modalism is equivalent to realism, and also to a stronger nihilist claim, that there are no correct philosophies of mathematics. Here again, the position only conflicts with indispensability realism on the surface. Putnam's modalist accepts evidence for mathematical statements, and this evidence is empirical.

As for deductivism, Putnam's underlying argument is for mathematical realism based on indispensability, as I shall show.

In the next section, I clarify my claim about Putnam's consistent adoption of the telltale position that Euclidean geometry is a physical hypothesis refuted by the rejection of Newtonian mechanics in favor of the theory of relativity. In the following four sections, I develop the unification thesis by indicating in more detail how each of the four Putnams relies on indispensability.

§1.2: The Telltale Position

In an early paper, Putnam argues that we should adopt three-valued logic because of its utility in accommodating quantum mechanics. Responding to the objection that we need to be shown the applicability of this logic, Putnam accepts the demand and implies that we will discover the application later. “This objection, however, cannot impress anyone who recalls the manner in which non-Euclidean geometries were first regarded as absurd; later as mere mathematical games; and are today accepted as portions of fully interpreted physical hypotheses.” (Putnam (1957) p 169)

Every one agrees that Riemannian geometry is a portion of a fully interpreted physical hypothesis, relativity theory. Putnam reveals his indispensabilism by implying that non-Euclidean geometries are mere mathematical games until they become portions of physical theory. More insidiously, he implies that geometry is a physical hypothesis. Though Putnam later rejects the proposal to adopt Reichenbach’s three-valued logic, the seeds of his indispensabilism are evident.

There are also seeds of anti-indispensabilism. In Putnam (1962), he seems to distinguish pure and applied mathematics. “What is often called ‘interpreting mathematical geometry’ is more aptly described as testing the conjunction of geometric theory and optical theory.” (Putnam (1962) p 49)

But then Putnam says that we can change either the geometry or the optics in response to trouble, that empirical evidence can weigh against mathematical principles. "After Einstein... [geometrical principles] have exactly the same status as cosmological laws: this is because general relativity establishes a complex interdependence between the cosmology and the geometry of our universe." (Putnam (1962) p 50)

Stronger statements of the telltale position are ubiquitous in Putnam's later work.

> If there are any paths that obey the pure [Euclidean] geometrical laws (call them 'E-paths'), they do not obey the principles from physics... Either we say that the geodesics are what we always meant by 'straight line' or we say that there is nothing clear that we used to mean by that expression. (Putnam (1968) p 177)
>
> The received view is that the temptation to think that the statements of Euclidean geometry are necessary truths about actual space just arises from a confusion. One confuses, so the story goes, the statement that one can't come back to the same place by traveling in a straight line (call this statement 'S'), with the statement that S is a theorem of Euclid's geometry... I find this account of what was going on simply absurd. (Putnam (1974) p ix)
>
> If space were Euclidean, doubtless the distinction between 'mathematical' and 'physical' geometry would be regarded as silly... Euclidean geometry is false - false of *paths in space*, not just false of 'light rays'. (Putnam (1975a) pp 77-8)
>
> Unless one accepts the ridiculous claim that what seemed *a priori* was only the *conditional* statement that *if* Euclid's axioms, then Euclid's theorems (I think that this is what Quine calls 'disinterpreting' geometry in 'Carnap and Logical Truth'), then one must admit that the key propositions of Euclidean geometry were *interpreted* propositions ('about form and void', as Quine says)... (Putnam (1976) p 94)
>
> Suppose someone had suggested to Euclid that this could happen: that one could have two *straight* lines which are perpendicular to a third *straight* line and which *meet*. Euclid would have said that it was a necessary truth that this couldn't happen. According to the physical theory we accept today, it *does* happen. (Putnam (1981e) p 83)

Putnam's concern is often not the falsity of Euclidean geometry, but the effect of the shift to non-Euclidean geometry on a priori knowledge. Traditionally, Euclidean geometry was taken to be known a priori. Apriority was taken to imply necessary truth. If geometric principles turn out false, we have a conceptual problem.

Dropping the indispensabilist's connection between mathematical and physical statements, making a distinction between pure and applied mathematics, solves the problem. The adoption of Riemannian geometry as the framework for relativity theory does not refute Euclidean geometry, but shows its inapplicability to space. Still, both Euclidean and Riemannian geometries can be known a priori.

On this traditional account, which Putnam calls absurd in the second quote above, Euclidean theorems hold necessarily of Euclidean space. When Newtonian mechanics was replaced by a theory based in Riemannian space, no necessary entailments were threatened. Only the empirical commitments of the physical theory were changed.

One does not sever the link between mathematics and science ad hoc, to maintain a priori knowledge. The distinction is independently plausible. Only the Kantian assumption that we have a priori knowledge of the physical universe supports the belief that our knowledge of mathematics was based on a priori knowledge of physical space. Whereas Hume may have stirred Kant from his dogmatic slumbers, Kant apparently did not awaken completely. Hume taught us that matters of fact could not hold a priori, including knowledge of the structure of physical space.

In part, Putnam defends his claim that an a priori claim turned out to be false by referring to the inconceivability of non-Euclidean geometries to pre-relativity thinkers. Inconceivability is a strong claim. More plausibly, one could claim that they merely failed to conceive of non-Euclidean geometries. Then Putnam's argument fails. Our intuitions about mathematical spaces and structures are constantly being extended. The resulting mathematics is not justified by finding physical correlates, but by its fruitfulness, among other factors.[118] Failure to consider consistent but unintuitive spaces is no argument against their mathematical existence. The traditional a priori account need not be dismissed.

Another way to resolve the problem, as I argued in Chapter 2, §3.5, is to sever the link between apriority and necessary truth. One could adopt a fallibilist apriorism, which separates issues about how we justify statements from questions of truth or necessity. Confusing these notions, as both Putnam and Quine do, encourages the adoption of the telltale position. I proceed to develop the unification thesis by showing how each of the four Putnams reveals commitments to the four Essential Characteristics.

§1.3: Putnam's Deductivism

The case for the ubiquity of Putnam's indispensabilism is hardest to make for his deductivism (Putnam (1967a). He refers uncritically to the pursuit of pure mathematics and appeals to non-empirical considerations when attempting to specify a standard model. Still, indispensability is present.

Putnam explores deductivism as a response to problems with Russell's *Principia* logicism. Putnam argues that logicism best accounts for the application of mathematics. But he recognizes several problems. Cantor and Gödel showed that we can not talk about all sets, or all the natural numbers, due to diagonalization arguments. The Löwenheim-

[118] See Chapter 5, §1.3 for a discussion of these factors.

Skolem theorem shows that we can not determine a standard model from within a preferred mathematical theory strong enough to serve the needs of mathematics. The independence of the continuum hypothesis shows that if we could determine a standard model for set theory, we would not know whether to add the continuum hypothesis or its negation; logicism commits to a unique solution to a question that should be left open.

In lieu of logicism, Putnam suggests returning to Russell's earlier deductivism, which he abandoned for logicism's account of application and the possibility of specifying a standard model. Since, Putnam argues, Russell erred about our ability to specify a standard model, we should revert to the earlier position, and devise an alternate account of application. Putnam proposes to take mathematics as the study of the consequences of mathematical axioms, using model theory. He reinterprets mathematical statements as referring to the possibility of a model for those statements. Any theorem of any mathematical theory T of the form '$\exists x \mathscr{F} x$' really means <u>if there is a model M of T, then there is something in M which is $\mathscr{F}^M$</u>. Deductivism does not make mathematics logic, in the logicist's sense, but it is logic in a broader sense which includes the set theory we need to construct models to determine the consequences of axioms.

Putnam only appeals to a deductivist interpretation for statements which can not be rendered in first-order logic plus empirical language. Consider, as Putnam does, a room with two apples on the desk, two apples on the table, and no apples on both the table and desk, or elsewhere. We can infer that there are four apples in the room with merely first order logic in addition to the empirical premises and so have no need for mathematics. Putnam has adopted one of the Essential Characteristics: EC.4, Subordination of Practice. Mathematics only requires and receives justification when it is useful to empirical science.

Discussing Kant, Putnam laments the impossibility of determining a standard model on the basis of empirical evidence. We could fix a standard model on the basis of intuitive chronometry, or geometry, if we took those principles to be known a priori. If we could guarantee the existence of a standard model, then we could know mathematical truths based on this intuitive grasp of the theorems of mathematics.

But, Putnam continues, Kant was wrong about our a priori grasp of geometry and chronometry, and there are non-standard models of our mathematical theories. Putnam here commits to EC.2, Theory Construction. Empirical evidence is insufficient to generate a standard model. By limiting his appeals to the models (standard or non-standard) and neglecting to include auxiliary mathematical evidence which could help fix the standard model, he again reveals his commitment to Subordination of Practice. A non-indispensabilist may accept non-empirical evidence for the standard model.

Putnam's preference for logicism, in his argument for deductivism, is really an expression of EC.1, Naturalism: the primary goal of the mathematician is to provide tools to assist with our understanding, or explanation, of the physical world. Logicism could explain the applicability of mathematics to the empirical world, by assimilating mathematics to logic, which applies to all possible states of the world. Putnam's deductivist has adopted three of the four Essential Characteristics of indispensability arguments.

In the course of resolving a problem for deductivism, Putnam seems to appeal to mathematics beyond that which the indispensabilist may justify. Deductivism has a prima facie difficulty selecting certain derivations, say the finite ones, from a given mathematical theory. Mathematicians often study only restricted areas like finite number theory. Since deductivism interprets mathematics as the study of entailments from axioms, it can not give priority to any sub-class of consequences.

In response, Putnam attempts to show that there is no need to make such distinctions formally, since the notions involved, like finitude, are relative to a broader model. We can not construct an absolute notion of finitude, or of a standard model, within a formal mathematical theory. Our interest in finite structures, or in only the standard model, is not based on any mathematical priority of those portions of our larger mathematical theory.

This blanket legitimation of all consequences of a set of mathematical axioms appears to avoid the indispensabilist's problem of UC.1, Restriction, by rejecting Subordination of Practice. But, Putnam continues, "None of this really presupposes the *existence*, in a non-hypothetical sense, of any models at all for anything, of course!" (Putnam (1967a) p 23) That is, deductivism does not generate any commitments, including ones to mathematical objects.[119]

Putnam is not justifying more mathematics than the indispensabilist; he is remaining agnostic on the question of mathematical existence. Still, he does have some existence questions in mind. Putnam considers models with empirical domains, and he

[119] Putnam also rejects a traditional realism when he refers approvingly to Weyl's contention that Russell's logicism makes logic theological by committing to an abstract universe when talking of predicates of all integers, or all models.

characterizes finitude and a standard model using set theory with empirical elements.

Here is one last consideration in favor of my claim that indispensabilism underlies Putnam's deductivism. Putnam rejects Hilbert's distinction between real mathematical statements, which admit of constructive proofs, and ideal statements, which can not be proven constructively. He prefers a distinction between which statements can be applied and which can not. Putnam wants to turn Hilbert into a kind of indispensabilist, limiting mathematical truth only to those statements which can be applied to empirical theory.

Putnam may appear to reject indispensability in embracing both Hilbert's real and ideal statements, but his real concern is to avoid privileging statements provable constructively. "I see *no* reason to agree with Hilbert that it matters in the least, to a statement's counting as a 'real' statement...whether it has been proved by Intuitionist means or not." (Putnam (1967a) p 36)

The indispensabilist thread in Putnam's deductivism appears in his reliance on empirical evidence for mathematical statements and in his adoption of the Essential Characteristics. Even when he looks to broaden the sources of evidence for mathematics beyond those to which the indispensabilist is entitled, he rejects the mathematician/ logician's terms 'property' and 'relation' as arcane, and arbitrary, and seeks to generate a "natural" concept of properties and relations. Putnam's rejection of deductivism in favor of modalism, which I consider next, is based in large part on even more clearly indispensabilist considerations.

§1.4: Putnam's Modalism

Putnam discusses modalism in two papers, (1967b) and (1975a). In both papers, he takes the telltale position on geometry. In the former, he notes that the development of non-Euclidean geometries showed that the axioms of Euclidean geometry were not truths. "The price one pays for the adoption of non-Euclidean geometry is to deny that there are *any* propositions which might *plausibly* have been in the minds of the people who believed in Euclidean geometry and which are simultaneously clear and true." (Putnam (1967b) p 50)

Putnam does not strongly defend modalism in either paper. He rejects the existence of a correct position, arguing that the modal account is equivalent to a realist one. "My purpose is not to start a *new* school in the foundations of mathematics (say, 'modalism')." (Putnam (1967b) p 57)

Putnam characterizes the equivalence between modalism and realism as 1) definability of the primitive terms of each theory in the primitive terms of the other; and 2) deducibility within each theory of the theorems of the other. He considers a (fantastic) counterexample to Fermat's theorem. It would be describable in object-realistic terms: the existence of four positive integers x, y, z, and n (where $n > 2$), such that $x^n + y^n = z^n$. We could also write it as schema of pure first-order modal logic: $\Box[AX(S, T) \supset \sim Fermat(S, T)]$, where 'AX' represents the conjunction of mathematical axioms required to generate the numbers required for the counter-example, 'S' and 'T' are dummy predicate letters, standing for the arithmetical primitives needed in both the antecedent and consequent but disinterpreted to emphasize the modal nature of the statement, and

'Fermat' represents the claim that there are no solutions to the given schema.

Both sentences, Putnam claims, assert the same fact while requiring different objects. Modalism requires no objects, merely describing entailments, whereas realism requires a vast universe of mathematical objects. "'Numbers exist'; but all this comes to, for mathematics anyway, is that 1) ω-sequences are possible (mathematically speaking); and 2) there are necessary truths of the form 'if α is an ω-sequence, then...' (whether any concrete example of an ω-sequence exists or not." (Putnam (1967b) p 49)

There are four claims on the table: nihilism (there is no correct position in the philosophy of mathematics); equivalence (there are equivalent modalist and object realist formulations of all mathematical claims); realism; and modalism. Putnam bases nihilism on equivalence, but nihilism is too strong to follow. A weaker claim that there are multiple correct positions is more plausible.

Still, the equivalence claim seems false. Putnam argues that we can take possibility as primitive and derive sets from it. Field argues that there is no acceptable modal operator to do the work that Putnam needs to establish equivalence.[120] Burgess and Rosen have other complaints about Putnam's project. "The technical details of the modal reconstrual he proposed are of no continuing interest, among other reasons because he did not deal with mixed, mathematico-physical language..." (Burgess and Rosen (1997) p 201)

The failures of Putnam's modalism need not concern us. The question here is how modalism relies on indispensability. The answer involves Putnam's proposal to

[120]See Field (1988), especially §6-§8.

cash out modal claims using concrete models. While the modal claim, which Putnam summarizes as "sets are permanent possibilities of selection," makes the models he uses abstract, these are models whose domains contain only concrete objects. Putnam relies on concrete models like inscriptions of a graph. "In constructing statements about sets as statements about standard concrete models for set theory, I did not introduce possible concrete models (or even impossible worlds) *as objects*. Introducing the modal connectives...is not introducing new kinds of objects, but rather extending the kinds of things we can say about ordinary objects and sorts of objects." (Putnam (1967b) pp 58-9, emphasis added) By 'ordinary', I believe that Putnam means concrete or physical, but in any case not pure mathematical.

In the later paper, he also explicates possibility using concrete elements. "A statement to the effect that for every number x there exists a number y such that F(x, y), where F(x, y) is a recursive binary relation, can be paraphrased as saying that it is not *possible* to produce a tape with a numeral written on it which is such that if one *were* to produce a Turing machine of a certain description and start it scanning that tape, the machine would never halt." (Putnam (1975a) pp 71-2) Turing machines and their tapes are concrete objects, even if their programs are abstract.

Also, Putnam discusses modalism in the context of quasi-empiricism; this is the paper in which he introduces Martian Mathematics. In Putnam's scenario, we discover alien mathematicians with a relaxed standard, relative to our own, for the acceptance of mathematical statements. The Martians accept statements which have not been proven, but only confirmed: counter-examples have not been discovered and the statements seem to cohere with a larger body of accepted results.

Putnam calls the Martian methodology quasi-empirical, because the accepted theorems are defeasible. As I argued in Chapter 2, §3.5, a statement's being defeasible is not necessarily indicative of its being empirical. Statements believed on the basis of a priori considerations may be ceded on the basis of further a priori considerations. Putnam's characterization of empirical science rules out this kind of fallibilist apriorism. According to Putnam's criterion, defeasibility makes mathematics empirical, even if our mathematical beliefs are known independently of experience.

Putnam's classification of Martian methods as quasi-empirical relies on his independent criticism of the a priori. But that argument focused on how beliefs which had been taken to be a priori turned out to be false.[121] He wrongly presumes that apriority entails necessary truth. Recognizing that we need a fallibilist account, he concludes that mathematics must be quasi-empirical.

Putnam mistakes the revisability of mathematics to entail a rejection of apriorism.[122] Instead of adopting fallibilism, and keeping necessity, he weakens necessity and apriority. He assimilates mathematics to empirical science. Mathematical necessity thus becomes empirical necessity. Mathematical truth becomes empirical truth. Putnam

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[121]Later, Putnam tries to rehabilitate the a priori within internal realism, which makes the resultant notion relative, or immanent. See §1.6, below.

[122]Later, throwing away the ladder, he rejects this link, seeking a more refined notion of fallibilism. "If fallibilism requires us to be sure that for every statement *s* we accept there is an epistemically possible world in which it is rational to deny *s*, then fallibilism is identical with the rejection of a priori truth; but surely this is an unreasonable conception of fallibilism." (Putnam (1979) p 136)

becomes an indispensabilist.

The defeasibility criterion for quasi-empiricism slightly revises his earlier discussion of the (sort of) empirical nature of mathematics, based on the availability of viable competitors. Here too, Putnam confuses empiricism with fallibilism.

> [T]he chief characteristic of empirical science is that for each theory there are usually alternatives in the field, or at least alternatives struggling to be born. As long as the major parts of classical logic and number theory and analysis have no alternatives in the field - alternatives which require a change in the axioms and which effect the simplicity of total science, including empirical science, so that a choice has to be made - the situation will be what is has always been. We will be justified in accepting classical propositional calculus or Peano number theory not because the relevant statements are 'unrevisable in principle' but because a great deal of science presupposes these statements and because no real alternative is in the field. Mathematics, on this view, does become 'empirical' in the sense that one is allowed to put alternatives into the field. (Putnam (1967b) pp 50-1)

Given Putnam's odd characterizations of the empirical, one might think that the difference between Putnam's allegation that mathematics is empirical (or quasi-empirical) and apriorism about mathematics is merely terminological. Consider further examples of mathematical claims which Putnam says were acquired empirically, or quasi-empirically: the postulation of a correspondence between real numbers and points on a line, well before the construction of reals out of rationals; the introduction of infinitesimals before epsilon-delta methods in the calculus were developed; Zermelo's use of the axiom of choice. Putnam claims that all of these were originally justified based on their success, fertility, and application, before formal proofs were generated.[123]

[123] Putnam's use of 'success' is ambiguous, referring sometimes to success within mathematics, and thus not indispensabilist, and sometimes to success within empirical theory. "The real justification of the calculus is its *success* - its success in mathematics,

He takes those criteria to be indicative of empirical (or at least quasi-empirical) justification. "The fact is that *we* have been using quasi-empirical and even empirical methods in mathematics all along..." (Putnam (1975a) p 64)

Calling mathematics empirical on the basis of defeasibility, or the presence of viable competitors, does not entail that it relies on empirical evidence, in a traditional sense. One may call it whatever one likes.

Moreover, consider Putnam's argument that mathematics is empirical because the acceptance of some mathematical statements is based on experimentation. The axioms of choice and replacement are not proven, but shown mathematically useful. Euler discovered that $\sum 1/n^2 = \pi^2/6$ through analogical reasoning, positing the equivalence of two terms on the basis of structural similarities, before he had a formal proof.

> [N]o mathematician doubted that the sum of $1/n^2$ was $\pi^2/6$, even though it was another twenty years before Euler had a proof. The similarity of this kind of argument to a hypothetico-deductive argument in empirical science should be apparent: intuitively plausible though not certain analogies lead to results which are then checked 'empirically'. Successful outcomes of these checks then reinforce one's confidence in the analogy in question. (Putnam (1975a) p 68)

Putnam's allegation that these results are checked "empirically" seems unjustified. Euler checked the results by calculating the series $1/n^2$ for finite results; Putnam says he checked them up to n = 30. There is nothing empirical about this. We can call it an experiment, in the same way one might call adoption of the axiom of choice an experiment. Then there are a priori experiments.

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and its success in physical science." (Putnam (1975a) p 66) This ambiguity reflects Putnam's failure to distinguish revisable a priori beliefs from empirical ones.

It looks as if Putnam's claim that mathematics is empirical, or quasi-empirical, may be merely misleading uses of those terms. To determine whether the modalist/ quasi-empiricist Putnam really accepts empirical evidence for mathematical claims, we must examine the details of the kind of evidence he accepts. Then, we see that Putnam really does allow empirical evidence. In the beginning of this section, I provided examples, especially Putnam's reference to concrete models. Consider also that when Putnam talks about the success of mathematical experiments, his focus is empirical success. "Today it is not just the axiom of choice but the whole edifice of modern set theory whose entrenchment rests on great success in mathematical application - in other words, on 'necessity for science'." (Putnam (1975a) p 67)

Putnam confuses reliance on factors like success and fertility with empirical justification. In constructing empirical theory, we look to such factors to explain sense experience. In constructing mathematical theory, we look to success and fertility within mathematics. This does not mean that the difference between Putnam's quasi-empiricism and apriorism is merely terminological, even if the root of Putnam's adoption of empirical evidence in mathematics is this confusion.

This discussion of quasi-empiricism has brought me away from Putnam's modalism for two reasons. Putnam's later discussion of modalism arises within his discussion of quasi-empiricism, although he leaves the connection obscure. Also, quasi-empiricism again evinces Putnam's indispensabilism, by admitting empirical justification for mathematics.

The main distinction between Putnam's modalism and his deductivism is his claim that we can fix a standard model within the modal picture. He asks us to accept it on faith that this can be carried out. Since his preference for deductivism over logicism was largely based on our inability to fix a standard model, Putnam could return to logicism, with the emendation that we can fix the standard model modally. The equivalence claim, and his concomitant nihilism, lead Putnam to give up on a correct philosophy of mathematics. Still, the equivalence claim is that modalism is equivalent to realism, and so he does, in a way, return.

Putnam's equivalence claim lessens the oddity of my claim that his modalism relies on indispensability. His anti-realist claims (e.g. "The modal logical picture shows that one doesn't have to 'buy' Platonist ontology..." (Putnam (1975a) p 72)) presuppose an equivalence with realism. In the later paper, Putnam presents his realist success argument, which I describe in the next section, and which I assess in Part 2.

§1.5: Putnam, Quine, and Intuition

That Putnam relies on indispensability for his mathematical realism needs little defense. But, in places he seems to make additional claims for an independent mathematical epistemology. "There are *two* supports for realism in the philosophy of mathematics: *mathematical experience* and *physical experience*." (Putnam (1975a) p 73) He describes mathematical experience on analogy with theological experience, independent of empirical evidence. In this section, after contrasting Putnam's position with Quine's, I show that such claims are misleading, and that Putnam's realism is fully indispensabilist, not traditional.

A paragraph after the above quote, Putnam makes his indispensabilism clear.

> If this argument [from mathematical experience] has force, and I believe it does, it is not quite an argument for mathematical realism. The argument says that the consistency and fertility of classical mathematics is evidence that it - or most of it - *is true under some interpretation*....The interpretation under which mathematics is true has to square with the application of mathematics *outside* of mathematics. (Putnam (1975a) pp 73-4)

Incidentally, I read the qualification above ("most of it") as an indication that Putnam recognizes the problem of UC.1, Restriction. This is another indication of his reliance on indispensability which is present more explicitly, earlier.

> Sets of a very high type or very high cardinality (higher than the continuum, for example), should today be investigated in an 'if-then' spirit. One day they may be as indispensable to the very *statement* of physical laws as, say, rational numbers are today; then doubt of their 'existence' will be as futile as extreme nominalism now is. But for the present we should regard them as what they are - speculative and daring extensions of the basic mathematical apparatus. (Putnam (1971) p 347)

Putnam's indispensability argument has strong affinities with Quine's. "This type of argument stems, of course, from Quine, who has for years stressed both the indispensability of quantification over mathematical entities and the intellectual dishonesty of denying the existence of what one daily presupposes." (Putnam (1971) p 347)

Putnam's commitment to Quinean holism arises early in his work, and leads him naturally into indispensability with Quine. "I should like to stress the monolithic character of our conceptual system, the idea of our conceptual system as a massive alliance of beliefs which face the tribunal of experience collectively and not independently, the idea that 'when trouble strikes' revisions can, with a very few exceptions, come anywhere." (Putnam (1962) p 40)

Differences between Quine and Putnam emerge slowly over the years. Putnam disparages Quine's commitment to a single, regimented, best theory, though this is clearer in his anti-realist phase, which I discuss in the next section. A second difference concerns truth. For Quine, truth is mainly a device for referring to several sentences at once, as in, 'Everything she said is true'. Putnam assumes a realist stance about truth in science. "I shall assume that one of our important purposes in doing physics is to try to state 'true or very nearly true'... laws, and not merely to build bridges or predict experiences." (Putnam (1971) p 338)

Putnam's independent contribution to the indispensability argument is also demonstrated in a third difference, his emphasis on the success of science, rather than on the construction of a monolithic best theory. "I believe that the positive argument for realism [in science] has an analogue in the case of mathematical realism. Here too, I believe, realism is the only philosophy that doesn't make the success of the science a miracle." (Putnam (1975a) p 73)

A fourth difference may be how Putnam argues that we may need only a predicative notion of set.[124] "[I]t appears *possible* (though complicated and awkward) to

[124]Predicative set theory quantifies only over previously defined sets in order to avoid the set-theoretic paradoxes. Russell's theory of types is a predicative set theory. Impredicative set theory allows quantification over all sets of individuals. Predicative set theory can not serve as the foundation for analysis, and so appears insufficient for physics; any continuous quantity would have to be represented using rational approximations, for example. Solomon Feferman (1998) has argued for the sufficiency of predicative set theory for science.

do physics using just predicative set theory." (Putnam (1971) p 346) He claims that the indispensability argument thus strongly commits us to predicative set theory, but more weakly to impredicative set theory. I am unsure whether to count this as a difference, or merely a refinement.

A fifth difference concerns the endeavors which impel mathematical commitments. In Putnam (1960), he argues that mathematics is indispensable to correspondence truth, which demands relations. In Putnam (1971), he considers formal logic and semantics, both of which do not, for Quine, demand mathematics. Putnam argues that we need mathematics in order to formalize metalogical notions like derivability and validity.[125] Consider: 'All Ps are Qs. x is a P. So, x is a Q.' Putnam argues that this inference is best understood as referring to sets P and Q. Quine and the nominalist prefer to interpret P and Q as schematic letters replaceable by predicates true of acceptable objects. Such attempts to avoid reference to classes within metalogic weaken our notion of validity. We can only define validity within a particular formalized language.

Putnam argues that formal logic requires abstracta in two more ways. First, we

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[125] Field takes Putnam's concerns in logic seriously, as his introduction of a modal operator to handle metalogical reasoning shows. I discussed Field's response in Chapter 1, §2.6. We also see Putnam's profound influence on Field in his detailed description of the measurement problem for the nominalist. Arguing for the inadequacy of nominalistic languages, Putnam indicates some of the requirements of Newtonian gravitational theory: statements of force, mass, and distance, as measured in rational numbers. "But no nominalist has ever proposed a device whereby one might translate arbitrary statements of the form 'the distance d is $r_1 \pm r_2$' into a nominalistic language." (Putnam (1971) p 339) Putnam's discussion of the virtues of fictionalism over verificationism, in the same paper, also surely influenced Field.

can not plausibly understand logical principles as referring to inscriptions, since they apply to sentences which have never been inscribed. Possible inscriptions, another option, are abstract objects. Also, logic requires truth, which again can not plausibly be ascribed to inscriptions.

Quine regarded higher-order logics, and metalogic, as mathematics. Putnam argues that this is a mistake, and inconsistent with philosophical tradition. If some higher order logic is our canonical language, we may commit to abstracta from the start. We can include validity and implication as logical notions.

> I believe that (a) it is rather arbitrary to say that 'second order' logic is not 'logic'; and (b) even if we do say this, the natural understanding of first order logic is that in writing down first order schemata we are implicitly asserting their validity, that is, making second order assertions... [T]he natural understanding of logic is such that all logic, even quantification theory, involves reference to classes, that is, to just the sort of entity that the nominalist wishes to banish. (Putnam (1971) pp 336-7)

Quine's choice of canonical language is ill-justified, but it is not as arbitrary as Putnam alleges. We can draw a firm line between mathematics and logic merely on the basis of ontic commitment, leaving logic as a subject with no commitments of its own.[126] This restriction may omit some of what was historically considered logic, since Aristotelian syllogisms may be taken to refer to classes, but this is no significant drawback. Fields of study change. In logic, which was revolutionized in the last 150 years, we should expect to find some disagreement with Aristotle, who did not hold a

[126] Lindstrom (1969) presents results that first-order logic can not be extended without losing completeness, compactness, and the Löwenheim-Skolem theorem. These form another basis for the line.

modern notion of set theory. The important point here is not whether we can draw a line between mathematics and logic, but Putnam's claim that mathematics may be indispensable to our best theories of validity and consequence.

I will here follow the theme of contrasts with Quine into Putnam's anti-realism, which I discuss in more depth in the next section.[127] Putnam cites two modifications of Quine's indispensability argument. The first is the addition of combinatorial facts to sensations, as desiderata of theory construction. "[T]he idea that what the mathematician is doing is contributing to a scheme for explaining sensation just doesn't seem to fit mathematical practice at all. What does the acceptance or non-acceptance of the Axiom of Choice... have to do with explaining sensations?" (Putnam (1994) p 504)

As for the other, "The second modification I propose to make in Quine's account is to add a third non-experimental constraint to his two constraints of 'simplicity' and 'conservatism'... The constraint I wish to add is this: *agreement with mathematical 'intuitions'*, whatever their source." (Putnam (1994) p 506)

The acceptance of mathematical intuitions looks like a break not only with Quine, but with earlier Putnam. He seems to have softened toward mathematical intuition and abandoned indispensability. But this appearance is misleading, since Putnam still understands intuition as essentially empirical.

Consider the quasi-empiricism which admits combinatorial facts, truths of

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[127] This material is appropriate in this section anyway, since the indispensability argument which Putnam holds in his anti-realist phase is the same as that which he holds as a realist. The difference between the two positions concerns only the weight of the conclusion.

number theory, geometry, and set theory, as facts to be explained by scientific or mathematical theory. If these combinatorial facts are not posits to explain sensations, as the indispensabilist has it, then where do they come from? How do we know which facts need explaining? How do we separate the truths from the falsehoods? To answer these questions, Putnam posits mathematical intuitions, and argues that they lead us to mathematical truth.

(PI) PI.1: We have intuitions about the truth of mathematical statements (combinatorial facts).
PI.2: These intuitions are justified quasi-empirically.
PI.3: Quasi-empirical justifications yield truth.
PI.C: So, mathematical statements about combinatorial facts are true, and justified.

Putting aside worries about Putnam's motives for quasi-empiricism, why should we believe that quasi-empirical justifications yield truth? Whether PI is an indispensability argument or an abandonment of EC.1, Naturalism, depends on the nature of these quasi-empirically justified intuitions. If they are derived from solely empirical experience, and the quasi-empirical facts are really empirical facts, then Putnam is just repackaging mathematical empiricism with new labels, as he did in his modalist phase.

Putnam's faculty of intuition is not like those of writers who do countenance an independent mathematical epistemology. It is not Gödelian insight, which Putnam rejects as mysticism. Neither is it Katz's notion of reason, which Putnam neglects. It is not even Platonic formal acquaintance. Rather, Putnam argues that we establish our mathematical intuitions quasi-empirically, by our attempts to understand the empirical world. Their justifications come from science, often inductively. Our knowledge of mathematics is on this account empirical.

Putnam's discussion of mathematical induction demonstrates his empirical grounding of mathematical intuition. "The principle of mathematical induction, for example, bears the same relation to the fact that when a shepherd counts his sheep he always gets the same number (if he hasn't lost or added a sheep, and if he doesn't make a mistake in counting) no matter what order he counts them in, that any generalization bears to an instance of that generalization." (Putnam (1994) p 505)

Putnam uses this example to justify his quasi-empiricism by blurring the line between mathematical reasoning and empirical reasoning. But it is wrong. The relationship of a mathematical generalization to an instance is immediate. The use of mathematical induction in a specific mathematical case yields a proof. In contrast, the relation between a mathematical generalization like the principle of mathematical induction and an empirical instance is mediated by the caveats provided by Putnam, and also by a Humean principle of uniformity of nature, that sheep do not spontaneously generate or disappear, for example.

Putnam only appears to admit mathematical intuitions. They are empirical, even if he calls them mathematical, or quasi-empirical. "[I]t is not clear how mathematical 'intuitions' do [constitute a link between acceptability and truth], if at bottom they are just generalizations from the finite on the basis of human psychology, reified forms of grammar, and so on." (Putnam 1994) p 507)

In this section, I compared Putnam's indispensabilism with Quine's, and urged that his talk of an independent epistemology for mathematics is misleading. Putnam is an indispensabilist, even if not a Quinean. Putnam's overtures to pure mathematics, combinatorial facts, and mathematical intuition, in both his realist and anti-realist phase, are at root just more mathematical empiricism, more indispensabilism.

§1.6: Putnam's Anti-realism

The differences between Quine and the realist Putnam were amplified from the late 1970s on, as Putnam turned away from realism. He accepted that there are a priori truths.[128] 'Not every statement is both true and false' serves as an example. "At least *one* statement is *a priori*, because to deny that statement would be to forfeit rationality itself." (Putnam (1979) p 129)

Putnam's acceptance of a priori statements was not, as it might seem, a move toward a more substantial realism than Quine's. His a priori statements are a priori only pragmatically, relative to a body of knowledge which may be swapped for an alternative body with different a priori truths. Permissible shifts in what we think is rational alter the class of a priori statements.

I call Putnam's later position anti-realism. Putnam's use of 'internal realism' is misleading, as he rejects claims which are central to realism in mathematics and ethics and science. These claims, elements of what Putnam calls metaphysical realism, are that

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[128] Two comments. First, Putnam uses 'a priori' as a modifier of statements, rather than as a description of how one justifies statements, and thus not as an epistemic notion. I shall follow him, for the purposes of discussing his work, though this usage leads to confusion. Second, Putnam had earlier claimed, in opposition to Quine, that there are analytic truths, but they are not interesting, while maintaining his rejection of the a priori. See Putnam (1976) p 95 et. seq.

we can assert language-independent, or conceptual-scheme-independent, truths. Putnam alleges that even our best scientific theory is interest-dependent.

> If there are many ideal theories (and if 'ideal' is itself a somewhat interest-relative notion), if there are many theories which (given appropriate circumstances) it is perfectly rational to accept, then it seems better to say that, in so far as these theories say different (and sometimes, apparently incompatible) things, some facts are 'soft' in the sense of depending for their truth value on the speaker, the circumstances of utterance, etc. (Putnam (1980) p 19)

Perhaps a better positive name for this position is pragmatism. Putnam himself calls it verificationism in Putnam (1980).

Whatever the name, Putnam maintains his indispensabilism. He holds that mathematical statements have as much truth, and mathematical objects have as much existence, as we can establish. In fact, Putnam broadens his use of the indispensability argument, in his anti-realist phase, and maintains the link between empirical evidence and mathematical truth.

> [R]ejecting the spectator point of view, taking the agent point of view towards my own moral beliefs, and recognizing that *all* of the beliefs that I find indispensable in life must be treated by me as assertions which are true or false (and which I believe are true), without an invidious distinction between *noumena* and *phenomena*, is not the same thing as lapsing back into metaphysical realism about one's own moral beliefs any more than taking this attitude towards one's beliefs about commonsense material objects or towards causal beliefs or mathematical beliefs means lapsing back into metaphysical realism about commonsense objects, or causality, or mathematical objects/modalities. It also does not require us to give up our pluralism or our fallibilism: one does not have to believe in a unique *best* moral version, or a unique *best* causal version, or a unique *best* mathematical version; what we have are *better and worse* versions, and that *is* objectivity. (Putnam (1987) p 77)

Putnam's anti-realism arises largely out of considerations from model theory, especially of the Löwenheim-Skolem theorem. The downward version of the theorem states that any formal theory whose theorems assert the existence of non-denumerably many objects, as any mathematical theory strong enough to include the real numbers will, also has denumerable models. Any sufficiently strong theory, including ones which might count as a best theory for Quine and the realist Putnam, has non-standard models. The theory can not determine an intended model.[129]

Furthermore, Skolemite reinterpretations may conflict, or appear to, with the intended interpretation. A term representing a non-denumerable set can be modeled by a countable set. Whether the term is countable or not, Putnam argues, is relative to the model. Truth values of terms are similarly relative.

Since an empirical theory and our empirical evidence for it can not fix a model, we might seek non-empirical evidence, though doing so might violate naturalist constraints. Putnam rejects this tactic, again affirming indispensabilist tenets.

> [T]he theoretical constraints we have been speaking of must, on a naturalistic view, come from only two sources: they must come from something like human decision or convention, whatever the source of the 'naturalness' of the decisions or conventions must be, or from human experience, both experience with nature (*which is undoubtedly the source of our most basic 'mathematical intuitions'*, even if it be unfashionable to say so), and experience with 'doing mathematics'. (Putnam (1980) p 5, emphasis added)

Putnam's anti-realism arises partly also from his recognition that indispensabilism entails Restriction. He argues that Gödel's Axiom of Constructibility, *V=L*, which says

[129] See Chapter 2, §1.2, for Putnam's related complaints about Quine's reliance on a single, best regimented theory.

that every set is constructible, and which is not derivable from any common axiomatization of set theory, lacks a truth value since empirical evidence does not settle the matter.

> [I]f the 'intended interpretation' is fixed only by theoretical plus operational constraints, then if 'V=L' does not follow from those theoretical constraints - if we do not *decide* to make V=L true or to make V=L false - then there will be 'intended' models in which V=L is *true*. If I am right, then the 'relativity of set-theoretic notions' extends to a *relativity of the truth value of 'V=L'* (and by similar arguments, of the axiom of choice and the continuum hypothesis as well). (Putnam (1980) p 8)

Michael Levin has argued that Putnam's contention that reference for scientific theories becomes unfixed is based firmly on this unfixing of reference within mathematical theories, but that Putnam does not succeed in establishing the mathematical case. He criticizes Putnam for claiming that empirical evidence can tell us anything about the truth value of V=L, since that claim is about pure sets, about pure mathematics. "Empirical facts about the results of measurement cannot confirm or refute V=L because V=L is a statement about pure sets." (Levin (1997) p 61)

Putnam's extension of his anti-realist conclusion to empirical theory is unmotivated, since we have external evidence in this case to nail down the references of our terms, and rule out unintended interpretations. The claim in mathematics is equally implausible, since the same empirical evidence is at issue. Again, Putnam has the Quinean indispensability argument in the background.

As I show in Part 2, Putnam presents an indispensability argument, the success argument, which is independent of QI and its use of formal theory to reveal ontic commitments. He maintains that formal results lead to anti-realism, but if he rejected the link between existence claims and formal theories and their models, then no anti-realist conclusion would follow from the puzzles of model theory. Putnam could follow his own advice. "So to follow this line - which is, indeed, the right one, in my view - one needs to develop a theory on which interpretations are specified *other* than by specifying models." (Putnam (1980) p 14)

One option would be a traditional realism not based exclusively on the construction of axiomatic theories. Putnam limits the realist's resources in such a way as to make this alternative unappealing. He compares two conflicting systems of set theory, while continuing to assume that no empirical evidence will settle the truth value of the axiom of choice. The first system includes the axiom of choice. The second he attributes to hypothetical extra-terrestrials who reject this axiom. Given indispensabilism, we have no basis to discriminate between the two systems. Putnam portrays the realist as committed to arbitrary decisions in mathematics. "But if both systems of set theory - ours and the extra-terrestrials's - count as *rational*, what sense does it make to call one *true* and the others *false*? From the Platonist's point of view there is no trouble in answering this question. 'The axiom of choice is true - true in *the* model, he will say (if he believes the axiom of choice)." (Putnam (1980) p 10)

This is, admittedly, one possible response, though it is not the best one. There is mathematical evidence available beyond the empirical constraints of indispensability. Even without further evidence, we can perform a Euclidean rescue and see both systems of set theory as independently true, of different universes of sets. The realist is not forced to make arbitrary and unjustified pronouncements on unanswerable questions.

Putnam attacks a straw man when he derides the realist for relying on mysterious faculties. "[T]here is the extreme Platonist position, which posits non-natural mental powers of directly 'grasping' forms (it is characteristic of this position that 'understanding' or 'grasping' is itself an irreducible and unexplicated notion)..." (Putnam (1980) p 1)

It is not essential to realism that its key notions are inexplicable, even if they are so far unexplained. The realist may explain mathematical understanding in terms of reliability, consistency, coherence, and intuition. These factors are not, with the possible exception of the last one, objectionable to the naturalist.

Putnam's success argument also relies on empirical evidence for mathematics. Putnam rightly sees that any indispensability argument is insufficient to justify mathematical knowledge without the Unfortunate Consequences. His anti-realist conclusion is to disparage all metaphysical realism.

Indispensability, in the guise of restricting mathematical justification to empirical evidence, is a major source of Putnam's anti-realism in mathematics. Since Putnam's general anti-realist case depends on the mathematical case, indispensability is a major cause of Putnam's broader anti-realism.

The anti-realist Putnam maintains naturalism and disparages non-indispensabilist mathematical realism. He subordinates mathematical practice to scientific practice. His worries about mathematical truth, and his consequent worries about metaphysical realism, arise from Restriction. His concerns about the Axiom of Constructibility come from his recognition that empirical evidence is insufficient to establish truth values for a variety of mathematical claims. These are all indications of indispensability.

Here is another way to the same end: Putnam's arguments that no statement is unrevisable, and that there is no absolute a priori, all rest on his analysis of Euclidean geometry. If we do not accept his claim that relativity theory showed that something we thought a priori turned out not to be so, then we have no reason to believe there is no absolute a priori. Putnam's anti-realism is unmotivated, unless he can make sense of indispensability. In Part 2 of this chapter, I argue that he can not.

§1.7: The Unification Thesis

In the introduction to *Mathematics, Matter and Method*, Putnam writes that the unity of his work to that point consists of four doctrines.

PD.1: Mathematical and scientific realism;
PD.2: Rejection of the absolute a priori;
PD.3: Rejection of the link between the factual and the empirical; and
PD.4: Mathematics is both empirical and quasi-empirical.

None of the themes Putnam cites are successful. He arrives at PD.4 by conflating empiricism with fallibilism. His arguments only establish that mathematical claims are defeasible, not that they are empirical. Once he adopts quasi-empiricism, though, he allows real empirical justification of mathematical claims.

Putnam takes the indispensability argument to support PD.3, which is Putnam's "[R]ejection of the idea that 'factual' statements are all and at all times 'empirical', i.e. subject to experimental or observational test." (Putnam (1974) p vii) Mathematical statements are supposed to be factual, but not empirical. Putnam fails to see that the empirical justification for mathematics at the core of his indispensability argument makes mathematics empirical, i.e. subject to experimental or observational test. Since mathematics is empirical, on Putnam's account, the link remains.

His claim to PD.1, including realism about mathematical objects and necessity, is a stretch. The volume which contains this claim includes papers which promote deductivism and modalism in addition to realism. Certainly his later work is a rejection of realism, even if he calls it internal realism. In the next part of this chapter, I show that Putnam's success argument, his clearest defense of mathematical realism, fails.

As for PD.2, the version of apriorism which Putnam rejects entails necessity and indefeasibility. Putnam is right that anyone who held this position must have been mistaken. It is plausible to read figures in the history of philosophy, like Descartes, as holding it. Kant's view of Euclidean geometry as the necessary, indefeasible structure of space was surely a mistake. Putnam takes the wrong lesson from this. He makes the a priori relative instead of fallible.

The failures of PD.1 - PD.4 aside, indispensability is a deeper unifying theme. Deductivism and modalism only commit to limited elements of the thesis, since those positions do not commit to mathematical objects. In terms of the Essential Characteristics, Putnam's deductivism contains Naturalism, Theory Construction, and Subordination of Practice, though it restricts the claim of Mathematization. Putnam's modalism has the same characteristics, though in place of Mathematization, he recognizes what one might call Modalization, that empirical science is committed to possibilia. Putnam's realism contains all the Essential Characteristics, as does his anti-realism, though he moderates their ramifications.

In the despairing, Putnam (1994), "Philosophy of Mathematics: Why Nothing Works," he criticizes both intuitionism and formalism for conflicting with tenets which are essentially indispensabilist. The intuitionist, Putnam argues, can not connect his mathematical logic with empirical logic. He changes the meaning of the logical connectives. Implication, in intuitionist mathematics, means that there is a constructive proof procedure in which the consequent follows from the antecedent. "While the assumption that there are such things as verifications ('proofs') of isolated statements may be all right in mathematics, it is not in physics, as many authors have pointed out. So what does $\supset$ mean in an *empirical statement*?" (Putnam (1994) p 509)

He assimilates formalism with a broader program, and again criticizes the program for failing to adhere to indispensabilist principles. "In short, the formalist seems to be really a kind of philosophical nominalist - and nominalism is (it is generally believed) inadequate for the analysis of empirical discourse." (Putnam (1994) p 502)

Putnam, despite flirtations with deductivism and modalism, and throughout his attempts to make sense of quasi-empiricism and internal realism, was always committed to the basic thesis of mathematical empiricism. "It is only when the language of mathematics is considered as an integral part of the language of science as a whole - in other words, considered in its relation to *empirical* science - that the reason for making these [mathematical] assumptions can become clear." (Putnam (1956) p 87)

Part 2: The Pragmatic and Success Arguments

§2.1: Introduction

In this part of the chapter, I consider two versions of the indispensability argument which do not depend on Quine's holism. Since holism is not a factor in the argument, one could deny or remain agnostic towards the claims of science, and still attempt to justify mathematical knowledge via indispensability. This argument is Resnik's pragmatic indispensabilism. Putnam's success argument also does not, despite appearances, appeal to science for its defense of mathematics. I show that both arguments are, for the most part, appeals to the practical utility of mathematics, and so are no arguments for mathematical truth. Also, I show that Putnam's allegation that mathematical truth accounts for unexpected applications of mathematics, its unreasonable effectiveness, is ungrounded.

§2.2: Resnik's Pragmatic Indispensability Argument

In Chapter 3, I attributed a holistic indispensability argument to the structuralist. Both Shapiro and Resnik make this holism explicit. Resnik also presents a pragmatic indispensability argument, which does not rely on holism. In this section, I show that the pragmatic argument fails, though not in the same way as the holistic argument.

Resnik first links mathematical and scientific justification.

(RP) RP.1: In stating its laws and conducting its derivations, science assumes the existence of many mathematical objects and the truth of much mathematics.

RP. 2: These assumptions are indispensable to the pursuit of science; moreover, many of the important conclusions drawn from and

within science could not be drawn without taking mathematical claims to be true.
RP.3: So we are justified in drawing conclusions from and within science only if we are justified in taking the mathematics used in science to be true. (Resnik (1997) pp 46-7)

Then, he adds an argument that we are justified in using mathematics because of its explanatory, predictive and technological fecundity. This yields an argument for mathematical truth.

RP.4: We are justified in using science to explain and predict.
RP.5: The only way we know of using science thus involves drawing conclusions from and within it.
RP.C: So, by RP.3, we are justified in taking mathematics to be true. (Resnik (1997) p 48)

One potential criticism of any indispensability argument is that by making mathematical justification depend on scientific justification, one makes mathematical truth depend on scientific truth. And, it is notoriously difficult to establish the truth of scientific theory.

Quine argues that mathematics is used to represent empirical findings and to infer non-mathematical conclusions from non-mathematical assumptions, and that scientists presuppose mathematics, even when constructing highly idealized versions of theories. Some critics, like Nancy Cartwright and Bas van Fraassen have argued that science, or much of it, is false, in part due to these idealizations. If our justification of mathematical truth is based on its use in scientific theory, then the mathematics requires an auxiliary defense.

The success of science may be explained without presuming that scientific theories are true, if van Fraassen and Cartwright are right. On the other hand, others like Devitt have argued that such skepticism is unfounded. I take the debate over scientific realism to remain open, and beyond the range of this project. Still, the problem with indispensability is the linking of justification for science and mathematics. The truth or falsity of science is moot.

Resnik's pragmatic argument avoids the potential problem of the falsity of science.[130] Even if our best scientific theories are false, their undeniable practical utility still justifies our using them. The pragmatic argument states that we need to presume the truth of mathematics even if science is merely useful.

The pragmatic argument, though, does not avoid the more pressing problem of instrumental interpretations of portions of scientific theory, which demonstrate that quantifications over mathematical objects can also be read only instrumentally. In the absence of QP, the inference to the truth of mathematics in RP.1 is unjustified. The utility of mathematics is not by itself an argument for its truth. We need a procedure for determining existence (or truth, or reference) to establish that these are to be found in science.

The same problem appears in RP.2. The scientist may work without considering the question of mathematical truth at all: without employing a truth predicate applicable to mathematical statements, and without taking mathematical theorems to be true.

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[130] Taking Resnik's pragmatic argument as a response to the problem of scientific truth is idiosyncratic, on my part. Resnik's stated aim in presenting the pragmatic argument is to avoid the problems Maddy and Sober raise for holism, which I discussed in Chapter 2, §1.3. But the argument appears to have the concomitant allure of avoiding the problems van Fraassen and Cartwright, among others, raise for truth in science.

Resnik can respond that the scientist's beliefs are irrelevant, and that his work entails those commitments anyway. But then, again, we need a procedure for determining commitment. Resnik has not provided a pragmatic argument independent of holism, but a reference back to Quine's holistic argument. One may want to be an indispensabilist without holism, but if knowledge of mathematics depends on knowledge of science, it does not seem possible.

Moreover, a pragmatic argument for the indispensability of mathematics seems like no indispensability at all. All scientists need, whether we interpret their work as true or merely instrumentally useful, is the practical utility of mathematics. They need not presuppose mathematical truth.

Resnik is right to notice the compelling questions about why mathematics is useful in science. "[The pragmatic argument] has the fairly limited aim of defending mathematical realism by pointing out that any philosophy of mathematics that does not recognize the truth of classical mathematics must then face the apparently very difficult problem of explaining how mathematics, on their view of it, can be used in science." (Resnik (1997) p 47)

The appropriate response for the nominalist is a project like Field's, which provides a tidy explanation. Mathematics is useful because it is just a convenient shorthand for more complicated statements about physical quantities. For any one who thinks that mathematical objects are causally isolated from the physical world, applicability remains a question.

The best non-indispensabilist account of application may be Balaguer's FBP, because it takes any consistent mathematical theory to be true.[131] The problem of application is solved merely by noting that for all consistent situations there is a mathematical theory which applies to it.

Resnik's pragmatic indispensability argument fares no better than his holistic argument. If it is not an enthymemic reference to the holistic argument, it is merely a weak appeal to the utility of mathematics. Putnam's success argument has similar problems, as I show in the next section.

§2.3: Putnam's Success Argument

Resnik hypothesizes that both Putnam and Quine may have intended to formulate indispensability arguments like Resnik's pragmatic one. This claim is unlikely when applied to Quine, who clearly relied on holism. In Putnam's case, Resnik may be right. In this section, I reconstruct Putnam's success argument, and in the next section, I show how it contains flaws like those in Resnik's pragmatic argument.

Putnam provides the following seed of his success argument. "I have argued that the hypothesis that classical mathematics is largely true accounts for the success of the physical applications of classical mathematics (given that the empirical premisses are largely approximately true and that the rules of logic preserve *truth*)." (Putnam (1975a) p 75)

Putnam's success argument for mathematics is analogous to, and may be

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[131] Balaguer's account is actually slightly more subtle. "[A]ll purely mathematical theories truly describe some part of the mathematical realm, but...it does not follow from this that all such theories are *true*." (Balaguer (1998) p 60)

compared with, his success argument for scientific realism, which I discuss briefly and set aside. The scientific success argument relies on the claim that any position other than realism makes the success of science miraculous.

(SS) SS.1: Scientific theory is successful.
SS.2: There must be a reason for the success of science.
SS.3: No positions other than realism in science provide a reason.
SS.C: So, realism in science must be correct.

Given the relatively uncontroversial SS.1 and SS.2, the argument for realism in science rests on SS.3, and the miracles argument. But, strictly false theories such as Newtonian mechanics can be extremely useful and successful. If realism were the only interpretation which accounted for the success of science, then the utility of many false scientific theories is left unexplained. An instrumentalist interpretation on which theories may be useful without being true better accounts for the utility of false theories.

There are probably good responses to this quick criticism, but refuting SS is besides the point, here. My point here is merely that the miracles argument, which is independent of Quine's holism, despite maintaining the connection between mathematical and scientific justification, is best understood as an argument for scientific realism, and not for mathematical realism. I now set it aside and examine Putnam's analogous but independent success argument for mathematics.

(MS) MS.1: Mathematics succeeds as the language of science.
MS.2: There must be a reason for the success of mathematics as the language of science.
MS.3: No positions other than realism in mathematics provide a reason.
MS.C: So, realism in mathematics must be correct.

To see that MS is independent of SS, consider that even if science were interpreted instrumentally, mathematics may be justified by its applications. The problems with scientific realism may focus on the incompleteness and error of contemporary scientific theory. These problems need not infect our beliefs in the mathematics applied. A tool may work fine, even on a broken machine.

MS.1 is not even offensive to the nominalist who thinks we can dispense with mathematics. MS.2 is just a demand for an account of the applicability of mathematics to scientific theory. MS, like SS, rests on its third step, which I evaluate in the next section.

§2.4: Problems with Success

MS goes wrong in two ways. First, the third premise is weak. Second, even if one could establish that premise, and the argument, the mathematics it would establish would suffer the Unfortunate Consequences.

I start with the second class of criticisms. Putnam's success argument for mathematics retains all the Essential Characteristics of an indispensability argument. It should be clear by the work in Part 1 of this chapter that Putnam's argument is an element of his commitment to EC.1, Naturalism. The argument commits to Theory Construction, EC.2, at MS.2. Putnam sees knowledge of mathematics as justified by the construction of that best science, which is EC.3, Mathematization. And Putnam defends the primacy of scientific practice, EC.4, mainly to avoid a mystical platonism.

Since MS has all the Essential Characteristics, it is burdened with the resulting Unfortunate Consequences. Restriction is even more of a problem for Putnam than it is for any Quinean holist. Quine was able to get slightly more mathematics on the basis of simplicity and rounding-out. He could justify extending our commitments on the basis of virtues of theoretic construction, since his holism extends justificatory weight around the whole theory. Putnam can not appeal to this rounding-out since he jettisons Quinean holism.[132] Only the mathematics which is used successfully in science would have any justification. Similar points apply to the rest of the Unfortunate Consequences.

But the Unfortunate Consequences are really moot, since MS.3 fails. Putnam's argument for that premise is essentially a rejection of the argument that mathematics could be indispensable, yet not true. "It is silly to agree that a reason for believing that p warrants accepting p in all scientific circumstances, and then to add 'but even so it is not *good enough*'." (Putnam (1971) p 356)

For the holist, Putnam's argument has some force. For, the holist has no external perspective from which to evaluate the mathematics in scientific theory as instrumental. He can not say, "Well, I commit to mathematical objects within scientific theory, but I don't really mean that they exist." Instrumentalism entails a rejection of holism.

For Putnam, who rejects holism, instrumentalist interpretations of the mathematics used in scientific theory are even more compelling. For, he is no longer constrained to limit existence claims to the quantifications of our best theory. He is free to adopt an eleatic principle, for example, as the fundamental criterion for existence.

More importantly, we need only one account of the applicability of mathematics

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[132] Putnam does not completely reject holism, maintaining the holistic thesis of disciplinary blur among logic, set theory, and mathematics.

to the empirical world other than the indispensabilist's to refute MS.3. Balaguer's FBP, suffices, since it claims that mathematics provides a theoretical apparatus which applies to all possible states of the world.[133]

One could amend MS.3:

> MS3*: Realism best explains the success of mathematics as the language of science.

This change does not help, though, since realism does not best explain the application of mathematics. Realism is just the claim that some mathematical claims are true, and some mathematical objects exist. It says nothing about the applicability of mathematics to the physical world. Moreover, Field's dispensabilist construction, in conjunction with the conservativeness claim, erodes confidence in MS.3 by presenting an alternate account of why mathematics is useful in science.

My rejection of MS contained two distinct elements. First, there are other, and better, accounts of the application of mathematics to physical theory. Any application which actually explains the connection between abstract mathematical objects and the physical world will be preferable to Putnam's, which takes this relationship as brute. Second, even if we were to accept the validity of MS, the mathematics yielded would still suffer the Unfortunate Consequences.

These elements, combined, reveal a tension in MS. The objects justified by indispensability are concrete, known a posteriori, and exist contingently and temporally.

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[133]See Balaguer (1998), Chapters 3 and 4.

So, indispensability can not establish mathematical knowledge. But, if it could, the account of why mathematics is useful in science would clearly be missing since the mathematical objects inhabit a separate, abstract realm.

§2.5: The Unreasonable Effectiveness of Mathematics

I consider one final attempt by Putnam to establish mathematical truth. Mathematics often develops in response to problems in empirical science. Independently of MS, Putnam argues that the truth of mathematics accounts for this phenomenon. But the fact that science can impel advances in mathematics is not a reason for saying that mathematical claims are true.

First, mathematical falsehoods may be useful in empirical reasoning. The paradoxes of set theory, for example, do not infect simple applied set-theoretic inferences. Even if I were to believe the axiom of comprehension, my inference from the union of two disjoint four-member sets of concrete elements to one eight-membered set would remain justifiable. I see four objects here, four there, count them together, perform similar operations, and develop a robust set theory including the axiom of comprehension. The mathematical theory arises from reasoning about empirical objects. It is useful, but false.

Second, and more generally, the origins of an idea are independent of its justification. Our mathematical ideas may arise from a variety of sources: analogy with other mathematical ideas, utility to scientific theory, solutions to purely mathematical problems. Justification is independent.

The relationship between mathematics and science is prima facie puzzling. Given a traditional, abstract, view of mathematics, including PCI, we seem to have two disjoint worlds and no reason to expect a relationship between them. The indispensabilist seems to solve this problem, since knowledge of mathematics is justified in the same way as knowledge of empirical science. That is, the indispensabilist rejects PCI. I have argued that the rejection of PCI is one indication of how the indispensabilist does not really generate mathematical objects. Still, even if we take the indispensabilist as providing an account of the connection between empirical science and the mathematics it uses, he has an even more difficult problem with a related problem. The indispensabilist lacks an account of why mathematics which has not been used in science can so easily come to be integral to an empirical theory. Putnam mentions this phenomenon.

> Descartes's assumption of a correspondence between the points on a line and the reals was a daring application of what we now recognize to be non-denumerable mathematics to physical space. Since space is connected with physical experience, it is perhaps not surprising that *this* found physical application. Likewise, the calculus was explicitly developed to study *motion*, so perhaps it is not surprising that this too found physical application; but who would have expected *spectral measure*, of all things, to have physical significance? (Putnam (1975) p 76)

Wigner's seminal article on this "unreasonable effectiveness" (Wigner (1959)) makes a similar claim. But effectiveness can only come as a surprise, or be apparently unreasonable, if we have a mistaken conception of the connection between science and mathematics. If we understood this relation correctly, then we would not be surprised, just as we are only surprised by physical phenomena when we hold an errant physical theory.

Whatever philosophy of mathematics turns out to be correct, it should yield that mathematics is somehow applicable to the physical world, often in ways the mathematician did not originally conceive. Indispensability is disfavored by this constraint. For, the indispensabilist has no explanation of why mathematics developed independently of science would find application. It is indeed a mystery why an idle game, unconnected to physical experience, would find application. The unreasonable effectiveness of mathematics is an artifact of the indispensabilist's subordination of mathematical practice to empirical scientific practice.

Relatedly, the discovery of a physical application can allay worries about a controversial new mathematical theory. This phenomenon may also mistakenly be taken as support for the indispensability argument. The discovery of a physical use for a controversial mathematical theory can allay worries because any model of a theory is evidence for the theory's truth. The physical world can serve as a model. So, if we find physical correlates, we can be confident that the mathematical theory is consistent. Any justification of mathematics will provide such an account. It need not be indispensabilist.

Whatever explanation we accept for the discovery of mathematics which only later finds application in physical theory would apply also to the mathematics discovered in the construction of scientific theory. If we have an account of the justification of unapplied results, we could use it to justify the applied results as well, and do away with the indispensabilist's indirect justification via scientific theory. Parsimony would entail a single, uniform account.

That mathematics may be developed for science is evidence neither for the truth of mathematics nor for its dependence on empirical evidence. The subsequent application of mathematics developed independently of science disfavors indispensabilism.

In this chapter, I have shown that the indispensability arguments presented by Resnik and Putnam which do not depend on Quine's holism are no more successful than the holistic versions. Without holism, we have only the pragmatic utility of mathematics, which is no argument for its truth.

Chapter 5: Towards Autonomy Realism

In this final chapter, I draw some conclusions about the direction that an account of mathematics must go, given the multiple failures of the indispensability argument. I use 'autonomy realism' for a position on which a) mathematical objects exist; b) some mathematical claims are true, but not vacuously so; and c) our knowledge of these objects and truths does not depend on our knowledge of empirical science. I defend the pursuit of autonomy realism.

In Part 1 of this chapter, I argue that mathematics shares important characteristics with empirical science, characteristics which confer legitimacy on an autonomous mathematical methodology. In Part 2, I characterize autonomy realism. In Part 3, I show how autonomy realism is preferable to nominalism, given the failure of the indispensability argument. Lastly, I indicate some areas for further research.

Part I: The Legitimacy of Mathematics

§1.1: The Limits of Logic as Mathematics

I begin Part 1 by exploring the mathematical tools that logic provides in the absence of an indispensability argument. Finding these insufficient, I proceed to argue for the legitimacy of mathematics in its own right.

Any current indispensability argument is speculative, since we do not know which mathematical claims will finally be required by empirical science. Furthermore, even an ideal indispensability argument will suffer the Unfortunate Consequences.

Those unwilling to pursue independent justification for mathematics might explore the resources provided by logic alone. We need logic in scientific theory anyway. The extent of the mathematics attainable from logic depends on which logic one takes as canonical. From first-order logic, we can generate logical dopplegangers for finite natural numbers. The would-be indispensabilist also has concrete templates at his disposal, which may perform practical geometric tasks. Of course, these tools are insufficient if scientific theory requires even the full theory of natural numbers, let alone analysis.

One might argue for a stronger logic. Shapiro recommends second-order logic, which yields rudimentary set theory. With any logic which generates some sort of set theory, one can construct objects which function like the natural numbers, and other mathematical objects.

Still, the claim that we can avoid mathematical commitments on the basis of a strong logic rings hollow. We can see the objects generated by second-order logic as replacements for sets and other mathematical objects only because we already possess profound information about mathematics. We take '{{ϕ}}' as a two, for example, on the basis of a translation between the objects intended as the models of the Peano axioms and a set-theoretic sequence. We can only take sets to serve the functions of numbers if they provably perform all the tasks that numbers do. Similarly, we can only take the objects of second-order logic as sets if they do the work of sets. Our prior knowledge of sets, or of numbers, is a constraint on the reductive claim. The would-be indispensabilist who attempts to restrict her ontology by adopting higher-order logics would disingenuously pretend to eliminate mathematics while using mathematical knowledge as a constraint on the construction of scientific theory.

Focusing on the logic used in science is instructive, though, for it shows how the appeal to scientific theory in even an ideal indispensability argument is insufficient to generate mathematical ontology. Consider a giant book in which is inscribed the spatio-temporal position of every object in the universe at every moment. This could be done with no mathematics, since we can use the dopplegangers constructible out of first-order logic. If one wanted to predict the position of any object at any time, say, or the direction of its motion, all you would have to do is look it up. I presume that the space-time is denumerable, that there are denumerably many space-time points, to avoid quibbling over whether the notion of a non-denumerably-long book is plausible, and whether one could look up what one wanted in it. But the argument could work with non-denumerable space-time, too.

Such a book would perform just about all the functions of scientific theory that we could want. It would be perfectly precise and perfectly predictive, if inelegant. It would need no mathematics beyond that generated by first-order logic.

The nominalist, we saw in Chapter 1, Part 2, is uncomfortable substituting logic for mathematics, since we will need a substantial account of our knowledge of any logic strong enough to do the work for science. Similarly, the autonomy realist resists resting with logical substitutes, because the strong logic hides mathematical claims. Scientists need more than first-order logical machinery, whether in the guise of axioms which govern the structure of a substantivalist space-time, or in the guise of traditional mathematical theories. In Chapter 1, I explored the first option, but it is unclear whether it suffices. I proceed to explore the latter alternative.

§1.2: Bootstrapping

In this extended section, I first present a too-quick argument from the rejection of reductive physicalism to the acceptability of mathematics in its own right. The argument is subject to a bootstrapping criticism. Maddy and Putnam have each also flirted with positions which fall to a bootstrapping criticism, and I discuss their proposals as well, with an eye to avoiding the problem.

§1.2.1: Scientific Pluralism

Quine's indispensability argument relied on both holism and physicalism: we have one best theory covering all our experience and we expect that all posits of that theory will be covered by some very general physical laws. In Chapter 4, I showed that dropping holism did not benefit the indispensabilist. In this section, I argue that dropping physicalism also disfavors indispensability, and pushes us toward autonomy realism. If we do not think that science can be reduced to physics, we have good reasons to accept mathematical objects on the basis of criteria for their existence which are independent of science. Mathematical objects should be admitted into our ontology, not because of their indispensable use in physics or economics, but because they are the objects to which mathematics itself refers.

Dropping physicalism, or accepting a supervenience form of physicalism on which the special sciences maintain their autonomy, opens up the possibility that different branches of science are independently justified. Biology no longer needs to be reduced to physics, for example. In the absence of reductive physicalism, we need criteria which determine whether a field of study is legitimate. In the philosophy of science, the problem of settling on these criteria is known as the demarcation problem. My quick argument is that any good solution to the demarcation problem makes mathematics an acceptable science.

This argument has a parallel in the philosophy of language. Consider whether we should believe in propositions, or other intensional objects. Quine's well-known arguments against synonymy and related notions like analyticity, in Quine (1951) and elsewhere, rely on naturalist, scientific constraints. Katz argues that intensional notions can be included in a scientifically acceptable theory. The disagreement between Quine and Katz comes down to the scientific merits of a linguistic theory which posits propositions. Quine recognizes this, in reply. "The question of assuming intensional notions in our theory comes down to the question of whether they would play a useful role in a theory that meets the test of prediction. That is where the doubts come." (Quine (1990a) p 198)

Katz argues that Quine's doubts result from the nascence of linguistics. A mature linguistics, especially, for Katz, a decompositional semantic theory, would include intensional notions essentially. In Katz's semantic theory, abstract objects are basic elements. Empirical science begins with common-sense physical objects, linguistics begins with abstract types, and mathematics begins with mathematical objects.

If we reject reductive physicalism, and accept the legitimacy of mathematics in its own right, even an ideal indispensability argument will be superfluous. Still, without a solution to the demarcation problem, or at least a demonstration that any solution must pronounce mathematics legitimate, any claim for the legitimacy of mathematics is just bootstrapping. In §1.3, I argue that a reasonable solution to the demarcation problem will declare mathematics legitimate. First, I examine two other bootstrappers.

§1.2.2: Maddy and Bootstrapping

In an attempt to legitimize pure mathematics, Maddy (1992) attempts to extend Quine's indispensability argument. She wants to avoid UC.1, Restriction, by accepting all of the Essential Characteristics except EC.4, Subordination of Practice.

Maddy's modified indispensability argument first appeals to the indispensable applications of mathematics to convince us generally that there are mathematical objects. She does not specify which version of the indispensability argument she intends; I presume she means some version of QI. Then, Maddy notes that the appeal to QI is inadequate to justify mathematical practice.

> [I]ndispensability theory cannot account for mathematics as it is actually done... [W]e must conclude that the indispensability arguments do not provide a satisfactory approach to the ontology or the epistemology of mathematics. Given the prominence of indispensability considerations in current discussions, this would amount to a significant reorientation in contemporary philosophy of mathematics. (Maddy (1992) p 289)

As part of this reorientation, Maddy appeals to mathematical practice to justify specific results, including the more abstruse, pure results. Maddy thus separates ontology, which is justified via indispensability, and methodology, which is justified via mathematical practice.[134]

> A modified indispensability argument first guarantees that mathematics has a proper ontology, then endorses (in a tentative, naturalistic spirit) its actual methods for investigating that ontology. For example, the calculus is indispensable in physics; the set-theoretic continuum provides our best account of the calculus; indispensability thus justifies our belief in the set-theoretic continuum, and so in the set-theoretic methods that generate it; examined and extended in mathematically justifiable ways, this yields Zermelo-Fraenkel set theory. (Maddy (1992) p 280)

Extending QI as Maddy suggests conflicts with its homogeny. The Quinean can not separate ontology from methodology. Maddy includes a backdrop of indispensability in order to connect mathematical methodology with the basic tenets of Quine's naturalism. But, as Maddy notes, there are two different interpretations of 'naturalism' which might be relevant. On a narrow interpretation, we only add mathematical axioms as they are required by empirical science; this is the indispensabilist's position. A broader version of naturalism includes mathematics as itself naturalistically defensible. The broader version justifies mathematical knowledge directly, since we just add the axioms yielded by our independent mathematical methodology to our best theory. We do not need the indispensability argument.

By itself, the broader version is incomplete. We must ask why the naturalist should accept mathematics as legitimate, since the practice can not justify itself. Maddy

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[134] Maddy may have taken her cue from some remarks in Field (1980) p 4, where Field suggests that the indispensabilist can have more than just a little rounding-out beyond the results needed to account for application.

writes, "Mathematics, after all, is an immensely successful enterprise in its own right, older, in fact, than experimental natural science. As such, it surely deserves a philosophical effort to understand it as practiced, as a going concern... [A] philosophical account of mathematics must not disregard the evidential relations of practice or recommend reforms on non-mathematical grounds." (Maddy (1992) p 276)

If justification comes only from mathematical practice, by parity of reasoning the psychic's reliance on the crystal ball, or divine revelation, can be justified. Appeals to psychic powers and revelation are also older than experimental natural science. While mathematical practice may be successful explaining mathematical phenomena, so may psychic powers be successful in explaining psychic phenomena, like ghosts and communication with the dead. The natural scientist claims that ghosts do not exist, and thus we need no account of our knowledge of them. Similarly, the mathematical nominalist claims that mathematical objects do not exist, and thus we need no account of them.

Even if we set aside this bootstrapping problem, indispensability is insufficient to get the ontic ball rolling. Maddy's modification may ameliorate the problems arising from UC.1, but it does nothing to solve the problems with the other Unfortunate Consequences. For example, the mathematics which results from Maddy's argument, like that which results from any indispensability argument, suffers from UC.6, Uniqueness. Various answers to the continuum hypothesis lead to various incompatible, though provably consistent, set theories. But, mathematical practice allows us to accept all of the competing answers, perhaps as competing results of independent axiomatizations. The indispensabilist is committed only to those which apply to the physical world. Maddy is forced to accommodate two irreconcilable views.

More importantly, Maddy does not provide sufficient criteria to distinguish mathematics, as a legitimate discipline, from illegitimate pursuits. She merely says that it is a longstanding pursuit successful on its own terms. Putnam makes a similar mistake.

§1.2.3: Putnam and Bootstrapping

In Chapter 4, I took Putnam's success argument, MS, as an indispensability argument. Another possible interpretation of Putnam's argument bootstraps.

(MS2) MS2.1: Mathematics succeeds in itself. That is, it is fruitful.
MS2.2: There must be a reason for the success of mathematics.
MS2.3: No positions other than realism in mathematics provide a reason.
MS2.C: So, realism in mathematics must be correct.

Like Maddy's modified argument, MS2 is insufficient to generate a justification of mathematics, since we need antecedent criteria to determine whether a theory is successful. If the criteria come from within the theory, as they do in MS2.1, then any theory can be deemed successful. The crystal ball can tell you to believe the crystal ball.

Within mathematics, mathematical criteria reign. We can justify a posit, for example, by its fruitfulness. Consider, as Putnam does, in (Putnam 1975a), Descartes's posit.

(AG) There is a one-one correspondence between points on a line and real numbers.

Even lacking proof that there are as many points on a line as there are real numbers, the fruitfulness of analysis makes it indispensable for the practice of mathematics. Thus we should believe AG. This kind of indispensability is inference to the best explanation and it yields justifications of particular mathematical theorems in the same way that a theoretical posit yields electrons. But we can only entertain a statement such as AG if we have prior commitments to points and lines and real numbers.

Nothing I have said in rejecting the indispensability argument applies generally to inference to the best explanation, which is essential to scientific and mathematical methods, justifying posits, of subvisible particles or fields in physical science, for example. It can not serve as a justification for an entire discipline, like mathematics.

Maddy's modified indispensability argument, Putnam's MS2, and my quick argument from the rejection of physicalism all suffer bootstrapping problems which can be solved if we find a reasonable solution to the demarcation problem. Criteria for good science should rule out obviously unacceptable fields, like parapsychology, and rule in obviously acceptable ones, like empirical science. Then, we can see what they say about mathematics.

§1.3: Demarcation

Exact characterization of acceptable scientific methodology is a notoriously intractable problem. Still, there are some relatively uncontroversial claims. Good science produces replicable results. These results cohere with other accepted results. The methods used in good science receive broad acceptance. When these factors are absent, empirical results are dubious.

Burgess and Rosen present a detailed list of theoretical virtues which may be taken as a step to solving the demarcation problem.

> BR.1: Correctness and accuracy of observable prediction.
> BR.2: Precision of those predictions and breadth of the range of phenomena for which such predictions are forthcoming, or more generally, of interesting questions for which answers are forthcoming.
> BR.3: Internal rigour and consistency or coherence.
> BR.4: Minimality or economy of assumptions in various respects.
> BR.5: Consistency or coherence with familiar, established theories.
> BR.6: Perspicuity of the basic notions and assumptions.
> BR.7: Fruitfulness, or capacity for being extended to answer new questions. (Burgess and Rosen (1997) p 209)

This list need not be taken as a revolutionary insight. These criteria are just the ordinary constraints on scientists in their daily work. Neither need we take this list as a categorical solution to the demarcation problem. It only needs to be a good working hypothesis, and it shows how mathematics should be classified with the good sciences.

Merely by omitting 'observable' from BR.1, or by interpreting that word to apply to our observations of mathematical results, like a token of a proof, or to intuitions of mathematical truths, the list is perfectly applicable to mathematics. The weights we ascribe to the different factors may differ from those we ascribe in any particular empirical case, but empirical cases will differ amongst themselves, too. Mathematical theorems must be perspicuous, as in BR.6, and proofs, or at least proof methods, must be available for scrutiny and receive broad acceptance, as in BR.5. Also as in BR.5, mathematical results must cohere, especially results which bridge mathematical sub-fields. Consider the mathematical virtues of Wiles's proof of Fermat's theorem, which bridged topology and number theory. Wiles proof increased the range of mathematical phenomena which topology predicts, as in BR.2. We seek alternative proofs of a theorem, which helps with BR.1, BR.3, and BR.5. A mathematical theory must be consistent, as in BR.3.[135] Axioms should be fruitful, as in BR.7, and few, as in BR.4. The consequences of axioms should be intuitively acceptable.

The omission of observation in BR.1 will seem like a major concession to those who deny mathematical intuition. They will argue that observation is essential for validating our beliefs, whereas mathematical intuition will seem like magic.[136] But, overwhelmingly the other criteria favor considering mathematics as a science. We should not stack the deck in favor of one criterion alone. Otherwise, it no longer seems that we are using the demarcation criteria objectively, but that we are using them to rule out mathematics.

One should not worry that a wider interpretation of BR.1 will actually allow magic. A defender of the legitimacy of parapsychology, or one who wished to show the illegitimacy of mathematics by assimilating it to parapsychology, would argue that similar criteria are present in the psychic case. This is wrong. Psychic practice posits conduits to knowledge which conflict with our best scientific theory *while attempting to explain the same phenomena*. Empirical evidence weighs heavily against such conduits. It is, or at least was, epistemically possible for psychic ability to be legitimate. There are interpersonal, mind-independent truths sought by mathematicians, but the psychic can

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[135]At least, it must be globally consistent. Dialetheic systems might contain local inconsistencies, but these have to be somehow isolated from the rest of a theory.

[136] Michael Devitt made this argument, in personal communication.

say whatever he wants. The psychic's results are not replicable and fail to cohere with accepted science. The psychic's methods are suspect.

The application of BR.1 - BR.7 to mathematics varies from their application to science, but they are the same broad, naturalist criteria. Applied to mathematics, the specific constraints on theory construction are not empirical. The phenomena explained by mathematics are mathematical facts, not empirical ones.

Still, the list shows how mathematics works like good science. In the next section, I argue that we should include mathematical objects and truths among our starting points for theorizing.

§1.4: Starting with Mathematical Objects

BR.1 emphasized the predictiveness of good science. Mathematics, too, is predictive, of mathematical phenomena, of the behavior of mathematical objects.[137] If we accept mathematical methodology, we must also accept mathematical phenomena. Quine, and all indispensabilists, reject pure mathematical phenomena, though autonomy realists accept them. In this section, I argue that Quine adopts a kind of unacceptable foundationalism to reject autonomy realism.

We do not possess a complete account of A) what exists, and B) how we know what exists. A healthy attitude toward our speculation would be to seek an equilibrium between our best estimates of each. Quine's foundationalism consists of the insistence

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[137] Reference to the behavior of mathematical objects is language borrowed from observation, and is merely metaphoric. It is common in mathematics where one naturally talks of the behavior of functions, or how sets act under operations.

that we settle B according to strict empiricists tenets, and ensure that all claims A conform. QI presumes this method at QI.1. Quine's foundationalism does not pretend to establish certainty, but it does claim the epistemic cleanliness sought by traditional foundationalists. By leading with his empiricism, Quine maintains a latent positivist tendency to denigrate metaphysics, not by eliminating it, but by making it subservient to epistemology.

In linguistics as in mathematics, Quine leads with his empiricism to deny the existence of intensional objects. If we admit sense properties, as Katz argues we should, we can solve the supposed problems of indeterminacy of translation. "As a general argument against the determinacy of translation, Quine's argument fails because it rules out evidence about sense properties and relations by fiat. It begs the question against the intensionalist who does not understand intension extensionally..." (Katz (1998) p 90)

Beliefs about sense properties, like mathematical beliefs, are beliefs of spatio-temporal beings. The best account of these beliefs may posit non-empirical justification, as long as it is consistent with our limited sensory apparatus. Naturalism in this broad sense could account for mathematical facts.

There is a tension between Quine's naturalism and QI. QI relegates existence claims to the domain of quantification of our best empirical theory, and opposes autonomy realism, while naturalism in the broader sense can support an autonomous mathematics.[138] "[I]f the nominalistic reformer is to claim to be an adherent of

[138] Whether we call this an extension of naturalism to include mathematics, or a rejection of naturalism for its refusal to include mathematics, is merely terminological. I have chosen the former route. Katz preferred the latter.

naturalization or naturalism in epistemology, the 'naturalism' in question must be of a restricted variety, making invidious distinctions, marginalizing some sciences (the mathematical) and privileging others (the empirical)." (Burgess and Rosen (1997) p 211)

Given criteria which justify mathematics as an autonomous discipline, we need an account of our mathematical knowledge. Part of this account just refers to ordinary mathematical methodology. But, another part parallels Quine's account of the construction of empirical theory. On Quine's account, the naturalist starts with ordinary objects and constructs a theory (or theories) to account for his experience. The autonomy realist starts also with mathematical objects and constructs a theory (or theories) to account for mathematical phenomena. The question for the philosopher of mathematics, as for the philosopher of science generally, is to explain, "How it is that man works up his command of that science from the limited impingements that are available to his sensory surfaces." (Quine (1974) p 3)

Quine avoids sense-data reductionism in part due to the practical impossibility of a sense-data construction, but also on principle. The empiricist wanted unmediated, unassailable data as a starting point. But the notion of a sense datum is itself the product of a substantial theory about human perception and the way in which we gather information.

Quine thus accepts ordinary physical objects as a defeasible starting point. Scientific theories are to be judged both on the basis of their ability to account for this evidence, and, since evidence under-determines theory, on the basis of constraints on theoretical construction.

By eschewing mathematical objects or truths as starting points, Quine adopts a restrictive foundationalism. The alternative is transcendental. We lead with substantive claims in both epistemology and metaphysics, and seek a reflective equilibrium. Quine has already adopted this methodology in empirical science, by starting with objects. In the philosophy of language, one may lead with semantic claims, about truth or reference. The role for epistemology would then be to generate a plausible account of the links between the semantic claims and our abilities to learn and use language. Similarly, we should accept basic mathematical facts and look for good explanations of our knowledge of these.

Kant's attempt to generate epistemology transcendentally led him to make unjustifiable claims, both about our psychology and about the certainty of our knowledge. Neither of those are essential to a transcendental approach. Leading with one's metaphysics does not mean that one has to claim certainty for those metaphysical claims. Once we sever the link between apriority and necessity, the transcendental approach is even more attractive. We can err about a necessary truth, without taking either the original proposition or its flaw to be held a posteriori. The transcendental approach does not necessarily countenance a priori knowledge, although it can easily accommodate apriorism. Even a naturalist can adopt a transcendentalist approach.[139]

The foundationalist will demand to know how it could be possible to settle questions about the existence of mathematical objects before the epistemic issues are

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[139] This is Devitt (1984)'s strategy for defending scientific realism, for example. Also see Devitt (2003).

settled. He worries that leading with metaphysics encourages untoward speculation. Errors have arisen this way: Kant's claim that Euclidean geometry is a necessary aspect of our psychology, Cartesian theism and dualism, Plato's denigration of the sensible world. This is a serious worry. The transcendentalist has to have at least a working hypothesis as to the nature of human epistemic capabilities.

Historically, the foundationalist is on equally tenuous footing. Hume's epistemology led to extreme skepticism, and Goodman's elaboration of the problems of induction show just how intractable those problems are, for the empiricist. All sorts of claims have been made about our epistemic capabilities, in the absence of a clear and complete understanding of psychological and physiological data. Somehow, we perform ordinary inductions. Somehow, we have knowledge of ordinary objects around us. Somehow, we have mathematical knowledge. The job for the epistemologist, in conjunction with the empirical scientist, is to account for these abilities.

Devitt makes a similar point in response to sense-data reductionism. He notes that we can either take sense data to be mental, which leads to idealism, or non-mental, which leads to another sort of anti-realism. "Thus the price for knowledge turns out to be the abandonment of familiar reality. Epistemology determines metaphysics." (Devitt (1984) p 70)

The same criticism should be levied against Quine for his omission of mathematical objects as starting points. By requiring that only empirical objects require an account, Quine weakens his defense of mathematics. If calling this foundationalism seems a stretch, it is no matter. The point remains that Quine started with ordinary objects because he recognized that reductionism failed, but when he adopted the common objects of human knowledge, he should have included mathematical ones.

We should start our transcendental theorizing with a full mathematical ontology, including sets, numbers, and abstract spaces. The specific commitments are unclear, for at least three reasons. First, mathematicians discover new theorems and generate new proofs which contain existential assertions. Second, debates over foundations, especially in set theory, have not generated universal agreement on the extent of the set-theoretic universe. Third, and related, depending on what we take as a criterion for mathematical existence, we may expand the mathematical universe as far as FBP, or contract it significantly.

In Part 1, I have argued that mathematics is a legitimate discipline independent of empirical science and that we should pursue a transcendental approach to epistemology and metaphysics, seeking a reflective equilibrium between the claims of mathematics and our epistemic abilities. In Part 2, I sketch the elements of this autonomy realism.

Part 2: Autonomy Realism

§2.1: Introduction

A detailed development of autonomy realism is beyond the scope of this dissertation. Still, since I take the moral of the failure of the indispensability argument to be that we must pursue autonomy realism, a brief elaboration is warranted. I first review some motivations for autonomy realism. Then, I examine one autonomy realist theory, Balaguer's FBP, and find it wanting. Lastly, I present some of the central elements of any acceptable autonomy realism, including mathematical intuition, mathematical necessity, and a fallibilist a priori.

§2.2: Motivations for Autonomy Realism

My overriding goal in attacking the indispensability argument was to eliminate empirical support for mathematics in order to motivate autonomy realism. Despite my earlier claim that the Unfortunate Consequences are not decisive against the indispensability argument, I do take them as reductios. If the indispensability argument were the best way to justify mathematical knowledge, then this dissertation would be an argument for mathematical anti-realism.

Autonomy realism, in contrast, is motivated by the following considerations already discussed in the sections cited:

AR.1: Mathematical posits are different kinds of posits than those of empirical objects, including odd empirical objects like fields and space-time points. They arise differently, and they are tested differently. (Chapter 2, §1.3; Chapter 2, §3.2; Chapter 3, §5 - §6)

AR.2: Autonomy realists approve of Euclidean rescues, on which changes in scientific theory do not tell against mathematical theories. (Chapter 3, §3)

AR.3: Autonomy realism does not suffer, though the indispensabilist does, from the Unfortunate Consequences. (Chapter 2, Part 3; Chapter 3, §2 - §3)

AR.4: Autonomy realism can accept mathematical diversity, which dissolves indeterminacy problems like that of Benacerraf (1965). (Chapter 2, §3.7; Chapter 3, §5)

AR.5: Autonomy realism allows a semantics which treats mathematical sentences generally according to their surface grammar. (Chapter 3, §4)

AR.6: Autonomy realism best accounts for the application of mathematics to science. (Chapter 3, §3; Chapter 4, §2.2; Chapter 4, §2.5)

AR.7: Autonomy realism provides the best account of mathematical facts, taken as starting points in our theorizing. (Chapter 5, §1.4)

AR.8: The criteria which legitimate science also apply in mathematics. They distinguish good sciences, like physics and mathematics, from bad science, like parapsychology. Mathematics is one of the good sciences. (Chapter 5, §1.3)

If AR.1 - AR.8 were not enough reasons to prefer autonomy realism to indispensabilism, remember that the indispensability arguments I have examined do not even work. QI fails due to problems with holism and QP. The structuralist does not make a successful case for ontic blur. The non-holistic indispensabilists provide only a weak appeal to the utility of mathematics in science. The argument for autonomy realism against indispensabilism is really moot. The autonomy realist's main opponent is the nominalist. I show how autonomy realism compares favorably to nominalism in Part 3 of this chapter.

Still, I suspect readers of this work may resist the anti-indispensabilist conclusions. Burgess and Rosen understand as well as any philosophers the limits of the indispensability argument and the motivations for autonomy realism, but they resist what I take to be the obvious conclusion. They take naturalism and its descriptive methodology as a sufficient to reject a priori, or "alienated" epistemology. "The two stand to each other rather as the descriptive grammar of Chomsky stands to the prescriptive grammar of Fowler." (Burgess and Rosen (1997) p 209) They rule out dispensabilist reformulations on the basis of their unattractiveness. They reject fictionalism, too, leaving only indispensability as an option for an account of mathematics. They find indispensability unacceptable on the grounds that parsimony is not a scientific merit. They have shown all possible positions untenable.

The lesson for Burgess and Rosen is to reconsider which elements of alienated epistemology might be acceptable. They should pursue autonomy realism.

§2.3: Problems with FBP

There are many potential versions of autonomy realism. One of these is Balaguer's FBP, which claims that every consistent set of mathematical axioms truly describes a universe of mathematical objects. For example, ZF + CH and ZF + not-CH each describe set-theoretic universes, despite their conflicting claims. In this section, I argue that FBP is not the best form of autonomy realism, because it generates too many objects in too many true theories.

FBP is similar to Field's fictionalism. Field argues that, "[M]athematicians are free to search out interesting axioms, explore their consistency and their consequences, find more beauty in some than in others, choose certain sets of axioms for certain purposes and other conflicting sets for other purposes, and so forth; and they can dismiss questions about which axiom sets are *true* as bad philosophy." (Field (1998a) p 320)

FBP accepts all of these claims, except the last. While Field thinks that the mathematician is free because all his theories are false or vacuous, Balaguer thinks that the mathematician is free since all his theories are true. In either case, truth is no constraint. FBP and fictionalism are correct that certain questions that the indispensabilist takes as having a unique answer, like the question of the size of the continuum, may in fact have many conflicting answers. There can be several set-theoretic universes, each with their own defining set of axioms, which taken together are inconsistent. Still, we need not jettison all talk of truth.

FBP provides no account of our focus on the standard model of the Peano postulates. "FBP-ists maintain that all consistent purely mathematical theories truly describe some collection of mathematical objects, but they do not claim that all such theories are true in a standard model." (Balaguer (1998) p 60) Every model of the axioms, on FBP, is equally acceptable. While focus on the standard model might be explained sociologically, and by its ubiquity, we may find gradations among different consistent theories. Some theories may be better mathematical theories than others.

Distinguishing between consistency and goodness is the main problem facing the autonomy realist. FBP only requires apprehension of consistency, not a contentious mathematical intuition. But it avoids the hard question of why we privilege certain systems. Justifying our interest in a standard model, if it is not to rest on application, will require appeal to intuition.

§2.4: Mathematical Intuition

The general form of my argument for autonomy realism is:

(AR)
- AR.1: We have mathematical knowledge.
- AR.2: Our mathematical knowledge must either be strictly derived from our scientific theories, or it must be autonomous.
- AR.3: Our mathematical knowledge is not strictly derived from our scientific theories.
- AR.C: Thus, autonomy realism.

I focused on AR.3 in Chapter 1 though Chapter 4. AR.2 is uncontroversial. AR.1 remains a worry.

FBP attempts to account for mathematical knowledge on the basis of our pre-theoretic apprehension of consistency. The autonomy realist who wishes to explain our focus on the standard model will hone this criterion.

> One *might* adopt the ontological position that there are multiple 'universes of sets' and hold that nevertheless we have somehow mentally singled out one such universe of sets, even though anything we say that is true of it will be true of many others as well. But since it is totally obscure how we could have mentally singled out one such universe, I take it that this is not an option any plenitudinous platonist would want to pursue. (Field (1998b) p 335)

On the contrary, this is exactly the position I have been pursuing. The obscure mental process to which Field refers is mathematical intuition. Any account of our knowledge of science must refer to our ability to reason about our commonsense beliefs. This ability to reason can not plausibly be limited to our knowledge of formal logic, since we must have some basis on which to develop such theories. One aspect of the evidence we have for science, for mathematics, and for logical theories must be our ability to reason. This ability is where the autonomy realist must look for accounts of mathematical intuition.

Field pointedly rejects intuition. "Someone *could* try to explain the reliability of these initially plausible mathematical judgments by saying that we have a special faculty of mathematical intuition that allows us direct access to the mathematical realm. I take it though that this is a desperate move...” (Field (1989a) p 28)[140]

Part of the worry about autonomy realism which makes it seem desperate is that it allegedly relies on a mysterious psychic ability. “The naturalism driving contemporary epistemology and cognitive psychology demands that we not settle for an account of mathematical knowledge based on processes, such as a priori intuition, that do not seem to be capable of scientific investigation or explanation.” (Resnik (1997) pp 3-4)

We must naturalize epistemology by debarring mystical and mysterious elements. Mathematical intuition must be compatible with a mature psychology. An epistemology which includes intuition is more than merely plausible. We reason all the time, in mundane matters as well as mathematics, logic, and linguistics. We can not rule out the possibility of a scientific, naturalistic explanation of our ubiquitous ability to reason.[141]

Naturalism has become a dear doctrine to many philosophers as a way to avoid both mysticism and an unsatisfying empiricism. On the empiricism side, one is faced with the failures of logicism and positivism to provide uncontroversial justifications of mathematical knowledge. No one wants to return to a pure Millian account of mathematics.

On the mysticism side, we see accounts of mathematics from Plato, Descartes,

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[140] See also Field (1989a) p 21, where he derides the sui generis solution to the Benacerraf puzzle about identifying numbers with sets; Field (1982a) pp 66-67, on Gödel’s view, which Field misleadingly calls indispensabilist; and Field (1985b) p 190.

[141] Parsons (1980) assimilates mathematical intuition with our ordinary ability to identify types. “At least one type of essentially mathematical intuition , of symbol- and expression-types, is perfectly ordinary and recognized as such by ordinary language.” (155)

and Gödel. Consider Putnam's remarks about Gödel's platonism. "The trouble with this sort of Platonism is that it seems flatly incompatible with the simple fact that we think with our brains, and not with immaterial souls. Gödel would reject this 'simple fact', as I just described it, as a mere naturalistic prejudice on my part; but this seems to me to be rank medievalism on *his* part." (Putnam (1994) p 503)

If naturalism is to do any work at all, it must forestall an autonomous mathematical epistemology, as long as that is seen as entailing mysticism. Also, every one wants to avoid Kantian psychologism, which is like mysticism in positing substantial mental structures without empirical evidence.

One reason why one might think that autonomy realism requires mysticism is if one makes unreasonable demands on what counts as a non-mystical (i.e. naturalist) mathematical epistemology. For example, Putnam thinks that the autonomy realist requires a dedicated brain structure for mathematical perception. "We cannot envisage *any* kind of neural process that could even correspond to the 'perception of a mathematical object'." (Putnam 1994) p 503) Further, Putnam writes that appeals to intuition are, "[U]nhelpful as epistemology and unpersuasive as science. What neural process, after all, can be described as the perception of a mathematical object? Why of one mathematical object rather than another?" (Putnam (1980) p 10)

Any account of mathematical reasoning must be consistent with neuroscience, but this connection may be many degrees more subtle than the discovery of a region of the brain dedicated to mathematical perception. Putnam's demand for the details of neural processes which account for our apprehension of mathematical objects is too stringent.

Indeed, the claim that there are neural processes which provide mathematical perception would be part of an empirical account of mathematics.

I do not pretend to have a neuroscientific account of mathematical intuition. But even a professed naturalist like Putnam recognizes the utility of appeals to intuition, though he grounds them unhelpfully in empirical science. The alternatives to autonomy realism are too unsatisfying. Lacking full accounts of hard neuroscientific issues like consciousness, let alone apparently easier ones like perception, dismissing autonomy realism is too hasty, considering both the robustness of pure mathematics and the need for intuition to account for it.

In exactly the same way in which our knowledge of the external, physical world begins with an apprehension of physical objects in the absence of any plausible reductionist account to sensory stimulation, we begin our exploration of mathematical knowledge in the absence of an account of intuition. In both cases, the account must be judged on the basis of what is the best overall account of our common-sense views of both mathematics and the physical world. It may turn out that the best account of our purported mathematical knowledge involves denying that we have any. But such a denial must carry with it an account of why we seem to have mathematical knowledge, and this is a task which no eliminativist has accomplished.

Mathematical intuition, or, less contentiously, mathematical reasoning, allows us to distinguish among consistent mathematical theories. It will, of course, be an a priori method of belief formation. A traditional objection to intuition relies on the impossibility of forming substantial, yet indefeasible, mathematical beliefs. This objection does not affect the fallibilist a priori I have touted.

The autonomy realist requires an a priori faculty of mathematical intuition. The account of this faculty will include a detailed examination of all our belief-forming processes, and a partition of these into a priori and empirical classes. I have not provided such an account in this dissertation. Relatedly, autonomy realism should include an account of mathematical necessity, which I describe in the next section.

§2.5: Necessity

For the indispensabilist, mathematical objects exist as contingently as the physical world. This Unfortunate Consequence pushes us toward autonomy realism. But if we adopt FBP as our autonomy realism, then, according to Balaguer, we still suffer from Modal Uniformity. In this section, I argue that this is another reason to reject FBP, and to seek a more refined autonomy realism.

Balaguer bases his rejection of necessity on its obscurity. Mathematical existence claims are not logically or conceptually true, he argues. Adopting Field's stance, he says there is no other coherent view of necessity.

Balaguer argues that the realist only employs necessity to ground his epistemology, as Katz and David Lewis do. They argue that since mathematical objects exist necessarily, there is no need to account for the conditions of their existence.

> There problem here is that we just don't have any well-motivated account of what metaphysical necessity consists in. Now, I suppose that Katz-Lewis platonists *might* be able to cook up an intuitively pleasing definition that clearly entails that the existence claims of mathematics - and, indeed, all purely mathematical truths - are metaphysically necessary. If they could do this, then their claim that

> mathematical truths are necessary would be innocuous after all. But (a)...the claim would still be epistemologically useless, and (b) it seems highly unlikely (to me, anyway) that Katz-Lewis platonists could really produce an adequate definition of metaphysical necessity. (Balaguer (1998) pp 44-45)

Claim (a) might be right, though it is not far from Balaguer's own claim. FBP says that every consistent mathematical theory truly describes a mathematical universe. This is very close to saying that the theorems of mathematics, when true, are necessarily true, and that mathematical objects exist necessarily. Moreover, the necessity of mathematics can help explain why consistency entails truth.

Balaguer's real argument here is (b). Balaguer and Field are right that mathematical necessity can not be logical necessity. But I am not as skeptical as Balaguer about the prospects for an account of necessity for mathematics, and I do not see his argument against it.

There are at least two reasons to want an account of mathematics on which mathematical objects exist necessarily. Besides the Katz-Lewis view, which uses necessity to ground an epistemology, one might merely wish to account for the intuition that there is a difference between what might have been different and what could not have been different. Field appealed to a similar intuition in support of his modal operators. Autonomy realism need not concede necessity.

In Part 2 of this chapter, I have merely sketched the view to which I believe we are compelled by the findings of the first four chapters of this dissertation. In fact, the main opponent of the autonomy realist is not the indispensabilist, but the fictionalist. In Part 3, I show how autonomy realism compares favorably to Field's fictionalism.

Part 3: Nominalism vs Realism

§3.1: Introduction

Given the failure of indispensability, philosophers of mathematics really have two choices: either deny that mathematical objects exist, or show how we can know of them independently of science. In this part of the chapter, I compare what I take to be the most viable form of the first option, Field's fictionalism, with autonomy realism.

First, I examine Field's dispensabilist construction. This time, I consider more general arguments than those I considered as a response to QI in Chapter 1, Part 2. Then, I describe the difficulties with Field's resulting position. In effect, here, I am considering the value of Field's dispensabilist construction, not as it affects the indispensability argument, but as it affects autonomy realism.

The value of dispensabilist reformulations has been questioned before. Any assessment must presume some way of measuring that value. Colyvan (2001) argues that standard science is more attractive than Field's reformulation. Attractiveness is a vague criterion which can be tailored to fit one's preconceptions. The use of 'best' in QI.1 and QI.4 encourages such inexactitude. Pincock (forthcoming) argues that standard science is better confirmed than Field's reformulation. This seems implausible, since the reformulation makes fewer commitments. My concern is different than either of these.

More plausibly, though also not my concern, Burgess and Rosen argue that for Field's theory to be accepted as a better one, it should be publishable in scientific journals, and adopted by working scientists. Since dispensabilist reformulations are not preferred by practicing scientists, they are no better. Burgess and Rosen presume that the arbiters of better theories are scientists. This is also the wrong way to measure the value of a theory. The practicing scientist wants a useful theory which can generate empirical results. The 'best' in QI, which is concerned with ontic commitments, is not to be decided on the basis of practical merit. Field's defense of his reformulation correctly emphasizes concerns about ontic commitments of scientific theories. Dismissing these concerns, as Burgess and Rosen do, begs the questions raised by QI, about whether scientific theories need to quantify over mathematical objects. Still, I think that Field's principle of intrinsic explanation, on which his defense rests, is wrong.

§3.2: Intrinsic and Extrinsic Explanations and Theories

Field argues for his nominalism on the basis of a preference for intrinsic explanations over extrinsic ones. In this section, I clarify these terms and show that Field's emphasis on explanation is misleading. His real goal is an intrinsic theory.

Field defines his terms as follows:

> If in explaining the behavior of a physical system, one formulates one's explanation in terms of relations between physical things and numbers, then the explanation is what I would call an *extrinsic* one. It is extrinsic because the role of the numbers is simply to serve as labels for some of the features of the physical system: there is no pretence that the properties of the numbers influence the physical system whose behavior is being explained. (The explanation would be equally extrinsic if it referred to *non-mathematical* entities that served merely as labels...) (Field (1985b) pp 192-193)

Field uses 'intrinsic' and 'extrinsic' to apply to entities, theories, and explanations. The application to entities appears basic, since he classifies explanations and theories depending on the types of objects involved. An explanation is intrinsic, presumably, if it relies only on intrinsic entities. We take a theory to be intrinsic if we see in it no demand for extrinsic entities. For example, Field claims that numbers are extrinsic to physics, while physical objects are intrinsic. Numbers are extrinsic to geometry, too, while line segments and their ratios are intrinsic.

The application of 'intrinsic' within mathematics proper begs several questions about the relationships among mathematical subtheories. Are real numbers intrinsic or extrinsic to the theory of natural numbers? Are categories extrinsic to set theory? Are topological spaces extrinsic to Euclidean geometry? These questions should make us worry about the viability of a commonsense intrinsic/extrinsic distinction. I shall not pursue these questions here, though I wonder, in §3.4, whether Field's distinction can withstand problems of ontic blur.

Field relies on the presumption that numbers do not influence physical systems to classify them as extrinsic to physical theory. "If, as at first blush appears to be the case, we need to invoke some real numbers... in our explanation of why the moon follows the path that it does, it isn't because we think that the real number plays a role as a *cause* of the moon's moving that way..." (Field (1980) p 43)

Field's preference for intrinsic explanations and theories is a broad methodological principle.

> Extrinsic explanations are often quite useful. But it seems to me that whenever one has an extrinsic explanation, one wants an intrinsic explanation that underlies it; one wants to be able to explain the behavior of the physical system *in terms of the intrinsic features of that system*, without invoking extrinsic entities (whether mathematical or non-mathematical) whose properties are irrelevant to the behavior of the system being explained). If one cannot do this, then it seems

rather like magic that the extrinsic explanation works. (Field (1985) p 193)[142]

Call this principle PIE: we should prefer intrinsic explanations over extrinsic ones, where possible. Compare this to an analogous principle PIT: we should prefer intrinsic theories to extrinsic ones, where possible. Field focuses on explanations, rather than theories, which is puzzling since his project is clearly a response to the indispensability argument, which is normally formulated in terms of theories, not explanations. QI has force because of its reliance on Quine's demand that we find our ontic commitments in the theory which best accounts for our empirical experience. To reformulate the indispensability argument in terms of explanation would force us to argue that we determine what exists by consulting our explanations. Explanation is a complex topic, and a detailed discussion of criteria for explanatory merit is beyond the range of this dissertation. Still, it is worth noting that if explanations must be perspicuous, Field's dispensabilist reformulation is not explanatory. It is impossible to use, which is why he attempts to establish that mathematical theories are conservative over nominalist physical ones. Indeed the reformulation is hardly recognizable as NGT. If we were to adjust QI to focus on explanation, a preference for intrinsic explanations would not support a dispensabilist reformulation.

Still, we are here comparing Field's project to autonomy realism, not the indispensability argument, so either PIE or PIT might serve our purposes. Nevertheless, I will focus on PIT, taking Field's goal to be an intrinsic theory which may or may not

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[142] A less detailed version is found in the original Field (1980), p 44.

serve as part of an explanation. Field could argue that the best explanation consists in the standard theory plus the availability of the reformulation. In this case, the explanation is extrinsic, though one part of it is an intrinsic theory.

PIT explains Field's preference for synthetic physical theories, ones based on physical geometry, over analytic ones, which rely on real numbers and their relations.[143] Without PIT, we have no reason to think that the dispensabilist reformulation is a better theory than the standard one.

Notice that PIT can not do the work for which Field uses it, i.e. as an argument for preferring his reformulation over the indispensabilist's standard scientific theory. For, any dispensabilist must accept indispensability's ontic blur. With no sharp distinction between mathematical objects and empirical ones, the indispensabilist's mathematical objects are actually intrinsic to physical theory. Field illicitly presumes the commonsense distinction. "[I]f you think nominalism is correct, then nominalist explanations will seem intrinsic while Platonist ones will not. The Platonist need not concede this. Hilbert's explanation is indeed an *illuminating* explanation of a remarkable fact about space, but it is not clear to me that, short of begging the question against the Platonist, it is an *intrinsic* explanation." (Colyvan (2001) p 88)

[143] PIT also supports Field's argument for substantivalist space-time; see Field (1985). A space-time relationalist uses mathematics to represent measurements of physical properties, like temperature and motion, since he lacks the resources to represent these measurements nominalistically. The relationalist thus appeals to magnitude relations between matter and extrinsic mathematical objects. The substantivalist can appeal to intrinsic space-time points, which can model mathematical theories and so serve his measurement needs. PIT also helps explain Field's hostility to modal reformulations of science. Modal properties are extrinsic to physics, as are possible inscriptions.

Field argues that the posits of space-time points differ from posits of mathematical objects. "After all, from a typical platonist perspective, our knowledge of mathematical structures of abstract entities... is *a priori*; but the structure of physical space is an empirical matter." (Field (1980) p 31)

Field correctly describes the autonomy realist's perspective. PIT may be effective against autonomy realism. For the indispensabilist, though, mathematical objects are known a posteriori. Field also argues for the difference between mathematical and empirical posits on the basis of ideology. "[T]he ideology that goes with the postulate of points of space is less rich than that which goes with the postulate of the real numbers. With the postulate of real numbers goes the operations of addition and multiplication; no such operations are directly defined on space-time points in Hilbert's theory..." (Field (1980) p 32)

Resnik develops an impressive amount of mathematics within Field's space-time, given an arbitrary choice of points to serve as 0 and 1. Not only do we get addition and multiplication over the reals and the natural numbers, but we can set up a coordinate system, and define ordered n–tuples. "What bothers me about this approach most is that it is simply no longer evident to me that it is nominalism." (Resnik (1985b) p 196)

In the next four sections, I argue that PIT is false. Because of their breadth of application, extrinsic mathematical theories which autonomy realism provides are preferable.

§3.3: Field's Motivation for PIT: Hilbert's Intrinsic Geometry

Field's reformulation and PIT were inspired by Hilbert's axiomatization of geometry. Field argues that the success of Hilbert's axiomatization is explained by PIT, but the success of Hilbert's project is no argument for preferring Field's reformulation. The motivation for Hilbert's work was not the austerity of ontic commitment which underlies PIT, but worries about consistency.

Hilbert's axiomatization of geometry was part of a broader concern to defend infinitistic mathematics using only finitary reasoning. Euclidean formulations used real numbers to represent measurements of geometric objects. Hilbert wanted to sever geometry from analysis. The quantifiers of Hilbert's axiomatization thus range over regions of geometric space in lieu of real numbers, and the predicates of the theory include betweenness, segment congruence, and angle congruence, used for measurement in the way that real numbers, and their ordering, are used in analytic versions. Hilbert generated representation and uniqueness theorems which assured that his synthetic theory could adequately replace the analytic theories, which remained the basis of mathematical interest.

We can understand why Hilbert would prefer synthetic geometry over analytic versions without committing to PIT. By describing geometric phenomena in terms of geometric relationships, we avoid worries about the consistency of the hypothesis that the structure of the real numbers is isomorphic to that of the line.

Furthermore, there are no benefits of parsimony in Hilbert's program. Hilbert only shows that real numbers are avoidable in the axiomatization of geometry. Even if one were to reduce set theory to geometry, it would only be on the basis of the same kinds of translations commonly used to reduce real numbers to sets. The mathematical universe remains, sui generis.

Our main worry within mathematics is antinomy, not parsimony, and this may be more easily discovered writ large, in the discovery of impossible spaces in topology, say, than small, as indiscernible results from set-theoretic translations of topological matters. Wiles proved Fermat's theorem, for example, using extra topological ontology, and ideology, unavailable to Fermat.

That the success of Hilbert's program can be understood without appealing to PIT is further demonstrated by noting that it is the conjunction of Hilbert's construction with analysis, which maps geometric structures onto those of number theory, which really interests us. Neither number theory alone nor a pure geometry could really be called a best mathematical theory.

§3.4: Unification and Extrinsic Theories

In this section, I argue that the isolation of theories demanded by PIT conflicts with our preference to unify theories. A comprehensive theory unifies diverse disciplines. It simplifies by explaining how different commitments cohere.

For an example of the virtues of extrinsic theories within mathematics, consider how the fundamental theorem of calculus bridges geometry and algebra. Algebra could easily be seen as extrinsic to geometry, but uniting the two yields a better, more comprehensive theory.

In science, consider how welcome bridge laws between physics and chemistry or biology would be. The objects of biology appear extrinsic to physics, but if we had bridge laws, we would switch their classification, and see them as intrinsic. In fact, the classification of objects as intrinsic or extrinsic seems blatantly arbitrary and flexible. Consider how, for an Aristotelian, the objects in the heavens would have clearly been extrinsic to any principles covering terrestrial objects.

In this vein, Devitt argues that what exists is not best seen from individual scientific theories, but from the whole system.[144] "The best ontology will be that of the best unified science." (Devitt (1984) §4.9; see also §7.8)

Our desires to unify theories shows PIT, as a general principle, to be false. Extrinsic elements can improve a theory. Applied specifically to the mathematics used in science, PIT is false, also. The unification of mathematics with physics yields a simpler theory. The isolation of scientific theory from mathematics, especially on the basis of a dispensabilist reformulation, omits important relations among mathematical and physical objects.

For example, it is a mathematical property of a three-membered set that it has exactly three two-membered subsets which do not contain the empty set. Applying this property, we can account for why we can, with a red marble, a blue marble and a green marble, form exactly three different-looking pairs of marbles. The utility of mathematics to empirical theory inspired Maddy's physicalistic platonism. "[S]uppose you deposit

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[144] This claim is independent of holism. It is merely the claim that we look to all of our scientific theories, which might include mathematics, and their relations, to see what exists. It says nothing about the nature of evidence for those theories.

three quarters into a soft drink machine and a soda pops out. Which properties of that which you deposited are causally responsible for the emergence of the Pepsi? Well, the weight of the physical aggregate of metal, its shape, and also the number property: three." (Maddy (1988) pp 270-271)

These simple examples of the empirical utility of mathematics do not demand much mathematical machinery. The relevant conclusions may be derived from empirical axioms and first-order logic, though such inferences are not simple. The nominalistic reformulation of claims which refer to numbers or sets, written in terms of first-order logic is more complex than the theory which includes mathematical axioms. Its only virtue is parsimony. Field offers conservativeness to explain the utility of mathematics, but, again, simplicity weighs against his account.

Parsimony does not apply within mathematics proper. The desire for parsimony proceeds from a principle applicable in empirical science: do not multiply entities without good reason. When building empirical theories, formal or otherwise, it is important not to posit more than that which accounts for the phenomena. We reject superfluous elements of a theory, like phlogiston. Dispensing with celestial motion made scientific theory simpler, and explanatorily uniform.

The mathematician, in contrast, explores his universe with a desire to multiply entities. The virtues of extrinsic mathematical theories are obvious, since they not only unify different sub-disciplines in set theory, or category theory, but also bear fruit. In mathematics, it is a virtue to be plenitudinous, within certain limits. Once we have admitted abstracta into our ontology, we do not run out of room. Worries about the introduction of new mathematical entities, as with transfinites, or complex numbers, focus on their consistency, or the rigor with which they are introduced. "Issues about rigor, we suggest, cloud virtually every case of apparent 'ontological' debate in the mathematical sciences." (Burgess and Rosen (1997) p 225)

For example, Kripke models for modal logic have ameliorated mathematical worries about modality, even though they commit to the same kinds of possible worlds about which the philosopher worries. The background set-theoretic, or category-theoretic, reduction serves to allay worries about consistency, not ontology. The intuition that mathematical theories should not be parsimonious is also a basis for FBP.

PIT would simplify ontology at the expense of perspicuity, fruitfulness, and coherence with other theories. And it is not even clear that the simpler ontology is preferable. "It is at least very difficult to find any unequivocal historical or other evidence of the importance of economy of abstract ontology as a scientific standard for the evaluation of theories." (Burgess and Rosen (1997) p 206)[145]

Given a reformulation of standard science to eliminate quantification over mathematical objects, we have two competing theories: the intrinsic, nominalist one, and the extrinsic, mathematical one. The intrinsic theory is preferable if one has already a prior disposition to nominalism. But the standard, extrinsic theory is better, providing insight into relations among diverse objects.

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[145] Devitt (2006) argues for a principle he calls Pylyshym's razor, which states that we should not multiply representations beyond necessity. In personal communication, he urges that the principle should apply to objects in general.

§3.5: Science Can Help Explain Mathematics

Scientific theories can help explain, though not justify, mathematical ones. This favors extrinsic theories, and undermines PIT.

Mathematical theories are often difficult to understand without considering their applications. Appealing to how a theorem is applied can serve as part of an explanation of the pure mathematical theory. We can keep the bridge principles between the pure theory and its application in the background, and focus strictly on the mathematical relations. Thus diagrams are useful in abstract reasoning. When we use physical instantiations to help understand mathematical theories, we rely on an extrinsic theory.

Resnik points out that scientists can articulate a geometry in terms of light rays. “The success of the resulting optical theory is supposed to be evidence for the consistency of the geometry. Of course, if this is the way we ultimately establish the consistency of our mathematical theories, then our evidence is ultimately empirical...” (Resnik (1997) pp 113-114)

The failure of an optical theory would not affect the truth of the geometry. The geometry may be merely inapplicable in this case. We blame the bridge principles when application fails. If they hold, the concrete model may help explain the mathematics.

Our ability to hold bridge principles in the background helps explain why the indispensability argument is appealing. But appeals to empirical science can not help us to distinguish among rival mathematical theories, if all are applicable. It can not help us to distinguish between intended and unintended models, or between plus and quus.

Scientific theories can not justify mathematical statements. A physical theory can not explain how to separate the bridge principles from the mathematical ones, and it will not say anything about purely mathematical constraints. Scientific theory can only speak to the applicability of the standard model.

So, here is another way in which extrinsic theories are more valuable than intrinsic ones. This undermines PIT, and supports autonomy realism, which neatly coheres with extrinsic scientific theories.

§3.6: The End of Intrinsic Theories

In defending his preference for intrinsic explanation, Field granted the utility of extrinsic objects, but argued that explanations which use them seem like magic if there is no underlying intrinsic explanation presupposed. I presume that Field's idea is that explanations of physical phenomena should be possible which only refer to entities which are active in producing those phenomena. This claim is an instance of a more general demand on explanations: an explanation of a phenomenon should only refer to objects involved in it. This general demand is unnecessary, for the reasons I have discussed in this section. But the general demand on explanations is also irrelevant. Instead, we must examine the analogous demand on theories, that a theory which accounts for a phenomenon should only refer to objects involved. Why should one insist on this?

The obvious defense of the general demand comes from linking our theories with ontic commitment, as Quine does. But here we are not comparing Field's reformulation with an indispensability argument. A more casual appeal to intuitions about our commitments might suffice. A theory of geometry should not commit us to real numbers. A theory of gravitational force among concrete objects should not commit us to mathematical objects. A theory which accounts for causal phenomena should only refer to things that play a causal role.

On the other hand, we do not approach the question of what objects exist as a matter to be determined by isolated theories. These are broad questions about the nature of the universe. An extrinsic theory is harmless if we have the additional commitments already. We do not want mistakenly to impute causal powers to mathematical objects by using the extrinsic mathematical theory within physics. But, merely noting that mathematical objects are non-spatio-temporal blocks any confusion.

Explanations are less edifying if they are restricted to isolated, intrinsic objects. Resnik, reviewing Field's monograph and defending PIT, demonstrates this phenomenon.

> The Expected Utility Theorem, which underwrites the use of utility functions, establishes that if an agent's preference ordering satisfies certain conditions then it can be represented by a real valued function which is unique up to positive linear transformations. From this it is usually argued that there is no need to presuppose ill understood utilities in accounting for behavior which maximizes expected utility because an account can be given directly in terms of preferences. (Resnik (1983) p 515)

Resnik's intrinsic account is desirable because utilities are, as he says, ill understood. If they were better understood than preferences, then the account would go the other way. The principle underlying the Expected Utility Theorem is not PIT, but that we should explain things we do not understand in terms of things we do understand. It is ironic that Resnik uses an example which employs mathematics to characterize the elements we understand. If utilities were as well understood as mathematical theories, then accounts in terms of them would be welcome.

We can appreciate both intrinsic and extrinsic theories. The situation is like the relation between mathematical realists and intuitionists, from a realist perspective. The realist can appreciate the distinction between constructive and nonconstructive proofs, without concluding that only constructive proofs tell us what exists.

Philosophers with mathematically nominalist prejudices may see PIT as a common sense principle, and so may have neglected to recognize a gap in Field's argument. Extrinsic theories are useful and often desirable. PIT is false. Field's project is thus poorly motivated in two ways. Mathematical instrumentalism, which I argued was a viable alternative to QP, removes the need for the reformulation.[146] The failure of PIT undermines the nominalist position, generally. In the next section, I discuss problems with Field's positive account, fictionalism.

§3.7: Problems with Fictionalism

In this section, I discuss three problems with Field's fictionalism. It provides a weakened account of the difference between mathematical truth and falsity. It unavoidably assimilates mathematical statements to ones about which we get to say whatever we like. And Field defends fictionalism, which denies that mathematical objects exist, but he really only generates skepticism.

Normally, we distinguish between '2+3=5' and '2+3=6' by calling the former true and the latter false. If we call all statements which refer to mathematical objects

[146] Field considers a version of MI in case his nominalist reformulation were found untenable. See Field (1989) p 20.

false, then both of these claims are equally false.[147] The fictionalist requires that we distinguish the two sentences on the basis of applicability and utility. '2+3=5' is false, but useful, and conservativeness accounts for utility.

False mathematical theories may be useful, just as false scientific theories may be useful. If we distinguish what we normally call truth from what we normally call falsity based on utility and application, we assimilate mathematical truths to mathematical falsities.

The fictionalist denies that Wiles showed us something new about mathematical objects when he showed that there are no $n > 2$ for which $a^n + b^n = c^n$. We already knew that, since there are no numbers at all. "I imagine most mathematicians would be contemptuous of this speech and most philosophers - even most *nominalist* philosophers - embarrassed by it." (Burgess (2004) p 24).[148]

Field, of course, will credit Wiles with advancing our logical knowledge. This is a weakened account of what we learned.

In addition to the anemic account of mathematical progress, fictionalism wrongly assimilates mathematical sentences to other fictions which lack constraints about what we say concerning them. Fiction can defy physical and even mathematical possibility. In contrast, we do not have full freedom to say whatever we like in mathematics. A

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[147] I take these simple sentences as meaning that there are numbers two, three, and five, and the sum of two and three is five, and similarly for the second. If one takes them as entailments from Peano axioms, then the problem is that the former is vacuously true when it should be true because of the relations among 2, 3, and 5.

[148] Burgess credits George Boolos with the example.

mathematical theory must at least be consistent. We seek interesting problems in mathematics, but even uninteresting problems and solutions can be mathematically good. There is nothing wrong with demonstrating, in set-theoretic language, the sum of 43 and 171, as long as we get 214, in the way that there would be something wrong if we were to conclude that the answer is seven billion.

The fictionalist may respond that there are constraints on non-mathematical fictions, too, depending on how we take the metaphor between mathematics and fiction. Burgess considers a variety of options. Is mathematics like novels? If so, then we really should have full freedom to create it in whatever way we please; the metaphor fails. Or is mathematics like mythology, as Leslie Tharp suggested? Or metaphors, as Stephen Yablo suggested? Or fables? If we take fables or mythology as paradigms, we may defend a constraint on mathematics, derivative from the constraints on mythology and on fables. We can not make Athena the goddess of grain.

The fictionalist would be unwise to assimilate mathematics to myths or fables. Aligning the accounts of mathematical goodness and with mythology would lead to worrisome questions about the standards for establishing mathematical theorems. Myths are hardly evaluated at all. We can construct new myths, but these need not be consistent with the old myths.

Field argues that good mathematics is conservative, and conservativeness is close to consistency. The fictionalist thus has standards for distinguishing among theories. But the account of mathematical conservativeness is not as clean as Field would like. The fictionalist cares about consistency only as a pragmatic condition on conservativeness. An inconsistent mathematical theory is no longer conservative, implying new nominalistically acceptable conclusions.

The difference between our freedom to construct fiction and the constraints on mathematics is not decisive against the fictionalist. The fictionalist need not commit to a positive account of mathematics based on the positive account of novels, say. Field does not suggest that we abandon our standard mathematical criteria for acceptance of theorems, or revolutionize mathematical practice. But by making the analogy with fiction, he invites such comparisons.

The problems of calling mathematical claims false and assimilating mathematics to fiction apply to any fictionalism. An additional problem arises for Field's particular version. Field grants that mathematical objects could have existed. This is a result of his object-level modal operator.[149] The possible existence of mathematical objects leads to skepticism about mathematical objects, not fictionalism.

Consider the world as it is, and accept with the fictionalist the contingent non-existence of abstract objects. Now, imagine that numbers are suddenly created.[150] Field's modal account renders this possible. By causal isolation of abstract objects, we are in principle unable to know of them. The fictionalist can not say that abstracta do not exist, but only that we have no way of knowing.

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[149] See Chapter 1, §2.6.2. Field represents our knowledge of the consistency of the axioms of ZF as'$\Diamond AX_{ZF}$'.

[150] Azzouni argues that nothing would change if mathematical objects ceased to exist, in Azzouni (1994) p 56. David Lewis denies that we can say anything sensible about how the world would be if there were no numbers (Lewis (1986) p 111).

This problem does not arise for the ordinary nominalist who relies on considerations about our isolation from mathematical objects to deny their existence. The problem arises for Field because he conjoins fictionalism with an account of consistency which makes mathematical objects possible, but not actual.

None of the three problems I discussed in this section rely on taking mathematics to be indispensable to science, as other arguments against fictionalism do. For example, Maddy argues that fictionalism fails to account for why one mathematical story seems most important. "Oliver Twist, whatever his other virtues, lives only in one good story among others, but the characters of mathematics, no matter how we twist and turn, have a stubborn way of introducing themselves into a story that is our very best of all." (Maddy (1990b) p 204)

Maddy thus denies fictionalism on indispensabilist grounds. There is a best theory, she says, and we can not seem to avoid mathematics when formulating it. If we had to decide between fictionalism and indispensabilism on this basis, the fictionalist theory is preferable. The fictionalist can see the role of mathematics in a best theory as a pragmatic matter. Different mathematical stories apply differently to different physical worlds. But the fictionalist leaves us without an account of the ubiquity of mathematics to which Maddy alludes.

The autonomy realist, in fact, has a better account of application, since it is broader. The autonomy realism is best able to account for new applications of mathematics since it provides whatever mathematics might ever be needed in science.

Field argues that he can best account for the application of mathematics to empirical science, through appeal to conservativeness. As evidence, he cites Michael Friedman as having rejected Field's nominalism, while, "[E]ndorsing its account of the applications of mathematics." (Field (1985a) p 191)[151]

Field recognizes, though, that the realist can also take his representation theorems as an account of application. The realist does not deny the legitimacy of the fictionalist's tools; he just has more. Yet on his own terms, Field does not explain application, since the representation theorems are not available in the official, first-order version of his theory. If he can develop representation theorems for all applications of mathematics, then the realist and the fictionalist can use the same account.

The autonomy realist, ex hypothesi, has an epistemology for mathematical objects as well as the independent account for empirical objects. For FBP, for example, our knowledge of mathematical objects arises from our ability to recognize contradictions, or non-contradictory sets of theorems. FBP entails a mathematical description for every possible empirical situation. Thus, FBP can provide an additional account of the applicability of mathematical objects, in case the fictionalist's account fails.

Fictionalism denies any account of the axioms forcing themselves on us, beyond the applicability of mathematics. Gödel took the feeling of constraint to be evidence of mathematical intuition. "[D]espite their remoteness from sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true." (Gödel (1963) pp 483-484; see also

[151] Friedman actually says, "There is no doubt that it is a major *contribution* to our understanding of applied mathematics." (Friedman (1982) p 506), which need not be interpreted as endorsing the account.

Putnam (1994) p 503, quoting Hao Wang)

The feeling of discovering mathematical truths is not really an argument. The fictionalist can urge us to think of this feeling as of something else, like having discovered a logical entailment. But it adds to the amount of re-thinking that one must do to embrace fictionalism, and this coheres poorly with Field's claim that we are to take mathematics at face value.

The fictionalist denies that mathematical theories have a subject matter. Consider the problem of how to interpret conflicting consistent set theories, like ZF with the axiom of choice and ZF with its negation. The indispensabilist allows only one universe of sets, that which applies in science. Either there is a choice set for any set or there is not.

Fictionalism avoids this difficulty: there is no choice set. "Our different set theories 'have a different subject matter' only in that they are different stories. They differ in subject matter in the way that *Catch 22* and *Portnoy's Complaint* differ in subject matter; these differ in subject matter despite the fact that neither has a real subject matter at all...[N]either is properly evaluated in terms of how well it describes a real subject matter." (Field (1990) p 207)

The autonomy realist can also take this stance, accepting multiple set-theoretic universes, each with different objects, each with different theorems true of it. The problems with Field's fictionalism lead us to autonomy realism, just as the problems with indispensability did.

§3.8: What to Make of Nominalism

Field's dispensabilist reformulation of standard scientific theory is moot for three reasons. First, indispensability arguments do not succeed in justifying mathematical knowledge. Second, mathematical instrumentalism trumps the dispensabilist construction. Lastly, when it comes to mathematics and science, the extrinsic theory is preferable to the intrinsic one.

Field argues that whatever success the projects have weakens the case for indispensability to some degree. "If, which I take as true, the partial successes of the nominalization programme have been substantial, this very much weakens the case for the reliability of the mathematical beliefs that we apparently need in those cases where the nominalization programme has not been carried out..." (Field (1989a) p 30)

The success of dispensabilist programs plays no role in weakening autonomy realism. Further, the autonomy realist asks whether it is really believable that an adequate account of the non-existence of mathematical objects could be made on the basis of the dispensabilist constructions which appear irrelevant to pure mathematics. So, what can we make of projects like Field's?

Burgess and Rosen urge us to see reformulations as providing insight into ways an intelligent species might develop mathematics, or as accounts of how our beliefs might have arisen. "Devising alternatives distinct from and inferior by our standards to our actual theories, but in principle possible to use in their place, is a way of imagining what the science of alien intelligences might be like, and as such a way of advancing the philosophical understanding of the character of science." (Burgess and Rosen (1997) p

243)

Neither of these suggestions are plausible. Consider the pure modal strategy, which interprets the quantifiers of standard scientific theory as ranging over possible inscriptions instead of mathematical objects. It is possible, I guess, to imagine that an alien species could develop mathematical physics this way, but this does not make it plausible. Creatures intelligent enough to develop general relativity theory, for example, would be smart enough to see the facility of appeals to abstracta. Even the hardcore nominalist accepts that standard science is easiest to work with. It is more plausible to interpret our references to abstracta as primarily to modality, but if this required no re-structuring of the theory, then the difference would be merely terminological.

Neither is it plausible that modality plays a significant role in the account of our acquisition of mathematical beliefs. Any account of the origins of our mathematical beliefs will be much more mundane than appeals to formal theories about what objects could have possibly coexisted. Resnik's quasi-historical account (Resnik (1997) Chapter 9) is much more appealing, but no account is particularly interesting; the meat of the question is at justification.

Burgess and Rosen say that in the absence of the background indispensability argument, the nominalist faces a dilemma. If he takes the arguments against abstract objects seriously, then there is no need to reformulate mathematics in order to establish nominalism. He will be happy with instrumentalist interpretations. On the other hand, if he relies on the dispensabilist construction to establish nominalism, then we need an argument, in place of indispensability, to show why we are to take the construction as eliminative. "For after all, to say that such a project has succeeded is only to say that there is a nominalistic alternative to standard scientific theory that could be adopted in its place. But should it be?" (Burgess and Rosen (1997) p 63)

Burgess and Rosen possess a dry wit, so it may be that I have mistaken a sarcastic comment about possible alien intelligences for an earnest account of the virtues of reformulations. A positive account of their utility is unnecessary, anyway. They may just be interesting, demonstrating interconnections among different physical and metaphysical spaces, or structures. Mathematics helps explain these interconnections.

Quine writes, about reduction, "Clearly the system-maker in such cases is trying for something, and there is some distinction to be drawn between his getting it and not." (Quine (1969b) p 93) One must admit that something rides on whether Field's project, or others like it, is successful. I have argued that success does not eliminate justification for our mathematical knowledge. We need not grant that set-theoretic reductions or dispensabilist reformulations of science demonstrate that there are no numbers.

Field's supposition that mathematics should be excisable from scientific theory is right. Consider any physical system, like two balls connected by a spring. I use mathematics to explain the behavior of the system and can not, presently, write this explanation without using mathematics. So, abstract objects exist? The mathematics is just an instrument, a theoretical apparatus. The idea that one can base mathematical epistemology, either for or against the existence of mathematical objects, on this fact is nearly absurd.

Part 4: Conclusions and Debts

§4.1: Reductio and Validity

I have presented two strands of argument against indispensability. A reductio strand shows that the mathematics generated by the indispensability argument suffers Unfortunate Consequences. The indispensabilist accept these criticisms, implicitly or explicitly.

A validity strand prevents such acceptance. Here, my argument in part relies on the viability of MI. Indispensabilists desire an abstract mathematical ontology. But if mathematical objects are abstract, then any references to mathematical entities in our explanations of experience should be eliminable; and even if they are not eliminable, the uses of mathematics do not entail the existence of mathematical objects. Mathematical objects, since they are causally isolated from physical systems, play no role in the systems themselves. The indispensabilist avoids the causal isolation of abstract objects, but can not avoid the various methods of separating pure mathematical objects from empirical ones, which is all MI really requires.

The remainder of the validity thread consists of arguments against Quine's procedure for determining ontic commitment, his holism and physicalism, and against specific arguments from structuralists and from non-holistic indispensabilists. The structuralist failed to establish ontic blur. Resnik's pragmatic argument and Putnam's success argument may be veiled references back to Quine's methodology. Where they are truly independent, they are unconvincing appeals to the utility of mathematics, which the indispensabilist, the dispensabilist, and the autonomy realist all stipulate.

§4.2: Debts

I have presented a comprehensive rejection of the indispensability argument. Even an ideal indispensability argument will suffer the Unfortunate Consequences, which should be enough to dissuade any one interested in justifying mathematical knowledge. Consequently, one must pursue either autonomy realism, with its concomitant apriorism, necessity, and reliance on mathematical intuition, or fictionalism. I provided a few considerations favoring autonomy realism.

I owe an account of mathematical intuition, and its relation to ordinary reasoning. The main goal for such an account, in the philosophy of mathematics, is to explain the primacy of the standard model. The account of course would include an extended clarification of fallibilist apriorism. At the same time, I would like to develop a broad description of mathematical necessity, and its relation to both stronger and weaker notions; a deeper analysis of the demarcation problem; a pluralistic account of logic; and an explanation of mathematical and scientific explanation.

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