Autonomy Platonism and the Indispensability Argument

Russell Marcus

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Introduction

I am a mathematical platonist. By using this label, I intend to communicate both that I believe that there are many true mathematical sentences and that these sentences are naturally and rightly used to refer to existing mathematical objects. Among the mathematical sentences I believe to be true are 'the tangent to a circle in a Euclidean plane intersects the radius of that circle at right angles', '7+5=12', and 'the power set of a set is always strictly larger than the set itself'. Among the mathematical objects which I believe to exist are circles, numbers, functions, and sets. I believe that mathematical claims like the ones I mentioned are true, in part, because they express properties and relations of the given mathematical objects. I believe that the mathematical objects exist, in part, because they are the subjects of true mathematical sentences. Some anti-platonists might object to this reasoning, considering it circular. But the circle is not vicious; the truth of mathematical sentences and the existence of mathematical objects are, in my view, two ways of saying the same thing.

I can't quite explain why I am a mathematical platonist. I would like to believe that it has something to do with the strength of the philosophical arguments for platonism. But my discussions with anti-platonist philosophers of mathematics, my arguments and exhortations, are usually unconvincing to them. I naturally believe that it is stubbornness on the part of my anti-platonist friends which prevents them from seeing the strength of the arguments for platonism. But an uncharacteristic bit of humility convinces me that they believe that my own stubbornness prevents me from seeing the strength of their arguments against platonism. It is difficult in philosophy to convince anyone of anything. We're a stubborn lot.

The best that we can do, I think, in favor of our positions is to lay them out as clearly as possible, listen to objections, and respond as well as we can. And then we have to let the rare few of us who are both interested in the philosophy of mathematics and who are not stubbornly committed to a particular view to weigh the arguments as objectively and disinterestedly as possible. This book is for them.

As all books, this one is written in the context of a particular moment. At this moment in the philosophy of mathematics, there are two dominant views. One view, which we can call indispensability platonism, is the claim that our mathematical beliefs are justified by the uses of mathematics in empirical science. The second view, which we can call fictionalism or anti-platonism, is that our mathematical beliefs are not justified. There are lots of other views, of course, but I believe that these two are the most popular and, other than my own view, the most defensible. When philosophers write about one or other of these two views, they ordinarily neglect a third view, my own. *Autonomy Platonism and the Indispensability Argument* is my attempt to insert my view into the conversation.

My view about mathematics, intuition-based autonomy platonism, is, roughly and simply, that our mathematical beliefs are justified but that the justification of our mathematical beliefs does not depend on the uses of mathematics in empirical science. In other words, mathematics is a discipline which contains true sentences which refer to existent objects and is not subservient to or dependent on empirical science. I make this claim a bit more precise in Chapter One and I defend it, especially its invocation of mathematical intuition, in Chapter Nine.

Autonomy platonism has a long and storied history. The view has its roots in Plato (obviously), Descartes, Hume, Frege, and Gödel. More recently, my teacher, Jerrold Katz, and Mark Balaguer have explored autonomy platonist views too. The older philosophers have troubling ancillary commitments which I deny, like the Platonic view that the sensible world is somehow unreal or Descartes's view that God imprints knowledge of Himself and mathematics in our non-physical minds. But the views of Katz and Balaguer's plenitudinous platonist are quite similar to my own. It would not be inapt to think of this book as an attempt to promote Katz's view about mathematics and Balaguer has written that Katz's view is best seen as a version of his FBP.[1]

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[1] Balaguer 1998: 44.

Platonism has often been seen as a mystical, perhaps theistic view. Plato held mathematical objects in reverence. It's easy to read Descartes the same way, and Leibniz. Even Hume, who held nothing in reverence, saw that the limitations of our experience did not undermine our mathematical beliefs and sought an alternative route to understanding mathematics. It is reasonable to ask, as both the indispensabilist and the fictionalist does, for an end to the mystery.

But it seems to me that the debate between the indispensabilist and the fictionalist is at a stalemate. The indispensabilist is convinced by the range of applications of mathematics in science, its ubiquity, its practical indispensability, and its effectiveness. The fictionalist is convinced by the isolation of mathematical objects. The indispensabilist's appeals to quantification over mathematical entities fail to convince the fictionalist to abandon instrumentalism about them. The fictionalist's appeals to the causal inefficacy of mathematical posits fail to appear relevant to the indispensabilist.

It also seems to me that autonomy platonism can capture some core intuitions of both dominant views. I reject the indispensabilist's claim that our uses of mathematics in science provide a reason to believe in mathematical objects without denying that our references to mathematical objects within scientific theories are robust. By taking mathematical objects to exist, the autonomy platonist can not be accused, as the fictionalist may be, of ignoring the applications of mathematics, of regarding some portions of our best theories as false.

In contrast, the fictionalist has a point against the indispensability platonist: the uses of mathematics in empirical science do not have the ontological weight that the indispensabilist attributes to them. Scientists using mathematics in science, invoking Hilbert Spaces to represent states of quantum systems or simply using real numbers for measurement, do not seem to be committing themselves to a vast universe of abstracta. They are describing physical systems, not mathematical ones. The autonomy platonist agrees with the fictionalist that such uses of mathematics are not epistemologically relevant to our mathematical beliefs.

Some fictionalists are motivated by their embrace of the indispensabilist's conditional: if mathematics is indispensable to science, then we should believe it. But they deny the antecedent and urge that there are no other good reasons to believe in mathematical objects. Such folk are sometimes called dispensabilists. Much of philosophy of mathematics over the last thirty-five years has been centrally focused on dispensabilist arguments and constructions. This is not another book on such strategies. My concern is with the soundness of other premises of the indispensability argument, and maybe with its validity.

My autonomy platonist captures both the indispensabilist's intuition that some mathematical sentences are true and refer to existing objects and the fictionalist's intuition that uses of mathematics in science do not commit us to the existence of mathematical objects. I do, in a sense, assume platonism to conclude platonism. I motivate my intuition-based autonomy platonism in part by showing, in Chapter Five, that there are certain unfortunate consequences for platonism of adopting the indispensability argument. For the indispensabilist, the mathematical universe is unduly limited. But my deeper claim, which I make in detail in Chapter Ten, is that the circle is virtuous. We can justify mathematical beliefs without support from science, but we do need a little bit of philosophy. On my view, either you buy the whole story or you don't. Perhaps the open-minded philosopher of mathematics should reconsider the value of such a position. Or, at least, s/he might acknowledge its viability in the debate.

When I was a graduate student, I had two mentors without whom my career as a philosopher could not have begun. I don't know anyone who I believe was more right about more of philosophy than Jerrold Katz. I wish he were still with us. David Rosenthal continues to be a brilliant and generous mentor to me, and a friend.

I owe a great thanks to Jana Hodges-Kluck of Lexington Books for her encouragement and her patience as I struggled to recover from some personal difficulties as the draft of the book was due.

While this book is fairly polemical, especially in the last few chapters as I develop a traditional, mathematical intuition-based autonomy platonism, I hope that my explications of the arguments (from Quine and Putnam especially, but also of more recent indispensabilists) will be useful to those interested in the indispensability argument. There is no canonical version of the argument and subtle differences can be misleading. I've spent a lot of time on Quine's work and Putnam's work and in places, especially in the second and sixth chapters, I treat them as historical figures, reading broadly and closely and working hard on proper interpretation. In portions of those chapters, I put aside, as much as possible, my polemicism and try to get at the strongest, most charitable presentation of the argument as I can.

The book covers some of the same ground as my dissertation, especially in the earlier exegetical chapters. But it is new work. Chapter Seven is an extended version of my, "How Not to Enhance the Indispensability Argument." Etc.

Lastly, thank you especially to my wife, Emily, for her stalwart support and encouragement, and to my wonderful children, Marina and Isidor, for their patience and for not ever making me feel too bad about pursuing my professional dreams by thinking and reading and writing about mathematics and philosophy, even if it meant some distraction from what is most important.

Chapter 1: Platonism: An Overview

§1: Benacerraf's Dilemma

The most general questions in the philosophy of mathematics divide into two types. Epistemological questions concern how we know what we know about mathematics. We acquire mathematical beliefs in a variety of ways. We learn some mathematics from teachers and textbooks, authorities who provide what we take to be reliable testimony of mathematical facts. We quietly reflect on those facts, perhaps inferring others from them. We also generalize from our sense experience, counting apples, say, or measuring distances. When we get to the business of epistemology, we look for justifications of those beliefs, not their origins. How do we know that the mathematical statements that we learn in our different ways are all true, independent of how we learned them?

Ontological questions concern the subjects of mathematical sentences and theories. What are numbers and spaces and sets? What are their properties and relations? They do not seem to be like ordinary objects, since we do not see or touch them. Their properties, like primeness or being equilateral, are also unavailable to sense experience.

Perhaps the first step to determining what mathematical sentences are about is to look at semantic theories for our language. Semantic theories guide us whenever we want to understand the meanings and references of our language. In a first, shallow analysis, they tend to parse sentences into subjects and predicates, objects and their properties. A sentence like 'the grass is green' predicates a property, being green, of an object, the grass. Many mathematical sentences also seem to refer to objects, like numbers or tori, and to predicate properties of those objects like primeness or dimensionality.

The central problem which arises when thinking about answers to these two kinds of questions is that the most obvious answers to the questions of the first type conflict with the most obvious answers to questions of the second type. Knowledge of mathematical objects seems difficult to reconcile with our best accounts of our knowledge more generally. We can call this problem Benacerraf's dilemma, after Benacerraf 1973.

Much has been written about Benacerraf's dilemma, which has appeared to some philosophers to be intractable. The indispensability argument, one of the two central subjects of this book, is a popular and influential attempt to avoid the dilemma, to show how realistic (or platonistic) answers to the ontological questions about mathematics are compatible with our best epistemology. I agree with indispensabilists that our best accounts of mathematics and our knowledge of it avoid Benacerraf's dilemma. But I argue that ways of solving the problems raised by Benacerraf which do not appeal to the indispensability argument are preferable to solutions that invoke that argument.

Let's take a moment to see Benacerraf's dilemma more clearly. Consider a few mathematical beliefs: that the tangent to a circle intersects the radius of that circle at right angles, that the square root of two can not be expressed as the ratio of two integers, that the set of all subsets of a given set has more elements than the given set. Such propositions refer, when read in a straightforward manner, to mathematical objects such as circles, integers, and sets. These objects are in many ways unlike ordinary physical objects such as trees and cars. We learn about ordinary objects, at least in part, by using our senses. It is difficult to see how we could use our senses to learn about mathematical objects. We do not have sense experiences of integers or sets.

Even geometric figures, mathematical objects perhaps most closely related to physical objects, are not the kinds of things that we can sense. Consider any point in space; call it P. P is only a point, too small for us to see or otherwise sense. Now imagine a precise fixed distance away from P, say an inch and a half. The collection of all points that are exactly an inch and a half away from P is a sphere. The points on the sphere are, like P, too small to sense. We have no sense experience of the geometric sphere, a mathematically precise object.

In order to mark the differences between ordinary objects and mathematical objects, we often call mathematical objects abstract to contrast them with the concreteness of ordinary objects. There is some debate about what 'abstract' means and how best to characterize the abstractness of mathematical objects. Still, there is some agreement on the properties ordinarily ascribed to mathematical objects: An abstract object lacks spatio-temporal location. Its existence is not contingent on our existence. It lacks causal efficacy, though our belief about an abstract object may affect our other beliefs and our actions.

When we study geometry, the theorems we prove apply directly and exactly to mathematical objects, like a sphere, and only indirectly and approximately to physical objects, like a ball. Numbers, too, are insensible. While we might see or touch a bowl of precisely eighteen grapes, we see and taste the grapes, not the eighteen. We can see a numeral, "18," but that is the name for a number, just as the term "Russell" is my name and not me. We can sense the elements of some sets, but not the sets themselves. And some sets are sets of sets, abstract collections of abstract objects. Mathematical objects are not the kinds of things that we can see or touch, or smell, taste or hear.

When we want to explain how we know about anything that we know, we ordinarily appeal to our sense experiences, especially to what we see or touch. I see the desk in front of me; I can bang my fist on it for emphasis. We are rightly suspicious of anyone who claims to justify any of their beliefs through non-sensory appeals: to psychic insight or an inexplicable feeling of rectitude. If we can not learn about mathematical objects by using our senses, a serious worry arises about how we can justify our mathematical beliefs.

Benacerraf originally constructed his dilemma in the context of a causal theory of knowledge. On the causal theory of knowledge, a person knows that p just in case s/he has a justified true belief that p along with a causal connection to the subject of that belief. On the causal theory, we are prohibited from claiming that we know anything about mathematical objects because we have no causal contact with them.

It is now ordinarily accepted that Hartry Field's more-general formulation of the dilemma is superior to the original. Field avoids Benacerraf's appeals to that contentious epistemology.

> Benacerraf's challenge...is to provide an account of the mechanisms that explain how our beliefs about these remote entities can so well reflect the facts about them. The idea is that if it appears in principle impossible to explain this, then that tends to undermine the belief in mathematical entities, despite whatever reason we might have for believing in them (Field 1989: 25-6).

Field thus attempts to generalize the problem away from the causal theory of knowledge. Instead, he claims it is a problem about constraints on our beliefs, about accounting for how we could know about mathematics given our best views about epistemology, whatever they turn out to be. How could our beliefs about abstract objects be reliable if we are causally (or otherwise) isolated from those objects?

That is, then, the Benacerraf-Field dilemma: our mathematical knowledge seems to be beyond our epistemic abilities. On the one hand, our knowledge of mathematics appears to be among our most secure: who could deny that seven plus five is twelve? On the other hand, justifying those beliefs seems to demand appeals to mysterious, or at least non-sensory, epistemic capacities. It seems as if we must either cede our beliefs about mathematical objects or give up our best theories of knowledge.

Before we move on to responses to Benacerraf's dilemma, it will be useful to characterize a bit further the account of mathematical objects I presented in this section, one which I take to be standard and traditional, if not widely accepted due to concerns like the ones Benacerraf and Field discuss. This standard view is ordinarily called platonistic. Platonism, as I will use the term, is composed of two main claims: a semantic thesis (PS) and a related ontological thesis (PO).

> PS: Some existential mathematical sentences are true and others are false. Universal and conditional mathematical claims may be non-vacuously true or false.

PO: There are abstract mathematical objects, possibly including (but not limited to) sets, numbers, and spaces.

As I understand PS and PO, they are tightly related: mathematical sentences are true or false because they include references to mathematical objects with particular characteristics. 'Seven is prime' is true because the number seven is prime. The truth of 'there is an even prime' entails the existence of the number two.

Seeing a tight connection between PS and PO is not an artifact of a platonist view about mathematics. It is just the result of a literal reading of mathematical sentences and a standard theory of truth. One need not be a platonist to see PS and PO as linked. A fictionalist, one who takes sentences like 'seven is prime' to be false, also naturally accepts the link.[2]

It is possible to try to sever the semantic and ontological theses, especially by adopting PS without PO. One might, for example, take mathematical sentences to be about concrete objects or to be shorthand for complex logical sentences. One could then take mathematical sentences to be true or false without committing oneself to the existence of abstract objects. I don't believe that this is a promising route and I will sketch why I believe that standard, literal readings of mathematical sentences are preferable, where possible, in the next section. But this book is mainly focused on the contrast between two different ways to account for standard semantic analyses of mathematical language and I will not spend much time on such proposals.

§2: Anti-Platonistic Responses to Benacerraf's Dilemma

Philosophers have developed a range of strategies to deal with Benacerraf's dilemma, from embracing the epistemic horn and denying that there are abstract mathematical objects to embracing the ontological horn and positing a contentious capacity called mathematical intuition. These responses can be divided into four main groups and arranged, somewhat artificially, according to whether they are more closely aligned with Benacerraf's worry about our epistemic capacities or whether they are more closely aligned with Benacerraf's concern about our ontological commitments to mathematical objects. I will call these groups fictionalists, reinterpreters, indispensability platonists, and autonomy platonists.

At the epistemic edge, fictionalists give up hope of accounting for knowledge of abstract objects and argue that mathematical objects do not exist and that mathematical theories about those objects are, strictly speaking, false. More precisely, fictionalists believe that existential claims about mathematical objects are false while universal or conditional claims about mathematical objects are vacuously true. So, 'seven is prime' is false because there are no numbers. 'Every rhombus is a parallelogram', understood as the universally quantified conditional, '$(\forall x)(Rx \supset Px)$', is true because there are no rhombuses and so the antecedent is unsatisfied by any object; a conditional with a false antecedent is vacuously true. Mathematical theories, for the fictionalist, are convenient, useful stories, but not to be taken literally.

Contemporary fictionalism traces to Field's defense in *Science without Numbers* and his ensuing work. David Papineau joined Field in defending fictionalism, as did Stephen Yablo. More recently, Joseph Melia, Mary Leng, and Octavio Bueno have urged that the fictionalist view of mathematics represents a properly austere ontology. Fictionalists are divided about the proper route to the defense of their view. Field, defending dispensabilism, believes that we must show how scientific theories can be developed without appeals to mathematical machinery. Melia and Leng, defending what has come to be

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[2] "From the importance and non-arbitrariness of [mathematical] axioms, it doesn't obviously follow that these axioms are true, *i.e.* it doesn't obviously follow that there are mathematical entities that these axioms correctly describe" (Field 1980: 5, emphasis added).

known as easy-road (or weasel) nominalism, argue that we can deny our beliefs in mathematical objects without formulating alternative scientific theories. All fictionalists agree that a central motivation for denying the existence of mathematical objects is epistemic: we have no hope of describing access to mathematical objects and so no reason to believe that they exist.

Reinterpreters agree with fictionalists that there is no hope for an account of knowledge of abstract mathematical objects and so that mathematical objects, as standardly conceived, do not exist. They do not agree with fictionalists that mathematical theories are false or vacuous. Instead, they take mathematical terms to refer elliptically to non-mathematical objects. Rather than deny the truth of mathematical sentences, they proffer alternative objects to model mathematical theories.

Historically, Locke, who believed that mathematical theories were about psychological objects, is best classified as a reinterpreter.[3] More recent reinterpreters, like the early Penelope Maddy, rely on physical objects to construct their models. Other reinterpreters, including Charles Chihara and Geoffrey Hellman, appeal to modal properties: possible structures or possible constructions.

I will not say much about either fictionalists or reinterpreters in this book. One reason that I will not is that I find these views repugnant. The fictionalist claims that 'five plus seven is twelve' is false. The reinterpreter says that it is about ideas, or concrete objects, or possible arrangements of those objects. These claims seem highly implausible. They are best seen as interesting ways of re-interpreting claims which are literally about mathematical objects. But finding these views repugnant is mainly dogmatic stubbornness on my part.

More reasonably (I hope), both categories of views share a debilitating skepticism about our epistemic capacities. Fictionalists and reinterpreters despair of any defensible account of our access to mathematical objects. Since abstract objects are causally isolated from us, knowledge of them appears to be beyond our abilities and platonism appears to involve us in mystery. Whether we put the worries of the fictionalists and reinterpreters in terms of Benacerraf's causal theory of knowledge or in terms of Field's demand for an account of the reliability of platonist mathematical beliefs, the concern is the same.

> Special 'reliability relations' between the mathematical realm and the belief states of mathematicians seem altogether too much to swallow. It is rather as if someone claimed that his or her belief states about the daily happenings in a remote village in Nepal were nearly all disquotationally true, despite the absence of any mechanism to explain the correlation between those belief states and the happenings in the village (Field 1989: 26-7).

In the next section, I discuss two reasons that I will not spend much time on the worry about our access to mathematical objects in this book. First, I believe that the worry is overstated. A proper attention to the ways in which our ontological commitments are justified neatly deflates the problem of access. Second, in the absence of a settled account of human epistemology, it seems reasonable not to dismiss the platonist alternatives until we can see whether they really are workable.

Even more importantly, here, this book is about the differences among two kinds of platonist accounts of mathematics: autonomy platonism and indispensability platonism. Problems of access, if there really are any, will apply to any version of platonism and so can not help us decide between competing platonistic views. But because the access problem is well-known and because it motivates anti-platonist responses to Benacerraf's dilemma, it is worth a moment.

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[3] "I do not doubt but it will be easily granted that the *knowledge* we have of *mathematical truths* is not only certain, but *real knowledge*, and not the bare empty vision of vain insignificant *chimeras* of the brain. And yet, if we will consider, we shall find that it is only of our own *ideas* (Locke, *Essay*, §IV.4.6).

§3: The Access Problem

§3.1: Against Naive Empiricism

The access problem is often taken as the central challenge facing the platonist. Whether framed in terms of Field's Nepalese village example or Benacerraf's causal theory of knowledge, the problem is essentially the same. An intuitively pleasing view of how our beliefs are acquired and justified on the basis of our sense experiences seems impossible to reconcile with the standard, platonist view of mathematics. We don't have sense experiences of abstract objects.

The access problem is really a vestige of a false, if intuitively pleasing, naive empiricist view about the relationship between our sense experiences and our ontology. The naive empiricist view is that the things we know most securely are those we perceive, with our senses, directly. All other objects that we countenance must have some constitutive or causal relation to those objects. The access worry arises because mathematical objects are neither constitutive of nor causally connected to the things we know best. But this view about what objects we believe exist is utterly misleading.

We do not know categorically both what there is and how we know about it. Even if we seem to know those things, we do not know that we know them. We have good reasons to believe that sense experience is somehow central to our epistemology and we have a rough picture of how sensory stimulation and cognitive processing lead to our theories of the world: our senses are stimulated sending signals to our brains; we perceive these events by having thoughts with some content; we somehow organize and arrange these various contents, comparing and contrasting our current experiences with previous ones which we have stored in memory (and which we are able to reconstruct); we look for unifying principles that explain or predict past or future experiences. Many details of the story of how sense experience leads to robust theories of the world remain to be discovered and organized.

However the simple story is expanded or completed, it is clear that our beliefs about the world are not limited to the dictates of immediate sense experience and memory. When I open my eyes in my front yard, I perceive a maple tree. The neurophysiological process that leads to my perception of the tree is more complicated than a simple appeal to the stimulation of my retina by light. The same retinal stimulation can lead to all sorts of different perceptions, depending on my attention and background beliefs. The ways in which our cognitive apparatus participates in developing perceptions is significant and still poorly (even if increasingly less poorly) understood.

Any complete account of human epistemology will have to take into account both the bare physical facts of the stimulation of our sense organs and the contribution of our cognitive apparatus to our beliefs. Once we discard our naive empiricism, accounts of our beliefs which posit abstract objects are possible; indeed they can be compelling. Along with the admission of abstract objects, we can countenance *a priori* (i.e. non-empirical) justifications of our beliefs about them. For example, some good accounts of our knowledge of language posit both abstract linguistic objects (e.g. propositions) and native structures in the brain which are consistent with *a priori* apprehension of those objects.[4] In parallel, our best accounts of mathematics may posit both abstract mathematical objects and native structures in the brain which are consistent with *a priori* apprehension of those objects.

Once we start exploring the role of contemporary psychology in learning mathematics, we are moved to examine the role of our cognitive apparatus in determining our experience. We can seek an

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[4] Chomsky's account of language learning is sometimes called rationalist, due to its denial that grammar and vocabulary are learned by behavioral conditioning. But Chomsky's view is most aptly labeled nativist and/or conceptualist, rather than rationalist, due to his antipathy toward *a priori* epistemology and linguistic platonism. A better view along these lines is that of Katz 1990. Both Katz's linguistic platonism and Chomsky's linguistic conceptualism are consistent with the original poverty-of-the-stimulus argument against linguistic behaviorism.

understanding of the ways in which our brains work as we acquire and justify *a priori* knowledge. It is an open empirical question whether such a project can be completed. But countenancing *a priori* knowledge need not commit us to mystery. It is just a natural fact about human cognition that reasoning in the formal sciences, about the abstract objects of mathematics, is independent of sense experience. It appears, to those in the grip of the intuitively pleasing naive empiricism, to be epistemically responsible to deny that we have any knowledge of abstract objects, given their unavailability to our senses. But such a claim is premature given the security and independence from empirical refutation of mathematical knowledge.

§3.2: Constructive and Transcendental Epistemologies

Epistemologists often debate a variety of contrasting views: foundationalism and coherentism, internalism and externalism. Putting those two particular pairs aside, we can contrast two kinds of approaches to epistemology which I will call constructive and transcendental. In a constructive approach, we start with our best theories about our psychological capacities and then speculate about what we can learn using those capacities. In a transcendental approach, we begin with some tentative beliefs about our psychological capacities and some tentative claims about knowledge and attempt to bring these beliefs in line.

Empiricists from Locke, Hume, and Mill to both Quine and Field adopt constructive approaches to epistemology within an empiricistic framework, assuming that our evidence is limited to sense experience, in some way. On an empiricist's constructive approach to epistemology, we start our epistemology with our sense experiences and work our way forward, constructing unifying explanations out of our sense experience, often in the guise of scientific theories. We commit to the flames or disregard as meaningless any claim that can not be traced back to original sense experience. Quine, though he abandoned the ontological reductionism of the logical empiricists, maintained their constructive approach to epistemology by continuing to hold that all experience, all evidence, is sense experience. We can see the non-reductionist version of a constructivist epistemology, for example, in his *The Roots of Reference*.

Field, in denigrating platonistic ontology for its mystery, in accusing the platonist of having a problem of access, adopts the same constructive approach. While Field does not defend a causal theory of knowledge and does not state explicitly that all experience is sense experience, the best way to understand the Nepalese village criticism is as a demand for some sort of contact, some sort of interaction, between ourselves and the objects of our ontology. In empirical theories, such a demand might be reasonable, though it is difficult to see how a story of contact or interaction could be constructed for our beliefs about, say, electrons or magnetic fields. In mathematical theories, such a demand begs the question regarding abstract objects. Of course we have no story about our knowledge of abstract objects that parallels the story of how we know what happens in remote or nearby villages. Even to demand such a story is to rule out an account of abstract objects by insisting that our knowledge of a proposition must arise from some contact with or experience of the subjects of that proposition. Anti-platonistic reinterpreters, despairing of knowledge of abstract mathematical objects, also seem to be constructivists in the same way. We have no contact with, or direct access to, abstract objects, so true mathematical sentences must have other subjects.

Philosophers motivated by worries about access present a latent, overly-simplistic view that appeared in the work of both the modern and the logical empiricists. They assume that there must be some way to connect any object that exists with our sense experience, at least in principle. Defenders of the indispensability argument, like Quine, persist in this assumption by arguing that our mathematical beliefs are justified by their utility to science. Much of this book is an attempt to show that Quine got the account of our knowledge of mathematical objects wrong. But this much he got right: ontology is a question of interpretations of how best to interpret our theories, not a question of with which objects we come into contact. Ontology is more complicated than simple stories about access. The lesson of Quine's rejection of logical empiricism is not holism, but that the proper methods for determining our ontological commitments are not direct and unmediated.

In contrast, a transcendental approach to epistemology starts with assumptions about the kind of knowledge that we have and then either speculates or empirically researches our neuropsychological capacities for attaining that knowledge. We know that we exist, that there is a hand in front of me, that acorns become oaks, that two and two are four. We know these propositions more thoroughly, more securely, more reliably, than most of us know about human psychology and neuroscience. On the transcendental approach, such claims are fallible constraints on our epistemology. Any account which denies such claims is deeply suspicious.

Rationalists tend to be transcendentalists, in my sense of the term, though rationalists can be constructivists, too. A rationalist constructivism would start epistemology with settled views on the roles of sense experience and rational reflection. But rationalists tend to invoke reason as an inference to the best explanation rather than as a settled matter. For example, Descartes concludes that some ideas are innate from his inability to explain the presence of certain ideas on the grounds of fantasy or sense experience. Contemporary nativists (often classified as rationalists, *pace* fn. 4) often invoke similar poverty-of-evidence arguments. Such arguments are best seen as transcendental, as I use the term, as inferring the nature of our capacities rather than presuming them.

Kant's transcendental deduction of the categories of the understanding is a paradigmatically transcendental approach. Kant made errors in the details of his conceptual analysis. Most instructively, he aligned the notions of apriority and necessity, claiming that all *a priori* judgments hold necessarily. Since some of the judgments Kant thought were *a priori* turned out to be false, Kant's approach seems doomed.

While I do not recommending reviving Kant's epistemological project in detail, I do pursue a transcendental approach to epistemology in this book, including appeals to a fallibilist *a priori*, which I will discuss in the next section. Such an approach is perfectly compatible with, indeed best suited for, empirical psychology. On the transcendental approach, we start with defeasible assumptions about what we know. Then we learn how our brains and bodies work to generate that knowledge. What are the environmental and neuro-psychological conditions that make such knowledge possible? By adopting a transcendental approach, we trade the epistemic hubris of the constructivists for epistemic humility. We see that epistemology is an ongoing project which balances our best claims about the world with our best knowledge of our cognitive apparatus. It is not a project of constructing whatever ontology we can construct given a settled account of our cognitive apparatus.

The major problem with the constructive approach is that we lack substantial information about our cognitive capacities. The empiricist's constructive approach rules out *a priori* methods of justification, and rules them out, ironically, *a priori*, leaving us neglecting the ontological horn of Benacerraf's dilemma. If we want to take the references of our mathematical theories seriously, if we want to account for mathematical knowledge, we should adopt a transcendental method for epistemology.

In the absence of a completed cognitive science, we may explore *a priori* cognitive abilities, including the capacity which I call mathematical intuition and discuss in Chapter 9. As Chomsky's approach in linguistics led to a fruitful linguistic nativism, a transcendental approach in the philosophy of mathematics leads to mathematical apriorism, whether in the thin form required for Balaguer's FBP (which requires only a bare ability to recognize consistency) or in thicker appeals to mathematical intuition.

Positing *a priori* epistemic capacities like mathematical intuition is often seen as approaching mysticism. It's all very well, says the constructive epistemologist, to wave one's hands at our ignorance of neuroscience and promise an account of our knowledge of mathematical objects. But even now, we can see that there is no hope for any kind of contact between ourselves and the abstract objects of mathematics. Whatever is happening in the brain, says the constructive epistemologist, it won't give us access to mathematical objects. The case is like that of the purported knowledge of Nepalese villages. The transcendental project is doomed.

Empiricist constructivists believe that they can either account for the kinds of knowledge that have variously been called relations of ideas, *a priori*, *necessary*, or analytic, or that they can deny that any knowledge has such characteristics. They claim that they can avoid positing cognitive abilities like mathematical intuition through programmes such as fictionalism. This Ockhamist approach is noble, to be sure, avoiding *ad hoc* ascriptions of speculated cognitive capabilities. But it may leave all but the most recalcitrant naturalist hungry for something more substantive.

Do we allow our ontology to lead our epistemology, as the transcendental approach does? Or shall we let our epistemic speculations lead our metaphysics? The proper approach balances the two kinds of considerations. Overemphasis on the transcendental approach puts us in the company of the psychics and astrologers and the speculative metaphysicians from whom Kant sought to distance himself. Overemphasis on the constructive approach is unwarranted in the absence of a complete understanding of our cognitive abilities. We need not wait until cognitive science is finished to get a good epistemological theory, but we should not proceed as if we know exactly how cognitive science is going to come out.

§3.3: The Fallibilist *A Priori*

So my first response to the worry about access to mathematical objects is that some patience while cognitive neuroscience proceeds is prudent. Neuroscientists are unlikely to discover some heretofore hidden sixth sense of mathematical intuition. But neither is an account of our *a priori* abilities to know about mathematics without causal contact between us and non-spatio-temporal mathematical objects impossible. I have no detailed story to tell about the neuro-psychological basis of mathematical reasoning; I leave that to the professional psychologists and neuroscientists. If there turn out to be good empirical reasons to believe that no such account is possible, then what I say about *a priori* knowledge of mathematics will have to be abandoned. But unless and until there are such reasons, until we have a much better picture of how we reason and a much better picture of how the apparent security of mathematics is consistent with a denial of *a priori* knowledge, platonism is an open question. To put it bluntly if a bit oxymoronically, I am defending a naturalistic kind of rationalism, one which is consistent with a non-mystical view of human beings.

Such a proposal raises in some philosophers a worry like the following: If we are merely the products of evolutionary forces, and our ability to reason is developed (and thus constrained) by environmental pressures to reproduction, then there is no reason to believe that our *a priori* reasoning will track truth. Since *a priori* justifications are supposed to be infallible, and since there is no reason to believe that any of our reasoning has that characteristic, there is no reason to believe that any of our reasoning is *a priori*.

This objection is flawed due to its reliance on an implausible conception of the *a priori*, though one which many philosophers have held. On this implausible conception of *a priori* reasoning, the dictates of reason are supposed to be immune from error. If we consider the concept of *a priori* justification, we can see clearly that infallibility is too strong a condition to place on such reasoning. Many contemporary defenders of *a priori* knowledge have adopted a fallibilist view. On the fallibilist notion that I invoke, apriority is a property of belief justification. A belief is justified *a priori* if the justification does not appeal to sense experience.

Pure mathematical proofs are *a priori* on this conception. To see this claim, let's look at a simple example, the classic proof that the square root of two is irrational.[5] It starts by supposing, for

[5] Compare to Brown 2008, Chapter 1.

reductio, that √2 is rational. By the definition of 'rational', √2 should be expressible as a/b, where a and b are integers. We can suppose a/b to be in lowest terms, which means that a and b have no common divisors, a supposition whose ground, that any fraction can be expressed in lowest terms, is independently provable. Since a/b = √2, $a^2 = 2b^2$. It follows (by definition) that a^2 is even and then that a is even, since any number whose square is even is itself even. Since a is even, a = 2c, for some c. Then a^2 is also equal to $4c^2$. So $2b^2 = 4c^2$ and thus $b^2 = 2c^2$, which entails that b is also even. We have shown that a and b are both even, which contradicts our assumption that a/b is in lowest terms. Thus, our initial assumption is false; √2 is not rational.

While we use our senses to perceive this proof, either by reading it or hearing it, say, none of the steps of the proof require or even could be justified by sense experience. The singular terms refer to mathematical objects which we can not perceive with our senses. There are universal claims, like the claim that any number whose square is even is even, which can not be justified by particular experiences, enumeratively, since it concerns infinitely many particular results. Moreover, the notion of an irrational number seems impossible to acquire empirically. Even if we took terms like 'a' and 'b' to refer to objects available to our senses (like lengths of the edges of fields), we could never discover irrational numbers by sense experiences in the way that we could discover the rationals. Given any unit distance, there will be objects whose length is no multiple of that unit. But given the density of the rationals, we could always some ratio of multiples of any unit distance to serve for any measurement purposes.

These considerations favoring taking mathematical proofs to be *a priori* do not entail that such claims are infallible. We sometimes get proofs wrong, mistaking an invalid inference for valid, say, or making an unjustifiable assumption. Such errors, too, may be *a priori*. That is, they may themselves be the result of errors in reasoning which are not explicable in terms of the fallibility of sense experience.

To see how a fallibilist *a priori* applies in mathematics, let's accept, for the purposes of argument, that we have some beliefs which are held *a priori*. Among those beliefs, let's say, are some mathematical propositions, for example the Euclid-Euler theorem that every even perfect number is of the form $2^{p-1}(2^p-1)$ where 2^p-1 is prime. The ways in which we can learn about the Euclid-Euler theorem include a variety of sense experiences: reading words and symbols on a page, say, or hearing discussions of the theorem. These experiences are irrelevant to the justification of the theorem which depends only and essentially on the production of a proof. If we believe the theorem without understanding a proof of it, perhaps on the basis of some testimony, then our belief is only as secure as the relevant testimony and the proof behind it.

The nature of mathematical proof is a deep and fecund question. Some proofs are derivations from axioms in an accepted deductive system. These are rare; it took Whitehead and Russell nearly 400 pages to prove that 1+1=2 in the *Principia*. Most proofs that mathematicians read or construct contain a wide variety of shortcuts and implicit lemmas. Indeed, mathematical training is, in significant part, an education in which lemmas must be proven and which may be assumed. Perhaps we assume that all lemmas could be traced back to initial axioms. Still, such derivations leave open the status of the axioms themselves. As many philosophers and mathematicians have noted, we choose axioms and rules of inference on the basis of the theorems they yield. Other proofs may invoke pictures, either as supplemental to deductive inference or with a more-central role.[6]

However we may answer the question of the nature of mathematical proof, it will involve grasping both the content of a mathematical theorem and its relation to other mathematical propositions.

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[6] Proofs with an exclusively central role for diagrams are often called proofs without words. See Nelson 1993 and Nelson 2001 which collect instances of the regular feature on proofs without words from *The Mathematics Magazine* published by the MAA. See also Brown 2008: Chapter 9 for a discussion of the role of diagrams in mathematical reasoning.

These processes, on the assumption that there are *a priori* justifications of any beliefs, are the kinds which would qualify as *a priori*. But there is no reason to infer that grasping the content of a theorem and its relation to other theorems must be infallibly secure. Such reasoning is *a priori* because it is independent of sense experience and is ceded only on the basis of further *a priori* considerations. In other words, we might hold a false proposition *a priori*.

Among false propositions most plausibly held *a priori*, for example, is an unrestricted set-theoretic axiom of comprehension: every well-defined property determines a set of things that have that property. When we first hear the axiom, it appears, to many of us, to be true. Further *a priori* reasoning shows that the axiom of comprehension leads to the inconsistency first noticed by Russell: there is no set that has the property of containing all and only sets which do not contain themselves. Thus, *a priori* reasoning is not infallible. Because beliefs which are justified *a priori* are independent of experience, we do not cede such beliefs on the basis of empirical evidence. But as long as reasoning may proceed *a priori*, we can err and correct errors among our *a priori* reasoning.

Still, it may not be clear that removing the infallibility condition on ascriptions of apriority will mitigate worries about a naturalistic account of apriorist reasoning. As I expressed the worry, the opponent of apriorism is concerned that such reasoning, if the product of merely evolutionary forces, may not track truth. Recent psychological evidence shows convincingly that people often think and behave in ways that psychologists and behavioral economists and some philosophers describe as irrational.[7] Irrational behavior, though, would only be a problem for the fallibilist apriorist in mathematics if it were based on *a priori* mathematical reasoning. People do infer erroneously, especially when using statistics. But even to call an inference erroneous is to assert that there is a correct inference, a standard against which the erroneous inference may be measured. Popular disregard for mathematical inferences is no reason to worry that mathematics itself is fallacious.

The real worry about the reliability of mathematical reasoning for the apriorist is more global. Suppose that arithmetic were shown to be inconsistent. We might, parallel to the case of an unrestricted axiom of comprehension, be able to cede some portion of our arithmetic in order to restore consistency. But if our best, most secure mathematical theories turned out to be inconsistent, our claims to their apriority might seem hollow: what good is apriority if our theories turn out to be so insecure?

No one really thinks that our best mathematical theories are liable to turn out inconsistent. Such a discovery would undermine most philosophy of mathematics as well as mathematics proper. More importantly, the point of defending *a priori* reasoning in mathematics, whether in the guise of a thin ability to recognize consistency or in a more robust mathematical intuition, is not to achieve some goal. The point of defending *a priori* reasoning in mathematics is that it best represents the ways in which we practice mathematics and the actual differences between empirical science and mathematics.

§3.4: No Access Problem

My first response to the concern about access to mathematical objects was that the worry about access is overstated, largely because our ontological commitments do not depend on access. Our ontology is best seen in our best theories; what exists is what our best theories say exists. Mathematical objects are posited as the subjects of mathematical theories which are known on the basis of fallibilist *a priori* reasoning, reasoning which is justifiable given a properly humble (and transcendental) approach to epistemology.

My second response to the worry about access to mathematical objects is that it is ancillary to the central questions of this book. We should be able to compare the various responses to Benacerraf's

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[7] Work on the topic is legion. See Kahneman et al. 1982, Gilovich 1991, Stein 1996, and Ariely 2008 among many others.

dilemma, both those (like fictionalism) which embrace the epistemic horn and those (like autonomy platonism) which embrace the ontological horn. In recent philosophy of mathematics, focus on the ontological horn has been mainly and errantly on indispensability platonism. This book is an attempt to re-focus philosophers of mathematics on what I take to be a more plausible version of platonism. I wish at least to make it clear, as we balance our desires for an intuitively-pleasing ontology with a scientifically respectable epistemology, that autonomy platonism is a better option than its indispensabilist cousin. If access is a worry for one version of platonism, it will be a worry for the other, and so not a reason for choosing one over the other.

The access worry motivates anti-platonist views like those of the fictionalist and the modal reinterpreter. Insofar as this book is an attempt to contrast two versions of platonism, the so-called access problem and anti-platonism are irrelevant. But insofar as criteria for a good platonist view include an ability to answer anti-platonist criticisms, a solution or dissolution of the access problem is essential. I return to the topics of access and ontology both in my introductory comments on autonomy platonism, later in this chapter, and, in more depth, in Chapter 9.

§4: Platonistic Responses to Benacerraf's Dilemma

The first two kinds of responses to Benacerraf's dilemma, fictionalism and reinterpretation, are anti-platonist views. The remaining two kinds of responses are platonist. They are distinguishable by their epistemologies. Indispensability platonism (IP) is the view that our mathematical beliefs are justified by their uses in scientific theory. Autonomy platonism (AP) is the view that our mathematical beliefs are justified independently of their applications in scientific theory. Broadly speaking, IP is an empiricist's view and AP is a rationalist's view, though the terms 'rationalist' and 'empiricist' are not very useful and not all versions of autonomy platonism are what their proponents would call rationalist.

More informatively, IP is a reluctant platonist's view. The indispensabilist agrees with the fictionalist and the reinterpreter that beliefs about abstract mathematical objects are odd and uncomfortable. Since, the indispensabilist argues, all of our evidence is sensory evidence, one would like to eschew commitment to abstracta if one could.[8] Principles of parsimony, essential to good theorizing, force us at least to try to avoid mathematical references in our best theories. If we could get away without mathematical commitments, we should. But, the indispensabilist argues, there's no way to do so.

IP is rooted in the mid-twentieth-century work of Quine and Putnam. Indeed it has been called the Quine-Putnam argument. Nevertheless, there are earlier hints of the argument. For example, Frege, arguing against formalism in the *Grundgesetze*, asserts that the applicability of mathematics elevates it from a game to a science (§91). Frege makes no claim that our knowledge of mathematics depends on our knowledge of empirical science.[9] Any platonist view which appeals to the applicability of mathematics in science to justify our mathematical beliefs is indispensabilist in spirit. But the plausibility of the indispensabilist's inference really depends on Quine's work on the connection between the construction of formal scientific theories and ontological commitments.

Early in his career, Quine and Goodman 1947 attempted to show how to eliminate mathematical ontology. Assessing that project as a failure, Quine soon, and for the remainder of his career, defended including mathematics in our best theories and, consequently, believing in mathematical objects.

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[8] While nearly all indispensabilists would agree that all evidence is sense evidence, one might employ an indispensability argument while admitting purely mathematical evidence in addition to sensory evidence, as Maddy 1992 does. Putnam also hints at a blended version of the argument. I argue that such appeals to purely mathematical evidence make the indispensability argument otiose in §10.1.

[9] See Garavaso, 2005.

Unfortunately, Quine never rigorously presented an indispensability argument and we have to piece one together from the many places in which he alludes to it.

Putnam explored at least four different and incompatible philosophies of mathematics, four different ways of dealing with the Benacerraf dilemma, in written work. Still, his discussions of the indispensability argument, while crediting Quine, are more carefully presented as an argument. Putnam's version of the argument differs from Quine's, though, and in ways which have been difficult for philosophers to distinguish. We will look at Quine's argument in depth in Chapters 2-5 and at Putnam's in Chapter 6.

After Quine and Putnam, proponents of versions of the indispensability argument include Michael Resnik and Penelope Maddy. More recently, Mark Colyvan and Alan Baker have been working on what they take to be a new version of the indispensability argument: the extended or explanatory version. Details of the different versions of the argument are the subjects of the bulk of this book. Here, I will characterize the argument in general form.

The following are essential characteristics of all indispensability arguments which conclude that we should believe that mathematical objects exist.

IPC1 Evidentiary Naturalism: The job of the philosopher, as of the scientist, is exclusively to explain or account for our sensible experience of the physical world; all evidence is sense evidence.

IPC2 Theory Construction: In order to explain our sensible experience we construct a theory or theories of the physical world. We find our commitments exclusively in our best theory or theories.

IPC3 Mathematization: Some mathematical objects are ineliminable from our best theory or theories.

It follows from Evidentiary Naturalism that we never need to explain mathematical phenomena for their own sake. Using mathematical evidence to support our mathematical beliefs is acceptable mathematical practice, says the indispensabilist. But when it comes to asking about whether that practice is legitimate in its own right, appeals to purely mathematical evidence beg the question. Just as a psychic may urge that the crystal ball tells us to believe the crystal ball, mathematicians may urge us to believe our mathematical inferences. To avoid such question-begging appeals, we must also seek extrinsic justification for taking mathematical inferences seriously in a way in which we do not take the crystal ball seriously. Only the mathematical beliefs which are supported by this external justification, and for the indispensabilist this means applicability in empirical science, will be legitimate.

For an example of the role of Evidentiary Naturalism, consider that some mathematicians believe that there are surprisingly many twin primes, prime numbers, like 59 and 61, separated by two whole numbers. Some mathematicians have even conjectured that there are infinitely many twin primes. Unlike the proposition that there are infinitely many primes, the twin prime conjecture has never been proven. Moreover, the question of whether there are infinitely many twin primes is purely mathematical. It has no empirical scientific importance. Thus, unless some application of the question whether there are infinitely many twin primes can be discovered, the mathematical phenomenon of twin primes is not an explanandum for the indispensabilist. Ultimately, for the indispensabilist, the justification of any mathematical belief must be grounded in our sense experience.

Theory Construction tells us where to look for our ontological commitments, but does not settle a particular procedure for determining them. Quine's well-known method for determining ontological commitments of a theory is ordinarily implicit in the writings of indispensabilists, as well as in the writings of many of those who oppose the argument. Other procedures for determining the commitments of our theories are possible. Stewart Shapiro, for example, urges that we adopt a structuralist criterion.[10] Many indispensabilists either leave their methods for determining commitments obscure or implicitly rely on Quine's criterion.

Mathematization is an empirical claim about the needs of theory construction. The best formulations of most good scientific theories invoke mathematical tools: real numbers for constants or measurement, functions, geometry, axioms governing statistical inference. Mathematization is an empirical claim since it is an empirical question whether we can formulate nominalist alternatives to all good scientific theories, including future theories. Much has been written about Mathematization and I will not say much about it in this book.[11]

Indispensability arguments portray a particular picture of the relationship between mathematics and empirical science. Suppose one is considering whether to believe that there are Woodin cardinals, a type of large cardinal number. On the one hand, axioms governing Woodin cardinals are provably consistent with the standard axioms of ZFC set theory. The existence of infinitely many Woodin cardinals implies projective determinacy, an intuitively pleasing regularity property for the projective sets absent from ZFC. On the other hand, the axioms of ZFC alone are sufficient, and sufficiently elegant, for all of the mathematics used in physical science. By Theory Construction and Evidentiary Naturalism, the indispensabilist indicates that the former, purely mathematical considerations are irrelevant to the question of whether to believe in Woodin cardinals. The latter considerations, whether Woodin cardinals appear in our best theories of the physical world, are the only ones which are relevant. In other words, IPC1-IPC3 rule out independent, non-empirical justifications of mathematical claims. These considerations lead to a fourth characteristic, implicit in the others but worth noting separately.

IPC4 Subordination of Practice: Mathematical practice depends for its legitimacy on empirical scientific practice.

Rejecting Subordination of Practice while retaining the legitimacy of mathematical practice would entail adopting an alternate means of justification for our mathematical beliefs (viz. allowing purely mathematical evidence for mathematical claims). To be clear, the indispensabilist does not urge that all mathematical claims must have some sort of direct empirical evidence in their favor before we adopt them. Mathematicians are, for the indispensabilist, free to practice mathematics as usual. But when it comes to interpreting their theories, to assigning truth values to mathematical claims or to specifying our ontological commitments regarding mathematical propositions and practice, we must look to see which mathematical theories, and thus which mathematical objects, are applicable to our best scientific theories.

The characteristics IPC1-IPC4 apply to all versions of the indispensability argument and should suffice to give a picture of the way the arguments work, especially how they attempt to avoid Benacerraf's dilemma. Benacerraf's dilemma is supposed to show that knowledge of the abstract objects of mathematics is inconsistent with our best epistemology. If some version of the indispensability argument succeeds, then knowledge of mathematical objects will have been shown to be consistent with our ordinary epistemic capacities. Whatever our ultimately best epistemology turns out to be, it will have to account for our knowledge of empirical science. If mathematical knowledge is justified in the same way, an epistemology for natural science will suffice to account for our mathematical beliefs about

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[10] See Shapiro 1993.

[11] Burgess and Rosen 1997 elegantly compiles the most important attempts to show Mathematization to be false.

abstract mathematical objects. So the allure of the argument is strong.

In contrast to the empiricist epistemology undergirding the indispensability argument, autonomy platonism has an apriorist epistemology which appears more contentious. Autonomy platonism derives part of its name from Plato, obviously, but has only a loose relation to his views, mainly to the claim that some portion of reality is non-sensible. The views of Descartes and Leibniz are clearer antecedents, since their epistemologies for mathematics are rationalist. Still, the empiricist Hume's distinction between relations of ideas and matters of fact is more properly precedental for autonomy platonism's separation of mathematics and empirical science. More recently, Gödel, who posited mathematical intuition analogous to sense perception, can properly be called an autonomy platonist, as can Jerrold Katz, John Burgess, Mark Balaguer, and Mark McEvoy.

Autonomy platonism, like indispensability platonism, is a family of views with a variety of conflicting versions. Again, we can distill the common characteristics of autonomy platonism to a several essential characteristics.

APC1 Mathematical Evidence: There is purely mathematical evidence for mathematical claims. Such evidence may include our recognition of inconsistency and intuitive judgments and is independent of our sense experience.

APC2 Theory Independence: Mathematical theories are independent of empirical theories. Mathematical theories are true or false regardless of the nature of the physical world. Mathematical theories are never refuted by empirical evidence.

APC3 Independence of Practice: Mathematical methods, including proof and possibly including intuition, are independent of empirical scientific methods.

The key idea behind APC1 is a denial of the naturalist's claim that all experience is sense experience. Of course, there are sense experiences associated with any mathematical claim: we read sentences and symbols on a page, for example. But mathematical propositions, the claims which mathematical sentences represent or express, are independent of any particular representation and thus of any particular sense experience. When we consider, say, the law of cosines for Euclidean triangles, the evidence for the law holds whether or not there are any sensible triangles. The law holds even though actual physical space is non-Euclidean. The truth of the law of cosines depends instead on its provability from axioms for Euclidean geometry and on however we justify our beliefs in those axioms.

Theory Independence, APC2, is the claim that mathematical theories are never ceded or refuted by empirical discoveries. Scientists use and discard equations and formulas. They refine their calculations of constants and seek improvements of their models of data. A scientist's abandonment of a mathematical tool is no evidence of any flaw in it, no sign of mathematical trouble. When we hypothesize a particular empirical model or measurement, we merely query its applicability. Mathematical theories are kept in the background whenever we test scientific theories and are never considered for refutation by empirical scientists.[12]

APC2 is sometimes called into question by discoveries that certain mathematical theories, presumed to apply to physical situation, are no longer applicable. For example, after space was discovered to be curved rather than flat, so that axioms for Lobachevskian hyperbolic geometry provide the proper framework for the structure of physical space while flat Euclidean axioms do not, some philosophers made the audacious claim that Euclidean geometry had been refuted by the theory of relativity. Similar claims have been made for the failure of the law of excluded middle from considerations in quantum mechanics. The autonomy platonist simply holds that in such cases, physical

[12] See Sober 1993, Sober 1999, and Sober 2005 for more on how science isolates mathematics.

scientists have found that some mathematical axioms were inapplicable to our best physical theories. The failure of a mathematical theory to apply to a scientific theory does not entail that there is any error in the abandoned mathematical theories.

By claiming that mathematical evidence is independent of sense evidence, that mathematical theories are independent of empirical theories, I adopt traditional distinction which has been called into question. The independence of mathematics is partially just that we can isolate mathematical theories from empirical theories and evaluate their consistency or truth without appealing to evidence from physics, biology, or any other empirical theory. Such mathematical theories should be consistent with any physical theories, as long as the physical theories themselves are consistent. Independent of whether space is continuous or discrete, real analysis, with its axioms of continuity, retains its plausibility.

It is difficult to state precisely how to delimit empirical evidence for a mathematical claim from mathematical evidence for that claim. Particular cases, though, can be easy enough to decide. For example, inductive evidence in favor of a mathematical hypothesis is insufficient whereas a proof is sufficient. Consider the inductive argument for the truth of Goldbach's conjecture. Goldbach's conjecture is that every even number greater than two can be expressed as the sum of two (odd) prime numbers. Relevant pairs of primes are called Goldbach pairs. Inductive evidence in favor of Goldbach's conjecture is strong: as of May 26, 2013, all even numbers through 4×10^{18} have been verified to have Goldbach pairs and all numbers through 4×10^{17} have been double-checked.[13] Furthermore, the numbers of Goldbach pairs for each even number increases with the given number and appears to have a lower bound. Still, despite this compelling inductive evidence, and despite the capacity of this evidence to support our beliefs in Goldbach's conjecture, mathematicians do not consider the theorem proven.

Indispensabilists and autonomy platonists agree that mathematical proofs are sufficient as mathematical evidence. But indispensabilists believe that there is a further kind of evidence for or against a mathematical hypothesis, one that comes from the application of a mathematical theory within science. Until and unless a mathematical theory is invoked within a scientific theory, indispensabilists look upon mathematical proofs as recreational, taking the term from a comment of Quine's.[14] In contrast, the autonomy platonist denies that there is any further kind of evidence for a mathematical claim beyond its mathematical evidence. There is no external perspective on mathematics from which one can distinguish the true (i.e. applied) portions from the merely recreational (i.e. unapplied) portions.

Last, APC3 is just the claim, perhaps implicit in APC1 and APC2, that mathematical theories are methodologically independent of empirical scientific theories. The deep question of whether scientific theories can function without mathematical ones remains unresolved. But even those philosophers who believe that we can eliminate references to mathematical objects in our best scientific theories (by eliminating purely mathematical axioms) agree that the theories which include mathematical references are practically, and so methodologically, dependent on mathematics. APC3 is the claim that the reverse dependence does not hold: we need not invoke empirical theories at all in order to justify our beliefs in our mathematical theories.

There is one small caveat to this last claim. If we presume that our physical world is consistent and we can show that a certain mathematical theory applies to the physical world, then we have good reason to believe that our mathematical theory is both consistent and interesting, two important mathematical virtues. If a theory has a model, it is consistent, whether that model is mathematical or physical. So we can, in a sense, find evidence for our mathematical theories in empirical science. But

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[13] See Oliveira e Silva. Also, see Baker 2007: 70 for a graphical representation of the numbers of Goldbach pairs and a discussion of enumerative induction in mathematics.

[14] See Quine 1986 and Leng 2002.

the importance of the physical model is just as evidence for the existence of a mathematical model. In the end, it's the mathematical model which does the work.

This book is an attempt to compare and contrast the two families of views, indispensability platonism and autonomy platonism, and defend the latter over the former. To some platonists, this argument will seem fruitless. IP is often seen as contrasting with some version of nominalism, as a leveraging argument against the nominalist. Such an objection is fair enough; I have little to say here to the nominalist. I merely hope that some of what I say in defense of AP will convince all but the most recalcitrant nominalist that it is a reasonable and defensible position.

Chapters Two through Seven of this book examine problems with various versions of IP. Then, in Chapters Nine and Ten, I develop a version of AP which is preferable to any version of IP. Before we get to the criticisms of IP, I'll sketch autonomy platonism in a bit more detail so that the contrast can be clear along the way.

§5: Autonomy Platonism: Intuition

Since autonomy platonism eschews all appeals to sense experience in justifying mathematical beliefs, proponents of autonomy platonistic views must provide an alternative account of their justifications. Bare appeals to the derivability of theorems from axioms are acceptable to the autonomy platonist, but they are insufficient without an account of our knowledge of the axioms. Simple appeals to the immediacy and obviousness of the axioms are unsatisfying and often misleading, since some axioms are neither immediate nor obvious. Moreover, mathematical theories are variously axiomatizable, with different equivalent axiomatizations having distinct virtues. Our most secure mathematical beliefs may not be our best axioms.

While no plausible autonomy platonism will depend on the certainty of even obvious axioms, some versions (indeed one I will defend) do rely on the claim that some mathematical propositions are to be taken as secure, if defeasibly so. Such simple of basic claims are intuitively obvious and play an important, if not axiomatic, role in justifying our mathematical beliefs.

Since autonomy platonism is a family of views, and since the members of this family disagree about the role of mathematical intuition in justifying our mathematical beliefs, the weakest and thus most plausible versions of APC1-APC3 will avoid reference to intuition. Since the versions of autonomy platonism which do not invoke intuition are most plausible, those wary of intuition may wish to focus on the weakest version when contrasting autonomy and indispensability platonisms.

Still, some philosophers, considering the methods of mathematics and the ways in which mathematical theories are developed, may find appeals to mathematical intuition not only palatable, but appealing. Kant was perhaps the first philosopher to invoke something called intuition in his account of mathematics. Inspired largely by Kant's work, the so-called mathematical intuitionists of the early twentieth century (e.g. Brouwer, Heyting) also relied on a concept of intuition derived from that which appears in Kant's work, as does, more recently, Parsons.

By 'intuition', Kant means something particular to his epistemology: our cognitive faculty of receiving unconceptualized content. For Kant, we develop mathematics by reflecting on our pure forms of intuition, space (for geometry) and time (for arithmetic, which also requires spatial intuition). Here is Kant characterizing intuition and describing a mathematician's construction of mathematical objects in intuition:

> The determination of an intuition *a priori* in space (figure), the division of time (duration), or even just the knowledge of the universal element in the synthesis of one and the same thing in time and space, and the magnitude of an intuition that is thereby generated (number), - all this is the work of reason through construction of concepts, and is called *mathematical* (*Critique* A723-4/B751-2).

> Let the geometrician take up [the questions of what relation the sum of a triangle's angles bears to a right angle]. He at once begins by constructing a triangle. Since he knows that the sum of two right angles is exactly equal to the sum of all the adjacent angles which can be constructed from a single point on a straight line, he prolongs one side of his triangle and obtains two adjacent angles, which together are equal to two right angles. He then divides the external angle by drawing a line parallel to the opposite side of the triangle, and observes that he has thus obtained an external adjacent angle which is equal to an internal angle - and so on. In this fashion, though a chain of inference guided throughout by intuition, he arrives at a fully evident and universally valid solution of the problem (*Critique* A716-7/B744-5).

Such invocations of intuition, whether by Kant or by more recent philosophers inspired by Kant, are in the service of a conceptualist account of mathematics. Conceptualism is the view that mathematical propositions are not discovered but constructed in thought. On conceptualism, mathematical objects are really mental objects. Such a view would best be classified as reinterpretive, taking references to mathematical objects to be references to mental acts or constructions. Such uses of 'intuition' in mathematics are not compatible with the ontological thesis of autonomy platonism, PO. For the autonomy platonist who countenances it, mathematical intuition is a capacity for acquiring, *a priori*, beliefs about non-sensible abstract objects. I will not consider Kantian, conceptualist intuition further.

Gödel famously characterized a version of mathematical intuition which is consistent with PO, taking it on analogy with perception: a non-inferential awareness, grasping, or understanding.

> [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. I don't see any reason why we should have less confidence in this kind of perception, i.e. in mathematical intuition, than in sense perception, which induces us to build up physical theories and to expect that future sense perceptions will agree with them, and, moreover, to believe that a question not decidable now has meaning and may be decided in the future. The set-theoretical paradoxes are hardly any more troublesome for mathematics than deceptions of the senses are for physics...
>
> Evidently the "given" underlying mathematics is closely related to the abstract elements contained in our empirical ideas. It by no means follows, however, that the data of this second kind, because they cannot be associated with actions of certain things upon our sense organs, are something purely subjective, as Kant asserted (Gödel 1964: 268).

In recent years, Gödel's view has been derided as incompatible with the epistemic constraints underlying the Benacerraf problem. Benacerraf examines Gödel's view specifically and in detail and concludes that any view of its type is untenable. Field characterizes Gödel's move as 'desperate'.[15] Still, as I have claimed, the worry about access underlying these complaints about intuition are overstated.

While there has been very little recent work on mathematical intuition, and even less on the kind of intuition relevant to an apriorist, as opposed to conceptualist, view, we can find a useful variety of characterizations of philosophical intuition in recent work by Ernest Sosa, George Bealer, and others.[16] These characterizations of philosophical intuition, as a non-inferential, non-perceptual process, are all easily adapted to cases in which an autonomy platonist might want to invoke mathematical intuition.

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[15] See Field 1989: 28.

[16] See §9.4 for details.

Intuitions are immediate inclinations to belief and they take as their subjects concepts and objects, including modal properties, which are unavailable to sense experience. Unlike earlier platonists like Descartes, no contemporary defender of intuition, philosophical or mathematical, claims that intuition is infallible. But when I have an intuition that p, it is desirable to find a theory which can accommodate p, or to find an explanation of why that intuition is wrong but appears to be correct.

The strongest recent defender of mathematical intuition is Jerrold Katz.

> In the formal sciences, it is common to refer to seeing that something is the case as "intuition" and to take such immediate apprehension as a source of basic mathematical knowledge (Katz 1998: 43).

> The notion of intuition that is relevant to our rationalist epistemology is that of an immediate, i.e. noninferential, purely rational apprehension of the structure of an abstract object, that is, an apprehension that involves absolutely no connection to anything concrete (ibid 44).

A satisfying theory of mathematical intuition is far from available at the moment, though mathematicians and philosophers have a pretty good sense of when and how it is used. The role of intuition in forming mathematical theories is like the role of sense experience in forming empirical scientific theories: it provides particular data points which our best theories should accommodate maximally. Just as not all sense experience is veridical, not all intuitive claims will turn out to be true. But without mathematical intuition, it seems difficult account for the ways in which our theories are constructed.

I will not be providing a full account of intuition in this book. My goal is to show that autonomy platonism, a family of views which can accommodate mathematical intuition as an element of its epistemology of mathematics, is better motivated and more desirable than indispensability platonism. To that end, I will return to the concept of intuition in the last chapter. But it is important to remember that mathematical intuition is not an essential element of autonomy platonism.

§6: Autonomy Platonism: The Question-Begging Version

In the previous section, I mentioned in passing Field's derision, as desperate, of Gödel-Katz-style appeals to mathematical intuition. In this section, I want to explore Field's complaint a bit further.

Field's defense of fictionalism, which I take to be the most plausible anti-platonist view of mathematics, is framed in opposition to the indispensability argument. Field dismisses autonomy platonism and calls indispensability platonism the only non-question-begging version of platonism (Field 1980: 4). Field's view, I believe, is that using mathematical evidence for the existence of mathematical objects is objectionably circular, perhaps in the following way:

> We should believe that the theorems of mathematics are true because they are intuitively acceptable. But we should believe that our intuitions in mathematics are reliable because they are consistent with our mathematical theories.

The same sort of argument could hold for the psychic or astrologer. More famously, the problem of scriptural circularity has the same form.

> Granted, it is altogether true that we must believe in God's existence because it is taught in the Holy Scriptures, and, conversely, that we must believe the Holy Scriptures because they have come from God... Nonetheless, this reasoning cannot be proposed to unbelievers because they would judge it to be circular (Descartes, Letter of Dedication).

Thus the accusation that autonomy platonism is question-begging is related to an accusation of mysterianism or theology. In all such cases, without some sort of external grounding, for example in sense experience, the evidence and the theories seem dangerously closely related. Just as we should not believe the psychic's entreaties to believe the psychic, we should not accept, without further grounds, mathematical evidence for mathematics.

Katz argues that such objections confuse mysterianism with philosophical puzzlement. The nature of mathematical intuition and the source and justification of our mathematical intuition is an unanswered philosophical puzzle. For answers regarding the source of intuition, we look toward neuroscience. For answers regarding their justificatory status, we look toward philosophical theories of mathematics. But to claim that intuition is inherently mysterious is to overstate the problem.

More importantly, no philosopher wants to commit a logical fallacy, as begging the question seems to be. The philosopher of mathematics who embraces autonomy platonism is liable to that sort of accusation. Nevertheless, and despite the problems with the circularity underlying the inferences of both the psychic and the theist, I believe that the autonomy platonist should embrace the circularity inherent in the view. Not all circles are vicious circles.

Autonomy platonism is question-begging in the sense that the autonomy platonist believes that mathematical evidence supports our beliefs about mathematical claims and that no sort of empirical evidence can weigh either for or against mathematical claims. The independence of mathematical theories from empirical theories, a central thesis of autonomy platonism (APC1), entails that the justifications of mathematical theories are not found in empirical evidence or theories. We believe that '7+5=12' is true in part because we believe that it follows from the Dedekind/Peano axioms of arithmetic. But we also believe the Peano axioms in part because they yield true propositions of arithmetic, e.g. that seven plus five is twelve. Field's view, and the view of many other anti-platonists, is that mathematical evidence for the truth of mathematical propositions is insufficient. That's why Field claims that indispensability platonism is non-question-begging: the evidence for mathematical propositions is, according to the indispensabilist, either directly or indirectly empirical, like all other evidence.

A parallel accusation of circularity can be levied against the empiricist: we believe that there are trees in part because our senses tell us that there are trees. We believe that sense experience is accurate in part because it tells us that there are things like trees, which we take as manifest. If mathematical evidence is insufficient to support our beliefs in the truth of mathematical theories, then sense evidence is insufficient to support our beliefs in the truth of empirical theories.[17] The accusation of circularity, as we are considering it, is supposed, by Field and others, to hold against the autonomy platonist and not the indispensability platonist. Such an accusation can hold only if we have non-question-begging justifications for our empirical theories.

The obvious response to the charge of circularity for empirical theories is to claim that the evidence of sense experience is somehow basic or secure from error. The logical empiricists, for example, tried to ground all scientific theories in the evidence of foundational sense data. Since, they believed, sense experience has a special status and is secure from error, unlike mathematical intuition, say, we can build our theories on their foundations without worries about circularity. But such projects, like that of Carnap's *Aufbau*, do not appear to succeed.

One problem with logical empiricism is that it takes observations as evidence when the very notion of an observation is laden with theory. As Quine observed, any particular claim can be held as true, even if it is inconsistent with our other beliefs, as long as we make appropriate adjustments to our

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[17] A scientific anti-realist could consistently hold that there is a circularity problem for mathematics but not for empirical science. But scientific anti-realism is another topic for another place.

background theory. Quine thus showed that confirmation holism better depicts the relations among theories and observations than the logical empiricist's dogma of reductionism. On Quine's holism, we start our theorizing with the tentative evidence of sense experience. As we add experiences, we construct increasingly plausible theories to account for them. We balance our tentative observations with our tentative theories, working in both directions to formulate the most attractive, comprehensive, conservative, and powerful system of beliefs that we can. We use our evidence to support our theories and we use our theories to judge and predict our evidence. As Quine realized, to deny the legitimacy of sense experience because of the circularity of such reasoning is self-defeating skepticism. Sense evidence is good, if not infallible, evidence. One can not defend appeals to sense evidence against the skeptic, but such worries are merely academic.

The autonomy platonist believes that mathematical evidence, like sense experience, is good, if not infallible, evidence. Parallel to empirical science, we begin our mathematical reasoning with some simple and particular mathematical claims: simple claims of arithmetic, including counting; basic geometric facts; rudimentary set-theoretic observations. We organize and regiment our basic claims, perhaps tracing them to some systematic sets of axioms which yields the desired theorems. We find some unifying claims among the various theories, set-theoretic (or category-theoretic) reductions, say. We judge our axioms by their yield and the particular claims by their relations to the axioms.[18]

Field and others accuse the autonomy platonist of circularity. But as Quine shows, not all circles are vicious circles. Quine's holism, perhaps the most plausible account of empiricist epistemology, has the same kind of circularity as autonomy platonism and is no less plausible for it. What separates the autonomy platonist from the indispensability platonism is the not form of argument, not the circularity of the epistemology, but the question of the nature of mathematical evidence. For the indispensabilist, all evidence is sense evidence. For the autonomy platonist, purely mathematical evidence is no less secure, no less reliable, and no less respectable.

I will return to a more detailed defense of the circularity inherent in autonomy platonism in Chapter Ten. For now, I just want to say that the autonomy platonist is going to have to learn to live with the charge of circularity. It is the price of autonomy.

§7: Autonomy, Access, and Application

Anti-platonist responses to the Benacerraf problem are motivated largely by concerns about access to mathematical objects. As I argued in §3, I do not think that the access problem is a serious worry. The important question for the platonist is not, as some critics seem to think, whether we have causal (or other) contact with mathematical objects. The important question is whether we can have some account of the applications of mathematics to the physical world. Given the independence of mathematical theories from empirical theories and the causal isolation of abstract mathematical objects, it seems surprising to many people that mathematics has the kind of application in empirical theories that it does. Earlier, I promised a positive account of an epistemology for abstract objects which does not depend on contact with those objects. Let's take a moment to see a sketch of that epistemology and how it leads away from the problem of access and toward the problem of application.

The access problem is a general worry about how we know about anything at all. It seems easy enough to explain our access to the ordinary objects in front of us, like trees and tables. We just open our eyes or reach out our hands. But those objects are composed of smaller objects, say carbon atoms, to which our access requires explanation. Or perhaps the atoms (or quarks or strings) are what we directly observe while our access to trees and tables requires explanation. The mere claim that sense experience provides access to objects does not fairly settle the question of what exactly we are observing in sense

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[18] Bertrand Russell noted this phenomenon early; see §9.5.

experience. Even if we believe that the world exists independently of our experiences of the world, as we should, what we consider to be the objects of our direct experience depends largely on how we conceptualize the world, on how we carve it up.

The access problem, in its most general form, is a demand to connect the objects we take as directly perceived, whatever they are, with the ones whose existence we infer. We can, very roughly, divide our ontological commitments into three groups. In the first group are ordinary objects like trees and tables. In the second group are the theoretical posits that are causally or compositionally related to ordinary objects, like electrons. In the third group are mathematical objects. There might be objects which do not clearly belong to any of the three groups, but let's put those aside for the moment and notice that there are two distinct ways of responding to the access problem for any object.

We begin our epistemic reasoning with strong (if defeasible) commitments to the objects in the first group. As we learn more about the world, we discover that our claims about the world are made more powerful and uniform when we appeal to objects in the second group. As we reflect on our claims about the world, we notice that we appeal to objects of the third group. We know that there are trees; we discover that they are made of carbon atoms (and other stuff); we use mathematical tools to represent our best theories about trees and their atoms. Still, our original commitments are to the objects of the first group and we don't appear to have direct knowledge of the objects of either the second or third groups. So, we tell some sort of story about the relations among the objects of the three groups. Two ways of responding to the access problem are two different kinds of stories we can tell about our beliefs about objects in the second and third groups.

The first kind of story appeals to the causal or constitutive relations between objects of the second group and objects of the first: trees are made of atoms; we know about the trees; so we have knowledge of the atoms. Philosophers who favor eleatic principles of ontological commitment tend to invoke stories of this first kind. Eleatic principles are claims that, approximately, only causally active entities exist. On this view, our access to electrons is explicable in terms of our access to ordinary objects. Our knowledge of objects of the second group is derivative from our knowledge of objects of the first group.

But stories of composition and constitution are unavailable for the objects of the third kind. We can not derive knowledge of mathematical objects from their constitutive relations to ordinary objects. Trees aren't made of mathematical objects even if we use mathematics to formulate our best scientific theories. Thus, those who invoke causal or constitutive relations among trees and atoms to explain our knowledge of atoms are left with an insoluble puzzle about our knowledge of mathematics. Some philosophers thus go on to deny the existence of mathematical objects.

The second kind of story, in contrast, makes ontological commitment homogeneous: all of our commitments are made together as a unified account of our sense experience. First, we have some experiences. We organize and systematize these experiences into an attractive, explanatory theory. Lastly, we interpret the claims of that theory to determine our ontology. This second kind of story should be familiar to those who know Quine's work. It is at the core of the indispensability argument.

Notice that such stories make the posits of the objects of all three groups equal. The lack of causal contact or constitutive relations between trees and electrons, on the one hand, and mathematical objects, on the other, is completely moot. There is no access problem for mathematical objects on the second kind of story.

All perception involves both the physical impact of the world on our sense organs and the processing and construction of an experience based on that sensory input. The fantasy of a simple, unmediated experience, a sense datum, is a myth. Once we discard the fiction of a pure sense datum, once we recognize that our conscious experiences are mediated by the conceptual schemes with which we meet the world, the access problem takes on a new appearance. Quine's deep insight in "Two Dogmas of Empiricism" and elsewhere is that ontological commitment is a matter of how one fits together the stimulation of our sense organs with the construction of our theories about the world. The access problem, seen in a post-Two-Dogmas light, dissolves.

Once we see, properly, that ontological commitment is a product of this interaction, the problems of access to sub-visible objects like atoms disappears and the traditional access problem for mathematical objects goes with it.

In place of the access problem, though, is a different pressing question: If mathematical objects are not causally or constitutively or compositionally connected to ordinary objects, why are they so useful, even essential, for our theories of our sense experience of those objects? We understand clearly the connections between atoms and trees; the Quinean doesn't throw out the causal or constitutive stories when discarding the first kind of story. To deny the eleatic's principles by seeing ontological commitment as a homogeneous product of theory construction is not to deny the facts about the ways in which the world is put together.

The connection between, say, real numbers and trees is less well-understood. If mathematical theories were completely independent of empirical science, their applicability to science, most prominently as tools for expressing scientific ideas precisely and as devices for measurement, would be a mystery. The access problem is replaced by a problem of application.

The problem of application is simply Wigner's question: How can we account for the unreasonable effectiveness of mathematics in physical theories?[19] When scientists model physical phenomena, mathematical devices for all their needs are available. Where they are not, they can be developed. Moreover, mathematical theories which seem unconnected to science have an uncanny way of finding applications. Complex numbers, widely used in electronics, fluid dynamics, and engineering, were variously labeled imaginary, fictitious, and impossible. They have become invaluable tools despite resistance from even those who developed theories of complex numbers.

The Wignerian problem of application, of explaining why objects which are not constitutive of or causally connected to the physical world are so useful in science, can easily be confused with the access problem. But it is a different problem and the solution to it has different parameters. Attempts to solve the access problem for mathematical objects are quixotic. Attempts to solve the problem of application are more tractable.

The most useful way of showing that the problem of application is tractable is to present a solution to the problem that is as broadly acceptable as possible. I can do no better than appeal to the solution presented by Mark Balaguer, which he calls variously the representational account or the theoretical apparatus account. Balaguer's solution is simple: mathematics provides a framework for any possible physical situation.

> The only reason it might seem surprising that mathematics can be used to set up a descriptive framework, or theoretical apparatus, in which to do empirical science is that this seems to suggest that there is an inexplicable correlation between the mathematical realm and the physical world. But within FBP, this illusion evaporates: the mathematical realm is so robust that it provides an apparatus for all situations. That is, no matter how the physical world worked, there would be a mathematical theory that truly described part of the mathematical realm and that could be used to help us do empirical science (Balaguer 1998: 143).

On Balaguer's view, any consistent mathematical theory truly describes some mathematical universe. There is no mystery about why mathematical theories apply to our world since mathematical theories apply to all possible worlds. Do you want to model mereological sums? There's a mathematical

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[19] See Wigner 1960. For more recent discussion of the phenomenon see Baker 2001: §2.2.

application for that. Do you want a way to represent the location relations among physical objects? There's an app for that too. Do you want to model market fluctuations, or properties of quantum particles, or the various possibilities of some physical event's occurrence? There are apps for all of those as well.

Moreover, the plenitude of mathematical theories entails that there are more mathematical tools than can be applied in scientific theory. It may seem surprising that a mathematical theory, previously uninteresting to scientists, can suddenly become essential for their work. But the structural or abstract properties of any and every physical situation are mathematically describable. Many mathematical theories are never applied because there are so many. Once we appreciate the vast number of mathematical theories which are never applied in scientific theory, the surprise dissipates.

We may be misled by simple mathematical models and their applicability in empirical science into thinking that there is something fundamentally Pythagorean about the world, something particularly mathematical about our physical universe. But the abstractness and generality of mathematics entails that this mystery is overstated. Consider, for example, Bode's purported law of planetary distances. In the late eighteenth century, Bode, influenced by some earlier astronomical observations, hypothesized a simple mathematical law governing the relative distances of the planets from the sun.

> Let the distance from the Sun to Saturn be taken as 100, then Mercury is separated by 4 such parts from the Sun. Venus is 4+3=7. The Earth 4+6=10. Mars 4+12=16. Now comes a gap in this so orderly progression. After Mars there follows a space of 4+24=28 parts, in which no planet has yet been seen. Can one believe that the Founder of the universe had left this space empty? Certainly not. From here we come to the distance of Jupiter by 4+48=52 parts, and finally to that of Saturn by 4+96=100 parts (Bode, in Jaki 1972: 1015).

The gap between Mars and Jupiter was temporarily considered to be filled by the dwarf planet/ asteroid Ceres, as astronomers motivated by Bode's reasoning attempted to replace the lacuna. Further evidence of Bode's law was supplied by the discoveries of Uranus and Pluto, which fit the sequence at least approximately. Neptune, which is not even close to the location Bode's law predicts, was a contravening datum, one which led astronomers to abandon the so-called law.

If one focuses on the fact that there are mathematical tools for all empirical scientific needs without recognizing the many tools which are unapplied, one might be surprised by the applicability of mathematics. Bode's law invoked a simple arithmetic sequence. That arithmetic sequence turned out to be inapplicable in this case. Many arithmetic sequences are inapplicable in this case. That there might be a mathematical tool to describe the relative positions of the planets should not be surprising because there are so many mathematical tools.

Reflection on the broad availability of mathematical theories should dissuade us that any account of application beyond that provided by Balaguer is required. Mathematics has far too many tools for us to believe that there is something particularly surprising or unreasonable about the effective applications of mathematics in empirical science.

Moreover, the fact that general principles of theory construction apply to mathematical and empirical theories in similar ways also contributes to the explanation of the applicability of mathematics. In both domains, we look to simple, elegant, unifying principles. The structure of our best formulations of scientific theories may appeal to simple, elegant, and unifying mathematical theories.

Alan Baker discusses, in this vein, Hamilton's discovery and development of theories of quarternions as a four-dimensional extension of the complex numbers.[20] Quarternions could be used in

[20] See Baker 2001: §2.3.

lieu of Cartesian coordinates, and more elegantly, so they had particular application to physical theories.

But quarternions were shown to be less powerful and elegant than other theories, in both mathematics and physics. In mathematics, the development of theories of quarternions was a useful stepping stone to more general, more elegant algebraic structures and to more general, more elegant vectorial systems. In physics, Maxwell's equations, Baker reports, are formulated as eight equations using quarternions of Cartesian components, but as four using vectors and even two using tensors. So there were parallel considerations in opposition to the adoption of quarternions in both domains.

Balaguer's theoretical apparatuses account of the applicability of mathematics is especially useful because it is compatible with a range of views about the ontology of mathematics. The fictionalist can use it, with the proviso that the mathematical references are not to be taken seriously. Indeed, contemporary fictionalists like Melia and Leng implicitly or explicitly adopt Balaguer's view. The autonomy platonist can employ Balaguer's view too, with the belief that some mathematical theories are true. Reinterpreters and indispensability platonists can also use Balaguer's account, since the references of mathematical terms and our justifications for believing mathematical propositions are irrelevant to the uses of mathematical theories as tools for representation.

Since the problem of access is dissolved by recognition that our commitments are not determined by a simplistic empiricist account of sensory experience and the problem of application is easily solved by Balaguer's plenitudinous account, it might seem that there are no major epistemological challenges to autonomy platonism. In a sense, that's how it should be. Mathematics is the most secure and eternal of disciplines. The view that there is a major epistemological problem with mathematics is an academic philosopher's construct, in the most pejorative sense. But there are two serious, if less-foundational, worries about mathematical epistemology that are worth study.

First, since Balaguer's account of application is consistent with a wide variety of mathematical epistemologies, it does not determine how to think about our knowledge of mathematics. In particular, here, it does not help us discriminate between indispensability platonism and autonomy platonism, between taking our mathematical beliefs to be justified by their utility to science or by purely mathematical evidence. The indispensabilist and the autonomy platonist agree that the access problem is moot and can both adapt Balaguer's account of application, which is independent of Balaguer's plenitudinous platonist account of the justification of our mathematical beliefs. So the question of justification remains open.

Second, and related, there are open questions within mathematics which have epistemological importance. For example, we don't know the proper axioms for set theory. Many large cardinal axioms, consistent extensions of ZFC carrying existential import, have been proposed. The proper methods for determining which axioms to adopt are not clear.[21] For another example, the role of category theory in providing a foundation for mathematics alternative to set theory is an open question and has ramifications for how we think about mathematics, ultimately. These questions are major, in that they will determine mathematical ontology. At least some of these questions seem to be answerable; we can at least make progress toward answering them. But answers will not come via the indispensability argument nor via a defense of autonomy platonism. Such questions are, rather, to be engaged independently of the questions of mathematical epistemology on which this book focuses.

I hope that the foregoing three sections suffice to characterize AP and distinguish this family of views from IP, its rival platonistic response to Benacerraf's dilemma. I'll return to a more-detailed discussion of autonomy platonism in Chapters Nine and Ten, after examining a variety of versions of the indispensability argument.

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[21] See Maddy 1988a and 1988b.

Chapter Two: The Quinean Indispensability Argument

Indispensability arguments are indirect justifications for beliefs. An indispensability argument concludes that we should believe in some thing or claim because that belief follows from some prior or more secure beliefs. In the philosophy of mathematics, the term has become attached to a particular kind of argument, one which concludes that we should believe in the existence of mathematical objects, or in the truth of some mathematical theories, on the basis of their uses in the construction of empirical theories.

Even this specific use of the term does not determine the precise nature of the argument. In the next six chapters of this book, I characterize, distinguish, and evaluate three different versions of the indispensability argument in the philosophy of mathematics. Chapters Two through Five concern what I will call the Quinean argument and its derivatives. Chapter Six is about what I will call the Putnamian argument and its derivatives.[22] In Chapter Seven, I discuss the new explanatory indispensability argument, sometimes known as the enhanced argument.

§1: Quine's Argument

Quine does not present a detailed indispensability argument, though he alludes to one in many places.[23] I first present a concise version of the argument. Then I proceed to discuss Quine's defenses of each premise. The focus of my discussion is how the argument relies on various aspects of Quine's broader methodology, especially his naturalism, his confirmation holism, and his procedure for determining ontological commitment. Each of these presumptions is central to Quine's work but contentious, especially from the point of view of the autonomy platonist.

My exposition of Quine's argument is both exegetical and revisionary and I have tried to achieve a difficult balance between the two. On the one hand, I have tried to be as faithful to Quine's views as I can, given that he never presented the argument in careful detail. But I also look toward the most charitable way to regiment the argument, to interpret it in its strongest sense. I believe that my version of Quine's argument represents his central intentions in the matter. But even Quine, whose central doctrines can be found in his earliest works and who did not shift his views radically through his career, did alter at least his emphasis on several key topics, including the question of whether mathematical claims have empirical content.[24] It might be best to call the argument Quinean rather than Quine's, but I beg the reader's forgiveness if I do not mark a strict distinction between the two.

The Quinean indispensability argument can be stated rather simply:

QI
- QI1. We should believe the theory which best accounts for our sense experience.
- QI2. If we believe a theory, we must believe in its ontological commitments.
- QI3. The ontological commitments of any theory are the objects over which that theory first-order quantifies.
- QI4. The theory which best accounts for our sense experience quantifies over mathematical objects.
- QIC. We should believe that mathematical objects exist.

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[22] Many philosophers do not distinguish the Quinean and Putnamian arguments. Melia 2000: 455-6 makes a slightly different distinction between them than I do.

[23] For examples, see Quines 1939, 1948, 1951, 1955, 1958, 1960, 1978, and 1986a.

[24] "No mathematical sentence has empirical content, nor does any set of them" (Quine 1995b: 53). For a discussion of Quine's odd shifts in view about mathematics in his latest works, see Isaacson 2006: §4.

The conclusion of the Quinean indispensability argument is thus that many of our mathematical beliefs are justified by the security of the empirical science at the core of our best theory of the world. We are justified in believing literally in some mathematical claims which refer to mathematical objects. QI does not indicate which mathematical claims we are justified in believing; that is a matter for empirical scientists, ones who preside over the construction of our best theories, to decide.

While it is obvious that scientists use mathematics in developing their theories, it is not obvious why the uses of mathematics in science should lead us to believe in the existence of abstract objects. For example, when we study the interactions of charged particles, we rely on Coulomb's Law, which states that the electromagnetic force between two charged particles is proportional to the charges on the particles and, inversely, to the distance between them.

CL $F = k\ |q_1q_2| / r^2$, where the electrostatic constant $k \approx 9 \times 10^9\ Nm^2/c^2$

CL refers to a real number, k, and employs mathematical functions like multiplication and absolute value. Still, we use Coulomb's Law to study charged particles, not to study mathematical objects, which have no effect on those particles. The plausibility of Quine's indispensability argument thus depends on both Quine's claim that the evidence for our scientific theories transfers to the mathematical elements of those theories, which is implicit in QI1, and his method for determining the ontic commitments of our theories at QI3 and QI4. The method underlying Quine's argument involves gathering our physical laws and writing them in a canonical language of first-order logic. The commitments of this formal theory may be found by examining its quantifications.

§2: Naturalism and A Best Theory

The first premise of QI is that we should believe the theory which best accounts for our sense experience, i.e. we should believe our best scientific theory. Quine's belief that we should defer questions about what exists to natural science is an expression of his naturalism. Quine describes naturalism 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 1981: 72).

Quine contrasts his naturalism, which he sometimes calls relative empiricism, to the so-called radical empiricism of the logical empiricists. Logical empiricists presume that any claim, to be justified, must be reducible to (or perhaps expressible, in principle, as) claims about sense data. Since we know our own sense data securely, even infallibly, they say, proper combinations of and inferences from them, using the tools of mathematical logic, can be known just as surely. Scientific theories are, for the logical empiricists, just complex representations of sense experiences.

Instead of starting with the presumption of the security of sense data and trying to show how to construct theories of the world from them, Quine assumes that ordinary objects exist and that empirical science is our best account of our sense experience of them. The job of the epistemologist, for the naturalist, is not to justify knowledge of either ordinary objects or scientific theory by tracing complex claims to their purportedly simplest roots in our sense data. The job of the epistemologist is mainly to describe the path from stimulus to science.

Quine's rejection of the logical empiricist's reductionist, or foundationalist, view is central to his indispensability argument, to his claim that mathematical beliefs are justified and his depiction of how they are justified. For the logical empiricist, there is no hope of reducing mathematical claims to sense experience; we have no experiences of mathematical objects. The logical empiricist may thus resort to calling mathematical claims contentless or even nonsensical. Quine's approach, in contrast, allows for mathematical beliefs by rejecting any requirement for individual reductions of scientific claims to sense data. Mathematical claims, like any claims, are justified by their roles in a broad theory of the world, not by any particular individual experiences.

Quine thus integrates his account of mathematics with that of empirical science. He does not demand that mathematical claims be justified by individual confirming experiences. He only requires that the justifications of our scientific theory, taken as a whole, be empirical. In part, he rejects the logical empiricist'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 logical empiricism on the basis of his insight that we can adjust any theory to accommodate any evidence. The logical empiricist describes a system of piecemeal theory construction, where particular claims are each independently assessed with regard to our individual experiences. In contrast, Quine defends the view that there are no justifications for particular claims independent of the justification of our entire best theory, a claim which has come to be known as confirmation holism.

§2.1: Holism

Confirmation holism is essentially just the uncontroversial observation that any sentence s can be assimilated without contradiction to any theory T, as long as give up any sentences of T that conflict with s. Such changes may entail further adjustments and the resultant theory may in the end look quite different than it did before we accommodated s. But we can, as a matter of logic, hold on to any sentence come what may. Typically, we will have lots of different choices of how to accommodate s to T. And, we are not forced to hold on to any statement, come what may; there are no unassailable truths.

> Just about any hypothesis...can be held unrefuted no matter what, by making enough adjustments in other beliefs - though sometimes doing so requires madness. We think loosely of a hypothesis as implying predications when, strictly speaking, the implying is done by the hypothesis together with a supporting chorus of ill-distinguished background beliefs. It is done by the whole theory taken together (Quine and Ullian 1978: 79).

For a simple example, suppose that I have a friend named Abigail. I have a set of beliefs about our relationship. Since a theory is just a collection of sentences, we can call these beliefs a theory of my friendship with her. Suppose that I overhear Abigail saying mean things about me. New evidence conflicts with my old theory and consistency demands a resolution. I could reject the evidence (e.g. "I must have mis-heard"). I could accommodate the evidence by adjusting my theory. I might give up the portions about Abigail being my friend. I might cede the general principle that friends do not say mean things about friends, either completely or by adopting a restricted version of that principle. I might even consider accepting the contradiction; "Do I contradict myself? Very well, then I contradict myself" (Whitman). I have a range of choices about how to eliminate the new contradiction in my beliefs and no one of those choices is immediately necessitated. To make the choice, I have to balance a variety of considerations, ones which affect lots of my beliefs about Abigail, about friendship, and about the importance of avoiding contradictions.

Similarly, when astronomical evidence in the 15th and 16th centuries threatened the geocentric model of the universe, people were faced with choices of how to respond. The could have accepted the evidence and given up beliefs about the Earth being at the center of the universe. The could have found grounds for rejecting the evidence. They could have maintained a geocentric view by complicating the mathematical models of the solar system. They could have claimed that the heliocentric model was a mere heuristic. The new astronomical data did not itself dictate how ti would be accommodated. Scientists had to determine how our whole theories of the universe , and our roles in it, were best constructed.

The confirmation holism underlying QI entails that there are no justifications for particular claims independent of the justifications of our entire best theory. Since we always have various options for restoring an inconsistent theory to consistency, our mathematical theories and our scientific theories are linked. Our justifications for believing in science and mathematics are not independent. When new evidence conflicts with our current scientific theory, we can choose to adjust scientific principles or mathematical ones. Evidence for the scientific theory is also evidence for the mathematics used in that theory.

§2.2: Physicalism and Pluralism

By assuming that the theory which best accounts for our sense experience is our best scientific theory, as opposed to, say, a theological account, Quine defers the work of determining what exists to scientists. Scientists naturally engage questions of the ultimate structure of the universe, whether explicitly and directly, as in theoretical physics, or less obviously, as in psychology or biology. Still, 'science' is a thin term for a broad set of endeavors. The proponent of QI must clarify what is to be taken as science for the purposes of determining our ontology.

QI itself makes no claims about whether science is really just physics or whether it has various, perhaps autonomous branches (e.g. biology, neuroscience, semantics, economics). Neither does QI make a claim about the relationship between physics and these other branches of science. Given the holism underlying the indispensability argument, which entails that our best theories are essentially interconnected, the proponent of QI must believe that there is some, perhaps quite strong, relationship among the various branches of empirical science.

A proponent of QI could assert a strong version of physicalism: every science is reducible to physics. On a version of this strong physicalism, what exists is just what our best physics says exists. Our best theories of physics are currently unsatisfactory. Quantum mechanics and relativity theory, as we understand them, seem to conflict. But for the strong physicalist, the objects of these theories are our best guesses about what exists. Other objects, tables and trees and animals and mental states, are just complex arrangements of the objects of our ontology.

Hedging, one might say that some sciences are irreducible to, but still supervene on, physics. Perhaps, say, mental states (the subjects of psychological sciences) are not completely explicable in terms of the basic laws of physics though they require no further ontology than that of physics.

Even more weakly, one might deny both that the special sciences are reducible to physics and that the ontology of our best theories of physics suffices for the explanations of every phenomenon that might be deemed scientifically explicable. Call scientific pluralism the position that various branches of science (biology, neuroscience, semantics, economics, etc.) are independent of each other, in some ways, and not reducible to physics. The pluralist sees our best theory as some sort of amalgam of various areas of science. In the absence of convincing evidence for stronger theses of reducibility or supervenience, it might be advisable for the proponent of QI to adopt scientific pluralism.

Deciding among strong physicalism, a weaker version, or even a pluralism, is important for the proponent of QI in order to determine which theories one should examine for mathematical content. In places, Quine seems to adopt a kind of scientific pluralism. He uses examples from casual discourse to illustrate how science invokes mathematical objects. He 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 1953: 115; see also Quine 1960: §48 and §55; and Quine 1981b: 14).

Quine's references to statistical generalities also make him appear pluralistic. He countenances groups of people rather than the collections of elementary particles. "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: 14).

Even in 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: 133).

Given the holism underlying the indispensability argument and its consequent connection of all aspects of our best theories, there is a natural tension between QI and pluralism. Perhaps Quine intends talk of common uses of mathematics as merely precedental of the kind of uses that one would have in a mature physical theory. Despite pluralistic appearances, Quine is ordinarily taken as a physicalist of a fairly strong kind, one who believes that our best theory will consist of the axioms of a completed physics. Putnam states Quine's physicalism explicitly. "Quine proposes to reduce logic, mathematics, and philosophy itself to physics" (Putnam 1981d: 183). 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: 98).

There are many different attitudes toward scientific theories between the strongest physicalism and the weakest pluralism. The question of whether to be a physicalist or a pluralist, or where in between one falls, is most relevant to QI4. The physicalist needs to determine 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.

This is not the place to determine the ultimate relationship among physics and the special sciences. But it is important to note that QI1 requires that there be a single, ultimate theory which comprehends and explains everything that can legitimately called science or knowledge. The proponent of QI can not be so pluralistic as to allow for the ontology of theories which are not strictly scientific or relevantly connected to science. If there were any claims that we could know which were outside the scope of our best empirical theory, independent of our best science, mathematical claims would seem to be among them. If we could know mathematical claims without appeal to science, the indispensability argument would be otiose. We would not need indirect justification of our knowledge of mathematics since whatever would justify our knowledge independently of scientific theory would suffice.

Such a pluralism is not Quine's view. But even though Quine is best seen as a physicalist, the proponent of QI might attempt to adopt one of the weaker views, perhaps in buttressing the claim, QI4, that the ubiquity of mathematics throughout all human endeavors should impel and justify our mathematical beliefs. So it is important to remember that steps away from physicalism while attempting to maintain the indispensability argument may create a kind of instability in one's view. Whatever pluralism the proponent of QI adopts, it will have to maintain the holistic connections among different theories.

§2.3: The Aesthetics of Theory Construction

Theories are generally under-determined by evidence. Simple examples include the fact that evidence often provides correlation without indicating causation. For example, a recent study shows that Facebook users get lower grades in college. We do not know whether to conclude that using Facebook causes lower grades or that people who use Facebook are those who are already likely to be less successful.

Sometimes new evidence forces us to alter our theories. We ordinarily have choices among various hypotheses. As the holist emphasizes, we can even reject the evidence and hold onto our original theory. Even when an observation does not conflict with previously-accepted hypotheses, there are always conflicting theories that can accord with our claims. Data is not categorical and can be accommodated by a variety of theories. Some theories can be ruled out for carrying extraneous elements. We discount theories that refer to ghosts, for example, and seek an explanation of the noise in the attic which appeals only to natural phenomena, like wind or the expansion and contraction of materials due to humidity. We invoke principles of parsimony, or Ockham's razor: do not multiply entities beyond necessity.

The construction of any theory is guided by several further principles, which Quine sometimes calls immanent virtues. When constructing a theory, or restoring consistency to a theory in which a contradiction has been discovered, we balance accounting for the evidence with achieving elegance in formulation. We try to retain as much of our prior theory as we can while accommodating new evidence and we make our claims as weak as possible, in order that they be as defensible as possible. Still, we want our theories to be broad and unifying in order that they have maximal explanatory strength.

Among the immanent virtues which govern scientific reasoning generally, Quine mentions conservatism, modesty, simplicity, generality, and refutability. Conservatism tells us to only revise as little as we need to, in order to maintain as much as possible of our previous theory. We accept only the most modest principles as the most plausible. "The lazy world is the likely world" (Quine and Ullian 1978: 68).

Simplicity for our large theory trumps simplicity for any portion of that theory. The claim 'objects fall to the Earth' is simple, but conflicts, outside our atmosphere, with gravitational theory, which is simpler overall, and more general. "There is a premium on simplicity in any hypothesis, but the highest premium is on simplicity in the giant joint hypothesis that is science, or the particular science, as a whole. We cheerfully sacrifice simplicity of a part for greater simplicity of the whole when we see a way of doing so" (ibid: 69).

Some critics of the indispensability argument believe that the invocation of the immanent virtues in constructing our theories leaves room for denying the argument, for denying that mathematical objects are essential to the construction of our theories. Debates about the argument can detour into discussions of how virtues like simplicity or conservatism are best understood and applied. Such detours are unavoidable given facts about how evidence under-determines theories.

A formulation of a theory which includes mathematics may be simpler, in some ways, than a formulation which does not. But the latter formulation may be seen as simpler precisely because it does not include references to abstract objects. Appeals to simplicity and the other immanent virtues are not categorical. These theoretical properties have an aesthetic component which introduces a kind of plasticity into QI1 and the indispensability argument, one which we will examine in more depth in Chapter Four.

QI1 may best be seen as a working hypothesis in the spirit of Ockham's razor. We look to our most reliable endeavor, natural 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.

The question of how we justify our beliefs about mathematical objects arose mainly because we could not perceive them directly. By rejecting the logical empiricist's requirement for reductions of scientific claims to sense data, Quine allows for beliefs in mathematical objects despite their abstractness and inaccessibility. Quine rules out independent justifications for formal sciences like mathematics while allowing that mathematical knowledge can be justified as a part of our best theory. We do not need sensory experience of mathematical objects in order to justify our mathematical beliefs. We merely need to show that mathematical objects are indispensable to our best theory.

§3: Believing Our Best Theory and the Double-Talk Argument

The second premise of Quine's argument states that our belief in a theory extends to the objects which that theory posits. This second premise is important because it insists that we are required to be univocal and sincere in asserting a theory and interpreting its assertions. While there is some looseness in our applications of the immanent virtues while constructing a theory, in determining on scientific grounds the best systematization of our beliefs, once we have formulated our best theory, there is no looseness in our beliefs about its posits.

Against QI.2, one might think that we could believe a theory while remaining agnostic or instrumentalist about whether its objects exist. Physics is full of fictional idealizations, like infinitely long wires, centers of mass, and uniform distributions of charge. Other sciences also posit objects that we do not really think exist, like populations in Hardy-Weinberg equilibrium (biology), perfectly rational consumers (economics), and average families (sociology). Sometimes, scientists posit such objects, knowing full well that they do not exist, in order to simplify inferences. While no wire is infinitely long, for certain calculations in electromagnetism such an assumption is useful, with no practical drawbacks. Talk of average family sizes or incomes is similar, facilitating communication with no worry of being misleading. Indeed, much of scientific reasoning is fo this sort, which Robert Batterman calls asymptotic.

Given the utility and ubiquity of idealizations and asymptotic reasoning, we might think that we can believe our best theories while recognizing that the objects to which it refers, strictly speaking, are only ideal. If we hold this instrumentalist attitude toward average families and infinitely long wires, we might want to hold it toward circles, numbers and sets, too.

Against such instrumentalism, Quine argues that any discrepancy between our belief in a theory and our beliefs in its objects is illegitimate double-talk. One can not believe in only certain elements of a theory which one accepts. If we believe a theory which says that there are centers of mass, then we are committed to those centers of mass. If we believe a theory which says that there are electrons and quarks and other particles too small to see, then we are committed to such particles. If our best theory posits mathematical objects, then we must believe that they exist. We can not assert the existence of objects at one moment and then take back those assertions at the next, on pain of inconsistency.

The double-talk criticism of instrumentalism appears throughout Quine's work. For example, his response to Carnap's internal/external distinction relies on it. 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. Once one has accepted mathematical objects as an internal matter, one can not merely dismiss these commitments as the arbitrary, conventional adoption of mathematical language. There is no external perspective from which to stand and choose among languages or conceptual frameworks. "[N]atural science [is] an inquiry into reality, fallible and corrigible but not answerable to any supra-scientific tribunal" (Quine 1981c: 72).

Quine's response to the Meinongian Wyman in "On What There Is," is also a double-talk criticism. Wyman presents two species of existence in order to avoid saying something about nothing, for example that Pegasus is a horse. Quine distinguishes between the meaningfulness of 'Pegasus' and its reference in order to avoid admitting that Pegasus subsists while at the same time denying that Pegasus exists. Putnam, defending Quine's indispensability argument, 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 1971: 356).

Worries about double-talk bother Quine's critics as well as his supporters. Field applies the double-talk criticism directly to worries about 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: 2).

QI1 and QI2 together entail that we should believe in the objects that our currently best theory says exist. Any evidence applies to the whole theory and we can not pick and choose among the posits, distinguishing ones we prefer from ones we disdain using some extra-scientific criteria. 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. This univocality of ontological commitment 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: 22).

The univocality of ontological commitment underlying QI permits the argument's proponents to avoid some criticism. It was one of Quine's great achievements to notice that the access problem in the philosophy of mathematics becomes obsolete once we recognize that ontological commitment is a matter of constructing and formulating theories rather than grounding each individual claim in sense experience or rational insight. "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 1983: 377-8).

The proponent of QI thus denies that the idealizations ubiquitous in science motivate or support any kind of mathematical instrumentalism. Even if scientists require fictionalizations like centers of mass for their work, that work may be based on, or presupposing, a theory which makes no such claims. That latter, more parsimonious theory will be the proper place to look for existence claims since its role is primarily to express the nature of things. Theories with idealizations are more likely constructed to facilitate inferences and communication. The austere theory is the official version. If there is no austere version available, then it is too bad for the opponent of centers of mass; they turn out to be indispensable posits and deserve no extra-scientific denigration.

The question, then, between the proponent of QI and the instrumentalist is whether an alternative theory, one which eschews mathematical objects, can be formulated. If so, then our everyday and scientific uses of mathematics can be seen as merely instrumental. If not, then commitment to their existence seems unavoidable, on pain of double-talk.

In Chapter Four, I will return to the instrumentalist's attempt to find a middle path, a defensible position which accepts both that our best theories express commitments to mathematical objects and that we should not be so committed. For now, let's, with Quine, reject such double talk.

Even putting instrumentalism aside, we might have some reservations about the claims of our best theories. There will be conflict between our currently best theory and ideal theories future science will produce. Future theories are, of course, not now available. But what exists does not vary with our best theory. Thus, any current expression of our commitments is at best speculative.

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, is also speculative, a casual observation which is, strictly speaking, unavailable to the proponent of QI. Insofar as one accepts double-talk, one must reject QI. The indispensabilist is adrift on Neurath's boat, with no external, meta-scientific perspective from which to judge our best theory, from which to hesitate in our affirmation of its commitments. We know, casually and meta-theoretically, that our current theory will be superceded, and that we will give up some of our current beliefs. But, we do not know how our theory will be improved, and we do not know which beliefs we will give up. The best we can do is believe the best theory we have, and believe in its posits, and have a bit of humility about these beliefs.

QI1 and QI2 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.

§4: Quine's Procedure for Determining the Commitments of a Theory

The third step of Quine's argument is an appeal to his general procedure for determining the posits, the ontological 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 method. 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 misleading and incomplete.

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

QP
- QP1 Choose a theory.
- QP2 Regiment that theory in first-order logic with identity.
- QP3 Examine the domain of quantification of the theory to see what objects the theory requires to come out as true.

The previous two sections discuss the application of QP1 to the indispensability argument. But, Quine's method for determining our commitments applies to any theory. Theories which refer to trees, electrons, and numbers, and theories which refer to ghosts, caloric, and God, are equally amenable of Quine's general procedure.

The second step of Quine's procedure invokes first-order logic as a canonical language. First-order logic (with identity) is a formal language of predicates and variables, logical connectives, some optional punctuation, quantifiers, and an identity predicate. In arguing that we can use this language as canonical since we can use it to express anything we need to say, Quine credits first-order logic with unifying the referential apparatus of ordinary and scientific language. He emphasizes its extensionality, efficiency, and elegance, convenience, simplicity, and beauty.[25]

Quine's enthusiasm for first-order logic largely derives from some attractive technical virtues. A variety of definitions of logical truth concur: in terms of logical structure, substitution of sentences or of terms, satisfaction by models, and proof. First-order logic is complete, in the sense that any valid formula is 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 theorems, 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).[26]

Less technically, the existential quantifier in first-order logic is a natural cognate of the English term 'there is', and Quine proposes that all existence claims can and should be made by existential sentences of first-order logic. "The doctrine is that all traits of reality worthy of the name can be set

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[25] See Quine 1986: 79 and 87.

[26] See Mendelson 1997: 377.

down in an idiom of this austere form if in any idiom" (Quine 1960: 228). Moreover, "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: 534).

We should take first-order logic as our canonical language only if:

FOL1 We need a single canonical language;
FOL2 First-order logic is adequate to express our ontological commitments; and
FOL3 No language other than first-order logic is adequate.

In Chapter Three, I will deny each of the clauses FOL1-FOL3. Here, I present Quine's reasons for holding them.

FOL1 arises, for Quine, almost without argument from QI1 and QI2, from his holism and his double-talk argument. One of Quine's most striking and important innovations was his linking of our concerns when constructing formal theory 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 a quest of ultimate categories, a limning of the most general traits of reality" (Quine 1960a: 161).

Whether FOL2 holds depends on how we use our canonical language. First-order logic is uncontroversially useful for what Quine calls semantic ascent. When we ascend, we talk about words without presuming that they refer to anything; we can deny the existence of objects without seeming to commit to them. For example, on some theories of language, sentences which contain terms that do not refer to real things are puzzling. Consider:

CP The current president of the United States does not have three children.
TF The tooth fairy does not exist.

If CP is to be analyzed as saying that there is a current president who lacks the property of having three children, then by parity of reasoning TF seems to say that there is a tooth fairy that lacks the attribute of existence. This analysis comes close to interpreting the reasonable sentence TF as a contradiction saying that there is something that is not.

In contrast, we can semantically ascend, claiming that the term 'the tooth fairy' does not refer. To that end, TF may be conveniently regimented in first-order logic, using 'T' to stand for the property of being the tooth fairy. '$\sim(\exists x)Tx$' carries no implication that the tooth fairy exists. Similar methods can be applied to more serious existence questions, like whether there is dark energy or an even number greater than two which is not the sum of two primes. Thus, first-order logic provides a framework for settling disagreements over existence claims.

For FOL3, Quine argues that no other language is adequate for canonical purposes. Ordinary language is too sloppy, in large part due to its use of names to refer to objects. We use names to refer to some things which exist: 'Muhammad Ali', 'Jackie Chan', 'The Eiffel Tower'. But some names, like 'Spiderman', do not refer to anything real. Some things, like most insects and pebbles, lack names. Some things, like most people, have multiple names.

We could clean up our language, constructing an artificial version in which everything has exactly one name. Still, in principle, there will not be enough names for all objects. As Cantor's diagonal argument shows, there are more real numbers than there are available names for those numbers. If we want a language in which to express all and only our commitments, we have to look beyond languages, like ordinary natural language, which rely on names.

Moreover, in natural languages, 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 exclusively at the quantifiers of the theory.

We are forced to choose between languages with names and languages with quantifiers because we can not include names in a language with quantifiers. Consider the derivation AE, valid in standard languages which include both names and first-order quantifiers, which entails that anything named exists:

AE		
	1. $\sim(\exists x)x=a$	Assumption, for indirect proof
	2. $(\forall x)x=x$	Principle of identity
	3. $(\forall x)\sim x=a$	1, Change of quantifier rule
	4. $a=a$	2, Universal instantiation
	5. $\sim a=a$	3, Universal instantiation
6. $(\exists x)\ x=a$		1-5, Indirect proof [27]

AE shows that any language which includes both names and quantifiers is trouble. 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.

Higher-order logics have all of the expressive powers of first-order logic and more. Most distinctly, where first-order logic allows variables only in the object position (i.e. following a predicate), second-order logic allows variables in predicate positions, as well, and introduces quantifiers to bind those predicates. Logics of third and higher order allow further predication and quantification. As they raise no significant philosophical worries beyond those concerning second-order logic, I will focus solely on second-order logic.

To see how second-order logic works, consider the inference R.

R
R1. There is a red shirt.
R2. There is a red hat.
RC. So, there is something (redness) that some shirt and some hat share.

RC does not follow from R1 and R2 in first-order logic, but it does follow in second-order logic.

R_S:
$R1_S$. $(\exists x)(Sx \bullet Rx)$
$R2_S$. $(\exists x)(Hx \bullet Rx)$
RC_S. $(\exists \mathsf{P})(\exists x)(\exists y)(Sx \bullet Hy \bullet \mathsf{P}x \bullet \mathsf{P}y)$

Accommodating inferences such as R by extending one's logic might seem useful. But higher-order logics allow us to infer, as a matter of logic, that there is some thing, presumably the property of redness, that the shirt and the hat share. It is simple common sense that shirts and hats exist. It is a matter of significant philosophical controversy whether properties like redness exist. Thus, a logic which permits an inference like R_S is controversial.

Quine's objection to higher-order logics, and thus a central part of his defense of using first-order logic as canonical, is that we are forced to admit controversial elements as interpretations of predicate variables. Even if we interpret predicate variables in the least controversial way, as sets of objects that have those properties, higher-order logics demand sets. Thus, Quine calls second-order logic, "Set theory in sheep's clothing" (Quine 1986a: 66). Additionally, higher-order logics lack many of the technical

[27] I owe the derivation to David Rosenthal.

virtues, like completeness and compactness, of first-order logic.[28]

Second-order logic and logics of higher orders have a variety of appealing and intuitive uses, especially in mathematics and logic. For the purposes of QI, though, they are contentious since their adoption would seem to beg the question of the existence of mathematical objects. Quine's preference for first-order logic, given its neutrality on that question, seems prudently conservative.

Once we settle on first-order logic as a canonical language, we must specify a method for determining the commitments of a theory in that language. Reading existential claims seems straightforward. For example, R2, read naturally, says that there is a thing which is a hat and which is red. But theories do not determine their own interpretations. Quine relies on standard Tarskian model-theoretic methods to interpret first-order theories. On a Tarskian semantics, we ascend to a metalanguage to construct a domain of quantification for a given theory. We consider whether sequences of objects in the domain, taken as values of the variables bound by the quantifiers, satisfy the theory's statements, or theorems. The objects in the domain that make the theory come out true are the commitments of the theory. "To be is to be a value of a variable" (Quine 1939: 50, and elsewhere).

The move to a metalanguage means that we do not directly interpret first-order theories to find their ontological 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 collected 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 commitments are the byproducts of this neutral process.

§5: Mathematization

The final step of QI involves simply looking at the domain of the theory we have constructed. When we write our best theory in our first-order language, we discover that the theory includes physical laws which refer to functions, sets, and numbers. Consider again Coulomb's Law: $F = k\ |q_1q_2|/ r^2$. Regimenting Coulomb's Law, or any sentence of physics, all the way down into first-order logic would make it quite complicated, but a partial first-order regimentation suffices to demonstrate the commitments of the law, using 'Px' for 'x is a charged particle'.

CLR $\forall x \forall y\{(Px\ \&\ Py) \rightarrow \exists f\, [f(q(x), q(y), d(x,y), k) = F]\}$
where $F = k\ |q(x)\ q(y)|/ d(x,y)^2$

In addition to the charged particles over which the universal quantifiers range, there is existential quantification over a function, f. 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, our ideal theory must include set-theoretic axioms, perhaps those of Zermelo-Fraenkel set theory with choice,

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28 For a defense of second-order logic, see Shapiro 1991.

ZFC. The full theory ZFC is unnecessary for scientific purposes; there will be some sets which are never needed, some numbers which fail to measure any real quantity. But we adopt a full set theory in order to make our larger theory as elegant as possible. ZFC is tidily axiomatized where a theory which only provided the sets one actually uses in science would be a gerrymandered mess. We can derive from the axioms of any adequate set theory a vast universe of sets. So, CL contains or entails several existential mathematical claims.

CL, with its mathematical commitments, is representative of the kind of physical law that motivates Quine's indispensability argument. Such examples abound. Real numbers are used for measurement throughout physics, and other sciences. Quantum mechanics makes essential use of Hilbert spaces and probability functions. The theory of relativity invokes the hyperbolic space of Lobachevskian geometry. Economics is full of analytic functions. Psychology uses a wide range of statistics. According to QI, then, we should believe that these mathematical objects exist.

Opponents of QI4 have developed strategies for re-interpreting apparently ineliminable uses of mathematics, especially in physics. Some reinterpretations use alethic modalities (necessity and possibility) to replace mathematical objects. Others invoke space-time points or regions. Some of these projects are motivated by fictionalism, in order to show why quantification of mathematical objects is unnecessary and that we should thus take existential sentences which quantify over mathematical objects to be false. Other projects are reinterpretative, with the goal of arguing that our first-order quantifications over mathematical objects should be taken as true, when properly interpreted. It is quite easy, if technical, to rewrite first-order theories in order to avoid quantifying over mathematical objects. It is less easy to do so while maintaining Quine's canonical language of first-order logic.

For example, Hartry Field's reformulation of Newtonian gravitational theory (Field 1980) replaces the real numbers which are ordinarily used to measure fundamental properties like mass and momentum with relations among regions of space-time. Field replaces the '2' in claims like, "The beryllium sphere has a mass of 2 kg" with a ratio of space-time regions, one twice as long as the other. In order to construct the proper ratios of space-time regions, and having no mathematical axioms at his disposal, Field's project requires either second-order logic or axioms of mereology, both of which are controversial extensions of first-order logic.

Work on QI4 is legion and I will not add to it here. As I mentioned in the first chapter, this book concerns the relationship between the two different kinds of platonism: autonomy and indispensability platonism. Whether QI4 is true or false, and on what interpretation, is mainly irrelevant to my goals, though if it were decidedly false, the platonist would lose the option of relying on QI and would be forced either to adopt autonomy platonism or to abandon platonism. For the purposes of argument, to make my defense of autonomy platonism stronger, I grant the indispensabilist QI4.[29]

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[29] For an excellent survey of dispensabilist strategies, and further references, see Burgess and Rosen 1997; for more recent work, see Melia 1998, MacBride 1999, and Melia 2000.

Chapter Three: Problems for QI

In the 1980s and 1990s, most attention to the indispensability argument was paid to the premise QI4, the question of whether mathematical or scientific theories could be rewritten as attractive theories without quantifying over mathematical objects. I believe that the problems with QI arise earlier in the argument. Recently, some voices have emerged which also challenge other premises. Philosophers including Azzouni, Melia, and Leng have questioned the claim that quantification over mathematical objects, even in good or best theories, should entail or justify belief in mathematical objects.

In this chapter, I discuss three distinct problems with Quine's indispensability argument. The criticisms of this chapter do not presume autonomy platonism or indeed any alternative to QI. Some of the criticisms I discuss originate in the works of people who deny platonism but most of those criticisms are also consistent with at least some forms of platonism.

I focus on three kinds of criticisms. First, following work of Penelope Maddy, I argue that Quine's naturalism has a natural internal tension and at least some understandings of his naturalism should not lead to the indispensability argument. On one interpretation, Quine's abandonment of first philosophy should leave mathematical justifications alone rather than deferring the question of whether we should believe that mathematical objects exist to the question of how and when they are invoked in scientific theories.

Second, following work of Elliott Sober, I argue that the holism underlying the indispensability argument does not extend to mathematics. The ways in which we isolate mathematical theories from empirical refutation show that evidence for scientific theories does not transfer to mathematics as the indispensabilist claims. Unlike Sober, though, I do not extend my criticisms of holism to its application within empirical science.

Last, I raise some worries for Quine's method for determining ontological commitments of theories. Quine's method purportedly allows us to reveal our commitments without prejudice: we regiment our best theory using scientific principles of theory construction and the commitments fall out of it, naturally, univocally, and unequivocally. As some recent critics argue, Quine's method misrepresents the way in which we determine our commitments. Instead, we regiment our pre-considered judgments about what exists. If, like Quine, we are predisposed to nominalism, QI, with its reliance on Quine's method for determining the ontological commitments of a theory, should not commit us to beliefs in mathematical objects. If we want to justify our mathematical beliefs, we must look elsewhere.

§1: Two Interpretations of 'Naturalism'

The indispensability argument depends, especially in its first premise, on Quine's claim that all evidence is sense evidence.[30] While Quine, in places, calls himself an empiricist, and the indispensability argument is clearly an empiricist's argument, Quine prefers to call his views 'naturalist'. As with any distillation of a variety of complex views to a simple term, there are different interpretations of 'naturalism'. Quine characterizes naturalism as the rejection of the view that philosophy is independent of science or is a way of evaluating science.

One important aspect of Quine's naturalism, central to his use of the term, is the assimilation of epistemology to empirical psychology. If philosophy can provide no extra-scientific criteria with which to evaluate science, then there is no first philosophy (no metaphysics) and no epistemology independent

[30] It is for this reason that Putnam calls Quine, "The Greatest Logical Positivist." Isaacson notes that Quine's views about evidence really are verificationist, if not reductionist: "Of course, not in the form rejected by him in 'Two Dogmas'... What was wrong with the Vienna Circle's verificationism was not the role it assigned to verification but the unit of language to which verification was taken to apply" (Isaacson 2006: 215).

of our best scientific theories. Quine's holism ensures that claims in philosophy, physics, biology, economics, and mathematics, are all inter-related. No claim is prior to, or independent of, any other. Epistemology, our theory of knowledge, is continuous with neuroscience, our theories of the brain, and cognitive psychology, our theories of our thought, and biology, our theories of sensation, and information processing.

> Naturalism does not repudiate epistemology, but assimilates it to empirical psychology. Science itself tells us that our information about the world is limited to irritations of our surfaces, and then the epistemological question is in turn a question within science: the question how we human animals can have managed to arrive at science from such limited information (Quine 1981c: 72).

Concomitantly, Quine views philosophy as continuous with the sciences. Philosophers can make progress in mathematics, say, as Quine did in set theory, or in empirical science, by contributing to the understanding of empirical research. Or they can work in areas traditionally studied by philosophers. But there is no special, independent discipline of philosophy. If we want to do metaphysics, we just construct the best scientific theories, and interpret them. We look to scientists for their work.

This rough sketch of Quine's naturalism, so central to the indispensability argument, is not uncontroversial. In particular, it is not clear how strictly we are to take Quine's claims about epistemology being just empirical psychology. Psychology is generally a descriptive discipline, exploring brain functions, thoughts, and personality. Epistemology is a normative pursuit, distinguishing better and worse accounts of belief formation and justification. Indeed, the conclusion of QI is explicitly normative: we should believe that mathematical objects exist. It is difficult to see how such a normative claim could follow from an empirical field like psychology.

Most relevantly here, Maddy argues that two interpretations of Quine's naturalism are in tension with one another, especially around the question of the status of our mathematical beliefs. On one interpretation, Quine privileges empirical science, understood holistically, as the locus of all our ontological commitments; mathematicians must defer to scientists on ontological questions. If mathematics is required for science, then our mathematical beliefs are justified. But insofar as portions of mathematics remain unapplied, the relevant beliefs remain unjustified.

On the other interpretation, since Quine defers all questions of what exists to the scientists and scientists isolate mathematics from the rest of their theories, the applications of mathematics seem to be irrelevant to the justification of mathematical beliefs. Scientists do not act as if all the objects over which they quantify have the same status. They make distinctions between the real elements of their theories and the instrumental elements. In particular, they seem to rely on something closer to an eleatic principle when evaluating the commitments of their theories.

The central idea behind the eleatic principle is that only those things which are causally active are real. The principle, which has its roots in Plato's *Sophist*, is notoriously difficult to articulate precisely. This difficulty arises in part because necessary and sufficient conditions are always hard to formulate and in part because the principle relies on a concept, causation, which is itself notoriously unclear. Eleatics may emphasizes either causal activity or spatio-temporal location. If we truly defer our questions about what exists to science, as Quine urges, then on the second interpretation of naturalism, we are moved to reject Quine's view that scientific evidence transfers to mathematical claims. In practice, scientists reject Quine's holism, at least insofar as they do not reject as unfounded mathematical results which turn out to be unapplied. The naturalist seems to have to decide between accepting Quine's holism or rejecting it in favor of the instrumentalism that scientists actually use.

Such instrumentalism about mathematical objects employs double-talk about ontological commitment which is strictly forbidden by Quine and others who defend QI. According Maddy, the

Quinean response to instrumentalism unjustifiably privileges philosophy over scientific practice. The holistic response elevates philosophical reflection over scientific practice, and thus conflicts with a proper understanding of naturalism. A consistent naturalism, Maddy argues, would not reject instrumentalism if that view is a part of scientific practice; and instrumentalism is an accepted principle of scientific practice.

Maddy thus concludes that the indispensability argument fails because the ontological commitments of a theory are not to be found exclusively in the quantifications of the theory. Instead, naturalism entails that epistemology is to be assimilated to empirical psychology, and that metaphysics is to be assimilated to empirical science. The ontological commitments of a theory should be discovered by looking at the actual practice of scientists.

The arguments for naturalism in philosophy can thus lean in two different directions. Some philosophers believe that naturalism supports a denial of platonism. After all, mathematical objects are not natural objects; being abstract, they are outside the natural world. Mary Leng, for example, defends fictionalism as a result of privileging the naturalism which respects the practice of scientists over Quinean holism and the double-talk argument.

But naturalism can work in the other direction as well. As Maddy writes, if we take the work of mathematicians seriously and defer ontological claims to the science of mathematics, naturalism seems to support platonism. For mathematicians and scientists quantify over mathematical objects all the time. If there really is no higher tribunal than science itself, the anti-platonist has no position from which to deny such existential quantifications.

Moreover, the naturalism which defers questions about ontology to practicing scientists does not entail that ontological questions must be answered by empirical scientists. If we defer such questions to the mathematical scientists constructing mathematical theories, without detouring through empirical science, Quine's naturalism can, ironically but not implausibly, support autonomy platonism. I return to this view, my own, in the last chapter of the book.

§2: Confirmation Holism and Disciplinary Boundaries

The interpretation of Quine's naturalism which I hold, the one which supports autonomy platonism, is contentious and I do not wish my argument against QI to rely on it. There are more important flaws in the argument as well as problems with the conclusion which I discuss in Chapter Five. One more-serious worry about QI concerns its holism.

Quine argues for the holism which underlies QI1, 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 background theories. Within empirical science, this holism is not unreasonable. But Quine's holism ignores the important differences between posits of mathematical objects and posits of empirical objects. In practice and in principle, we shield mathematics from empirical refutation, even if the logical point holds.

Holism is suspect precisely when it comes to mathematics. Empirical evidence never leads us to give up our mathematical beliefs. In contrast, we have given up beliefs in all sorts of other theoretical posits. Moreover, it is easy to imagine evidence that would force us to give up our current beliefs in non-mathematical theoretical posits, in objects which are neither causally nor constitutionally related to sensible objects. There seems to be a serious, principled, methodological difference between mathematical and empirical posits.

Quine argues that the commonsense distinction between mathematics and empirical science is illusory. All objects are posits, including ordinary ones. "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: 45).

In lieu of the commonsense distinction, Quine presents a continuum of commitments from our most firm and central to our most tenuous and peripheral: the web of belief. As we are unlikely to give up our beliefs in ordinary objects, we are unlikely to cede our mathematical beliefs. But any posit may be questioned. Starting our reasoning about the world with our beliefs about ordinary objects, as Quine rightly does, is no guarantee of ending with them. If on a scientific basis, say, Berkeleyan idealism turned out to be a better theory than physicalism, if it were more useful and simpler, say, then we should in fact abandon beliefs in physical objects. We can cede any belief, including our basic mathematical ones.

In contrast to Quine's view, since no empirical, scientific reasons sway us to abandon our mathematical beliefs, then we have the basis for a distinction which undermines his holism. We act as if the traditional conception of mathematics is true. On the traditional conception, mathematical claims are necessarily true and mathematical objects, the subjects of those claims, thus exist in all possible worlds. No empirical evidence transfers to our mathematical beliefs, either for the existence of mathematical objects or against it.

Appeals to necessity and possible worlds are notoriously tendentious. But the concept of necessity is essentially the one which underlies even some austere work, in particular Field's claim of conservativeness for mathematics. In defending fictionalism, Field argues in detail that good mathematical theories are conservative over our best physical theories: The addition of mathematics to a theory with no mathematical axioms should license no additional nominalistically-acceptable conclusions. If a mathematical theory is conservative over a nominalist physical theory, then we can use the mathematics to facilitate derivations in the physical theory with assurance that we will not derive any unacceptable empirical consequences. Mathematical theories are supposed to be compatible with any empirical theory.

By itself, an appeal to our evident respect for the traditional conception of mathematics is insufficient to establish its truth. It ignores the strength of Quine's position. Quine accounts for the immunity of mathematics by referring 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: 15).

Sober calls holism into question with an explanation of why we never cede mathematical beliefs on the basis of empirical experiment. We subject mathematical claims to different kinds of tests. We do not, in practice or in principle, 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. But we are able to do this only 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:

Y1 Space-time is curved.
Y2 Space-time is flat.
Y3 Space time is not curved, although all evidence will make it appear that it is.

Empirical evidence will discriminate between Y1 and Y2, but no evidence will discriminate between Y1 and Y3. 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: 45).[31]

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 and implausible, Quine's doctrine appears suspect.

In later work, Sober calls Quine's holism bizarre for its consequence that evidence for an empirical theory is supposed to extend to all background beliefs, not just those in mathematics.

> If I believe relativity theory, and this theory is confirmed by some observation that I make, then *everything* I believe is also confirmed. To say otherwise is to say that the observation impinges only on part of what I believe; my total system of beliefs then would not have confronted the tribunal of experience as a corporate body (Sober 2005: 266).

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: 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. For cases of empirical confirmation of empirical beliefs, Quine's response is perhaps defensible. But for the case at hand, whether mathematical beliefs are called into question or confirmed by empirical evidence, Sober's contention that mathematics is immune to disconfirmation is more plausible.

Sober's argument against holism relies on differences in testing, differences which one might esteem merely practical and thus no evidence against holism. Resnik argues that such differences in practice do not 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: 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.

Moreover, Sober can accept Quine's assertion that confirmation holism holds as a logical matter without ceding the claim that empirical evidence may undermine our mathematical beliefs. Quine's claim only holds on the presumption that our beliefs in mathematics are based exclusively on their applicability in science. If we have other reasons to believe mathematical claims, ones, for example,

[31] Putnam similarly suggests that we can distinguish mathematics from science by the fact that scientific theories have viable competitors and mathematical theories do not. See Putnam 1967b: 50.

based on our estimation of mathematical methods, then Quine's logical point that we can excise mathematical objects from our theories is moot.

Azzouni also recognizes a difference in kinds of posits despite accepting Quine's 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: 481). Again, if our mathematical beliefs are justified in ways independent of their applications in science, they are not subject to abandonment for empirical reasons.

If Sober's criticism of holism is correct, as I believe it is, then at least some elements of our best theory are separate and isolatable from others, depending on how and why we adopt them. 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: 135).

Resnik's hopelessness is no evidence for the impossibility of the task. 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, perhaps fewer. Mathematics demands more. Also, the types of geometric space with which mathematicians are concerned exceed even the most tutored intuitions and the most arcane physical theory. And it is easy to determine which objects are purely mathematical. They are the ones to which purely mathematical theories (e.g. ZFC set theory or the Dedekind-Peano postulates) refer.

Some philosophers like Resnik see a blurred line between mathematical objects and other posits of scientific theory. Certainly some posits are oddly unfamiliar and inaccessible to our senses: space-time points, quarks, the equator. Such objects seem intractable, like mathematical objects. Some philosophers even claim that objects can be complex, consisting of both concrete and mathematical components.[32] But these kinds of puzzles in no way detract from our ability to identify purely mathematical objects.

There are two independent points here. The first is that we can make a principled distinction between mathematical objects and empirical objects and thus between mathematics and empirical science. 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. Consider how mathematical objects get added to a theory on the holist's picture. We do not take them as explananda, as we do with our experiences of 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 for its formulation.

The case is different with empirical posits. When we introduce electrons, say, 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 sense experiences of them, as we do an electron trail in a cloud chamber. We are just forced to quantify over mathematical objects by the desire for greater facility in manipulating descriptions of physical situations.

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

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[32] See Katz 1998: Chapter 5.

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: 152).

Quine insists that all philosophical questions are to be answered using scientific methods. To hold that scientists must test mathematical statements as they test empirical ones is to favor a methodology based not on scientific principles but on prior philosophical prejudice. Mathematical theories are, as a matter of practical fact, tested differently from empirical ones. Of course, mathematicians have their own methods which might be called scientific, in a broad sense. But mathematical methods require proof rather than inductive support and conclusions do not depend on experimentation except in a loose and metaphorical sense.

The criticisms of holism in this section have a limited scope. I am merely interested in establishing that holism does not extend to mathematics even if it accurately describes the relations among empirical claims within scientific theories. Sober believes that his methodological objections extend to the ways in which the holist depicts confirmation within science.[33] I need not take such a contentious position. For my purposes, Quine's confirmation holism may hold within empirical science. Justification may well be spread throughout empirical theory. The web of belief, restricted to our empirical beliefs, may remain a useful metaphor.

Similar observations hold, perhaps even more saliently, within mathematics. The holism with mathematics is clear. Equivalent mathematical theories admit of varying alternative axiomatizations: compare Hilbert's modern axiomatization of Euclidean geometry to those of Birkhoff, Tarski, and Friedman.[34] Projects of reverse mathematics, seeking minimal axiomatizations, are newly vibrant areas of mathematical research precisely because there are so many different formulations of mathematical theories. Such projects proceed holistically within the discipline.

The extension of the holistic picture to wed mathematics and empirical science, though, is unjustified. Mathematical and empirical theories are independent. Their construction may proceed holistically within each theory without crossing disciplinary boundaries. QI1 tells us that we should believe our best scientific theory. It does not say that we shouldn't believe our best mathematical theory independently![35]

§3: Problems for the Quinean Method for Determining Ontological Commitment

The first two steps of QI involve settling on a single empirical-scientific theory in which to express our commitments. I have argued that our commitments are not all made in the same way by the same theory. Still, even if we grant QI1 and QI2, QI depends on Quine's general procedure for determining ontological commitments. Against Quine's method, I argue that we should not look to first-order versions of scientific theories 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 commitments. My criticisms apply to the use of first-order logic as canonical language, to the way in which Quine reads the commitments from a regimented theory, and to the invocation of formal languages as a tool for determining our commitments.

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[33] See Sober 1999 and Sober 2005.

[34] Harvey Friedman presents an alternative axiomatization based on equidistance on FOM.

[35] Some versions of Quine's indispensability argument invoke his naturalism to debar such beliefs. For example, see the 'only' clause in Colyvan's version at Colyvan 2001: 11.

Quine's argument that we should find the ontological commitments of theories in their existential quantifications has two distinct parts: an argument against the unclarity of natural language, especially the uses of names, and an argument against using higher-order languages as canonical.

§3.1: Names and Quantifiers

First, we should note that Quine's argument that the first-order existential quantifier is the best tool for indicating existence (since it is a cognate of the natural-language 'there is') is undermined by his rejection of names, which have common and useful roles in natural language. Against looking to names to find the commitments of a theory, Quine points to four problems.

PN1 Some names do not refer.
PN2 We often find reference in terms which do not look like names on the surface, in pronouns for example.
PN3 There are not enough names.
PN4 There is a profound conflict between names and quantifiers.

The problem of non-referring names, PN1, concerns terms like 'Pegasus' and '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, in natural or artificial languages.[36] Similarly, the problem of diffusion of reference, PN2, is not an argument for eliminating names, but an argument to be careful, in any language, when approaching questions of reference.

To the problem of not having enough names for all objects, PN3, we may respond by adopting an infinite language with enough names, by dropping the presumption that every object have a name, or by merely allowing infinitely long strings to serve as names. Quine's standard example for showing that natural language does not have enough names for objects is the real numbers. By Cantor's diagonal argument, any list of names of real numbers (e.g. in their decimal representations) is incomplete. Still, any decimal expression represents a real number and every real number can be written as a (possibly infinitely long) decimal (or binary or otherwise) expansion. Such representations of real numbers may be seen as sufficient even though there are non-denumerably many real numbers.

Given the incompatibility of names and quantifiers, PN4, Quine favors languages which eschew names. But avoiding names makes Quine's formal language less natural, since names are naturally taken as indicating reference and worries PN1-PN3 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. Quine's counsel amounts merely to advice to be careful with whichever language we choose. We have to ensure that the language of our best theory expresses the real commitments of that 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 be taken as indicating reference.

Quine's argument for taking the existential quantifier as indicating commitment is based on its natural equivalence with 'there is', but the argument from the naturalness of language is not categorical. Naturalness is an equivocal 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

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[36] Azzouni, for example, urges the introduction of a special predicate for formal languages to indicate when an existence assertion is to be taken literally. See, for example, Azzouni 1998.

'there is' is counterbalanced by the ease of using a familiar language with names.

We can adopt, for the purpose of revealing the commitments of a theory, a language less formal than first-order logic, a language with names and no quantifiers. In particular, we may use a cleaned-up version of our ordinary language. Indeed, our expressions of ontological commitment are generally formulated first in natural languages. We use these formulations as guides to understanding artificial formal languages.

Even if we must use a formal language as canonical, first-order logic is not the only option.

§3.2: First-Order Logic and Higher-Order Logics

Terms in quantificational logics contain two different syntactic places: for subjects and for predicates. These places may be filled by constants, ones which refer to particulars (either particular objects or particular predicates), or by variables which must be bound by a quantifier in order to form a closed term. In first-order logic, only the subject terms may include variables; we never quantify over a predicate position. In higher-order logics, we may quantify over predicate positions, essentially (if roughly) generalizing over properties.

Quine presents three concerns about higher-order logics.

HOL1 Higher-order logics make too many commitments.

HOL2 A constellation of technical results which hold for first-order logic fail in logics of higher-order: the concurrence of a variety of definitions of logical truth, completeness, that every consistent first-order theory has a model, compactness, and both upward and downward Löwenheim-Skolem features.

HOL3 Referential vagueness

Quine rejects higher-order logics in part for their ontological extravagance, HOL1, calling it, "Set theory in sheep's clothing" (Quine 1986c: 66). Consider a sentence of second-order logic, SP.

SP Some properties are shared by two chipmunks.
$(\exists X)(\exists x)(\exists y)(Cx \bullet Cy \bullet x \neq y \bullet Xx \bullet Xy)$

The value of the two lower-case variables are objects, chipmunks. The value of the upper-case variable 'X', though, is not an ordinary object. It is a property of an object. For SP to be true, there must exist two chipmunks and there must exist a property. By quantifying over properties, we take properties as kinds of objects; we need some thing to serve as the value of the variable, Platonic forms, perhaps, or eternal ideas. The second-order sentence reifies properties.

The least controversial way to understand properties is to take them extensionally, to be sets of the objects which have those properties. On an extensional interpretation, 'blueness' refers to the collection of all blue things; the taller-than relation is just the set of ordered pairs all of whose first element is taller than its second element. Thus, second-order logic, in its least-controversial interpretation, is some form of set theory.

Quine's view is that we can include sets in our ontology if we think that there are mathematical objects but that we need not include them under the guise of second-order logic. We can instead take them to be values of first-order variables. We can count them as among the objects in the universe, in the domain of quantification, rather than sneaking them in through the interpretations of second-order variables. Quine's complaints about second-order logic, that it is set theory in sheep's clothing, are based on this sneakiness.

Quantification over properties in higher-order logics, though, may be seen as a virtue, in contrast to Quine's view. It provides all the predicates we might need for science or mathematics. Moreover, we need not take quantifications of variables attached to those predicates as indicating untoward or unjustified commitments.

The quantifiers, whether of first-order or of 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, allowing us to distinguish universal and existential claims.

The translational role of the quantifiers involves the uses we make of the formal theory and the meanings we give to the quantifiers. It is common to take, with Quine, the existential quantifier as indicating existence. We need not do so. Instead, we can interpret them substitutionally, 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 uniquely 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: 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. Worries about excessive commitments of higher-order logics can thus be minimized by altering the way we read the commitments of a theory.

The technical virtues of first-order logic, HOL2, do not decide the matter, either. For example, the completeness which Quine believes favors first-order logic only entails 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. 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. Consider the claim EM1, a first-order expression of the logical law of the excluded middle.

EM1 $(\forall x)(Px \vee {\sim}Px)$

That a similar sentence can be written substituting any predicate for 'P' is a higher-order fact, which we can express as EM2

EM2 $(\forall \wp)(\forall x)(\wp x \vee {\sim}\wp x)$

Quine rejects EM2, though he accepts every instance of it like EM1, with any predicate in the place of 'P' (or given any interpretation of that predicate). He thus 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.

Moreover against the supposed adequacy of first-order logic, there are expressions whose first-order logical regimentations are awkward at best. Leibniz's law, that identical objects share all properties, seems to require a second-order formulation. Similarly, H resists first-order logical treatment.

H Some book by every author is referred to in some essay by every critic (Hintikka 1973: 345).

H may be adequately handled by branching quantifiers, which are not elements of first-order logic. The completeness of first-order logic is a technical virtue which can make first-order logic useful. Higher-order logics which can accommodate such inferences may also be useful.

There are other limitations on first-order logic. Regimenting a truth predicate in first-order logic leads naturally to the paradox of the liar. Propositional attitudes, like belief, create opaque contexts that prevent natural substitutions of identicals otherwise permitted by standard first-order inference rules. The technical virtues of first-order logic do not deflect these and other problems. Still, defenders of first-order logic have proposed a variety of solutions to these difficulties, some of which may not be due to first-order logic itself, but to deeper problems with language.

Quine complains that logics other than first-order logic may be referentially vague. "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 1986d: 534). 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. If reference is diffuse, then we can be explicit about which uses of terms are serious. In all cases, we must be clear, antecedently, about our commitments. But the concern about vagueness is a practical one and can be mitigated by a variety of techniques for explicit clarification in a variety of languages, including natural languages.

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 in serious theories as indications of ontological commitments. We may regiment into first-order logic to clarify our meanings when semantic ascent is useful or to reveal deductive relations. We may use other formal languages when they suit our purposes. Quine's reliance on first-order logic arises from overemphasizing its structural virtues. These characteristics do not justify choosing that language as the exclusive and somehow determinative way to reveal our commitments.

§3.3: The Regimentation of Commitment

The considerations of the previous two sections focus on the question of which language to choose as canonical. I have raised some general worries about Quine's particular choice of first-order languages: they have expressive limits and may not be properly tailored to the job of exclusively representing our ontological commitments. Now, I wish to consider some more general worries about Quine's procedure.

One reason to favor Quine's method is because the clarity of formal languages can help reveal the presuppositions of a theory and avoid our 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. Anyone who has invoked formal languages to reveal ambiguities in natural language knows that the virtues of its precision are compelling. Amb is ambiguous between an interpretation on which Duffin is an American who teaches history one on which Duffin is a teacher of American history.

Amb	Duffin is an American history teacher

Amb1 and Amb 2 can be used to express the first and second interpretations, respectively, and do not suffer the ambiguity of Amb on their intended interpretations.

Amb1	Ad • Tdh
Amb2	Tda

Similar examples abound.

Reasoning using a formal language could, in principle, affect our independent beliefs about what exists. Mathematicians could discover new and interesting theorems by generating inferences using a formal theory. But mathematical reasoning does not generally work this way. Regimentations are mainly used as a check on informal reasoning. Frege developed modern mathematical logic in pursuit of a gap-free system of inference which could secure the results of mathematics, not as a tool for producing those results. Of course, the resulting field of mathematical logic is itself a proper object of study. But our inherent interest in logic, in regards to other areas of mathematics, seems mainly for its relevance to justification and foundation and, perhaps, to see connections among different areas of mathematics. It is not itself a tool for discovery in, say, abstract algebra or topology.

The benefits of mathematical regimentation may translate as well to the mathematical portions of science. But it is even more unlikely, if not impossible, that writing science in a formal, canonical language would lead to any scientific advances. Indeed, the complexity which would result from translating scientific theories into formal languages debars such projects from being fruitful for discovery.

The Quinean view of how we determine our commitments, the method underlying QI, is thus misleading. 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. For example, consider Quine's rejection of propositional attitudes as "creatures of darkness" (Quine 1956: 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. But if we abandon the view that regimenting our scientific language into first-order logic is the source of our justification of our mathematical beliefs, the indispensability argument loses its force.

Quine recognizes the limitations of formal languages as tools for determining ontological commitments and accepts that they are best seen as ways of clearly expressing those commitments. "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 1960: 243).

The point here should be fairly obvious: translating ordinary language into regimented form can aid clarity, but the regimented language is not 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. Let us remember that one of the foremost developers of logic understood that formal languages were not canonical, but merely useful for certain purposes.

> I believe I can make the relationship of my *Begriffsschrift* to ordinary language clearest if I compare it to that of the microscope to the eye. The latter, due to the range of its applicability, due to the flexibility with which it is able to adapt to the most diverse circumstances, has a great superiority over the microscope. Considered as an optical instrument, it admittedly reveals many imperfections, which usually remain unnoticed only because of its intimate connection with mental life. But as soon as scientific purposes place great demands on sharpness of resolution, the eye turns out to be inadequate. The microscope, on the other hand, is perfectly suited for such purposes... (Frege, Preface to *Begriffsschrift*)

QI overextends our use of formal languages, especially in metaphysics. 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 accepted mathematical theorems prior to formalization or regimentation. We do not merely examine the domain of quantification of the regimented theory and discover them there. We construct formal theory knowing the references of the terms of the theory. Our ontology is a constraint on regimentation, not a result of it.

As a final concern about Quine's method of using formal languages to express our ontological commitments, consider the last stage of the process of determining the commitments of a theory. After formulating a preferred theory and regimenting it in our canonical language of first-order logic, we are supposed to examine the models of our theory to determine its commitments. This last step is the essence of Quine's slogan that to be is to be the value of a variable.

Quine defended taking first-order logic as canonical not only because it is refined and precise, but also because it is also easily interpreted. But he further denies that we should interpret ordinary language at surface value. Languages in which quantifiers have been translated away, or which do not contain quantifiers, are unable, he believes, to generate an ontology. For example, 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: 63).

The problem Quine raises here applies to all languages. A theory can not prescribe its own interpretation. We can only know the references of the names of a language lacking quantifiers by indicating an interpretation. Similarly, in a first-order theory, one must look to a domain of quantification to find values of its variables. Ontology is metatheoretic work.

These appeals to metalanguages generate infinite regresses 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 ontological 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: 50).

Ontological relativity weakens QI. Instead of justifying our mathematical beliefs, it justifies beliefs in a theory which may or may not be interpreted as making such commitments. The theory will have other interpretations, given that first-order theories of the sort a Quinean prefers admit of non-standard models.

To avoid ontological relativity, we could take the metalanguage of our first-order theory at homophonic face value: read it directly and naturally. But this pragmatic response is available for the original theory, too, 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: 50). Indeed, the direct and natural reading is available for natural languages without appealing to formal regimentation at all.

We have lots of ways to express commitments and to make statements whose commitments we deny. We can clarify matters at the level of the object language when pressed. And we need not be pushed into a metalanguage, whether our object language is natural or formal. Quine 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 (e.g. its perspicuity) natural language is much simpler.

Quine admits 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 1960: 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 ontological questions, for Quine, we have to look at the fully formal framework.

Quine's appeals to first-order logic as a canonical language produce a natural tension. On the one side, they are essential support for QI: without the demand that we find the commitments of a theory in its first-order regimentation by examining the domains of models of the theory, those who resist the conclusion of QI have little reason to admit that the uses of mathematics in science must be taken seriously; I will say more about this claim in the next chapter. On the other side, it is difficult to believe that any ontological questions can be settled by appeals to models of regimented theories. Our commitments arise prior to regimentation.

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

§4: Whither QI?

QI relies on two factors about which I have raised doubts: confirmation holism and Quine's method for determining the ontological commitments of a theory, especially his preference for a canonical language of first-order logic. Quine's holism, while perhaps applicable within empirical science, has its limits. Evidence from empirical theories does not extend to mathematical ones. And we are free to construct and interpret our formal theories as we wish. We can adopt semantic ascent for clarification without also believing that regimentation somehow forces our commitments. Ontology need not recapitulate philology. Moreover, naturalism does not unambiguously favor the indispensability argument by ruling out autonomous justifications for mathematical beliefs.

To my concerns about holism, the Quinean may naturally respond by digging in her heels: what appears to be a difference in kind (between mathematics and empirical science) is really just a difference in degree. I believe the burden of proof is on the holist to provide evidence of cases in which we cede mathematical beliefs in the face of recalcitrant empirical evidence. Given standard mathematical and scientific practice, such evidence is unlikely to be forthcoming. When we find that mathematical theories fail to apply in empirical science, as when space-time turned out to be hyperbolic rather than flat, we do not cede our mathematical theories.

In response to the problems of appeals to first-order logic in QI, the Quinean may drop references to a canonical language. Instead, the Quinean may just argue from the preponderance of evidence: mathematics is so widely used in science that it's preposterous to think that science isn't up to its ears in mathematical commitments. Such a move underlies the work of the most prolific contemporary Quinean indispensabilist, Mark Colyvan. In the next chapter, I will show that giving up QI3, appeals to Quine's specific procedure for determining ontological commitments, fully undermines QI. Despite my complaints in this chapter, the strongest version of the indispensability argument is one, like QI, which specifies how we determine our commitments. It is thus incumbent on the proponent of the argument to defend Quine's method or a reasonable alternative.

Chapter Four: The Weasel

In the previous chapter, I discussed some concerns about Quine's holism and about his method for determining ontological commitments. These criticisms are general, applying to any use of Quine's method. They do not presuppose any view of the indispensability argument or of mathematics. They are available to platonists and anti-platonists alike.

But the relevance of those concerns in this book is to the indispensability argument. In particular, the lesson we should learn from these criticisms is that, first, where confirmation holism may or may not properly characterize the relationships among claims within empirical science, it does not extend evidence from propositions of empirical science to the claims of formal sciences like mathematics. The indispensability argument, deprived of holism, is implausible.

Second, while Quine's method for determining the ontological commitments of a theory is useful, it should not be seen as somehow forcing us to believe in the existence of mathematical objects on the basis of their utility in scientific theory. The freedom we have to construct and interpret our best theories limits the force of QI. QI is supposed to push the avowed nominalist into a commitment to abstract objects, but its leveraging force is illusory.

Recently, some philosophers have argued to the same end, that QI does not have the force that its proponents believe it to have, whether or not QI4 holds (i.e. whether or not attractive first-order formulations of scientific theories are available). Philosophers including Joseph Melia and Mary Leng deny QI3, Quine's claim that the commitments of a theory are to be found in its existential quantifications. These recent critics of the indispensability argument claim that we can believe a theory or portions of a theory without believing in the existence of all of the objects over which it existentially quantifies. In particular, they argue that our beliefs in the empirical portions of a theory need not extend to the purely mathematical claims of that theory.

The view that we can baldly deny the mathematical portions of our empirical theories has come to be known as weaseling or easy-road nominalism.[37] Weaseling may be used as a defense of nominalism against QI. Since my focus in this book is on the contrast between indispensability platonism and autonomy platonism, my interest in weaseling, as an anti-platonist response to the indispensability argument, is limited. Insofar as weaseling is used to support fictionalism, it is relevantly like the fictionalist or reinterpretation strategies with which I am not concerned in this book.

But the weasel preys on a weakness of some formulations of the indispensability argument which is worth examining and understanding for the autonomy platonist. In this chapter, I show that QI withstands the weaseling response, but that other, more recent versions of the argument do not. More importantly, the autonomy platonist can adopt the weaseling strategy while withstanding a Quinean response to the weasel unavailable to the nominalist.

Let's start by considering an early precedent for recent weaseling.

§1: The Original Weasel

The view that we need not believe all the claims of a theory which we generally do believe, and in particular that we need not believe the mathematical claims of an empirical theory, traces back at least as far as Carnap's work.

> A physicist who is suspicious of abstract entities may perhaps try to declare a certain part of the language of physics as uninterpreted and uninterpretable, that part which refers to real numbers as space-time coordinates or as values of physical magnitudes, to functions, limits, etc. More probably he will just speak about all these things like anybody else but with an uneasy

[37] Weaseling strategies can also be found in the work of Azzouni, Balaguer, Maddy, Mortensen, and Yablo. See Colyvan 2010: 2. Patrick Dieveney calls it Separatism; see Dieveney 2007: 113 et seq.

> conscience, like a man who in his everyday life does with qualms many things which are not in accord with the high moral principles he professes on Sundays (Carnap 1950: 205).

Carnap defended the physicist's uses of mathematics by distinguishing between contentful internal questions and contentless external questions. Internally, it is analytic and obvious that there are numbers. One we adopt mathematical theories, the existence of mathematical objects follows directly. We can, of course, question whether or not to adopt mathematical theories, but such questions are not internal; they are not answered by examining the claims within the theory. Instead, they are external questions about whether to adopt a theory.

From the external perspective, the question of the existence of mathematical objects is, according to Carnap, meaningless. We might have pragmatic reasons to adopt mathematical language, but such reasons are not properly scientific. Thus Carnap eases the guilty conscience of the scientist by allowing her to speak in two distinct modes. We can choose to adopt mathematical language, in which case we assert the existence of mathematical objects, or we can choose to reject mathematical language, in which case we do not. The answers to the internal question is easy and obvious. The answer to the external questions is just pragmatic and no indication of any facts about the world.

As we saw in Chapter Two, Quine responded to Carnap's distinction by accusing his mentor of double-talk: we can not on pain of contradiction speak equivocally. If we believe a theory, we must believe in its commitments, from the most natural to the most artificial. There is no internal/external distinction and no separation of questions about our choices of language and questions about what uses of that language imply.

Field and other dispensabilists agree with Quine that we can not just deny some portions of a theory which we otherwise esteem. We must remove the mathematics from science if we hope to ease consciences regarding abstract objects. Underlying Quine's position and that of his dispensabilist critics are both his naturalism and his holism which support his denigration of double-talk. There is no external perspective from which to stand to evaluate linguistic frameworks. Evidence for any part of a theory is evidence for every part of the theory.

Other philosophers have attempted to revive Carnap's view against the indispensabilist. Mark Balaguer, anticipating even more recent work, argues that fictionalists about mathematics can adopt a position on which mathematics merely provides a theoretical apparatus for empirical scientists to make claims about the physical world; our mathematical assertions can be seen as different in kind from other assertions of a theory.[38] The two most forceful recent proponents of weaseling are Melia and Leng and I will focus on their defenses of the strategy.

§2: Melia's Weaseling Strategy

Melia attempts to reclaim Carnap's attitude toward the uses of mathematics in science without his more general view concerning linguistic frameworks. He claims that the proper interpretation of scientific theories which include mathematical theorems does not take the mathematical references seriously. We should, rather, interpret mathematicians and scientists as taking back all *prima facie* commitments to abstracta. "It is quite common for both scientists and mathematicians to think that their everyday, working theories are only partially true" (Melia 2000: 457).

Melia's weasel accepts that mathematical claims are ineliminable from scientific theory but maintains that we need not believe that such claims are true or that mathematical objects exist. The weasel can accept, say, that vectors in Hilbert space are indispensable to the practice of quantum mechanics. The weasel just adds that we can, when speaking most seriously and parsimoniously, deny

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[38] See Balaguer 1998: Chapter 7, §3.3.

that our best theory really posits them. Thus, the weasel denies QI3, Quine's claim that our ontological commitments are (all of) those objects over which we first-order quantify in our best theories.

Melia defends the weaseling strategy by claiming, as Balaguer did, that scientists use mathematics merely to represent or express facts that are not representable without mathematics. Mathematics, for Melia, is just a tool for communicating or representing non-mathematical claims. When constructing theories of the physical world, it is sometimes necessary for us to invoke mathematics. We should not be misled by such invocations into beliefs in mathematical objects. Such representations are not supposed to be ontologically serious. "The mathematics is the necessary scaffolding upon which the bridge must be built. But once the bridge has been built, the scaffolding can be removed" (Melia 2000: 469).

Melia begins by noting that we can easily reformulate theories to avoid quantification over mathematical objects if we ignore constraints on the languages used in the resultant theories. Such reformulations may not result in theories that are as elegant or compelling as the originals. For example, consider a trivial rewriting: just take standard scientific theories and eliminate any sentence which claims that there are mathematical objects. Such a theory would be awkward and gerrymandered. It would not be elegantly axiomatized. But it would not quantify over mathematical objects.

To make the theory which results from a trivial reformulation slightly less awkward, we could use a Craigian re-axiomatization to show that the new theory is recursively axiomatizable.[39] Field considered and rejected this strategy for eliminating mathematics for science. Such theories, even in their Craigian reformulations, are unattractive. They ignore the structure of standard scientific theories. Still, the debatable ugliness of the resultant theory is irrelevant to the question of whether we can eliminate mathematical references from standard science; we can.

> Even if he is right, in a very short time we have come a long way from the view that quantification over abstracta is *indispensable*. Quantification over abstracta can be dispensed with - and easily dispensed with at that - but the theories which do quantify over abstracta are more attractive than the theories which don't. This is a considerably weaker claim and one much more vulnerable to a nominalist assault (Melia 2000: 458).

Melia's own reasons for rejecting such trivial strategies of nominalization depend on a purported counter-example to the trivial strategy. Melia constructs a simple mereological theory T and shows that extending T by adding set theory to it, even (he claims) adding it conservatively, allows us to derive further nominalistically-acceptable consequences. Melia's construction uses model theory, and the cleavage between model theory and proof theory to construct the result. Melia claims that his construction shows that we can not merely remove the mathematics from physical theories without losing inferential strength.

> There can be more to the nominalist consequences of a theory than the set of sentences entailed by that theory in the nominalist vocabulary. If the nominalist simply takes his theory to be the set of nominalistically acceptable sentences entailed by some platonist theory, he has no guarantee that his theory actually has the same nominalist content as the platonist theory (Melia 2000: 461).

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[39] Craig's reaxiomatization theorem says that any recursively enumerable set of well-formed formulas of a first-order language is recursively axiomatizable. Thus, we can delete particular sentences from any (elegantly formulated) first-order scientific theory (say, the ones with existential quantifications over mathematical objects) and re-axiomatize the remaining (awkward) theory recursively.

At their strongest, mereological theories, the logics of parts are wholes, are equivalent to second-order logic and its full theory of sets. At their weakest, they are tempting to nominalists. The relations of parts to wholes is less controversial than the theory of sets. But, those relations aren't quite as uncontroversial, or purely logical, as the theory of identity, which is ordinarily included among first-order logic. Thus, mereology is somewhere between pure logic and full mathematics.

Melia's mereological theories are thus not first-order logical. They include claims about combinations of regions among their logic and include variables that range over regions and their mereological sums. Mereology contains significant mathematical ontology and ideology, built-in.

Again, this is not the place to evaluate competing strategies for eliminating mathematics from scientific theories.[40] But it is useful to notice the strategy used by Melia since it illustrates, specifically, the importance of QI3, Quine's choice of a canonical language, in the indispensability argument. It is fairly easy to invoke alternatives to first-order logic to rid a first-order theory of its quantifications over mathematical objects without anything like a Field-style reformulation of science.[41] For example, Burgess and Rosen sketch a rewrite which uses predicate functors to replace quantification. Appeal to such theories is laughable as a way to avoid mathematical commitments. The change in logic obscures the theory's commitments. Without settling carefully on how we are to determine the ontological commitments of a theory, without agreeing on a canonical language, we can construct theories which seem to make no mathematical commitments but which are really just obfuscating.

Still, Melia's goal is to demonstrate the failure of this trivial strategy in order to introduce an even-more trivial strategy. His neo-Carnapian weaseling strategy consists of a simple denial of the existence of mathematical objects, whatever the quantifications of whatever theory. He posits a hypothetical nominalist who prefers standard theories, with their mathematical axioms, because of their inferential strength, but who also rejects the mathematics used in those standard theories.

> Joe does *not* simultaneously hold contradictory beliefs. Just because, in the process of telling us his beliefs about the world, Joe asserts all the sentences of T* [the mereological theory which includes mathematical axioms], it does not follow that Joe believes all the sentences of T*. Indeed, since Joe believes there are no abstract objects, he will explicitly say that T* is false (Melia 2000: 467).
>
> Compare Melia's view to that of an earlier philosopher.
>
> Some contemporary nominalists label the admission of variables of abstract types as "Platonism". This is, to say the least, an extremely misleading terminology. It leads to the *absurd consequence*, that the position of everybody who accepts the language of physics with its real number variables (as a language of communication, not merely as a calculus) would be called Platonistic, even if he is a strict empiricist who rejects Platonic metaphysics (Carnap 1950: 215, emphasis added).

To support his revival of Carnap's view, Melia provides a helpful analogy for those, like Quine and Field, who believe that weaseling is double-talk. Consider the two-dimensional surface of a sphere. From a three-dimensional perspective, we can, simply and elegantly, describe the surface as the locus of

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[40] See Daly and Langford 2010 and Melia 2010 for the continuing debate over whether Melia's counter-example to the trivial strategy succeeds.

[41] See Burgess and Rosen 1997: 186-7.

all points equidistant from the center of the sphere. In order to describe the spherical surface, we appeal to the center point of the sphere and its three-dimensional properties. But the center is not part of the two-dimensional surface. From the point of view of the surface of the sphere, we can appeal to the center despite its not being part of the world. "We do successfully and unproblematically describe a particular non-Euclidean world by taking back some of the implications of what we earlier said" (Melia 2000: 468).

Similarly, consider Melia's story about angels and stars, his riff on a story of Putnam's.[42] "In charge of each star is an angel, no two angels are in charge of the same star, and at the precise moment that each star is created the corresponding angel is also created. Moreover, the angels in charge of stars a and b were created at the very same time" (Melia 2000: 470).

From the story of the angels, we can infer that two stars are created at the same time, even though the story never says so. Melia's weasel, who tells the story, is a nominalist about angels so he retracts all the consequences about them. But he can continue to hold the inference about the stars, which he believes exists, seeing the talk of angels as merely instrumental.

In Melia's angels example, we can easily eliminate references to the undesirable objects by saying directly that there are two stars created at one time. Melia's primary claim, though, is that in stating that claim in terms of angels, and then taking back the parts of the claim that refer to angels, one is not speaking incoherently or in a contradictory manner. One is speaking poetically or metaphorically but not inconsistently as long as one is clear about one's real commitments, weaseling away the non-literal portions of one's speech.

Melia claims that our ability to weasel is fortunate since nominalistic reformulations of our best theories are not always available. That's why reliance on the less-trivial strategy of reformulating one's theory to eliminate all insincere references is unacceptable for the committed nominalist. "Sometimes, we just cannot say what we want to say first time round. Sometimes, in order to communicate our picture of the world, we *have* to take back or modify part of what we said before" (Melia 2000: 468-9).

Melia really makes two distinct claims. First, the indispensabilist's invocation of aesthetic criteria in developing our scientific theories entails that arguments like QI are weaker than they appear to be. The attractiveness of a theory can be evaluated in different ways, with different outcomes, especially regarding mathematical references.

Second, in part because of the looseness introduced by appeals to attractiveness, the Carnapian denial of some portion of a theory we otherwise believe (i.e. the mathematical portion) is not inconsistent. Properly understood, the content of our scientific theories does not include mathematics.

Regarding the first claim, Melia erroneously infers that the indispensabilist has a choice whether to believe in the existence of all the objects over which we first-order quantify from the fact that we use aesthetic and pragmatic considerations in choosing among theories. As I showed in Chapter Two, the looseness of our choice of theory does not entail, for the Quinean, that there is any looseness in the choice of whether to believe in the objects which that best theory posits. For the indispensabilist, there is no perspective external to scientific theory from which to choose among various formulations of a theory. The indispensabilist engages the important question of which language to use as canonical independent of which theories are best. Once our language is chosen and our best theory is selected, we have no choice about its yield.

But Melia's mistaken inference is independent of the plausibility of his second claim, that weaseling is not self-contradictory. It is also independent of the high plausibility of its premise, that aesthetic criteria are relevant to our choices of which theories to believe.

We are often faced with competing theories. Sometimes these theories are empirically equivalent: no evidence favors one over another. In such cases, we may suppose ourselves to be like

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[42] See Putnam 1975a: 74-5.

Buridan's ass between the two theories, unable to choose. Mere counsel to favor simplicity lacks categoricity. Quine's method directs us to consider the immanent virtues of the theories such as simplicity and elegance. Some theories are simpler ontologically. Some theories are simpler in formulation. These factors of simplicity are in many cases inversely proportional: the simpler the ontology the more complex the formulation (and vice-versa).

Melia claims that we should prefer ontological simplicity over formulational simplicity. He compares a theory he calls T_1, which has no numbers (in object positions) but lots of numerical predicates ('is six cm long', 'weighs eight pounds', etc.), to another theory, T_2, which accomplishes the same tasks with few predicates and an arithmetic ontology.

> I accept that considerations of simplicity play an important role in theory choice. But I prefer the hypothesis that makes *the world* a simpler place. For sure, all else being equal, I prefer the simpler ontology. For sure, all else being equal, I prefer the theory that postulates the least number of fundamental properties and relations. But the simplicity I value attaches to the kind of world postulated by the theory - not to the *formulation* of the theory itself (Melia 2000: 473).

Quine, with his stated Okhamist preference for desert landscapes would agree with Melia's preference for the ontologically simpler theory, all else being equal. But, it does not seem to the indispensabilist as if all else is equal in this case. For Melia's T_1, every distance relation is a different property. For T_2, the same predicate may be used for any distance since we can use mathematical objects to differentiate measurements.

We can, it is true, reduce ontology (values of the variables) at the expense of ideology (predicates). Thus, one must be very careful to choose a proper language. As Quine says, our choices of language are not distinguishable from our choices of what we say is the fundamental nature of the world. "The quest of a simplest, clearest overall pattern of canonical notation is not to be distinguished from a quest of ultimate categories, a limning of the most general traits of reality" (Quine 1960: 161).

Quine and Melia agree that parsimony is important in choosing a theory. But Melia urges that a reduced ontology is more important than elegance of formulation where Quine believes that the two factors can be weighed against each other. Still, it is impossible to formulate precise guidelines for when and how to adopt a mathematical ontology in order to achieve gains in elegance.

I'll turn to another approach to adjudicating the debate between the indispensabilist and the weasel after looking at a related weaseling proposal from Mary Leng.

§3: Leng's Recreational Weasel

Like Melia, Leng defends a weaseling version of mathematical fictionalism against the indispensability argument. Relying on the work of Maddy and Sober describing the relationship of scientific practice and attitudes toward mathematics, criticisms of the extension of holism to this relationship which we saw in Chapter Three, Leng presents three characteristics of mathematics with regard to science, which I will call insularity, Euclidean rescue, and bridging.

> [Insularity] 1. Mathematics is insulated from scientific discoveries, in the sense that the falsification of a scientific theory that uses some mathematics never counts as falsification of that mathematics (beyond simple cases of calculation error).
>
> [Euclidean Rescue] 2. In particular, a scientific observation that conflicts with some scientific theory may suggest a move to a different background mathematics, but does not suggest that mathematicians should abandon that mathematics...The success of a scientific theory does not confirm the mathematics used in that theory.

[Bridging] 3. What does seem to be disconfirmed by the failure of a scientific theory that relies strongly on a background mathematics is the claim that this mathematics is applicable to the scientific phenomena that it has been used to describe (Leng 2002: 411).

As we saw in Chapter Three, Sober's argument against Quine's holism derives from the observation that in practice we never choose to give up the logical or mathematical principles. This practical fact is not contentious. Quine explains that fact as the consequence of a methodological principle of theory choice which he calls the maxim of minimum mutilation: we hold logical and mathematical principles fixed in order to do as little damage as possible to other portions of our theory. Leng, following Sober, takes the fact that we never give up our mathematics in light of recalcitrant data to indicate that mathematics is insulated from science at the most basic level.

The term 'Euclidean rescue' comes from the work of Michael Resnik. Before the nineteenth century, people generally thought that there were only one geometry. When it became clear that there were consistent, non-Euclidean spaces, some philosophers and mathematicians denigrated them as somehow lesser theories. Such people sometimes thought of non-Euclidean geometries as consistent but false or uninterpreted. When it became clear that physical space is non-Euclidean, in the early twentieth century when relativity supplanted classical mechanics and flat Euclidean geometry was replaced by a curved, hyperbolic space-time, some philosophers inferred that hyperbolic geometry is true and Euclidean geometry is false or uninterpreted. In retrospect, both claims to prefer on geometry over others appear un-mathematical. Neither has been vindicated. From the mathematician's perspective, there is no reason to prefer one or the other geometry.

We perform a Euclidean rescue when we resist distinguishing among different geometric theories in terms of their applicability and we accept all three consistent geometries that result from the three different parallel postulates as equally true. In this paradigmatic case, the Euclidean rescue entails that the discovery of which geometry is actually applied in our space is irrelevant to the truth of the mathematical theory.

> It is...difficult to maintain that the empirical discoveries confirmed the truth of non-Euclidean geometry and showed the falsity of Euclidean geometry in any sense other than that one was a correct model of the physical world and the other was not. But this is not *mathematical* truth: the applicability of non-Euclidean geometry did not falsify any mathematical theorems in Euclidean geometry - the Pythagorean theorem still holds for Euclidean triangles - it merely confirmed the assumption of Gauss and others that the scope of the theorems of Euclidean geometry only covers systems that assume the parallel axiom (Leng 2002: 402).

Euclidean rescues are not limited to this one case. They are available in all cases in which a mathematical theory is shown inapplicable in science. Leng cites the case of catastrophe theory. Initially, the mathematical theory called catastrophe theory was thought to have profound implications for physical science. Later, it was seen not to apply as broadly as was initially thought. Still, the mathematics itself was not impugned.

Euclidean rescues are related to insularity in that if one thinks that mathematics is insular, then one is predisposed to perform Euclidean rescues. One might perform Euclidean rescues because one thinks that mathematics is and should be held insular.

Similarly, Euclidean rescues are supported by Bridging. For a mathematical theory to be used in a physical theory, there must be bridge principles which map some mathematical claims into some physical claims. When one finds an inconsistency in one's physical theory, one can always restore consistency without falsifying one's mathematical claims by denying just the bridge principles and not the actual mathematical theorems. By denying only bridge principles, we perform a Euclidean rescue, holding mathematics to be insulated from falsification of an empirical theory. In Leng's case of catastrophe theory, only the bridge principles, the claims of its applicability to physics, were denied. "Catastrophe Theory became a much less popular area of research, but no one would claim that the mathematics of Catastrophe theory had been *falsified* by its magnificent scientific failures" (Leng 2002: 407).

Leng's fictionalist proposal rests both on her exploitation of the tension in Quinean naturalism, which we saw in §3.1, and these three characteristics of the relation between mathematical practice and science. It also relies on Quine's attitude toward the mathematics which is not used in empirical science. Quine calls such unapplied mathematics recreational.

> My view of pure mathematics is oriented strictly to application in empirical science...Pure mathematics extravagantly exceeds the needs of application...but I see these excesses as a simplistic matter of rounding out...I recognize indenumerable infinites 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 1986a: 400).

Strictly speaking, the indispensabilist has no justification for beliefs in mathematical claims that are not used in physical theory. There is some room for extending our beliefs in the disjoint pieces of mathematics that are applied in science to full mathematical theories. There is no room for beliefs in portions of mathematics which have no relation to our empirical science. Quine and the fictionalist agree that such sub-theories of mathematics are false or merely vacuously true. Mark Colyvan follows Quine in appealing to a fictionalist interpretation of the un-applied portions of mathematics.[43]

Leng's proposal is simply to extend the fictionalist attitude to all of mathematics. From the characteristics of Insularity, Euclidean Rescue, and Bridging, Leng concludes that mathematics plays merely a modeling role in science. Mathematical theories lack any serious ontological rights because they are used merely as models, without any presumption that they are true or refer to real objects.

> When we use mathematics to model physical situations in this way, we never refer to mathematical objects or assume the (mathematical) truth of their relations. Rather, we interpret our mathematical stories physically and assume that our model is good enough in the relevant respects that the theorems derived in our mathematical recreations, when transcribed into physical language, will give us truths about the physical phenomena we are considering...If Colyvan is right (and I think he is) that mathematics that is not assumed by science to be true should be seen as recreational (and given some important status as such), then it follows from the modeling picture of the relationship between mathematics and science that *all* mathematics is recreational (Leng 2002: 411-412).

Later, Leng discusses the modeling aspect of mathematical objects in terms of their ability to represent physical objects.

> We are not committed to belief in the existence of objects posited by our scientific theories *if their role in those theories is merely to represent configurations of physical objects*. Fictional objects can represent just as well as real objects can (Leng 2005: 179).

[43] See Colyvan 1998: 56.

Where Quine claimed that un-applied portions of mathematics could be considered recreational, Leng argues that all of mathematics should have that status. There is no mathematical reason to distinguish between applied and un-applied results in mathematics and the work of mathematicians and scientists need not entail our beliefs in the truth of mathematical claims. Like Melia's weasel, Leng's defense of recreation revives the Carnapian double-talk of denying that our beliefs in the empirical portions of a theory which includes mathematical axioms extends to the mathematical claims of that theory.

§4: The Weasel at Work

We have seen two different kinds of support for the revival of Carnapian weaseling against the indispensability argument. Melia argues that denials of aspects of our assertions (or portions of a theory) are ordinary ways in which we take back some unacceptable portion of a claim (or theory) which is only constructible with the use of that unacceptable portion. Leng argues that the indispensabilist's understanding of unapplied mathematics as recreational or ontologically unserious, is best extended to all of mathematics. Both of these claims are mere denials of QI3, Quine's claim that the ontological commitments of a theory are to be found in all and only its existential quantifications.

Quine, responding to Carnap and anticipating the neo-Carnapian strategy, attempted to block such weaseling with his double-talk argument, as we saw in §2.3. Where Melia claims that double-talk is not inconsistent, Quine insists that the surface contradiction is not to be dismissed as a mere way of speaking. Where Leng invokes the limitations of the indispensability argument to extend to un-applied portions of mathematics, Quine insists that only the unapplied portions are recreational; our commitments to the mathematical theorems used in science and the objects to which they refer is unequivocal. How are we to decide in favor of either Quine or the weasel?

In the next section, I argue that Quine's double-talk argument suffices to reject the weaseling strategy when used against QI. But the weasel has some teeth and is effective against a wide variety of indispensability arguments, ones which abandon QI3 or otherwise fail to specify (in a clear and unequivocal way) how we are supposed to determine the ontological commitments of our theories. The feature of QI which distinguishes it from other versions of the indispensability argument is Quine's insistence on specifying the details of how and when we are to be taken as speaking most seriously about our ontological commitments. These details arise out of a combination of his holism and his naturalism, as well as his methods for determining and representing our ontological commitments. The broad way in which the indispensability argument is sometimes represented masks these central claims.

As an example of an indispensability argument which may succumb to weaseling, consider Colyvan's version.

CIA
1. We ought to have ontological commitment to all and only those entities that are indispensable to our best scientific theories.
2. Mathematical entities are indispensable to our best scientific theories.
Therefore:
3. We ought to have ontological commitment to mathematical entities (Colyvan 2001: 11).

Colyvan's first premise does not answer the question of when an entity is indispensable to our best theory: How does a theory make its posits? How do we read those posits? Are all the posits made in the same way, with the same force? By suppressing Quine's criterion for ontological commitment, Colyvan's argument is liable to weaseling criticisms based on these questions.

For example, a nominalist can respond to CIA by accepting that, say, vectors in Hilbert space are indispensable to the practice of quantum mechanics, indeed that we quantify over such vectors in our best theories, but adding that we can, when speaking most seriously and parsimoniously, deny that our best theory really posits them. Indeed, Melia's claim that one can weasel out of the indispensability argument, accepting that mathematics is ineliminable from scientific theory but maintaining that we need not believe that mathematical objects exist, is precisely an effective response to CIA.

Colyvan argues that any easy-road strategy actually presumes a hard-road strategy behind it. If we lack a way of showing how to eliminate references to the unwanted entities in a story, the story loses its sense. We couldn't understand *The Lord of the Rings* without the hobbits. It is misleading sloppiness to assert a theory which includes poetic metaphor and not indicate precisely where an account is to be taken as literal.

> We can change the story we are narrating by adding or subtracting minor details, but we can hardly be thought to be telling a consistent story (or in some cases, any story at all) if we take back too much. In short, there are limits to how much weaseling can be tolerated...I simply do not know what to make of sentences such as [There exists a differentiable function that maps from the space-time manifold to the real numbers, but there are no mathematical objects] where no obvious paraphrase presents itself (Colyvan 2010: 10-11).

Colyvan's objection to the weaseling strategy is indefensibly strong. *Pace* Colyvan, it is clear what the weasel wants to say: the references to mathematical objects within our scientific theories are to be taken as merely instrumental, not serious, and the uses of mathematics in science should not give us a reason to believe that mathematical objects exist. Colyvan's denial that he can make sense of the weasel's claim can not be taken seriously.

The underlying and more important objection to weaseling is the one that Quine already makes to Carnap: the double-talk objection. Indeed, CIA includes an implicit appeal to Quine's argument in its use of 'all' in its first premise: We ought to have ontological commitment to *all* and only those entities that are indispensable to our best scientific theories. The weasel can deny the first premise by pointing out that some entities over which our theories naturally quantify may be reasonably taken to be instrumental posits. Indeed, as Batterman 2003 argues, asymptotic reasoning which involves idealizing is essential to much of science. The indispensabilist must respond by defending the premise. Claiming that the weasel's claims are nonsense is insufficient.

The weasel can accept Colyvan's first premise by arguing that even if mathematical axioms are essential to our formulations of our best scientific theories, our commitments to the objects to which those axioms (and the theorems which follow from them) refer are not to be seen as indispensable to those theories. The weasel claims that even ineliminable uses of mathematical sentences should not be taken as grounds for belief in those sentences, taken literally. Yes, says the weasel, we should have ontological commitment to all and only those entities that are indispensable to our best scientific theories. But mathematical entities are not indispensable to our best theories because those theories should not be taken as indicating serious commitments to mathematical objects.

Indeed, Azzouni, whose work Colyvan also characterizes as weaseling, makes exactly this claim.[44] He argues that we can distinguish among our commitments to a theory's posits. We have observational access to thick posits (e.g. apples, trees) and excuses for not being able to observe thin posits, like objects outside of our light cone. So, we should believe in the thick and thin posits of a theory. In contrast, Azzouni claims that we should not believe in the existence of posits, like

[44] Azzouni distinguishes his claims from those of Melia and other weasels. He claims to deny Quine's method where Melia and others like Yablo merely evade it. See Azzouni 2012: §1. This difference is not relevant here.

mathematical objects or centers of mass, which we can call very thin, the result of mere casual talk (Azzouni 2004: Chapter 6).

Colyvan responds to Azzouni's weaseling with a demand for criteria to distinguish the serious from the casual posits. "Whether [mathematical objects] are thin or very thin depends on what can count as an excuse for not being accessed thickly... Unfortunately Azzouni doesn't give us any guidance; he offers no systematic story about acceptable excuse clauses" (Colyvan 2010: 7). In order to adjudicate between weasels and indispensabilists we need a general account of when we are speaking seriously.

§5: Speaking Seriously in Mathematics

The weasel preys on a weakness in indispensability arguments which do not insist explicitly on a method for determining the commitments of a theory. To defend itself from weaseling attacks, the indispensabilist must make a clear and explicit case about when and where to find one's ontological commitments, about when we are taken to speak most seriously.

To illustrate the problem a bit more carefully, consider that the autonomy platonist believes that we are speaking most seriously in pure mathematics. Mathematicians say things like, "There are infinitely many primes." When we take such claims at face value, the inference to the existence of mathematical objects is manifest. Call the direct inference from pure mathematical sentences to the existence of mathematical objects the mathematical-practice argument.

Alan Baker, calling deference to the practice of mathematicians strong mathematical naturalism, observes that such deference renders the indispensability argument moot (Baker 2003: 63-4). The proponent of the traditional indispensability argument engages the difficult question whether mathematical objects are required in our best scientific theories. But mathematical objects are obviously required for face-value interpretations of mathematical theories. The defender of the mathematical-practice argument has no need to wonder whether uses of mathematics are eliminable from our scientific theories or explanations or practices.

The central problem with the mathematical-practice argument is that we require justification for taking mathematical practice itself, the claims of pure mathematics, seriously. Even if we accept mathematical practice generally, the references to mathematical objects by mathematicians in their work may be unserious. Their posits may be, as Azzouni alleges, ultrathin. To understand how thin the mathematician's posits may be, consider the work of Geoffrey Hellman (Hellman 1989) and Charles Chihara (Chihara 1990) in reformulating mathematical claims as modal claims. For Hellman, mathematical claims which are naturally and most literally seen as referring to objects are really assertions about possible structures. For Chihara, such claims are really references to possible inscriptions. The work of Hellman and Chihara is meaningful precisely because we need not take mathematical claims at face value. Michael Potter extends the point: "What mathematicians *say* is not always a reliable guide to what they are doing: what they mean and what they say they mean are not always the same" (Potter 2007: 18).

The weasel argues that we can interpret both mathematicians and scientists as taking back all *prima facie* commitments to abstracta. If we want to know whether the use of some theory or discourse commits us to the existence of mathematical objects, and we want to avoid begging the question, we need an independently-motivated account of when we are speaking most seriously. Colyvan's indispensability argument is most charitably interpreted as including an implicit appeal to such an account. But, it succumbs to the weasel precisely because of its neglect of QI3, Quine's procedure for determining when we are speaking most seriously. Many indispensabilists ignore QI3.[45] Such elision opens the argument to attacks from the weasel.

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[45] Liggins 2008 and Resnik 2005 are notable exceptions.

§6: Quine Against the Weasel

Quine's indispensability argument is of central importance in the philosophy of mathematics because of how it attempts to reconcile two conflicting but natural intuitions, about the security of mathematical beliefs and the exhaustiveness and sufficiency of science to tell us what there is. The autonomy platonist favors the first intuition and the weasel nominalist favors the latter and no mulish proponent of either side is likely to be swayed by arguments. But we should evaluate those arguments anyway and to do so we must see them in their full strength. I take Quine's argument to be indispensability at full strength.

A significant and perhaps underappreciated aspect of Quine's argument is its insistence not just on the controversial doctrines of naturalism and holism, on at least some interpretations of those terms, but also on the need to speak seriously, to settle on a canonical language and use that for debates about what there is. We cannot, says Quine, succumb to mulish defenses of our untutored intuitions. By insisting that we speak most seriously in our canonical formulations of our best scientific theory, Quine preemptively blocks the weasel.

For an example of how Quine's argument resists weaseling, consider the role of debates over simplicity in evaluating the indispensability argument. Colyvan and other indispensabilists argue that standard scientific theories with their mathematical tools are simpler than nominalist alternatives, like those of Field. The nominalist versions require gerrymandered physical axioms to do the work (especially of measurement) that elegant mathematical theories can do more simply. Melia argues that the indispensabilist is favoring the wrong kind of simplicity: a simpler theory rather than a simpler world.

Simplicity is a notoriously context-sensitive criterion for theory evaluation, as recent work especially by Elliott Sober shows.[46] Applications vary by sub-discipline within science and by particular context. It seems impossible to settle on a categorical application of the criterion, as Wesley Salmon argues.

> The most reasonable way to look at simplicity, I think, is to regard it as a highly relevant characteristic, but one whose applicability varies from one scientific context to another. Specialists in any given branch of science make judgments about the degree of simplicity or complexity that is appropriate to the context at hand, and they do so on the basis of extensive experience in that particular area of scientific investigation. Since there is no precise measure of simplicity as applied to scientific hypotheses and theories, scientists must use their judgment concerning the degree of simplicity that is desirable in the given context (Salmon 1990: 186).

Salmon does not conclude that there is something unscientific about the concept of simplicity. His conclusion, which I take to be the proper one, is that applications of the concept of simplicity in theory choice are complicated and context-sensitive. The complexity of the concept of simplicity is relevant to the indispensability argument because such aesthetic considerations within science must be evaluated in order to settle on a theory from which to read one's ontological commitments. If the concept of simplicity is too plastic, it is easy for the weasel to claim, as Melia does, that simplicity speaks in the nominalist's favor.

QI, with its insistence on a specific method of determining the commitments of a theory, can block, at least to some extent, weaseling based on the plasticity of 'simplicity'. If the indispensabilist insists that our best theories are as close to first-order as possible, then we can refine and distill the complexities of the concept of simplicity. The indispensabilist can insist that simplicity in constructing the theory is to be understood as minimizing the number of its distinct predicates or that simplicity in

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[46] See Sober 1996.

constructing our model is to be understood as minimizing the number of objects in the domain. These clear criteria may not be categorical in favor of a unique theory. But they severely restrict the weasel's latitude. Without such criteria, without any refinement of the criterion of simplicity, the weasel roams free.

To sum up, let's remind ourselves of QI.

QI QI1. We should believe the theory which best accounts for our sense experience.
QI2. If we believe a theory, we must believe in its ontological commitments.
QI3. The ontological commitments of any theory are the objects over which that theory first-order quantifies.
QI4. The theory which best accounts for our sense experience quantifies over mathematical objects.
QIC. We should believe that mathematical objects exist.

If the weasel says that we need not believe our theories, QI1 denies that we can avoid such belief. If the weasel says that we can believe a theory without believing that the objects to which the theory refers really exist, QI2 denies that we can make such discrimination. If the weasel says that we can differentiate between legitimate and merely instrumental posits, QI3 provides a specific method, neutral to prejudice about what exists and deferential to the actual workings of science, for revealing the commitments of a theory.

Anyone can merely deny that the commitments of a good scientific theory impel belief in the mathematical quantifications of that theory. Whether such a response is compelling or mere gainsaying depends in part on whether its proponent presents a defensible alternative method for determining one's ontological commitments. An eleatic who defends a causal criterion for ontological commitment may have some standing against QI3. Some weasels do appear to have such an alternative criterion in mind. But if the weasel hopes to convince the indispensabilist to give up the argument, she must defend the alternative criterion.

Whatever the weaknesses of QI are, it is not the mere expression of a preference for a mathematical ontology and it can not be refuted by the weasel's mere preference for a simpler world.

§7: Autonomy Platonism and the Weasel

The weasel denies the legitimacy of any inference from the applications of mathematics to its justification. The indispensabilist's central response is the double-talk argument: one can not use the tools of mathematics in our most sincere empirical theories without also taking the references of mathematical terms seriously. The weasel responds that double-talk is ordinary scientific practice; instrumentalism is standard operating procedure and mathematical objects are so obviously not the kinds of things that exist that to take mathematical terms to refer is absurd.

If the topic of this book were whether the indispensability argument succeeds in convincing the fictionalist to believe in mathematical objects, we would have to pursue further the question of whether the eleatic weasel can present a viable alternative to Quine's criterion and thus defend instrumentalism. But the subject of this book is a contrast between the indispensabilist and the autonomy platonist. What can the autonomy platonist take from this debate between the indispensabilist and the weasel?

The role of Chapter Three is to show that there are problems with the arguments underlying QI which should be apparent to philosophers with all sorts of views about mathematics, to nominalists as well as platonists. The role of this chapter is to show some problems with the argument from the instrumentalist's perspective. Instrumentalists tend to be nominalists about mathematics. But one need not be an anti-platonist to believe that the inference from the applications of mathematics to their justification does not follow. The autonomy platonist agrees with the weasel that the inference is invalid.

Still, there is something uncomfortable about the double-talk that Melia and other weasels defend. Colyvan seems correct to claim that there are limits to what one can take back from a story without turning it into nonsense. The strength of Quine's indispensability argument arises in part from his proper insistence that we cannot invoke the physicist's theoretical commitments to electrons as reasons to believe in electrons without also being serious about the references to mathematical objects used in those theories. One's natural suspicion of the existence of abstract objects can only go so far. Melia claims that our expressive resources may be too impoverished to say what we want to say without invoking mathematics. But we must, at some point, speak seriously. Weaseling remains awkward despite the instrumentalist's assurances.

Autonomy platonism, as we will see, provides a middle ground. It allows justification of mathematical beliefs which does not depend on the applications of mathematics in science and so allows us to avoid double talk about the mathematical references in our theories without also taking those uses of mathematics as grounds for our beliefs. Insofar as the weasel finds the arguments for autonomy platonism persuasive and consistent with her position, they may be considered friendly amendments to the weasel's cause. Insofar as the weasel denies the success of QI, or weaker versions of the indispensability argument, the autonomy platonist can sympathize. But insofar as the weasel is interested in defending some version of anti-platonism, the autonomy platonist can not agree.

One might believe that the autonomy platonist, being a platonist, would welcome an argument, like QI, for platonism. In the next chapter, we will put both the problems for QI from Chapter Three and the criticisms from instrumentalists aside to look at some concerns about the yield of the argument from the autonomy platonist's perspective. By the end of the next chapter, it should be clear why the autonomy platonist must reject the indispensability argument.

Chapter Five: The Unfortunate Consequences

§1: Traditional Platonism

In Chapter Two, I presented in detail what I take to be the strongest version of the indispensability argument, QI. In Chapter Three, I discussed what I take to be the major weaknesses of QI. For the purposes of this chapter, I will set aside those criticisms, grant the soundness and validity of QI, and look at the purported yield of the argument, which beliefs the argument justifies even when we read it most charitably. I show that even if QI were successful, even if the indispensabilist can defend naturalism, holism, and the Quinean criterion for determining the ontological commitments of a theory, the argument yields an anemic version of platonism, one which suffers from what I call the unfortunate consequences of the argument.

The goal for those who defend the indispensability argument is often to convince the nominalist to be a platonist. In this chapter, I have a different goal. I want to show the platonist who has cottoned to the indispensability argument that s/he's getting a raw deal.

I start by characterizing a traditional view of mathematical objects. Though I broadly characterized that traditional view in Chapter One, I will now specify it, in part by characterizing the objects of mathematics more clearly. While some readers may find the traditional conception contentious, I claim that it is defensible, that it is possible to establish with an autonomy platonism, and that the indispensability argument does not justify mathematical beliefs as traditionally conceived.

For simplicity, I focus the characterization of mathematical objects on sets. By doing so, I adopt some common, though not universal, presumptions about the reducibility of all mathematical objects to sets. Critics of the reductive presumption include structuralists motivated by Benacerraf 1965, which reasonably denies the existence of a unique reduction. In another direction, category theorists may hold that mathematical reductions should point instead to more fundamental categories. But nothing I say depends on the reductive presumption; all my claims could be generalized to any pure mathematical objects.

Sets are abstract objects, lacking any spatio-temporal location.[47] The universe of sets is described by various standard axiomatizations; where different axiomatizations conflict we find disagreement about the nature and extent of the set-theoretic universe.[48] Their existence is not contingent on our existence, nor is it contingent on the existence of any physical objects. (I speak here of pure sets. The existence of sets with ur-elements, sets of grey dogs or kings of France, is contingent on such objects.)

Furthermore, mathematics is a discipline autonomous from empirical science; mathematical standards are independent of the uses of mathematics in science. We do sometimes pursue mathematics in order to solve specific problems in empirical science. But we have criteria for determining whether to accept a mathematical assertion which are independent from the application of that assertion to empirical science.

Mathematical methodology largely proceeds *a priori*. There are empirical aspects to mathematical methods, of course: knowledge of who proved which theorems, say, and observations of inscriptions. We may even come to believe a mathematical claim on empirical grounds. But, such empirical claims do not suffice for mathematical justification. A mathematical proof is independent of any empirical grounds.

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[47] Attempts to locate mathematical objects with their concrete members lead to substantial difficulties, as Frege argued against Mill (see Frege 1980, §7-§9), and as Mark Balaguer argues against Penelope Maddy (see Balaguer 1994).

[48] Or universes. Balaguer 1998 argues that any consistent axiomatization truly describes a universe of sets, even if it conflicts with other consistent axiomatizations; see Chapter Nine.

Each of the properties of sets or traditional mathematics that I have mentioned has been denied of mathematical objects, just as the existence of mathematical objects has been denied. Still, these characteristics constitute, at least in part, the standard starting point for discussions of the nature of mathematics and mathematical objects. They are consistent, for example, with what James Robert Brown calls the "mathematical image" and with Stuart Shapiro's traditional picture.[49] I have described, though not defended, the traditional view. My goal here is to show that the indispensabilist can not support this view. Later, I argue that the autonomy platonist can support the traditional view. So for those for whom the traditional view is appealing, autonomy platonism is preferable to one based on the indispensability argument.

§2: The Indispensabilist's Mathematics

In Chapter 1, I described the following essential characteristics of any indispensability argument which concludes that we should believe that mathematical objects exist.

IPC1: Evidentiary Naturalism: The job of the philosopher, as of the scientist, is exclusively to explain or account for our sensible experience of the physical world; all evidence is sense evidence.

IPC2: Theory Construction: In order to explain our sensible experience we construct a theory, or theories, of the physical world. We find our commitments exclusively in our best theory or theories.

IPC3: Mathematization: Some mathematical objects are ineliminable from our best theory or theories.

IPC4: Subordination of Practice: Mathematical practice depends for its legitimacy on empirical scientific practice.

These essential characteristics entail some unfortunate consequences for the mathematics to which the indispensability argument refers. Most of these unfortunate consequences have been noticed and discussed elsewhere, but it is worthwhile to collect them here.[50]

§2.1: Restriction

First, since Mathematization and Evidentiary Naturalism rule out any alternate justifications for mathematical claims, the indispensabilist has no commitments to mathematical objects which are not required for empirical science. Call this consequence Restriction.

It is difficult to say precisely which mathematical objects the indispensability argument would justify, i.e. how much mathematics empirical science actually needs. Burgess and Rosen suggest that there is historical consensus that science needs no more than analysis. Feferman 1998 argues that predicative set theory will suffice. The point at which the indispensabilist draws the line between justified and unjustified mathematical beliefs is unimportant here. What is relevant is the existence of a division, one which Quine embraces. "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 1986a: 400).

[49] See Brown 1999: 1-7 and Shapiro 2000: 21-23.

[50] "The [indispensability] argument certainly does not provide a stairway to Platonic Heaven" (Decock 2002: 246).

There are really three problems of Restriction. First, the existence of a division, with no mathematical basis, between justified and unjustified mathematical beliefs is itself counter-intuitive. This oddity appears both to the autonomy platonist and to some anti-platonists. For example, Leng, attempting to extend Quine's view about mathematical recreation to all of mathematics, argues against Quine's division.[51]

The second problem is that the restrictions on the indispensabilist 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 can grow. These changes can occur though no mathematical progress need be made.

Lastly, the third problem of Restriction is that such changes can, in principle, decrease the scope of legitimate mathematics. Maddy 1992 suggests that all of science could, in principle, become quantized. In such circumstances, we could lose justifications for our beliefs about the real numbers, even though no mathematical problem with the theory of real numbers is discovered.

§2.2: Ontic Blur and Causality

A second unfortunate consequence, call it Ontic Blur, arises directly from Theory Construction, which entails that the indispensabilist can not differentiate between abstract and concrete objects. The indispensabilist's theory is constructed to explain or 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 1981: 9).

As these analogies dwindle, the abstract/concrete distinction blurs. Indeed, the terms 'abstract' and 'concrete' become rather meaningless for the holist, vulgar terms in which the learned may only lightly indulge. 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 1983: 377)

Furthermore, Quine himself wonders if such distinctions are 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 1986a: 402)

For Quine, the abstract/concrete distinction must be made within science. The indispensabilist's naturalism, required to leverage the empiricist into beliefs in mathematical theorems, debars a first-philosophical perspective from which to make an abstract/concrete distinction, from which to develop theories of different categories of objects. And scientific theory does not support the distinction. The quantifier univocally imputes existence. All commitments are made as values of bound variables.

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[51] See Leng 2002: §6.

It is tricky even to formulate a precise version of the abstract/concrete distinction: intermediary objects (e.g. space-time points, games, the equator) undermine some typical attempts. The indispensabilist thus emphasizes the continuity among posits of a physical theory, from ordinary objects, to atoms, to quarks, to strings, to space-time points, to sets and numbers. There is, for the indispensabilist, no clear boundary between physical and mathematical objects as there is no boundary between real and merely instrumental posits.

As the abstract-concrete distinction erodes, so does the causal/non-causal distinction. Independently of the indispensability argument, we can establish a criterion for abstractness, e.g. on the basis of what Balaguer calls the principle of causal isolation (PCI) of mathematical from empirical objects. With an epistemology for mathematics separate from that for empirical science, the claim that mathematical objects are distinctly abstract is more plausible. But, PCI is off limits to the indispensabilist. In fact, Ontic Blur is precisely the rejection of PCI. Balaguer even sees it as definitive of the indispensability argument. "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: 110).

So it is not clear that the indispensabilist's mathematical objects lack causal powers. Ordinary scientific theories do not include predicates indicating which objects have causal powers and which do not. So there is no reason to believe that the references of terms in pure mathematical theorems included in an empirical theory are excluded from the causal realm. Minimally tractable objects like space-time points may have minimal causal powers, depending on one's account of causation. The same holds for the indispensabilist's mathematical objects. "The indispensability argument may yet be compelling, but it would seem to be a compelling argument for the existence of entities *with* causal powers" (Cheyne and Pigden 1996: 641).

Of course, the indispensabilist can call mathematical objects abstract and other objects concrete and it is typical, even for indispensabilists, to deny that mathematical objects have causal powers.[52] We can even introduce a predicate into a canonical language to indicate those classifications, though it is difficult to imagine what work such a predicate would do. For example, it could not be invoked to indicate any difference in the ways we know about such objects. But it is not clear how the indispensabilist may ground such an assertion beyond mere fiat. Such fiat would contradict the basic tenets of the indispensabilist argument that our scientific theories are designed to explain our sense experiences and not to explain, as ends in themselves, mathematical phenomena.

It is thus no accident that many indispensabilists accept Ontic Blur or even invoke it to support their argument.

§2.3: Modal Uniformity and Temporality

When we combine Theory Construction with Mathematization, we find that the indispensabilist's mathematical objects do not exist necessarily. Mathematical objects are posited to account for our experience of a world which exists contingently. If the world were different, if it contained different physical laws, then scientific theory could require different objects. Call this consequence Modal Uniformity.

To illustrate Modal Uniformity, suppose that charge is a real property of particles in this world properly measured by real numbers. The indispensabilist thus alleges that the world contains continuous functions. Further, suppose that in a different possible world, there are no continuous properties. In that world, charge and all other properties are accurately and satisfactorily measured using rational numbers, eschewing continuity. In that world, says the indispensabilist, there are no continuous functions. Whether there are continuous functions in any particular world depends on the contingent fact of whether

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[52] See, for example, Baker 2005: 234.

the world contains properties with particular qualities.

All hope for modality may not be lost for the indispensabilist. There are several notions of necessity. When one asserts that the world is possibly Newtonian, even if relativistic, one may refer to a physical necessity on which phenomena in accord with scientific laws follow necessarily. Perhaps more strongly, a statement may be logically necessary, which may be construed as entailing a contradiction when negated, or as being either a logical law or following from one. And perhaps even more strongly, a statement may be metaphysically necessary, or true in all possible worlds. Kripke alleges that some identity statements, ones flanked by rigid designators, like the identity of water and H_2O, are metaphysically necessary. Some naturalists claim insight into Kripkean metaphysical necessities. Perhaps the indispensabilist could claim, similarly, that some mathematical claims are necessary.

There are two reasons to be skeptical about an indispensabilist's attempt to avoid modal uniformity by appeal to Kripkean metaphysical necessities. First, the indispensabilist's naturalism would seem to debar such claims. Explanations of Kripkean metaphysical necessities tend to rely on a non-naturalist *a priori* intuition unavailable to the indispensabilist. Second, even if the indispensabilist could establish that some mathematical identity statements are metaphysically necessary, it would not follow that mathematical objects exist necessarily, or that we should believe that they do.

By linking the justifications for our beliefs in mathematics to the physical world, the indispensabilist may retain a weaker modality, like physical necessity, for mathematical claims. Unfortunately, the weaker notion is not the one traditionally imputed to mathematics, and is unsatisfactory from the traditional perspective. It would follow that under a different set of physical laws, two and two might not equal the square root of sixteen. While this idea may be alluring to some, it seems absurd. Only a stronger necessity will do justice to intuitions that mathematical truths are broader than physical ones.

As a corollary of Modal Uniformity, mathematical objects are temporal too. For, if mathematical objects exist contingently, then there can be a time when they do not exist. If the existence of continuous functions depends on the existence of continuous physical quantities, then if the physical quantities were to be extinguished, the mathematical functions would disappear as well, and would have to be removed from our list of commitments. Moreover, since no other objects to which scientific theory refers are atemporal, the indispensabilist has no grounds for calling indisets atemporal. Again, mathematical objects are traditionally taken to be 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: 21).[53]

The temporality of the indispensabilist's mathematical objects apparent also from considering Ontic Blur. The best accounts of the abstract/concrete distinction rely on the atemporality of abstract objects.[54] Since the indispensabilist can not defend the claim that mathematical objects are abstract, s/he can not use that claim to support the atemporality of mathematical objects.

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[53] David Lewis called the non-spatiotemporality of sets an unofficial axiom of set theory. See Lewis 1993: 13; also see Paseau: 2008: 302.

[54] "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: 124). One distinct advantage of Katz's distinction is his ability to account for objects intermediary between abstract and concrete: artworks, games, natural languages, the equator. Katz classifies such objects as composite, generated by a creative relation between abstract and concrete objects.

§2.4: The Indispensabilist's Methods

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, Subordination of Practice entails that any mathematical debate, like that over the axiom of choice, should be resolved not on mathematical terms, but on the basis of the needs of science. Chihara criticizes this indispensabilist subordination of mathematical practice. "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: 15).

For another example, consider the introduction, by Cardan, of complex numbers as solutions to quadratic equations with missing real roots. So-called imaginary, or impossible, numbers were derided, despite their mathematical uses. Complex numbers 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 not accommodate them. Even the analogy with negative numbers, which arose from similar disrepute, serves as no argument for the indispensabilist. Lacking application, work with complex numbers was just mathematical recreation.

The indispensabilist will describe the discovery of an application for any mathematical objects as one of an empirical confirmation of its existence. Not all mathematical objects will be as lucky as the complex numbers. Consider two conflicting mathematical theories, like ZF + CH and ZF + not-CH (in any of the many different ways it can be denied). It is possible, in this case and others like it, that neither theory will admit of application in empirical theories and thus of confirmation for the indispensabilist. It seems safe to presume that there will not be application for all of the transfinitely many, presumably consistent axiomatizations which result from adding axioms asserting different sizes of the continuum to ZF. There is no mathematical reason not to multiply set-theoretic universes. Perhaps there are multiple set-theoretic hierarchies; in some the continuum hypothesis holds, while in others it fails, and in different ways. The indispensabilist, committed to austerity in abstracta, adopts a mathematical theory only when it has physical application.

The problems of Subordination of Practice are even worse for theories which seem to suffer from empirical disconfirmation. Subordination of Practice entails that the indispensabilist's appeal to Euclidean rescues is limited. As we saw in §4.3, we naturally perform a Euclidean rescue any time a mathematical theory loses application in science. In such cases, the indispensabilist generally rejects the newly-unapplied mathematics. The traditional response is the Euclidean rescue, unless the mathematics is shown inconsistent.

We can be sure that mathematicians working today in the farthest reaches of pure set theory do so without knowing that their work has any physical application. One may arise, or their work may never find use in empirical science. 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 makes the mathematician dependent on the scientist for the justification of his or her work.

§3: The Unfortunate Consequences

Summarizing the results of the previous section, given the essential characteristics of the indispensability argument, the following are thus unfortunate consequences:

UC1 Restriction: The indispensabilist's commitments are to only those mathematical objects required by empirical science.
UC2 Ontic Blur: The indispensabilist's mathematical objects are concrete.
UC3 Causality: The indispensabilist's mathematical objects may have causal powers.
UC4 Modal Uniformity: The indispensabilist's mathematical objects do not exist necessarily.
UC5 Temporality: The indispensabilist's mathematical objects exist in time.
UC6 Aposteriority: The indispensabilist's mathematical objects are known a posteriori.
UC7 Methodological Subservience: Any debate over the existence of a mathematical object will be resolved, for the indispensabilist, by the needs of empirical theory.

Given the unfortunate consequences together with the traditional characterization of mathematical objects, it is clear that the indispensability argument does not justify beliefs in mathematical objects, even if our best scientific theory includes mathematical axioms. The indispensabilist's so-called mathematical objects retain none of their traditional characteristics. Still, the indispensabilist asserts that any regimentation of physics will require set-theoretic axioms in order to provide the required functions. So, what are the objects which satisfy these axioms, if not mathematical objects?

The inclusion of mathematical axioms in a theory is no indication that the theory is committed to mathematical objects. Theories do not determine their own models. Moreover, lots of objects can serve as models of mathematical axioms. Appropriately arranged peas can serve as models of finite portions of number theory. Field 1980 proposes using space-time regions to model geometry, including an axiom of continuity. Of course, peas will not suffice for ZF, but some less tractable and more plenitudinous objects can.

Quine urges a doctrine of gradualism from observable objects like trees, through sub-visible objects like electrons, to space-time points and sets. The central point of this chapter is that just as space-time points are not mathematical objects, neither are the indispensabilist's sets. They are ordinary empirical posits, somewhat less tractable, but no different in kind, than trees.

I hesitate to coin a name for these concrete, contingent, temporal, known-a-posteriori denizens of the indispensabilist's restricted universe. Still, a name will help us distinguish them from real sets. 'Quasi-sets' is taken. One might call them 'indisets'. Indisets are concrete and temporal empirical posits, which unlike space-time points lack spatial location. As set theory may be used to generate all the objects of pure mathematics, so indisets may be used to generate all the objects of applied mathematics.

I disdain the profligacy which leads me, tentatively, to introducing a further category of objects into our ontology. But the introduction of objects to model applied mathematics is independently motivated by considerations of physical geometry. The objects which model our pure physical geometry seem to lie between space-time points and pure geometric points. They are the indispensabilist's geometrical posits.

Given the results of Chapter Three, there are questions whether the indispensability argument can justify belief even in indisets and other objects of applied mathematics. But, the indispensability argument surely does not justify belief in mathematical objects.

§4: Other Quinean Indispensability Arguments

Because its earliest proponents never constructed a precise indispensability argument, everyone who writes about the argument formulates it for her/his own purposes, and so gives the argument certain idiosyncratic attributes. Some versions are explicit about the argument's reliance on naturalism or Quine's criterion for commitment. Some versions leave those premises implicit or suppress them. As we will see in the next chapter, some version even imprudently try to eschew appeals to holism. To this point, we have examined what I take to be the strongest version of the argument, QI, and I have been ascribing this version of the argument to Quine. Before we proceed to examine more of the unfortunate consequences facing this argument, let's pause to look at two related versions of the argument, ones which not only have the essential characteristics of indispensability arguments but which, like QI, adopt Quine's holism.

Mark Colyvan presents an influential version focused on the core claims.

CIA
1. We ought to have ontological commitment to all and only those entities that are indispensable to our best scientific theories.
2. Mathematical entities are indispensable to our best scientific theories.
Therefore:
3. We ought to have ontological commitment to mathematical entities (Colyvan 2001: 11).

In CIA, holism is the 'all' portion of the first premise and naturalism is the 'only' portion, so those commitments are fairly explicit. Colyvan suppresses Quine's invocation of a method for determining the ontological commitments of our theories, but I take him to have no deep disagreement with Quine over how we determine the commitments of a theory.

Michael Resnik characterizes holism and naturalism in a version of the argument explicitly dependent on them.[55]

> Confirmation Holism: The observational evidence for a scientific theory bears upon the theoretical apparatus as a whole rather than upon individual component hypotheses.
> Naturalism: Natural science is our ultimate arbiter of truth and existence.
> ...
> Mathematics is an indispensable component of natural science; so, by holism, whatever evidence we have for science is just as much evidence for the mathematical objects and mathematical principles it presupposes as it is for the rest of its theoretical apparatus; whence, by naturalism, this mathematics is true, and the existence of mathematical objects is as well-grounded as that of the other entities posited by science (Resnik 1997: 45).

Like CIA, Resnik's Quinean argument lacks an explicit appeal to the ways in which we determine the commitments of a theory. But it is clear that Resnik's argument also adopts the essential characteristics. Thus both Colyvan's CIA and Resnik's Quinean argument suffer the unfortunate consequences. They may justify belief in some limited mathematical theories most aptly modeled by the contingent and temporal indisets. But they can not justify robust beliefs in mathematical theories or mathematical objects.

§5: Embracing the Unfortunate Characteristics

The Unfortunate Characteristics are important to keep in mind when evaluating the indispensability argument. The argument, if successful, would justify, to some degree, beliefs in the

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[55] Resnik also develops a non-holistic version of the argument which I discuss in the next chapter.

theorems of the portions of mathematical theories which must be included in our best scientific theory. But the objects which we can posit to model those theorems are odd.

But complaints such as UC1-UC7, while compelling to the traditional platonist and the autonomy platonist, do not suffice to reject the indispensability argument. Indeed, some indispensabilists embrace some of the unfortunate characteristics. Quine embraced Restriction in the division between legitimate mathematics and recreational mathematics, as we saw in §4.3, and Ontic Blur, as we saw in §5.2.2. In rejecting modalities, Quine may be seen as embracing Modal Uniformity, though his position is more properly described not as embracing one modal status but rejecting all modal claims.

More recent indispensabilists have explored other of the Unfortunate Consequences. Maddy presents what she calls a modified indispensability argument on which the mathematical beliefs which are applicable in science are justified directly by their use, but the unapplied portions of mathematics are justified indirectly, via their relations to those beliefs which are directly justified. She calls the modified indispensabilist's mathematical beliefs *a posteriori*.

> [T]he indispensability theorist should adopt some version of [the claim that there is a determinate answer to open mathematical questions]. Notice, however, that this acceptance of the legitimacy of our independent question and (for the modified theorist) the legitimacy of its pursuit is not unconditional; it depends on the empirical facts of current science. The resulting mathematical beliefs are likewise a posteriori and fallible (Maddy 1992: 285).

Maddy seems to confound two distinct characteristics: the purportedly *a posteriori* nature of mathematical beliefs and their fallibility. As I mentioned in Chapter One, the most plausible account of *a priori* knowledge is fallibilist; mathematical beliefs can be held *a priori* without our believing that they are infallible. But Maddy may just be rightly sensitive to the ways in which the mathematics yielded by an indispensability argument, even her modified argument, inevitably suffers the unfortunate characteristics.

Colyvan presents an extended defense of the empirical nature of mathematics.[56] Since the question whether mathematical knowledge is *a priori* or empirical is a central point of disagreement between the autonomy platonist and the indispensabilist, it is worth a moment to examine Colyvan's arguments against the apriorist's account. Colyvan frames his argument in part as a response to Bigelow who argues that G, for example, can be believed *a priori*.

G $$\sum_{j=1}^{n} (2j - 1) = n^2$$

Bigelow provides a pictorial proof of the equation which involves arranging objects like pebbles in a series of shapes called gnomons.[57]

Against, Bigelow, Colyvan despairs of an explication of '*a priori*' which would support the such a characterization of the proof.

> [H]e clearly does not intend the traditional (Kantian) sense, in which a priori knowledge is

[56] Colyvan 2001: Chapter Six.

[57] See Colyvan 2001: 119; Bigelow and Pargetter 1990: 350-1; and Kline 1972 (vol. 1): 28-34 for discussions of the gnomons at issue.

> gained independently of experience, for no matter how you cash out 'experience', gazing at and manipulating pebbles *must* count as experience. Some other accounts of "a priori" on offer are: (1) a priori knowledge is rationally unrevisable; (2) a priori knowledge involves necessity in some way (either as a necessary condition, a sufficient condition or both); or (3) a proposition is a priori if one is justified in believing it once one understands it (Colyvan 2001: 120).

None of the three numbered characterizations of *a priori* knowledge in this quote are plausible. Any account of the *a priori* must be fallibilist in order to be plausible. The unrestricted set-theoretic axiom of comprehension, for example, may plausibly be held *a priori*. But it has turned out to be false (since it leads to contradiction) and so not necessarily true. But the falsity was discovered by further *a priori* reasoning and so should not lead us to revise our claims about the basis on which one (Cantor, say, or Frege) believed it initially.[58] So any plausible account of *a priori* knowledge could not entail either unrevisability (1) or necessity (2), though it might be the case that *a priori* beliefs are necessarily true if they are true.

The characterization of an *a priori* belief as one that is justified once one understands it, (3), is more plausible. But understanding comes in degrees which do not seem proportional to the degrees to which a belief is justified. I have some measure of understanding, of, say, the axioms governing Woodin cardinals. I had a greater understanding of them when I was studying set theory carefully. I can achieve a greater understanding of them by going back to my notes and texts. But I do not know if I am justified in believing that there are Woodin cardinals, in large part because I am not sure how to think about the existence claims of many large cardinal axioms. There is just too much work on those axioms and their relationships left to do. Whatever my degree of understanding of a proposition, my justification for believing that proposition seems to be an independent matter.

The first characterization of the *a priori* which Colyvan considers is even more plausible, but he seems to misunderstand it and put it to poor use in rejecting Bigelow's claim. Colyvan claims that since manipulating pebbles counts as experience, we can not consider that picture proof as *a priori* in the sense of knowledge gained independently of experience. There are two problems here. First, no plausible account of *a priori* knowledge could deny that some experience is relevant to our understanding. That we have visual experiences of inscriptions of a token of a proof, for example, does not undermine the *a priori* status of that proof.

Second, Colyvan seems to be committing a genetic fallacy of confusing the origins of our beliefs with their justifications. A characterization of *a priori* knowledge as independent of experience does not entail that our beliefs have to be acquired without experience; that would be an impossible and quixotic task. Even having thoughts is, in some sense, having experience. A proper characterization of *a priori* knowledge as independent of experience merely entails that a belief held *a priori* admits of justification which does not depend exclusively on empirical evidence.[59] Much remains to be said about *a priori* knowledge, and I will say more in Chapter Nine about what I take to be an *a priori* capacity of mathematical intuition. But Colyvan dismisses the possibility of such knowledge by examining only implausible characterizations.

Colyvan's characterization of mathematics as empirical, though, need not depend on his rejection of the apriorist account. That mathematics is empirical (or *a posteriori*) follows directly from the indispensability argument, as one of the Unfortunate Characteristics.

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[58] For a similar account, see Bealer 1998.

[59] See, for attempts to cash out this characterization even further: Bonjour 1998 and Casullo 2003.

Colyvan embraces not only the claim that mathematics is empirical, but that mathematical theorems are contingent. Colyvan hints at two arguments for the contingency of mathematical objects. The first is just the argument of §5.2: the contingency of mathematics follows, for the indispensabilist, from the contingency of the physical world and the theories we use to describe that world. "[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: 116).

Colyvan's second argument is via Field's embracing of contingent nominalism. According to Field, mathematical objects are fictions, but contingently so. Field's argument that mathematical objects only contingently fail to exist is an artefact of his account of the applicability of mathematics. Briefly: In order to explain the utility of mathematics, despite its dispensability, Field develops a position he calls conservativeness: good mathematics is conservative insofar as it does not allow us to derive further nominalistically-acceptable consequences when added to a nominalist theory. Conservativeness entails consistency; if mathematics were inconsistent then adding it to a consistent nominalist theory would allow the derivation of every theorem of the language (including many nominalistic statements not already derivable). So Field needs an nominalistically-acceptable account of consistency, which he provides in terms of a modal operator understood as representing logical possibility. It follows from the use of this modal operator that mathematical theories are (logically) possibly true, but actually false, and so only contingently false.

Colyvan defends Field's embrace of contingency, but not for any reason intrinsic to mathematics or to the indispensability argument. Field's view is that mathematical objects are logically possible, but the contingency of mathematical objects which arises from the indispensability argument is of a different sort. For the indispensabilist, mathematical objects are contingent on the nature of the physical world, not merely logically contingent.

This discrepancy aside, Field's defense of contingency is not without its critics. Hale and Wright criticize Field's claims, focusing on the oddity that mathematical objects do not seem to be contingent on any other facts. Field agrees that there is no good explanation of on what the existence of mathematical objects depends, but alleges that there is no reason to believe that such an explanation is required; Colyvan concurs.

Thus nothing in Colyvan's defense of contingency serves to motivate a view on which it is intuitively acceptable to call mathematical objects contingent. It is very well for an indispensabilist to embrace the contingency of mathematical objects. Indeed, I believe that it follows from the indispensability argument. But if one is going to argue that contingency is an acceptable result, it would be useful to have an intuitively pleasing account of their contingency.

Balaguer's plenitudinous platonist, a kind of autonomy platonist, embraces the contingent existence of mathematical objects too, though not from an indispensabilist's position. For Balaguer's FBP-ist, mathematical claims are necessarily true in worlds in which abstract objects exist. But Balaguer believes that it is possible for there to be worlds in which there are no abstract objects.

The difference between the claim that true mathematical sentences are true in every world (i.e. that mathematical truths are necessary truths) and the claim that they are only true in worlds in which mathematical objects exist (Balaguer's contingency claim) may seem minor. Still, I do not know how to understand the claim that it is possible to have a world without, for example, an empty set. In any world in which there are objects, or even if there are just fields with various strengths, there are concepts of those objects or fields, or at least possible concepts of them. But once there are even possible concepts, we can reason *a priori* about them. Such reasoning leads to beliefs about mathematical objects.[60] We do not even have to be in the world in order to impute the concepts and thus the mathematical objects to the world.

Many worlds have some things in them, so many worlds have mathematical objects. But the empty world does not have any thing in it and so we might think that there are no mathematical objects in that world. But any account of mathematics will appeal to immanent virtues of theories like simplicity and uniformity. It would be awkward gerrymandering to posit that all worlds have mathematical objects except the empty world (or empty worlds). So, on the basis of simplicity and uniformity, we might believe that there are mathematical objects in empty worlds too.

The contingent existence of mathematical objects which arises as a consequence of the indispensability argument is different from the accounts defended by Field, Colyvan, and Balaguer. For the indispensabilist, mathematical objects exist contingently because we only posit them to account for the physical world. If the world were to change so that such posits were no longer essential, then we would no longer have reasons to believe in their existence. In contrast, the autonomy platonist can, I believe, account for the necessary existence of mathematical objects if that turns out to be a desirable characteristic. In any case, the autonomy platonist is not constrained to say that mathematical objects exist contingently which I take to be an unfortunate consequence.

§6: The Way Forward

I have not presented positive arguments that the Unfortunate Consequences are truly unfortunate, i.e. a direct defense of traditional platonism. It seems to me that the burden is on those who deny the traditional characteristics of platonism to provide not just a reluctant acceptance of the Unfortunate characteristics, but positive reasons to embrace those characteristics. I have not seen such arguments.

But philosophy shouldn't be burden-of-proof volleyball. Some people believe that the advantages of the indispensability argument, its empiricist epistemology for a platonist ontology, outweigh the Unfortunate Consequences. My suspicion is that those philosophers who embrace the unfortunate consequences do so for the epistemological benefits they see in avoiding the posits of traditional platonism. Those benefits have been overrated. I have mentioned some of the reasons for my belief in Chapter One and I will return to the topic in Chapter Nine. My goal in this chapter was merely to make sure that autonomy platonism remains in play and that, when evaluating competing philosophies of mathematics, all the consequences of each position are considered.

The strongest version of the indispensability argument, QI, depends on controversial claims about believing in a single best theory, finding our commitments by using a canonical first-order logic, and the ineliminability of mathematics from scientific theories. Other versions of the argument attempt to avoid some of these controversial claims. In the next two chapters, I will examine some recent attempts to develop alternatives to QI.

[60] I'll have more on this argument in §9.3.

Chapter Six: The Putnamian Indispensability Argument

I have called the version of the indispensability argument on which we have focused so far, QI, a Quinean argument. While Quine never formulated this argument precisely, I believe both that it represents his views and that it is the strongest version of the argument available.

Still, partially as a result of the argument's earliest defenders never formulating canonical versions of the argument, many alternative versions of the argument have been developed. Some of these, like the versions by Colyvan and Resnik which we saw briefly in §5.4, are variations on QI. It's best to think of such arguments as Quinean, differing from QI mainly in emphasis rather than in content.

In this chapter, I will examine versions of the indispensability argument which I trace to the work of Hilary Putnam and which I will call, instead, Putnamian arguments. My differentiation between the Quinean argument and the Putnamian arguments is not standard, because there is no standardly accepted distinction. But I believe that my division usefully and accurately tracks the central differences in the intentions of Quine and (at least one thread of work by) Putnam. I will support that claim with a bit of exegesis in §1-§2 and show how my division is more useful than another interpreter, Joseph Melia, in §3.

Nothing really hangs on the names of the arguments. The important distinction is really between arguments which fully rely on holism and Quine's criterion for ontological commitment and those which either leave those commitments implicit or deny them. The central theme of this chapter is that, in contrast to the hopes of their proponents, attempts to avoid criticisms of QI by removing such premises weakens the argument.

§1: Putnam and the Quinean Argument

Putnam is both lauded and criticized for exploring a variety of competing views, especially in the philosophy of mathematics: deductivism (1967a), modalism (1967b, 1975a), realism (1971 and 1975a again), and anti-realism (1980, 1981, 1994). Despite the differences among these views, a reliance on mathematical empiricism in the guise of one or another form of the indispensability argument is a deep underlying theme through his work.

There is a quick way to see this unifying thread. Repeatedly through his work, Putnam argues that the theory of relativity shows that Euclidean geometry is false. This inference links the truth of mathematical claims with empirical evidence, a link at the core of the indispensability argument. An autonomy platonist would not make that inference; that physical geometry is curved only shows the autonomy platonist that flat geometry is inapplicable to our best theories of space-time. A fictionalist would not make that inference either; fictionalists argue that mathematical theories are false without appealing to such considerations. Only someone who believes that our justifications for mathematics are essentially empirical can make such a claim. That sort of mathematical empiricism, pairing a purportedly platonist ontology with an empiricist epistemology, is the core of the indispensability argument. No matter which of the four specific views Putnam is exploring, he consistently takes this telltale position regarding Euclidean geometry as a datum.[61]

Our focus here is on Putnam's indispensability argument for what he calls mathematical realism. That Putnam relies on indispensability for his mathematical realism needs little defense. "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: 67).

In places, Putnam seems to make additional claims for a traditional, autonomy realism. "There are two supports for realism in the philosophy of mathematics: mathematical experience and physical experience." (Putnam 1975a: 73) He describes mathematical experience on analogy with theological

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[61] For examples of how indispensabilism encroaches on Putnam's four different views of mathematics, see Putnam 1967b: 50; Putnam 1968: 177; Putnam 1974: ix; Putnam 1975a: 77-8; Putnam 1976: 94; and Putnam 1981: 83.

experience, independent of empirical evidence. In later work, Putnam even urges two modifications of Quine's indispensability argument which seem incompatible with indispensability: the addition of combinatorial facts to sensations as desiderata of theoretical construction and agreement with mathematical intuitions.[62]

These early allusions to mathematical experience and later modifications of the argument, though, are thin and do not alter Putnam's essentially indispensabilist framework. Putnam takes intuitions to be justified empirically, or quasi-empirically, and so not autonomously from scientific theory. Further, he does not abandon his criticisms of an intuition-based autonomy platonism: "[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: 1).

Moreover, Putnam insists on empirical verification of mathematical claims. "[T]he 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: 73-4)

Given Putnam's consistent rejection of pure mathematical intuition and his insistence that mathematical theories be supported by their applications in empirical sciences, it would be a mistake to read his attempts to accommodate pure mathematical phenomena or experience as a rejection of the indispensability argument. It's best, I think, to see them as attempts to assimilate aspects of traditional mathematical epistemology into the context of the indispensability argument.

So let's turn to Putnam's explicit and precedental defenses of the indispensability argument. Putnam held at least two specific versions of the indispensability argument. The first is the Quinean argument, which Putnam echoes both in its procedure for determining the commitments of theories and its holism. "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: 40)

Putnam's agreement with Quine on the illegitimacy of instrumentalist double-talk, especially as a response to Carnapian weaseling, is well-known. "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: 347)

Furthermore like Quine, Putnam embraces a division in our mathematical beliefs between those claims which we should believe because they are used in scientific theories and those which are speculative and about which we should remain agnostic. Quine calls the process of development of speculative mathematical claims recreation. Putnam concurs with Quine's distinction.

> 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: 347)

Putnam also developed a second, different indispensability argument, the success argument. Both versions of the argument contain the essential characteristics, EC1-EC4: Evidentiary Naturalism;

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[62] See Putnam 1994: 504-506.

Theory Construction; Mathematization; and Subordination of Practice. But the success argument attempts to simplify the indispensability argument by eschewing appeals to holism and to find an analogy between arguments for scientific realism and arguments for mathematical realism.

Other differences between Quine and Putnam over the indispensability argument emerged over time. Putnam abandoned Quine's commitment to a single, regimented, best theory and assumed a realist stance about truth in science, in contrast to Quine's view of truth as mainly a device for semantic ascent. Also unlike Quine, Putnam argued that realism in mathematics can be justified by its indispensability for correspondence notions of truth (which require set-theoretic relations) and for formal logic, especially for metalogical notions like derivability and validity which are ordinarily treated set-theoretically. Quine mainly focused on the needs of physical science. These differences may be more cosmetic than contentful and mainly concern the goal of theory construction: what mathematics is supposed to be indispensable for. We can, for the most part, put them aside and focus on the most influential of Putnam's modifications, his attempt to remove the argument's reliance on holism.

§2: Putnam's Success Argument

In this section, I develop the argument which I call Putnamian and show that it is, for the most part, a limited and weak appeal to the practical utility of mathematics. It might also be a demand for an account of the applicability of mathematics in empirical science. But it does not justify our mathematical beliefs. In later sections of this chapter, I show how the success argument has been precedental for many recent indispensability arguments and how it shares its weaknesses with them.

Putnam's argument focuses merely on the success of mathematics in developing science. "[T]he 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: 75).

Putnam's success argument for mathematics is analogous to, and may be compared with, his success argument for scientific realism. The scientific success argument, SS, relies on Putnam's famous claim, sometimes called the no-miracles argument, that positions other than realism are implausible. "The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle" (Putnam 1975a: 73).

SS
SS1. Scientific theory is successful.
SS2. There must be a reason for the success of science.
SS3. No positions other than realism in science provide a reason.
SSC. So, realism in science must be correct.

Given the relatively uncontroversial SS1 and SS2, Putnam's argument for realism in science rests on the no-miracles argument at SS3. 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 would be left unexplained. An instrumentalist interpretation on which theories may be useful without being true better accounts for the utility of false theories.[63]

There are probably good responses to this quick criticism of SS3. But the realism/anti-realism debate in science is ancillary to our purposes here. I mention SS mainly to distinguish the no-miracles argument for scientific realism from Putnam's secondary use of that argument for mathematical realism.

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[63] Recent work on the realism/anti-realism debate also claims that acceptance of the no-miracles argument may be the result of a base-rate fallacy; see Magnus and Callendar 2004 and references therein.

"I believe that the positive argument for realism 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 1975: 73).

Putnam's non-Quinean indispensability argument has the structure of an inference to the best explanation, where the explanandum is the utility of mathematics in science.

MS
MS1. Mathematics succeeds as the language of science.
MS2. There must be a reason for the success of mathematics as the language of science.
MS3. No positions other than realism in mathematics provide a reason.
MSC. So, realism in mathematics must be correct.

By 'realism', Putnam means a philosophy of mathematics on which both mathematical objects exist and many mathematical sentences are true. Thus MS provides, at least implicitly, an alternative to Quine's indispensability argument. Where Quine makes ontology the result of modeling our best theories, MS says that ontology is a consequence of understanding our explanations, our reasons for the success of mathematics in science.

To see that MS is independent of SS, consider that even if science were interpreted instrumentally, mathematics may be justified by its applications. On the standard, Quinean indispensability argument, our mathematical beliefs are only justified insofar as our scientific beliefs are. Some philosophers, like Nancy Cartwright, Bas van Fraassen, and Larry Laudan, have argued that science, or much of it, is false or idealized.[64] Even more recently, Robert Batterman provides convincing arguments that much of scientific reasoning is asymptotic or idealized. If our justification for mathematical realism is based on its use in scientific theory, and anti-realist philosophers are correct about science, then mathematics requires an auxiliary defense. But such problems need not infect our beliefs in the mathematics applied in scientific theory: a tool may work fine even on a broken machine. One could deny or remain agnostic towards the claims of science and still attempt to justify our mathematical beliefs using an argument like MS. While Putnam may see MS on analogy with SS, they are independent arguments.

MS1 is inoffensive even to the nominalist who thinks we can dispense with mathematics. MS2 is just a demand for an account of the applicability of mathematics to scientific theory. MS, like SS, rests on its third premise.

Even if one could establish that premise, and thus the argument, the mathematical realism it would establish would still suffer the unfortunate consequences: restriction, ontic blur, modal uniformity, temporality, aposteriority, and methodological subservience. Putnam's success argument retains all the essential characteristics of an indispensability argument and so would be burdened with the unfortunate consequences.

But the unfortunate consequences are really moot, since MS3 is false. Putnam's argument for it 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: 356).

For the holist, Putnam's argument, his rejection of double-talk, has some force. The holist has no external perspective from which to evaluate the mathematics in scientific theory as instrumental. Adrift on Neurath's ship, s/he can not say, as Carnap or the weasel can, "Well, I use mathematical objects within my best scientific theory, but I don't really mean that they exist."

For MS, which *ex hypothesi* rejects holism (in order to distinguish it from QI), instrumentalist

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[64] See Cartwright 1983; van Fraassen 1980; and Laudan 1981.

interpretations of the mathematics used in scientific theory are far more compelling. The proponent of MS is not constrained to limit existence claims to the quantifications of our best theory. She is free to adopt an eleatic principle, for example, as the fundamental criterion for existence.[65] The eleatic, who claims that only objects with causal efficacy (or spatio-temporal location) exist, rejects mathematical objects independently of (and despite) considerations of their applications.

More importantly, any account of the applicability of mathematics to the empirical world other than the indispensabilist's refutes MS3. For example, Mark Balaguer's plenitudinous platonism says that mathematics provides a theoretical apparatus which applies to all possible states of the world. It explains the applicability of mathematics to the natural world, non-miraculously, since any possible state of the natural world will be described by some mathematical theory. Dispensabilist constructions like that of Field 1980, which shows how mathematical theories can be taken as shorthand for statements about physical objects (spatio-temporal points and their relations) also erode confidence in MS3 by presenting an alternative account of why mathematics is useful in science.

In response, the proponent of MS could amend MS3 to MS3*.

MS3* Realism best explains the success of mathematics as the language of science.

The substitution of MS3* for MS3 does not help, though, since realism does not best explain the application of mathematics. In fact, it does not even explain applicability. Realism is just the claim that some mathematical claims are non-vacuously true. It says nothing about the applicability of mathematics to the physical world. Indeed, the existence of abstract objects of mathematics would seem to have little to do with the physical world. To see mathematical realism as explaining the successful application of mathematics to science would be to take the relationship between mathematical objects and the physical world, the most puzzling aspect of the question of application, as a brute fact.

So there are two distinct reasons to reject MS. 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 reasons, combined, reveal a tension in MS. The objects knowledge of which is justified by the indispensability argument are concrete, known *a posteriori*, and exist contingently and temporally; those are the unfortunate consequences. So, the indispensability argument can not establish pure mathematical knowledge. But, if it could, the account of why mathematics is useful in science would clearly be missing since mathematical objects inhabit an abstract realm, apart from the physical world.

Nevertheless, Putnam's MS has been precedental for, if not explicitly influential on, philosophers attempting to avoid what they see as a weakness of QI in its reliance on holism. Since the newer versions of the Putnamian argument do not differ relevantly from MS, I will not spend much time on them. But, it will be useful in tracking the motivations for recent modifications of the indispensability argument to examine Resnik's pragmatic indispensability argument as well as a general argument for why removing holism from the indispensability argument is fruitless.

First, I digress briefly to discuss a different division of Quinean and Putnamian indispensability arguments and thus complete the defense of my version of the distinction.

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[65] For examples of eleatics, see Armstrong 1978b: 46; Armstrong 1978a: 1:126; Azzouni 2004b: 150; Field 1989: 68; and Melia 2000.

§3: Melia's Two Indispensability Arguments

Melia also distinguishes a Quinean version of the indispensability argument from a Putnamian version.

> The indispensability argument comes in two main flavours: (1) We ought to believe the claims our best scientific theories make about the world - after all, they are our best scientific theories. But a casual glance through any book of theoretical physics reveals that our best scientific theories entail the existence of numbers, sets and functions... Since such claims entail the existence of abstracta, we cannot consistently assert or believe in our scientific theories whilst denying the existence of abstracta. (2) We ought to believe in abstracta for the very same theoretical reasons we believe in the concrete, unobservable entities postulated by scientific theory. We postulate such things as quarks and space-time points not because we directly observe these entities, but for pragmatic or aesthetic reasons: because doing so either increases the explanatory power of our theory, or increases the theory's simplicity, or increases the theory's strength-or a combination of all three (Melia 2000: 455-6).

Melia ascribes the first version to Putnam and the second to Quine. Such ascriptions are consistent with my division of the arguments: it's reasonable to put Melia's (1) into Putnam's mouth and Melia's (2) into Quine's. But Melia's distinction fails to get to the root of the difference between the two arguments. (1) is consistent with Quine's views and (2) is consistent with what I'm calling the Putnamian argument. Indeed, it is difficult to see Melia's (1) and (2) as distinct arguments; they look more like one argument, that we should believe in all our posits equally, with differing emphases.

Melia accurately represents Putnam's argument as lacking an explicit statement of the method one is to use to determine the objects to which a theory commits. But he represents, as a distinctive characteristic of the Putnamian argument, its reliance on the double-talk argument, its insistence on the inconsistency of mathematical instrumentalism in light of its ubiquity in our most serious scientific endeavors.

Putnam does makes use of the double-talk argument. As we have seen, though, that argument is also essential to the Quinean argument and really rooted in his work. One can not use this characteristic to distinguish Quine's version from that of Putnam.

For (2), Melia ignores Quine's premise that we must find our commitments in the first-order logical regimentations of our best theory. Melia thus proceeds to criticize a version of the argument which is not as strong as it can be. Quine's appeal to his criterion for determining the ontological commitments of a theory is essential for blocking exactly the kind of weaseling criticism that Melia uses against the indispensability argument and which Quine invokes against Carnap's proto-weaseling.

In characterizing the Quinean argument, Melia focuses on Quine's claim that the reasons for believing in mathematical posits are identical to the reasons for believing in any other posits, like quarks. This accurately represents Quine's view about posits. Melia also correctly presents Quine's claim that there are pragmatic and aesthetic reasons for choosing one theory over another. But Melia takes these aspects of Quine's method as evidence of a weakness in the argument. He claims that the introduction of considerations like simplicity in choosing a theory leave Quine's version open to the weasel.

Melia's reading of Quine is uncharitable. Quine's claims about so-called aesthetic considerations in theory construction are merely facts about the under-determination of theories by evidence. For any body of empirical evidence, there are many possible theories which can account for that evidence and we have to appeal to considerations like simplicity to choose among the competing theories. Quine is not leaving slack in the interpretation of our best theory. Once we choose a theory, we are committed to the objects over which that theory first-order quantifies.

The most salient differences between Quine's argument and Putnam's argument are, first, that

Quine provides an explicit method for determining the ontological commitments of a theory, while Putnam leaves that question open, and, second, that Quine's version of the argument is ineliminably steeped in holism while Putnam's attempts to avoid it. Quine's argument is thus resistant to alternative interpretations of the language of science. In particular, Quine's argument resists weaseling. We can not, for Quine, take back some of what we allege. Given Putnam's argument, for which we do not have explicit rules for interpretation of scientific discourse, a weaseling strategy might succeed. The weasel says that we can deny that we are really committed to the objects over which we first-order quantify. That contradicts the version of the indispensability argument that I presented as Quine's. But, it need not contradict the version I ascribe to Putnam.

Getting the difference straight between the Quinean argument and Putnamian argument is important in answering the question whether there is an opening for the weasel in any version of the indispensability argument. A version of the argument which is explicit about finding one's commitments in the first-order quantifications of our best theories, on pain of contradiction, resists the weasel. For versions which are more casual about our commitments, the weasel has a way in.

Another way to see the difference between the Quinean and Putnamian arguments is to ask whether looseness arises at the level of theory choice or at the level of commitments. For the Quinean argument, the only slack is at the level of theory choice. QI1, which refers to a best theory, does not fully describe the diverse complexities of theory choice. We must balance breadth and elegance and simplicity and unification and other factors. For Quine, balancing these factors is part of the scientific project. Once we have settled on a best theory, though, we are stuck with whatever objects best model it; there is no extra-scientific perspective from which to resist its commitments.

For Putnam's argument, there is slack in both the construction of our theories and in their modeling. We have to decide which philosophical theories (realism or its competitors) best explain the utility of mathematics in science. But only if there is slack at the level of commitments can the weasel get a toehold. Melia argues that we can choose the theory which makes the world simpler. But that claim is a *non sequitur* against the Quinean argument. The weasel enters the picture at the level of ontological commitment: we can deny the existence of objects over which we first-order quantify.

Melia seems to miss the key premise of the indispensability argument, that we find our commitments in the domain of quantification of the first-order version of our best theory. He correctly notes that we choose among empirically equivalent theories. But, once we choose one of those theories, we can not, according to the Quinean indispensabilist, choose to take back any of what we stated.

Of course, Melia is free to abandon that key premise of QI, as Putnamian arguments do. In that case, one has to provide an alternative method of determining one's ontological commitments. Putnam argued that we should believe in the objects which best explain the success of science; that method fails to establish mathematical realism. Melia can choose an eleatic principle. But such a choice is not an argument against QI. It is the result of an antecedent rejection of Quine's method for determining the commitments of a theory.

Quine knew that in choosing alternative criteria for determining one's commitments, the choice of one's logic is of utmost importance. Once we give up first-order logic havoc reigns. If we adopt a second-order logic, we are already committed to sets or properties. By adopting other logics, our commitments can be obscured. Or, we can abandon all hope of finding our commitments in regimented theories, and find ourselves back at the point before Quine wrote "On What There Is," fussing with names in ordinary language or letting bare metaphysical prejudices lead the way.

In evaluating indispensability arguments, only their content is important. It does not matter whether one version or another is best attributed to Quine or to Putnam. Whether my exegesis better tracks the intentions of Quine and Putnam better than Melia's is moot. Moreover, Melia's claim that our uses of mathematics in scientific theory do not commit us to belief in the existence of mathematical objects may be a more or less reasonable response to different versions of the argument. The way I understand the differences between the Quinean and the Putnamian argument, weaseling is more successful against the latter. Both Quine and Putnam defend their arguments from such an objection by appealing to the double-talk argument. But Quine supports his appeal to the double-talk argument by both his arguments for holism and appeals to his criterion for determining the ontological commitments of theories. Putnam, attempting to avoid Quine's full holism and views on best theories, weakens these requirements and thus opens his version of the argument to instrumentalist weaseling.

§4: Resnik's Pragmatic Argument

Michael Resnik, like Putnam, presents both a holistic indispensability argument, like Quine's, and a non-holistic argument. He calls the latter the pragmatic indispensability argument.

RP
- RP1. In stating its laws and conducting its derivations, science assumes the existence of many mathematical objects and the truth of much mathematics.
- RP2. 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.
- RP3. 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.
- RP4. We are justified in using science to explain and predict.
- RP5. The only way we know of using science thus involves drawing conclusions from and within it.
- RPC. So, by RP3, we are justified in taking mathematics to be true (Resnik 1997: 46-8).

The potential benefits of RP are twofold. First, RP, like MS, may avoid the problems which arise from undermining our beliefs in the truth of science. Even if our best scientific theories are false, their practical utility may still justify our using them. RP alleges that we need to presume the truth of mathematics even if science is merely useful. Second, and more explicitly, Resnik presents RP as a response to criticisms of the indispensability argument, especially criticisms of its underlying holism, by Maddy and Sober, arguments which we saw in Chapter 3.

The key premises for RP are the first two. If we can take mathematics, like science, to be merely useful, then those premises are unjustified. The question for the proponent of RP, then, is whether science presumes the truth of mathematical claims and the existence of mathematical objects. How does the proponent of RP determine the commitments of scientific theory?

This question is important because the mere inclusion of mathematical axioms within a scientific theory does not entail that those who use the scientific theory are assuming, say, the truth of the existential quantifications within that theory. The utility of mathematics is not by itself an argument for its truth. We need, further, a reason to believe that the inclusion of mathematical axioms within a scientific theory is serious enough to support beliefs in the objects to which those axioms refer. Otherwise, we are free to take the mathematics as merely instrumental, for representation and modeling, but not as an aspect of our most sincere, austere ontology. The inference to the truth of mathematics in RP1 is unjustified in the absence of a clear explanation of how science assumes the existence of objects.

The same problem appears in RP2. 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. The proponent of RP can respond that the scientist's beliefs are irrelevant, and that his work entails those commitments anyway. But then, again, we need a reason to believe that the uses of mathematics are supposed to be serious, entailing justifications of our beliefs in those axioms.

Against both Putnam and Resnik, the weasel, unchained by the abandonment of holism, attacks the strong premise that science presupposes or requires either the truth of mathematical claims or the existence of mathematical objects. A pragmatic argument for the indispensability of mathematics is 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 the truth of mathematical claims or the existence of mathematical objects.

The instrumentalist argument against RP is the same as that against MS. This result holds generally. For indispensability arguments which give up Quinean holism, instrumentalist interpretations of the mathematics used in scientific theory are compelling. Without holism and Quine's criterion for determining the ontological commitments of a theory, our existence claims need not be restricted to the quantifications of our best theory. We are free to adopt an eleatic principle, for example, as the fundamental criterion for existence.

Putnam and Resnik tried to save the indispensability argument from problems arising from holism, but they open the argument to weaseling criticisms to which Quine's original argument was resistant. QI resists the weasel because of Quine's claims that the ontological chips are down precisely in our single best theory and that we find our commitments in the quantifications of that holistic theory. Chris Pincock has noticed this role of holism in the argument. "Holism...is useful because it blocks any attempt to allot confirmational support to the various parts of our best scientific theories in different ways" (Pincock 2004a: 62).

Still, there may be other benefits of considering RP. Resnik presents the argument in part as a response to 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: 47).

As we have seen, the mere assumption of mathematical realism does not explain the applicability of mathematics and there are alternative, satisfying explanations available. For the mathematical nominalist, Field 1980 explains that mathematics is useful because it is a convenient shorthand for more complicated statements about physical quantities. For the non-indispensabilist realist, Balaguer's FBP notes that for all physical situations there is a mathematical theory which applies to it. If the goal of the pragmatic argument were merely to show that there is a problem of application, it can be solved without justifying mathematical beliefs on the basis of their utility to science.

Colyvan defends Resnik's invocation of a non-holistic argument, despite skepticism about whether a non-holistic version is strong enough to do the work that the indispensabilist needs.

> This argument has some rather attractive features. For instance, since it doesn't rely on confirmational holism, it doesn't require confirmation of any scientific theories in order for belief in mathematical objects to be justified. Indeed, even if all scientific theories were disconfirmed, we would (presumably) still need mathematics to do science, and since doing science is justified we would be justified in believing in mathematical objects. This is clearly a very powerful argument and one with which I have considerable sympathy (Colyvan 2001: 15).

But it is implausible that our mathematical beliefs would be justified by their appearances in scientific theories without some way of transferring evidence from science to mathematics. Holism facilitates that transfer. Colyvan sees the virtue of RP as showing that we need not think of evidence as transferring from individual sentences of a scientific theory to other individual sentences. Instead, evidence is supposed to transfer from the practice of science to our mathematical beliefs. Resnik and Colyvan claim that scientific practice is justified, whatever the truth values of its theories, so even if all scientific theories were disconfirmed, our uses of mathematics in scientific practice would still justify our mathematical beliefs.

Setting aside worries about the validity of RP (or MS, which has a similar goal), I do not believe that the argument is plausible. Resnik wants us to believe that the uses of mathematics in science should justify our mathematical beliefs, even if scientific theory were false. But there must be some kind of epistemic virtue transferring from science to mathematics if our mathematical beliefs are going to be justified by their applications. If scientific practice were no more justifiable an intellectual pursuit than, say, playing *Call of Duty*, and mathematics were needed only in order to play the game (a core claim for the indispensabilist), we would see mathematics as a merely instrumental practice for a merely instrumental pursuit. It is only because we are so thoroughly convinced that our scientific practice is justifiable (even if the laws are not precisely true) that the indispensabilist can suppose that we do not need a premise like holism to transfer justification. If the indispensability argument were the only reason to believe mathematical claims and the practice of science had the value of, say, astrology, we would have no reason to believe them.

§5: Indispensability Without Holism

Both MS and RP attempt to give the proponent of the indispensability argument some freedom to avoid two important objections to the standard indispensability argument, from anti-realists about science and from anti-holists. But the flexibility gained by eschewing QI's invocation of holism comes at a cost to the effectiveness of the argument.

While Putnam and Resnik attempt to avoid a contentious holism, any indispensability argument has to give holism or something like it some role. The indispensabilist claims that evidence for our scientific theory extends to the mathematical elements of that theory. Without holism, or something else to do the work that holism does for QI, it is difficult to make the case that evidence transfers from science to mathematics.

Mathematical objects are causally inert. They have no spatio-temporal location and no effects on anything that does. They are, in Hartry Field's words, absolutely insular, both brute and barren.[66] The evidence we have for our scientific theories is essentially sensory. We have theories of physical objects insofar as we have sense experiences of physical objects. Our theories extend to distant and small objects of which we have no (direct) sense experience. But we have explanations of our distance which do not entail that, say, quarks and dark matter are insular. Indeed, we posit such objects in order to explain their constitutive or causal relations with the objects we do experience.

Whatever mathematics we use in our scientific theories, it is difficult to see how our evidence can extend to absolutely insular mathematical objects. *Pace* the odd Pythagorean, trees are not made of numbers or sets. Numbers and sets are not causally efficacious, even in deep space. It seems overwhelmingly more likely that the mathematics is a second-class artefact of our formulations of those theories.

Moreover, it is not typical practice to think of empirical evidence as extending to mathematical claims, except in one trivial sense. Scientists seeking a mathematical framework for a physical theory, a proper set of differential equations, say, will ordinarily test and discard many mathematical hypotheses. They struggle to find the correct constant (e.g. the fine-structure constant) for an equation or formula. We do not ordinarily understand their rejected hypotheses as evidence against any mathematical theory. Conversely, the adoption of a mathematical theory by scientists is not naturally seen as providing mathematical evidence for that theory, except insofar as (this is the trivial case) mathematicians might

[66] "Let us call a claim brute if its obtaining or not obtaining doesn't depend on anything else; barren if no phenomena from a different domain depend on it; and absolutely insular if it is both brute and barren: (Field 1993: 296).

seek a model for a theory and the physical world can be a model, given appropriate bridge principles. The results of experiment and observation and theory construction seem irrelevant to our mathematical beliefs.

But not if we are, like the proponent of the Quinean argument, holists. Once holism is invoked, it is no longer puzzling how empirical evidence can extend to mathematical claims: all evidence for any portion of our theory is evidence for every portion of our theory. Our evidence for our scientific theories extends to their mathematical theorems and objects. Unless one introduces holism into the argument, either implicitly or explicitly, empirical evidence does not transfer to mathematical claims. Holism bridges the disciplines.

It is understandable that the proponent of the indispensability argument might want to eschew holism. But the holism in question is not particularly problematic. As a logical matter, it is undeniable: in response to recalcitrant experience, any statement of a theory may be held true as long as the truth values of others are adjusted to compensate. Sober's argument against holism, that we hold some premises in the background whenever we perform an experiment or an observation, may be practically accurate. We don't bring all of our background beliefs to play in any given observation. Such a practice can be observed without holism being false. If holism is correct within science, the indispensabilist still has a case to make for the extension of evidence from science to mathematics.

§6: Other Non-Holistic Indispensability Arguments

In recent years, a variety of philosophers have developed versions of the indispensability argument which do not explicitly rely on holism. As we will see in the next chapter, Alan Baker introduces his recently influential explanatory (or enhanced) indispensability argument in part as an attempt to develop a version of the argument that does not rely on holism.[67] Patrick Dieveney argues that the argument is better without holism. "[T]here is a stronger version of the argument...that does not include confirmational holism as a premise" (Dieveney 2007: 126).

Similarly, David Liggins presents two arguments which he claims eschew appeals to confirmation holism, one focusing on measurement and the other on law statements.[68] Jacob Busch and Andrea Sereni propose what they call a minimal indispensability argument, similar to those of Liggins, which does not invoke holism.[69]

None of these so-called non-holistic versions of the indispensability argument, these Putnamian arguments, avoid the problems we saw facing Putnam's MS or Resnik's RP. Without holism, the transfer of evidence from the scientific portions of a theory to its mathematical axioms is unmotivated. On a non-holistic (or atomistic) view, mathematics is a discipline independent of empirical science. Our evidence for mathematics is independent from our evidence for empirical theories, as independent and dissimilar as we can imagine. The indispensability argument seems to require holism or some other argument to the same end, either implicitly or explicitly.[70]

In formulating indispensability arguments, proponents may reasonably hope to weaken the controversial premises of the argument so as to make it maximally defensible. Quine's reliance on naturalism, holism, and his criterion for determining the commitments of a theory are all controversial. It

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[67] See Baker 2005 and Baker 2009.

[68] See Liggins 2008.

[69] See Busch and Sereni 2012.

[70] See Marcus in preparation.

makes perfect sense for proponents of the argument to avoid or downplay the argument's essential reliance on these premises. But weakening the argument to make it more defensible only makes it more vulnerable to attack. Avoiding explicit commitments to the controversial premises to make it more resistant to one sort of attack makes it more liable to other kinds. In particular, an indispensability argument which rejects the legitimacy of double-talk is resistant to weaseling. QI, in its explicit defense of the relevant controversial premises, including the commitment to Quine's criterion for determining the commitments of a theory, is maximally strong.

There is more than a hint of truth in these Putnam-inspired attempts to justify our mathematical beliefs independently of our beliefs about science. But insofar as one wishes to justify mathematical beliefs without appealing to their uses in science, one does not invoke an indispensability argument; one is looking for autonomous justifications, toward autonomy platonism.

We have almost reached my discussion of autonomy platonism. First, there is one more version of the indispensability argument to examine, one which has received a lot of attention recently: the explanatory (or enhanced) indispensability argument.

Chapter Seven: The Explanatory Indispensability Argument

§1: Indispensability, Theories, and Explanations

One of the consequences of Quine and Putnam not formulating a canonical version of the indispensability argument is the contemporary proliferation of versions of the argument. Another consequence is that scholarship on the argument did not develop in earnest until Hartry Field's seminal *Science without Numbers* appeared in 1980. In that book, Field denies QI4, the claim that our best theories first-order quantify over mathematical objects. To support his denial, he shows how to reformulate Newtonian gravitational theory (NGT) without using standard mathematical axioms. Field demonstrates that the physical structure of space-time with certain assumptions about continuity can be used in lieu of real mathematics in relatively attractive formal versions of NGT.

Field's project was precedental for a range of further research. John Burgess worked on refining and improving Field's project, using two-sorted logics to streamline the construction. Charles Chihara and Geoffrey Hellman rewrote mathematical theories as modal ones. Mark Balaguer attempted to show how quantum mechanics could be nominalized. Much has been written about these projects, all surrounding the claim QI4. We can call these projects, generally, dispensabilist.

I will not attempt to answer to the deeply interesting questions of whether dispensabilist projects are technically successful. My focus is on the other premises of the argument. But we should keep in mind that even for those who accept that first-order quantification over mathematical objects in an attractive theory is sufficient to justify mathematical beliefs, the further question of whether our best theories actually do quantify over mathematical objects is an open one.

One assumption of Field's project, central to QI as well, is that we should believe in all and only the posits of our best scientific theories. We have seen that some versions of the indispensability argument are more liable than others to rejections of that claim via weaseling, to denying that we should believe in the existence of all of the objects to which our theories refer. In particular, versions of the argument which give up on Quine's holism lose the ability to respond to the weasel by appealing to the ways in which evidence for a theory spreads to all its components.

Still, proponents of weaseling may see even the standard indispensability argument, with its emphasis on uses of mathematics to represent physical quantities, as vulnerable. Some such opponents of the indispensability argument claim that the representational role of mathematics in science is insufficient to justify our mathematical beliefs. "We are not committed to belief in the existence of objects posited by our scientific theories *if their role in those theories is merely to represent configurations of physical objects*. Fictional objects can represent just as well as real objects can" (Leng 2005: 179).[71]

Thus, some recent work on the indispensability argument, by Alan Baker and Mark Colyvan especially, has sought either to replace or augment the central assumption of standard indispensability arguments, like QI or CIA, that our commitments are to be found in canonical versions of our theories. Instead of considering how mathematics is used in scientific theories, they suggest, we should look at how mathematics functions in scientific explanations. Proponents of the explanatory indispensability argument believe that uses of mathematics in scientific explanations are not as easily explained away as the merely representational uses of mathematics in scientific theories: nominalism is less plausible if one can show that mathematics plays an explanatory role in addition to its representational role.

My central goal for this chapter is to show that either the explanatory argument succumbs to instrumentalist weaseling, like the versions of the indispensability argument discussed in Chapter Six, or it is no stronger than the standard argument, QI. Along the way, we'll look at the origins of the

[71] See also Balaguer 1998; Pincock 2004b; and Daly and Langford 2009. The latter call this claim the indexing argument: we are not committed to mathematical objects if we use them in a merely representational (or indexing) role.

explanatory argument and the ways in which evidence is supposed, by its proponents, to support the argument.

As we proceed, it is important to remember that explanation already has a central role in the standard indispensability argument as a prominent factor in our choices of theories. Among the criteria we use to compare theories are their abilities to explain empirical phenomena. Other factors include the simplicity of the theory, the breadth of its application, and the ways in which it can unify disparate phenomena. Proponents of the new indispensability argument urge that explanation should play an even more prominent role.

§2: The Explanatory Indispensability Argument

Paolo Mancosu presents a tidy version of the explanatory argument, which is sometimes called the enhanced indispensability argument.

EI
EI1. There are genuinely mathematical explanations of empirical phenomena
EI2. We ought to be committed to the theoretical posits postulated by such explanations; thus,
EIC. We ought to be committed to the entities postulated by the mathematics in question (Mancosu 2011: §3.2.)[72]

In addition to using EI as a response to instrumentalist concerns about the representational role of mathematics in scientific theories, proponents of EI are motivated by dispensabilist criticisms of the standard argument, like those of Field. The proponent of EI sets aside the question of whether scientific theories can be rewritten without mathematical theorems. Instead, she argues that non-mathematical explanations of physical phenomena are either unavailable or less preferable.

The literature on the explanatory argument is divided between two ways to view EI in relation to versions of the argument, like QI, which appeal to theories. Bangu and Melia see EI as an additional demand on the platonist, and thus an additional option for the nominalist. They argue that even if dispensabilist constructions like those of Field are not available, we should withhold commitments to mathematical objects since there are no genuinely mathematical explanations.[73] On the Bangu/Melia view, the platonist may have to show mathematics indispensable from both theories and explanations; the nominalist may need only to show that mathematics is eliminable from explanations or theories.

In contrast, one can see the argument as an additional option for the platonist, and thus an additional demand on the dispensabilist. Baker and Lyon and Colyvan argue that even if dispensabilist constructions of scientific theories are available, we should believe in mathematical objects as long as there are genuinely mathematical explanations of the physical phenomena entailed by those theories. For example, exploring David Malament's claim that phase-space theories resist dispensabilist constructions, Lyon and Colyvan write, "Even if nominalisation via [a dispensabilist construction] is possible, the resulting theory is likely to be less explanatory; there is explanatory power in phase-space formulations of theories, and this explanatory power does not seem recoverable in alternative formulations" (Lyon and Colyvan 2007: 242). On the Baker/Lyon and Colyvan view, the platonist must show that mathematics is indispensable only from explanations (or, perhaps, only from theories); the nominalist must show how we can eliminate mathematics from both theories and explanations.

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[72] Compare to Baker 2009: 613.

[73] See Melia 1998: 70; Bangu 2008: 14; and Bangu 2013: 259. Melia 2002 and Leng 2005: 179, though working with explanation as a theoretical virtue, can also be seen as taking this route.

Since, I will argue, the explanatory argument is at best an elliptical allusion to the standard argument, it will not matter here whether EI is taken as an additional burden on the nominalist or as extra work for the platonist.

§3: The Origins of the Explanatory Argument

Before analyzing the explanatory argument, I want to clear up a matter of some dispute about its origins. This confusion contributes to a continuing misunderstanding about the relation between the standard argument and the explanatory argument.

Baker and Mancosu both misleadingly credit Field as originating the explanatory argument. "Hartry Field, one of the more influential recent nominalists, writes that the key issue in the platonism-nominalism debate is 'one special kind of indispensability argument: one involving indispensability for explanations' (Field 1989, p. 14)" (Baker 2005: 225).

Field does allude to a special indispensability argument for explanations in the text cited. But what makes the argument special, for Field, is not its difference from the standard argument, QI. Field contrasts the special indispensability argument for explanations in science with an indispensability argument which alleges that mathematics is required for logic, specifically metalogic. Putnam had argued, in his influential "Philosophy of Logic," that we should believe in mathematical objects because of their utility both in science and in logic.

> [A]t present reference to 'classes', or something equally 'non-physical' is indispensable to the science of logic. The notion of logical 'validity', on which the whole science rests, cannot be satisfactorily explained in purely nominalistic terms, at least today (Putnam 1971: 333).

Field thus augmented his reformulation of NGT with demonstrations how to understand concepts like consistency, derivability, and truth without appealing to abstract objects like sets or argument types.[74] When Field mentions the special indispensability argument for explanations, he is not contrasting the explanatory argument with one for scientific theories. Field is distinguishing the argument that we need mathematics for science from the argument that we need mathematics for metalogical notions like consistency and truth.

Bangu follows Baker and Mancosu on this misdirection. "Field noted that even if, contrary to what he argued in his (1980), mathematical posits turn out to be indispensable to scientific theorizing, they still can't be granted ontological rights until they are shown to be indispensable in a stronger, more specific sense; in particular, the realists should be able to show that mathematical posits are indispensable for scientific explanations (Field, 1989, pp. 14-20)" (Bangu 2008: 13-4).

A careful reading of the selection cited by both Baker and Bangu shows no such argument by Field. Field makes no claim that there is a heavier burden on the dispensabilist than recasting standard scientific theories to remove quantification over mathematical entities. Field's interest in explanation is exclusively on how explanatory merit factors into evaluations of our theories. His concern is with QI, where one factor in determining whether a theory is best is its explanatory force. Other factors include breadth and simplicity. For Field, once we have settled on a best theory, the only important question for the indispensabilist is whether that theory can be written to avoid quantification over mathematical objects.

In the section cited by both Baker and Bangu, Field frames the indispensability argument as an inference to the best explanation and makes clear that his allusions to explanation are not meant to distinguish an explanatory argument from the standard one.

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[74] See Field 1989: Chapter 1, §5 and Chapter 3.

> If our belief in electrons and neutrinos is justified by something like inference to the best explanation, isn't our belief in numbers and functions and other mathematical entities equally justified by the same methodology?... I think that this sort of argument for the existence of mathematical entities (the Quine-Putnam argument, I'll call it) is an extremely powerful one, at least *prima facie*... [I]t says that the very same explanations in which the postulation of unobservables is essential are explanations in which the postulation of mathematical entities is essential: mathematics enters essentially into our theory of (say) electrons. There seems to be no possibility of accepting electrons on the basis of inference to the best explanation, but not accepting mathematical entities on that basis (Field 1989: 17).

Field also explicitly refers to his own work rewriting NGT as a response to this so-called explanatory argument. And he moves directly from talk about explanation to talk about theories.

> What we must do is make a bet on how best to achieve a satisfactory overall view of the place of mathematics in the world... My tentative bet is that we would do better to try to show that the explanatory role of mathematical entities is not what is superficially appears to be; and the most convincing way to do that would be to show that there are some fairly general strategies that can be employed to purge theories of all reference to mathematical entities (Field 1989: 18; see also fn 15 on p 20).

Field is clearly thinking of explanation on a traditional covering-law account, in which explanations are done by our best scientific theories. Field even says that an explanation is, "A relatively simple non-ad hoc body of principles from which [the phenomena] follow" (Field 1989: 15).

The philosopher's interest in scientific theories derives in part from their uses in explanations. We have a related interest in theories because of their uses in making predictions. We might also have a brute interest in the ways in which scientific theories can represent facts about the world. These considerations are linked, especially in regards to the indispensability argument. The question to which the indispensability argument is relevant is whether we should believe in the existence of mathematical objects. We appeal to theories or explanations in attempting to answer this question only insofar as we think that theories or explanations are relevant to the question of what we should believe exists. QI refers to theories only because we might, with Quine, believe that what we should believe to exist is what our best theories say exists. For someone who thinks of explanation as focused on theories because our commitments are to be found in our best theories, as I believe Field does, the difference between explanation and theory is moot.

The same phenomenon, sliding inconsequentially between appeals to theories and appeals to explanations, appears in Colyvan's early work. In *The Indispensability of Mathematics*, he presents what he calls a scientific indispensability argument, which refers to theories, and then says that such an argument can be an explanatory argument.

> **Argument 1 (Scientific Indispensability Argument)** *If apparent reference to some entity (or class of entities) ξ is indispensable to our best scientific theories, then we ought to believe in the existence of ξ.*
> In this formulation, the purpose, if you like, is that of doing science. This is a rather ill-defined purpose, and I deliberately leave it ill defined for the moment. But to give an example of one particularly important scientific indispensability argument, with a well-defined purpose, consider the argument that takes providing explanations of empirical facts as its purpose. I'll call such an argument an explanatory indispensability argument (Colyvan 2001: 7).

For Colyvan in 2001, as for Field, there is no difference between the standard indispensability argument, like QI, and an explanatory argument. It is really not until Baker's 2005 paper, which introduced his influential cicada example, that talk shifted to a new explanatory indispensability argument, distinct from the standard one. This version was formulated in Mancosu 2011 (originally 2008) and then in Baker 2009. As Bangu rightly notes, Baker framed the new argument as a response to a debate between Melia and Colyvan.[75] "Despite their opposing sympathies, both authors agreed that it is not enough - for the purposes of establishing platonism - that mathematics be indispensable for science; it has to be indispensable in the right kind of way. Specifically, it needs to be shown that reference to mathematical objects sometimes plays an explanatory role in science" (Baker 2009: 613).

By that point, it had become clear that, for proponents of the new argument, explanation was not merely playing its old role as a theoretical virtue helping us choose among different theories. The eliminability of mathematical theorems from scientific theories, in light of the new argument, might not be decisive in favor of the dispensabilist.

Credit for the new argument, EI, seen as distinct from QI, should thus really go to Baker, rather than Field or Colyvan. Still, in evaluating the explanatory argument, its provenance does not matter. But the ways in which appeals to Field's so-called special explanatory argument seem to ignore how explanation functions in the standard argument are revealing. Proponents of the new argument believe that they are providing a new and strengthened version of the indispensability argument, but, as I show in this chapter, they are most charitably read as alluding to the standard one.

So let's move on to evaluating EI. I'll start by defending the first premise and then proceed to showing how the second premise either fails to distinguish EI from QI or just fails.

§4: Mathematical Explanations in Science

Debate over EI has mainly focused on EI1, the claim that there are genuinely mathematical explanations within science. Examples of mathematical explanations of physical phenomena can be used to support either QI, taking explanatory merit as a theoretical virtue, or EI. Examples which are proffered as support for QI can thus carry over as support for EI. Colyvan 2001, for example, presents three cases intended to support the claim that standard, mathematized theories have greater explanatory merit than their nominalist correlates.

ME1. Bending of light. The best explanation of light bending around large objects is geometric, rather than causal.

ME2. Antipodes. The Borsuk-Ulam topological theorem, along with appropriate bridge principles, explains the existence of two antipodes in the Earth's atmosphere with the same pressure and temperature at the same time.

ME3. The Fitzgerald-Lorentz contraction. Minkowski's geometrical explanation of the contraction of a body in motion, relative to an inertial reference frame, relies on equations in four dimensions, representing the space-time manifold.[76]

Colyvan 2007 presents three further illuminating examples.

ME4. Squaring the circle. That π is transcendent explains why we can not construct a square with the same area as a circle, using straight-edge and compass.

[75] See Melia 2000; Colyvan 2002; and Melia 2002.

[76] See Colyvan 2001: 81-6.

ME5. Mountaineering. A hiker, leaving base camp on one day and top camp the next, at the same time, will pass one point on the trail at the exact same time on both days.

ME6. Altruism. Simpson's paradox helps explain how a maladaptive trait like altruism can succeed despite the fact that altruistic populations, taken individually, are less fit.[77]

Not all of these examples are equally compellingly described as mathematical explanations of physical phenomena. Baker rightly worries about the status of the geometry on which ME1 and ME3 rely. If the relevant geometry is physical geometry, then the explanation may proceed without appeal to pure mathematics. Baker also argues that ME2 is a prediction rather than an explanation. There is no antecedent why-question, a requirement for explanations, since we are unlikely to discover the two antipodes: there are insurmountable limitations on the precision of our instruments and no independent interest in the phenomenon. Still, one can easily provide a deduction or unifying argument which yields the given phenomenon. Leng also complains that ME2 requires contentious idealizations, and so the requisite bridge principles will not apply. Still, these worries do not impugn the claim that if such antipodes were found on Earth, the Borsuk-Ulam theorem would help explain them.[78]

Leng also calls ME4 a prediction. But it is not a prediction of a physical fact. It is possible to construct a square with an area arbitrarily close to that of a given circle, by choosing arbitrarily close rational approximations of pi. We can draw a square-ish region with the same area as a given circle, within any given margin of measuring error. Still, if we had arbitrarily good measuring tools, we could always find a difference in the areas of the square and circle. If we found that we could not square the circle, the transcendence of pi could help explain that fact.

ME5 is more plausibly an explanation, rather than a prediction. But ME6 may not be even be true. Colyvan cites Malinas and Bigelow to support his claim. They conclude only that the mathematical result is worth examining since it *could* explain the persistence of altruism. "It is of considerable theoretical significance to explore the applications of Simpson's Paradox, to see whether this might help to explain not only the altruism but also the irrationality, inefficiency, laziness and other vices that may prevail in populations, and that can cause a population to fall short of the economic rationalist's or Darwinian's ideal of the ruthlessly efficient pursuit by each individual of its own profits or long-term reproductive success" (Malinas and Bigelow 2008).

Baker, interested in defending the claim that there are mathematical explanations of physical phenomena but worried about Colyvan's ME1-ME6, produced an influential cicada example.

ME7. Cicadas. That prime-numbered life-cycles minimize the intersection of cicada life-cycles with those of both predators and other species of cicadas explains why three species of cicadas of the genus Magicicada share a life cycle of either thirteen or seventeen years, depending on the environment.

The phenomenon of cicadas having prime-numbered life-cycles intrigued biologists, who sought an explanation. Baker claims that the phenomenon is explained with indispensable appeal to mathematics.

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[77] See Colyvan 2007: 120-1.

[78] See Baker 2005: 226-7 and Leng 2005: 181-2.

CP CP1. Having a life-cycle period which minimizes intersection with other (nearby/lower) periods is evolutionarily advantageous.
CP2. Prime periods minimize intersection (compared to non-prime periods).
CP3. Hence organisms with periodic life-cycles are likely to evolve periods that are prime.
CP4. Cicadas in ecosystem-type, E, are limited by biological constraints to periods from 14 to 18 years.
CP5. Hence cicadas in ecosystem-type, E, are likely to evolve 17-year periods (Baker 2009: 614).

Baker's argument is that the mathematical explanans at CP2 supports the empirical explanandum at CP3. As Baker notes, CP3 is a "'mixed' biological/mathematical law." He uses this law to explain the further empirical claim CP5.

Bangu explores a worry about ME7, one which would hold for other examples like Colyvan's ME3. He presents four desiderata of examples used to support EIA1. In addition to, first, their indispensable uses of mathematics and, second, their being genuinely mathematical explanations, they should, third, be fairly simple. Since rewriting theories to avoid quantification over mathematical objects is mainly a philosopher's project, not of compelling interest to many scientists or mathematicians, relevant techniques for eliminating mathematics may not yet be developed. The indispensabilist should avoid resting the case on a lack of nominalist strategies which is due only to the difficulty of the task.[79]

Most relevantly to the case of ME7, proponents of EI should not, fourth, beg the question by presenting examples in which the explanandum contains ineliminable uses of mathematics. Bangu argues that some purported mathematical explanations of physical phenomena, like ME7, are really mathematical explanations of mathematical phenomena and so question-begging. Specifically, to apply the mathematical theorem used in CP2 to the case at CP3, we need bridge laws to assurance that number theory applies to the cicadas' cycles; the pure number-theoretic premise refers to numbers and says nothing about life-cycles and their intersections.

Bangu argues that the explanandum in question at CP5 is, like CP3, a mixed statement composed of both mathematical and physical facts: a physical phenomenon (the time interval between successive occurrences of cicadas); the concept of a life-cycle period, expressed in years; the number seventeen; and the mathematical property of primeness. The mathematical explanation, he claims, only explains the mathematical portions of the explanandum. "Since primeness is a mathematical property, it is not surprising that we have to advance a mathematical explanation of its relevance, in terms of specific theorems about prime numbers" (Bangu 2008: 18).

Thus, Bangu presents ME8, the banana game, to avoid what he sees as Baker's violation of the fourth desideratum. In the game, two players compete to collect bananas by choosing among crates filled with unknown numbers of bananas. By adjusting the probabilities of choosing some crates over others, the game can be constructed so as to ensure the victory of one side over the other, even when the losing side has more bananas to choose from, as long as the probability of choosing the crates with large numbers of bananas is sufficiently low. In Bangu's case, the explanandum, that one side consistently wins, does not contain ineliminable uses of mathematics. But the explananda include mathematics in the

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[79] Relatedly: "As a consequence of nominalism's being mainly a philosopher's concern, this open research problem is moreover 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: 118).

forms of probabilities and expected values.

Recent work on EI has seen a profusion of further examples supporting EI1. Lyon and Colyvan 2007 discuss (ME9) how the honeycomb conjecture in mathematics explains the structure of some bee hives. The relevant geometric theorem, a conjecture for over two millennia which was proven in 1999, is that a regular hexagonal grid divides a surface into regions of equal area with the least total perimeter.

Mancosu 2011 notes (ME10) that the twisting tennis racket theorem explains why a tennis racket, held horizontally and tossed to rotate about its intermediate principal axis, will make a near-half twist around its handle. Mancosu also cites (ME11) Peter Lipton's observation that a simple geometric fact explains why a snapshot of a bundle of sticks tossed in the air will show significantly more of the sticks closer to the horizontal than to the vertical: there are more ways to be horizontal than vertical.

In response to cases such as these, Juha Saatsi argues that the proponent of EI has not shown that mathematics plays an explanatory role in such examples. Saatsi believes that the mathematics in these cases continues to be mostly representational. "[T]he fact that mathematics can give us knowledge (or better justified beliefs) of certain physical facts does not automatically entail that it thereby plays an explanatory role" (Saatsi 2011: 145).

The central claim of this chapter is that EI fails because its second premise is false. If Saatsi is correct, then the proponent of EI has problems with the first premise as well. But whether all of these examples work exactly as proponents of EI require is too strong a demand either for establishing EI1 or for satisfying Bangu's four desiderata. What is important is the underlying plausible claim that there are mathematical explanations of physical phenomena. The examples ME1-ME11 describe physical phenomena. They invoke mathematics to explain those phenomena. It may be possible to re-describe some of the phenomena or explanations either to eliminate or to isolate the mathematical elements, or to show that the mathematics continues to play a merely representational role. But as they stand, such examples provide decisive, unsurprising evidence for EI1.

§5: Two Concepts of Explanation

The real problem with EI is not at EI1, but at EI2, the claim that we should believe in the objects to which our explanations refer. Not all uses of mathematics in our discourse compel our belief in mathematical objects.

For example, the inference IM, though it uses a bit of elementary arithmetic, is of a type that should have little weight in our beliefs about numbers.

IM I have two mangoes.
Andrés has three different mangoes.
So, together we have five mangoes.

Such simple, adjectival uses of arithmetic do not do not justify our mathematical beliefs in part because they are so easily avoided in formal languages like those of first-order logic.

IN $(\exists x)(\exists y)(Mx \bullet My \bullet Hix \bullet Hiy \bullet x \neq y)$
$(\exists x)(\exists y)(\exists z)(Mx \bullet My \bullet Mz \bullet Hax \bullet Hay \bullet Haz \bullet x \neq y \bullet x \neq z \bullet y \neq z) \bullet (\forall x)[(Mx \bullet Hax) \supset {\sim}Hmx]$
$\therefore (\exists x)(\exists y)(\exists z)(\exists w)(\exists v)[Mx \bullet My \bullet Mz \bullet Mw \bullet Mv \bullet x \neq y \bullet x \neq z \bullet x \neq w \bullet x \neq v \bullet y \neq z \bullet y \neq w \bullet y \neq v \bullet z \neq w \bullet z \neq v \bullet w \neq v \bullet (Hix \vee Hax) \bullet (Hiy \vee Hay) \bullet (Hiz \vee Haz) \bullet (Hiw \vee Haw) \bullet (Hiv \vee Hav)]$[80]

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[80] Compare to Field 1980: Chapter 2 and to Baker 2009: 619.

The contrast between IM, the inference which contains mathematical terms, and IN, the parallel inference which contains no mathematical terms, demonstrates simply the underlying theme of dispensabilist responses to the indispensability argument: some statements which use numbers may be taken as convenient shorthand for complicated statements that do not. There may be statements more complex than those in IM from which mathematical objects are ineliminable. Indeed, this is the ongoing debate over the possibility of constructing dispensabilist projects for all scientific theories. But the eliminability of numbers is uncontroversial in some cases. If someone were to present IM as a reason for believing that there are numbers, anyone would be justified in denying the inference to the existence of mathematical objects by claiming that the uses of numbers in IM are merely casual and proffering IN to support that claim. Whether we believe in mathematical objects or not, IM is not a good reason for believing in them.

IM thus does not reflect our serious commitments. When we want to display our those commitments, we speak most soberly, invoking parsimony and rewriting such casual sentences. We reflectively remove from our language references to sakes and behalves and point-masses and frictionless planes, and to adjectival uses of natural numbers.[81] IN makes it clear that the essential subjects of IM are mangoes, not numbers.

Now, consider the question, "Why are there five mangoes here?" A fine explanation of the presence of five mangoes may be that I brought two and Andrés brought three; the inference IM may be taken to explain its conclusion. IM is not a complete, best theory of mangoes. It requires background assumptions about object constancy and that mangoes do not annihilate each other when, say, there are more than three together. But it is an explanation that will satisfy any ordinary person, and even a philosopher who is not thinking too hard about theories of explanation. In contrast, IN is ordinarily not a very useful explanation of anything. The only way for IN to have any explanatory force for most folks is to translate it back to something like IM.

Both IM and IN have their virtues. IM provides a perspicuous and easily-understood explanation. The conclusion of IN follows from its premises in first-order logic by purely computational means. Noting its formal elegance, a philosopher may insist that the first-order derivability of its conclusion contributes to making IN explanatory. To determine which inference is more explanatory, though, one would have to know more about the nature of explanation.

The philosophical literature on explanation is messy in part because of a natural tension in our concept of explanation, a tension exemplified by the contrast between IM and IN. In an ordinary sense, explanation seems highly contextual, perhaps even pragmatic. What one person takes as explanatory may be incomprehensible (and thus not explanatory) for another person. For example, while one can say that the principles of general relativity explain gravitation, they do no such thing for most of us, since we do not understand the fundamental laws. Still, on plausible models of explanation, general relativity may well explain gravitation. Similarly, for many people, IN lacks explanatory power while IM is explanatory, though the difference may be minimal for those proficient with first-order logic. But IN is the kind of inference on which many models of explanation rely.

On covering-law accounts, for example, the explanation of a state of affairs is an inference involving the laws of a serious theory combined with appropriate initial conditions. The theories to which such explanations appeal are ones in which we hope to speak most strictly, ones which attempt to cut nature at its joints. This is what Hempel called the empirical condition of adequacy for the deductive-nomological (D-N) model of explanation.[82] Refinements of the D-N model, like Railton's

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[81] On sakes and behalves, and the general strategy of speaking austerely, see Quine 1960: §50.

[82] See Hempel and Oppenheim 1948: 248.

model of probabilistic explanation (Railton 1978) or Kitcher's unificationist account (Kitcher 1981), work similarly. Kitcher, for example, invokes unifying argument patterns which also answer why-questions with inferences made by a serious theory. Salmon's causal-mechanical model focuses explanations on real causal processes as opposed to mere statistical generalities.

For proponents of such metaphysical models, the general principles and particular claims invoked by an explanation should be true (or empirically correct). Ordinary explanations may be either shorthand for proper explanations or loose invocations of the term. Proponents of such models of explanation aim to increase understanding for an ideal or sufficiently educated reasoner. But the central focus of such theories is on how they represent the world, not on how they foster people's understanding. Railton, for example, promises, "An account of probabilistic explanation *free from relativization to our present epistemic situation*" (Railton 1978: 219, emphasis added).

Part of the reason that some models of explanation fail to increase our understanding is that they explain lower-level particular phenomena by appealing to more universal and general laws. The laws are often more difficult to comprehend than particular events. Michael Friedman, hoping to improve on D-N accounts by seeking an objective yet psychological account of explanation, noticed this phenomenon. "As a matter of fact, many scientific explanations relate relatively familiar phenomena, such as the reflecting and refracting of light, to relatively unfamiliar phenomena, such as the behavior of electromagnetic waves. If the view under consideration were correct, most of the explanations offered by contemporary physics, which postulate phenomena stranger and less familiar than any that they explain, could not possibly explain" (Friedman 1974: 10).

Some proponents of theories of scientific explanation, especially of the standard D-N model, denigrate, at least at times, that aspect of 'explanation' which concerns actual cognition, actual understanding. Again, Friedman notices the phenomenon. "In some of their writings defenders of the D-N model give the impression that they consider such a task to lie outside the province of the philosopher of science, because concepts like 'understanding' and 'intelligibility' are psychological or pragmatic" (Friedman 1974: 7).

By pointing out that our accounts of explanation do not always aim at being epistemically satisfying, at explaining the unfamiliar in terms of the familiar, I do not intend to criticize them. Indeed, as Toulmin argues, it is essential to explanations that they fail to do so. "If we were to insist on accounting for the 'unfamiliar' in terms of the 'familiar', instead of *vice versa*, we should never be able to shake ourselves loose of Aristotelian dynamics... [A]s science develops, this turns into 'relating the anomalous to the accepted', and so in due course into 'relating the phenomena to our paradigms'. This is inevitable" (Toulmin 1961: 60-1).

Some proponents of standard accounts of explanation attempt to assimilate inference and understanding. To account for residual concerns that explanations should at least have some relation to understanding, the proponent of a standard theory may promise that when we understand the laws or causal structures or unifying principles underlying a phenomenon, we will understand why it occurred. But the attempt to provide an objective account of explanation which is independent of any particular agent remains strong and the attempts to account for human understanding within such a model fall short.

So, there are two distinct senses of 'explanation' on the table. One sense is represented by the formal inference IN and by standard accounts of scientific explanation. While IN is a simple logical inference which suppresses auxiliary presuppositions involving laws governing mangoes, we could easily tidy it up, appealing to the relevant laws. IN could thus play a central role in a formal scientific explanation of why there are five mangoes here. Such explanations seek, above all, to represent the way the world is.

The other sense is represented by the casual inference IM. Such explanations answer, or at least forestall, our why questions. They may refer to idealizations, like frictionless planes. They may explain the height of a flagpole by the length of its shadow, instead of the other way around. They may appeal to sakes or behalves. They may invoke models which work, like any metaphor, only so far. When we invoke metaphors in science or use models for explanation, we do not particularly care whether the objects to which we refer are real ones. For example, we do not think that the atom is literally constructed like the solar system; nevertheless, the image of electron orbits can be a useful heuristic. We know not to take it too seriously, that the metaphor breaks down at some point. Such explanations may be perfectly good answers to ordinary why questions. But they do not purport to track the fine structure of the universe.

When I explain the presence of three mangoes on the table as the result of my bringing four but you having eaten one, or when I explain my actions as having been done for someone else's sake, I may successfully communicate using language which does not reflect the structure of the world. Explanations in this epistemic sense are often agnostic concerning their commitments, including mathematical ones. They can invoke mathematical terms which may be interpreted variously by platonists, fictionalists, or those who believe that mathematical terms are oblique references to other things, like possible arrangements of concrete objects.

Let's call the kind of explanation that IM provides, but that IN does not generally provide, epistemic. Epistemic explanations need not be taken literally. In contrast, we can call the D-N and related kinds of explanation metaphysical for their attempts to get at the fundamental structure of the world. Given their central characteristic, metaphysical explanations are especially apt for the purposes of revealing our ontological commitments, for expressing what we think exists.[83]

While both senses of 'explanation' have legitimate uses, the metaphysical and epistemic senses are incompatible. Epistemic explanations, and epistemic aspects of explanation, are independent of the way the world is without failing, for that reason, to be explanatory. Criteria for good epistemic explanations will include intelligibility to a particular audience and familiarity. They will vary with the audience. Criteria for good metaphysical explanations include getting at the right laws and general principles. They transcend their audience. They are apt for expressing what we think exists. We must take the references of their terms seriously.

It is quixotic to try to capture the contrasting kinds of explanation with a single account. The essential tension in our concept forces any account to one side or the other, toward actual human understanding or toward representing or expressing the fundamental structure of the world.

There are two important claims in this section illustrated by the differences between IM and IN.

Claim 1: We are committed to mathematical objects not by our casual uses of numbers, but only when we are speaking most seriously. The mere presence of mathematics in an explanation or inference is, by itself, no evidence that we should believe in the mathematical objects to which it refers.

Claim 2: The theory we use to specify our ontological commitments may not be most useful when we want to explain facts about the world, in the epistemic sense of 'explain'. Thus, the degree to which a theory is explanatory, whether or not it includes mathematics, may not be proportional to the degree to which we should believe in the objects to which it refers.

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[83] Brown 2012 distinguishes these two senses of 'explanation' without naming them. Salmon 1984 distinguishes epistemic, modal, and ontic explanations, and traces that distinction to Aristotle. Salmon focuses on the D-N model to characterize epistemic explanation because of the expectations raised in us by considering D-N inferences; I take D-N explanations to be metaphysical because of their employment of empirically adequate laws. Salmon also focuses on causation for ontic explanation, a focus which precipitously rules out mathematical explanations.

The central thesis of this chapter is that the explanatory indispensability argument is no improvement on the Quinean argument because it depends on an equivocation between the two senses of 'explanation'. If it appeals to a metaphysical notion, then it is just a restatement of the original argument. The central question is whether mathematical objects can be eliminated from our best theories: can Baker's cicada case, or other supporting examples, be written in a canonical language apt for expressing our serious commitments without mathematical terms? Projects like that of Rizza 2011, which shows how to nominalize Baker's ME7, would be apt responses. Thus, if EI is going to be an enhanced or extended version of the indispensability argument, its proponents must appeal to a different model of explanation. But as soon as the proponent of the explanatory argument gives up the strictly metaphysical sense of 'explanation', she undermines the essential premise, EI2, that we should believe that all of the terms used in our explanations refer.

§6: Epistemic Explanation and the Explanatory Indispensability Argument

To see further how important the epistemic sense of 'explanation' is for the explanatory argument, reconsider the claim, from Lyon and Colyvan, that the explanatory power of phase-space formulations of theories is unrecoverable in nominalist reformulations. On any of the standard interpretations of 'explanation', ones in which explanation is provided by a deductive inference using laws of our best theories as premises, Lyon and Colyvan's claim is false, *ex hypothesi*. Conserving explanatory power is a standard, minimal requirement on nominalist reformulations, and it works unlike other theoretical virtues. One might wonder whether sacrifices in simplicity of ideology are worth parsimony in ontology. But dispensabilists may not give up explanatory power, in the standard sense, in their reformulations. Field constructs representation theorems precisely to support the claim that his reformulation lacks no explanatory power of the standard theory. One just could not successfully nominalize a scientific theory by producing an alternative with less explanatory power unless one is using a different non-metaphysical sense of the term.

Lyon and Colyvan's claim is plausible, though, if we interpret their use of 'explanatory power' in an epistemic sense. Unlike standard scientific theories, dispensabilist reformulations will be imperspicuous, useless to working scientists. Dispensabilists generally do not argue that scientists should adopt the reformulations. Indeed, Field grants that standard theories are more epistemically explanatory by arguing that mathematics is conservative over standard scientific theory, that adding mathematical axioms to nominalist theories will not allow one to derive any further nominalist conclusions. The nominalist wants to show that mathematics is conservative precisely because we are inevitably going to take advantage of the greater epistemic explanatory force of standard theories. Thus, the explanatory indispensability argument, in order to differentiate itself from QI, must rely on a notion of explanation that is not metaphysical.

We can also see that EI depends on an epistemic sense of explanation by noting that standard, metaphysical accounts of scientific explanation do not comfortably apply to mathematical explanation. Many mathematical inferences conform to standard criteria for scientific explanation without being explanatory. For example, one can derive '2+2=4' from basic axioms, but such derivations are not ordinarily taken as explanations. Indeed, the amusement with which we reflect on the fact that it takes several hundred pages in *Principia Mathematica* to arrive at the proof of '1+1=2' speaks directly to the ways in which we take such derivations to be non-explanatory.

Some mathematicians and philosophers of mathematics distinguish between mathematical proofs which are explanatory and those which are not. Often, when there are multiple proofs of a given theorem, mathematicians will compare them according to how well they explain the theorem.[84] Within

[84] See Steiner 1978 and Hafner and Mancosu 2005.

mathematics, it is clear that any evaluation of proofs as explanatory or not, if that is even possible, will have to take factors other than derivability from basic axioms into account. Mathematical explanation may be explicated psychologically or by appeal to unifying proofs, for example.[85]

An alternative approach for understanding the nature of mathematical explanation would be to claim that mathematical explanations of physical phenomena have two parts: a strictly mathematical explanation of a strictly mathematical theorem and a broader explanation of the physical phenomenon which invokes the mathematical theorem. The defender of the explanatory argument could claim that the nature of the pure mathematical explanation is isolated from, and thus irrelevant to, the nature of the broader explanation.[86]

But unless broader explanations are to take mathematical results as brute facts, the nature of purely mathematical explanations is not isolatable. Uses of mathematics in science naturally raise questions about why these results hold: their scope and limits and their relations to other mathematical theorems. While scientists often just want the proper formula or relevant set of differential equations, understanding the relations between one mathematical formula and another is central to understanding how and why the mathematics applies. Narrow mathematical theorems often generalize, from claims about, say, squares to claims about all polygons. The more general a theorem, the broader its applications. It is at least odd to say, as the proponent of EI here imagined does, that we should believe in the existence of mathematical objects because they play an ineliminable explanatory role in science while dismissing the nature of mathematical explanation as irrelevant.[87]

A third option for the proponent of the explanatory argument, faced with my charge that mathematical explanation in the sciences may not be metaphysical, is to claim that there is no such thing as explanation in pure mathematics. Then, the sense of 'explanation' invoked in EI will depend exclusively on the nature of the scientific explanations used to support the first premise. And again, if they are metaphysical explanations, EI is no improvement on QI, but if they are epistemic, we lack a good reason to take the references of that explanation most seriously.

Whether or not the nature of pure mathematical explanation is relevant, the proponent of EI must defend the claim that we should take our mathematical explanations of physical phenomena seriously. If they are metaphysical explanations, the proponent of EI can adopt Quine's argument for the claim. But then EI is no improvement on QIA. If they are epistemic explanations, the argument will be difficult to make: there is no good reason to take the references of epistemic explanations, especially the contentious mathematical references, literally. We need not be fully serious when we provide an epistemic explanation because explanations which facilitate our subjective understanding may not reveal our most sincere commitments.

§7: Weaseling Away the Explanatory Argument (But Not the Standard Argument)

Proponents of EI generally presume that their central challenge is to establish that there are mathematical explanations of physical phenomena, EI1. Even if not all of the examples ME1-ME11 work the way that their proponents want them to work, taken together with the epistemic interpretation of

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[85] Lange 2010 contains a useful discussion in the context of mathematical coincidence.

[86] Thanks to Alan Baker for raising this suggestion in conversation.

[87] Mancosu agrees: "It...appears that a proper account of explanations in science requires an analysis of mathematical explanations in pure mathematics" (Mancosu 2011: §3.2). Baker dissents: "[Science-Driven Mathematical Explanations] do not (and should not) incorporate proofs of the mathematical results to which they appeal" (Baker 2012: 264).

'explanation' they provide a compelling case for EI1. The real problem with such examples is that we want a reason to take such examples as expressing our commitments. EI2, which claims that we ought to be committed to the objects postulated by mathematical explanations of physical phenomena, is problematic for reasons I discussed in §5: we need not be ontologically serious when we provide an epistemic explanation. Once we realize that the sense of 'explanation' relevant in EI is epistemic, any force that EI2 is supposed to have is lost. There is little reason to believe that explanations which facilitate our subjective understanding are ones in which we reveal our ontological commitments by speaking most soberly. EI is thus highly susceptible to weaseling. EI seems plausible if we have a metaphysical sense of 'explanation' in mind; but then it's no improvement on QI.

Leng also argues that EI is susceptible to weaseling, but she derives her response to the explanatory argument from her response to QI. "If the original indispensability argument can be rejected on the grounds that some theoretical components can be good representations without being true (so that 'fictional' assumptions would do the representative work just as well), then the same considerations can be applied in the case of theoretical explanations" (Leng 2005: 187).

Leng relies on Melia's claim that mathematics merely provides a language for representing or modeling physical facts. Such representations need not be ontologically committing. "Nothing is lost in the explanation of cicada behavior if we drop the assumption that natural numbers exist" (Leng 2005: 186). She makes similar claims against several of Colyvan's particular examples, including ME2 and ME4. Such explanations, she claims, do not allow us to infer the truth of the mathematics involved.

> We model the earth as a sphere, and pressure and temperature as continuous functions on the surface of this sphere. Once we have done this, the Borsuk-Ulam theorem can be seen to apply, and, to the extent that are [sic] model is a good one, we can draw a conclusion about the existence of a pair of points on the earth's surface. Does the question of whether the sphere and the functions in our model *really exist* matter to the success of this piece of reasoning? It is hard to see how it should (Leng 2005: 182; see also 179).

While these weaseling claims from Leng and Melia are appropriate responses to EI, they do not extend to QI and so can not derive from a successful objection to QI. We can not expect our epistemic explanations to be the locus of our ontological commitments. But it is reasonable to expect our commitments to be represented by our best theories, in the sense required by QI or by a metaphysical sense of 'explanation'. We constructed IN from IM precisely to be clear about our commitments. Our best theories are our best attempts to get at the structure of the world. My claim that we can weasel away our commitments to EI is not the wanton claim of one who stubbornly denies that we should believe any reference to mathematical objects, even in our most serious theories. It is the more measured claim that only in our best theories can we be most confident in those references.

The weasel denies that we should believe in the objects in the domain of quantification of our best theory. Daly and Langford challenge the weasel in terms of a puzzle about rational belief. "How could it be rational to assert a theory if you believe both that the theory's description of an abstract domain is false and that that description is indispensable in describing the concrete world?" (Daly and Langford 2010: 1115).

Daly and Langford are correct that at some point, we must speak seriously. But since we should not expect our serious commitments to appear in our explanations, the liability of EI to weaseling does not extend to the original QI. Thus, the nominalist can not use the aptness of weaseling to EI as a general strategy for resisting the indispensability argument.

Since EI is susceptible to weaseling, its defenders might re-cast the argument in a Quinean style, including explicit instructions for speaking seriously. Quine's argument, re-cast for epistemic explanations, would say that our ontological commitments are to be found in our best explanations.

QEI
- QEI1. We should believe (epistemic) explanations of our sense experience.
- QEI2. If we believe (epistemic) explanations of our sense experience, we must believe in their ontological commitments.
- QEI3. The ontological commitments of any explanation are the objects over which that theory first-order quantifies.
- QEI4. The explanations of our sense experience first-order quantify over mathematical objects.
- QEIC. We should believe that mathematical objects exist.

But QEI is no help at all to the defender of EI, since the conjunction of QEI1 and QEI2 is implausible. We need only believe our explanations in the sense in which we believe that we can re-cast them in ontologically serious ways. The chips are not down in our epistemic explanations.

If the indispensabilist is tempted to believe in mathematical objects because of an explanation which uses mathematics, the explanation is not doing the work. The work is done by the background claim that there is a good theory supporting that explanation which requires those mathematical objects. Defenders of EI rely on a metaphysical notion of explanation in order to motivate the seriousness of our speech and then switch to an epistemic notion of explanation in order to defend the viability of the claim that there are mathematical explanations of physical phenomena. If there are mathematical explanations of physical phenomena, and we want them to be taken as ontologically committing, we could find a way to fit the explanation into a traditional model so that QI applies and so that the dispensabilist has a fair challenge to respond by removing mathematical references. Alternatively, the indispensabilist can find a way to argue not merely that there are mathematical explanations of physical phenomena, like ME1-ME11, but also that these should be taken seriously, that such explanations are not mere heuristics. Until and unless some such defense is developed, the explanatory indispensability argument is no improvement on the standard one.

§8: Toward an Autonomy Platonist Solution

We speak in ontologically serious tones only in the most austere version of our scientific theory. In a wide range of cases, scientific explanations are better if they include references to mathematical objects. But since our explanations need not appeal to our most parsimonious theories, we need not take even the indispensable presence of mathematical objects in such explanations as ontologically serious.

Still, proponents of the explanatory indispensability argument present a serious case for the claim that there are mathematical explanations of physical phenomena. Even though that claim can not support the argument for which they use it, we might wonder about the importance of such explanations. There are two distinct attitudes that one can take. The weasel nominalist believes that their uses of mathematics give us no reason to believe in mathematical objects. The mathematics plays a representational role, perhaps akin to idealizations in physics. The indispensabilist can trot out compelling examples of applications of mathematics, but the weasel is really a mule, refusing to admit that any of these uses of mathematics are worth taking seriously.

The strength of the standard Quinean argument, which Field realized but which more recent nominalists seem not to realize, is that we must, at some point, speak seriously. To Daly and Langford's claim that it is irrational to assert a theory without believing its description of mathematical objects, Melia responds that our expressive resources may be too impoverished to say what we want to say without invoking mathematics. "There is no a priori reason to suppose that the physical systems we wish to represent can *always* be characterized intrinsically using languages and theories that make no reference to the hypothesized abstract structures" (Melia 2010: 1119). But then, how do we determine the commitments of a theory?

The weasel thus seems to propose to return to the pre-"Two Dogmas" point when philosophers questioned the existence of electrons because we couldn't see them directly. The strength of Quine's indispensability argument arises from his proper insistence that we cannot invoke the physicist's theoretical commitments to electrons as reasons to believe in electrons without also being serious about the references to mathematical objects used in the same theories.

In his 1946 Harvard lecture on nominalism, Quine coins the term 'struthionism' to apply to those like Carnap (and now Melia and Leng), who refuse to take references to mathematical objects within serious theories seriously. 'Struthionism' has the Greek word for ostrich at its core, so there's a third creature in the nominalist's menagerie: weasels, mules, and ostriches.

I have defended these ragtag creatures in their claims that our explanations of physical phenomena need not impel us to believe in the referents of the mathematical terms they use. But there is still something uncomfortable about weaseling, despite Melia's assurances that we can take back some portion of our serious assertions. Consider again Colyvan's mountaineering example, ME5. The explanation of the existence of a point on the mountain which the hiker passes at the same time on consecutive days includes a topological fixed point theorem. In siding with the weasel, I argued that such mathematical explanations give us no reason to believe in the existence of mathematical objects. We are still left wondering, though, whether we should believe in their existence and whether we can learn anything from the fact that there are satisfying mathematical explanations of physical phenomena.

Explanations do seem to be more convincing if they do not refer ineliminably to fictional objects. Moreover, if we had an alternative justification of our beliefs in mathematical objects then we could avoid seeing explanations such as ME1 - ME11 as appealing to fictional objects without taking their invocations of mathematical objects as the grounds for our mathematical beliefs. It is not that the explanations themselves should be taken as ontologically serious. I am suggesting that an explanation which refers to fictional objects is less compelling than one which we can take fully literally.[88] Those who believe, like Field, that the indispensability argument is the only plausible justification of mathematical beliefs are stuck either denying mathematical beliefs, and thus seeing scientific theories as equivocal, or accepting the unfortunate consequences of Chapter Five.

Fortunately, we can maintain that the explanatory indispensability argument gives us no reason to believe in pure mathematical claims while also saying that our best explanations may use mathematical claims seriously. That is the position of the autonomy platonist.

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[88] Christopher Pincock also wonders about the status of mathematics used in science without, with the indispensabilist, taking it as justifying our mathematical beliefs. "I will not argue here for the strong claim that the only way to accept these theories is to be a platonist about mathematics, but only for the comparatively weaker claims (1) that the presence of mathematics in our best confirmed scientific theories forces us to offer some account of the subject matter of mathematics and (2) this account, whether it turns out to be platonist or nominalist, must be a realist account that assigns the statements of pure mathematics truth-values that accord with mathematical practice" (Pincock 2007: 265).

Chapter Eight: Motivating Autonomy Platonism

§1: From Indispensability to Autonomy Platonism

Mathematical platonism is among the most persistent philosophical views. (It's *plato*nism, after all.) Our mathematical beliefs are among our most entrenched. They have survived the demise of millennia of failed scientific theories. And unlike scientific theories, mathematical theories are, once established, rarely rejected, and never for reasons of their inapplicability to empirical science.

The purpose of this book is to contrast two different kinds of mathematical platonism: one based on the indispensability argument and one which takes the justification of our mathematical beliefs to be independent from, or autonomous of, our empirical beliefs. Chapters Two through Seven examined the indispensability argument from the perspective of a platonist who is asking whether and how her mathematical beliefs can be justified. I hope that it is clear that the indispensability argument fails to do so. The committed platonist must look for an alternative, autonomous view. The platonism-curious philosopher should explore other options.

I use 'autonomy platonism' for a position on which mathematical objects exist; some mathematical claims are true, but not vacuously so; and, most importantly, our knowledge of these objects and truths does not depend on our knowledge of empirical science. In the remainder of this book, I will defend autonomy platonism as the best account of these well-known phenomena and show that a proper epistemology for autonomy platonism need be neither mystical nor spooky.

A significant motivation for autonomy platonism is its ability to avoid the Unfortunate Consequences of Chapter Five:

- UC1 Restriction: The indispensabilist's commitments are to only those mathematical objects required by empirical science.
- UC2 Ontic Blur: The indispensabilist's mathematical objects are concrete.
- UC3 Causality: The indispensabilist's mathematical objects may have causal powers.
- UC4 Modal Uniformity: The indispensabilist's mathematical objects do not exist necessarily.
- UC5 Temporality: The indispensabilist's mathematical objects exist in time.
- UC6 Aposteriority: The indispensabilist's mathematical objects are known a posteriori.
- UC7 Methodological Subservience: Any debate over the existence of a mathematical object will be resolved, for the indispensabilist, by the needs of empirical theory.

Because, unlike the indispensabilist, the autonomy platonist does not restrict her justification for mathematical beliefs to the needs of empirical science, Restriction, UC1, is not a problem. Our mathematical commitments may be to those objects to which our best mathematical theories refer.

The autonomy of mathematical justification entails that there is no reason to assimilate mathematical objects with concrete ones; they are best taken, as is natural, to be abstract, with no spatio-temporal locations and no causal efficacy. Thus the autonomy platonist can avoid UC2, UC3, and UC5. While autonomy platonists may disagree about whether mathematical objects exist necessarily, the question of their necessity is not restricted by considerations of the contingency of the objects of empirical science; Modal Uniformity, UC4, is not inevitable. I will return to the question of necessity when contrasting two kinds of autonomy platonism, in Chapter Nine.

Similarly, the autonomy platonist need not adopt a strictly empiricist epistemology and suffer UC6, Aposteriority. The autonomy platonist is free to adopt an account of the justification of our mathematical beliefs which is naturally *apriorist*, as long as it is not spooky or mystical. Lastly, and obviously, the autonomy platonist adopts a strictly mathematical methodology in debates over mathematical claims. In asking about which large cardinal axioms to adopt, for example, the autonomy platonist looks for ways in which set theory can be naturally and intuitively extended without inconsistency.

Further regarding UC7, the indispensabilist paints an especially inaccurate picture of mathematical practice, one which is far better captured by autonomy platonism. The continuum hypothesis provides a clear example of how mathematical practice conflicts with the indispensabilist's philosophy of mathematics. According to the indispensabilist, mathematical questions are to be answered by examining our best (naturalist) scientific theory. Our current mathematical axioms do not settle the question of the size of the continuum, and provably so. Gödel famously suggested that we adopt new axioms to settle the question. The question arises about how to decide which axioms to adopt. The indispensabilist claims that the answers are to be found in the needs of our best scientific theory. But it is unlikely that the size of the continuum will be settled by developments in physics. Even if physical science did require that the continuum were, say, $\aleph_2$, mathematicians would be unlikely to adopt that result unless it were motivated by concerns beyond those of physics. The mathematician looks to purely mathematical criteria for such answers, and not to empirical science.

A further consideration motivating autonomy platonism arises from considerations in Chapters Two through Seven, but especially of the explanatory indispensability argument. While I have argued that the inference from our uses of mathematics in science to the truth of mathematical claims is invalid, it does seem that scientific theories and explanations are more satisfying when they include references to mathematical objects. The autonomy platonist, by distinguishing evidence for our mathematical beliefs from the presumption of truth for mathematical claims made within scientific theories, can account for this phenomenon without adopting an indispensability argument and its unfortunate consequences. For the indispensabilist, the desire for standard semantics forces us to commit to mathematical objects. But the desire for standard semantics should not compel our belief; it is merely is a wish that there are objects to stand for mathematical terms.

Let's imagine that the autonomy platonist can provide a justification for the beliefs in mathematical objects sufficient for the needs of science: sufficient for the construction of scientific theory and for scientific explanations. Such a justification will not depend on the uses of mathematics in science, *ex hypothesi*. Still, when the autonomy platonist refers to mathematical objects in a scientific theory, she is able to see such references as unproblematic, unlike the fictionalist, and unlike the indispensabilist, who rejects autonomous justifications. When the autonomy platonist is doing science, she can appeal to mathematical machinery. She can refer sincerely to Hilbert spaces and real numbers. But, she need not infer mathematical knowledge from these uses, and we need not so infer. We are already justified in believing that those mathematical objects exist.

Like the weasel, the autonomy platonist denies that we can justify our mathematical knowledge on the basis of our uses of mathematics in science. But we need not deny the existence of mathematical objects and we need not give up an explanation of why mathematics is applicable to physical phenomena. A denial of the inference to mathematical objects from their indispensable uses in science is compatible with the indispensabilist's claim that our best scientific explanations are more convincing if they refer only to real objects.

Thus the argument for autonomy platonism is distilled at AP.

AP

AP1. We have mathematical knowledge.

AP2. If we have mathematical knowledge, it is either justified exclusively by the uses of mathematics in science or is at least in part autonomous.

AP3. Our mathematical knowledge is not justified exclusively by the uses of mathematics in science.

APC. Thus, at least some mathematical beliefs are justified independently of empirical science (i.e. autonomy platonism).

AP3 is established by the failure of the indispensability argument. AP2 is uncontroversial. AP1 remains a worry.

Some philosophers, seeing the problems facing indispensabilism, reject platonism. For them, the indispensability argument makes platonism acceptable because it seems to provide metaphysical and semantic benefits with no epistemological cost: a platonist ontology with an empiricist epistemology. Such a position is too good to be true. It depends on controversial assumptions like holism and naturalism. Its yield is limited and fails to characterize mathematics properly, as the Unfortunate Consequences show. It purports to yield platonism on the basis of experience which is contingent.

Thus, if the indispensability argument fails to establish platonism, those unwilling to expand their epistemology to account for our apprehension of pure mathematics, may adopt an anti-platonist ontology, perhaps embracing fictionalism or a reinterpretive strategy. 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. The most important opponent of the autonomy platonist, then, is the anti-platonist. In Chapter 1, I said that I would not say much about anti-platonist views of mathematics. Still, if you're with me so far, and accept that the indispensability argument is untenable as a defense of platonism, you might be tempted to adopt such a view. A few words about anti-platonism are thus in order.

§2: From Fictionalism to Autonomy Platonism

The most important defender of mathematical fictionalism is Hartry Field. In this section, I discuss three salient 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. Field's arguments for fictionalism, which denies that mathematical objects exist, really only establish 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 false, then both of these claims are equally false. The former is true in the standard story of mathematics, perhaps because it follows from the Dedekind-Peano postulates, and the latter is false in the standard story of mathematics. But the standard story of mathematics is itself false.

Like the indispensabilist, the fictionalist can distinguish the two sentences by their applicability. For the indispensabilist, the applicable one is true while the other is false. For the fictionalist, the former is merely more useful than the latter. '2+3=5' is no less false than '2+3=6' since there are no mathematical objects; it is merely more useful. This distinction in utility fails to track an important mathematical distinction. Mathematical theories which are inconsistent with standard mathematics may be also useful, just as false scientific theories may be useful. If we attempt to replace the distinction between mathematical truths and falsities with a distinction based on utility and application, assimilating mathematical truths and mathematical falsities, we will get the distinction wrong.

The fictionalist also has difficulty explaining mathematical progress. Consider the proof of Fermat's theorem. 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. The fictionalist can only credit Wiles with advancing our logical knowledge, a knowledge that the theorem follows from certain axioms. That is a weakened account of what we learned and does not do the proof credit. It abandons all questions of why we choose particular sets of axioms or at least defers them, as the indispensabilist account does, to questions of application and empirical science.

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 mathematical possibility. In contrast, we do not have full freedom to say whatever we like in mathematics. A 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 mathematically 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 suggests? Or metaphors, as Stephen Yablo suggests? 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.[89] 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.

These problems apply to any fictionalism. An additional problem arises for any version which takes the non-existence of mathematical objects to be contingent. Field and others grant that mathematical objects could have existed. For Field, the contingent non-existence of mathematical objects is a consequence of his use of an object-level modal operator; he represents our knowledge of the consistency of the axioms of ZF as‘$\Diamond AX_{ZF}$’.[90] But 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.[91] Field’s modal account renders this

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[89] Conservativeness is the claim that the addition of mathematics to a theory which contains no mathematics should license no additional nominalistically acceptable conclusions. More formally: Let A be any nominalistically statable assertion, N any body of such assertions, and S any mathematical theory. Take ‘Mx’ to mean that x is a mathematical object. Let A* and N* be restatements of A and N with a restriction of the quantifiers to non-mathematical objects. 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. ‘∃x~Mx’, that there is at least one non-mathematical object, is a technical convenience. See Field 1980: 10-16.

[90] See Field 1984.

[91] Azzouni argues that nothing would change if mathematical objects ceased to exist, in Azzouni 1994: 56. David Lewis denies that we can say anything sensible about how the world would be if there were no numbers (Lewis 1986: 111).

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.

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: 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 platonist, in fact, can provide a better account of application, since it is broader. Autonomy platonism is best able to account for new applications of mathematics since it provides whatever mathematics might be needed in science.

Field argues that he can best account for the application of mathematics to empirical science, through appeal to conservativeness. For Field's fictionalist, mathematics is applicable to science because it is merely a convenient shorthand for claims which are properly empirical, about the structure of space-time for example. He constructs representation theorems to show that the quasi-mathematical theory based on space-time yields the same theorems as standard real mathematics. As evidence of the virtues of his account, Field cites Michael Friedman as having rejected Field's nominalism, while, "[E]ndorsing its account of the applications of mathematics" (Field 1985a: 191).[92]

Field recognizes, though, that the platonist can also take his representation theorems as an account of application. The platonist does not deny the legitimacy of the fictionalist's tools; he just has more.[93] If Field can develop representation theorems for all applications of mathematics, then the platonist and the fictionalist can use the same account.

The autonomy platonist, *ex hypothesi*, has an epistemology for mathematical objects as well as an independent account for empirical objects. For FBP, for example, our knowledge of mathematical objects arises from our ability to recognize contradiction and 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.

The platonist's central complaint about fictionalism is its denial that many mathematical claims seem to force themselves on us, beyond their applicability to empirical science. 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: 483-484; see also Putnam 1994: 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

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[92] Friedman actually says, "There is no doubt that it is a major *contribution* to our understanding of applied mathematics" (Friedman 1982: 506), which need not be interpreted as endorsing the account.

[93] Actually, on his own terms, Field does not explain application. The representation theorems are not available in the official, first-order version of his theory. See Shapiro 1983: 529-530.

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 in much the same way that Alexander cut the Gordian knot: 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: 207)

The autonomy platonist may demonstrate similar largess without giving up an account of content, accepting multiple set-theoretic universes, each with different objects, each with different theorems true of it. For the fictionalist, the lack of content allows us to say whatever we want. For the platonist, there are stakes to, say, positing multiple set-theoretic universes. There are constraints on theory-building in mathematics that prevent us from saying whatever we might want. Such constraints lead us to autonomy platonism.

§3: Mathematics as Logic?

Autonomy platonism is not the only alternative to fictionalism or indispensability platonism, though it is the one I pursue in the remainder of this book. One set of options are reinterpretive, mainly taking mathematical terms to be oblique references to modal properties. Such proposals are essentially fictionalist, denying the existence of mathematical objects and changing the subject of mathematical sentences. Modal accounts make little epistemic headway, trading knowledge of mathematical objects for knowledge of possibility, a perhaps-equally intractable problem. I will not consider modal interpretations since I believe, and will argue, that an autonomy platonist epistemology is even less contentious than an epistemology of modal properties.

More promisingly, some philosophers look to minimize the existential content of mathematics by taking mathematical knowledge to be, at root, logical knowledge. Attempts to take mathematical theories as complex logical theories trace back to Frege's logicism. Frege believed that by assuming some modest logical principles and developing a gap-free formal system of inference, he could both put arithmetic on a firm ground and solve any epistemological problems facing it. Unfortunately for Frege's project, the modest logical principles, in the guise of his Rule V (a version of Cantor's unrestricted axiom of comprehension), turned out to be inconsistent, as Russell showed. Russell's paradox deterred Frege from completing his work by showing that his purportedly logical fundamental principles had substantial mathematical content. Nevertheless, Whitehead and Russell persisted, helping to develop and apply axiomatic set theory. Contemporary work on neo-Fregean philosophies of mathematics have continued to refine and circumscribe the content required to ground axiomatic set theory and higher-level mathematics.[94]

I want to make three quick comments about neo-logicist projects. First, the technical

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[94] Work by Crispin Wright, Bob Hale, and Richard Heck have showed decisively that while Frege's Rule V leads to paradox, the majority of Frege's project can be carried out by just assuming what is variously and somewhat misleadingly known as Hume's principle or just HP. That assumption is essentially the claim that cardinality is to be understood in terms of one-one functions. No one, though, claims that Hume's principle is a simple logical principle.

developments of such projects have succeeded in casting important light on the foundations of mathematics. Second, even a quick glance at that work dashes any hopes of solving the epistemological problems facing platonistic mathematics by appealing to an uncontentious logic. Last, as Frege knew, grounding mathematical knowledge in logical knowledge does not eliminate the problem of explaining our knowledge of mathematical objects. This problem appears in the work of Frege and the neo-logicists as (or perhaps as a facet of) the Caesar problem. Mathematical objects exist in what Frege called a third realm: they are neither concrete objects nor mental objects. Neo-logicists should welcome an autonomy platonistic account of our knowledge of mathematical objects, since it could help respond to charges that knowledge of the relevant logical principles does nothing to explain how we could have knowledge of the abstract objects of mathematics.

Other attempts to ground our knowledge of mathematics in our knowledge of mathematics are more closely related to Field's work, an aspect of which is sometimes ascribed to Hilbert as well. On this view, mathematical knowledge is knowledge of which theorems follow from which others.

The unfortunate consequences of the indispensability argument do not extend to our knowledge of many logical theories, especially those, like standard first-order logic, which make no existence claims. Even weak logical theories, though, afford us some tools which might ordinarily be thought to require mathematics; one might make some headway in accounting for mathematical knowledge by examining logical knowledge. We need logic to govern inferences anyway.

The extent of the mathematics attainable from logic depends on which logic one takes as canonical. Propositional logic provides few mathematical resources. From first-order logic, if we include an identity predicate and its standard governing axioms, we can generate logical dopplegangers for finite natural numbers. Specifically, we can generate finite cardinality quantifiers, allowing us to distinguish the sizes of different finite collections. We can say that there are three, four, or seventeen thousand blue vases in the showroom, or planets in the galaxy.

Other resources for those attempting to avoid a truly platonistic ontology include concrete templates. Templates are and represent concrete patterns and can perform practical geometric tasks by serving as blueprints, allowing us to measure and design spaces.[95] But concrete templates and cardinality quantifiers are insufficient if scientific theory requires even the full theory of natural numbers, let alone analysis. We can not represent continuity or infinite sizes.

With stronger logic, one can accomplish more of the tasks for which mathematics is ordinarily used in science. George Boolos and Hartry Field, for example, have explored a logic of plural quantification, one which is equivalent to a mereological theory, to handle collections of space-time points and regions.[96] Stewart 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. Field's logic was criticized for being too mathematical; mereological axioms entail theorems too substantial to be called logical.[97] We can see the objects generated by mereological theorems and second-order logic as replacements for sets and other mathematical objects only because we already possess profound information about mathematics. We take '$\{\{\varphi\}\}$' as a two, for example, on the basis of

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[95] See Resnik 1997: 226 ff.

[96] See Field 1980: Chapter 9; and Field 1989: Chapter 3.

[97] See Malament 1982, Shapiro 1983, and Field 1984a: 141. More generally, Quine 1986 famously derided second-order logic as set theory in sheep's clothing.

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 claim that we can substitute logical theories for mathematical ones. Anyone who attempts to restrict her ontology by adopting stronger logics would disingenuously pretend to eliminate mathematics while using mathematical knowledge as a constraint on the adoption of a logic and the consequent 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. 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. If our knowledge of mathematics truly depended on its indispensable utility for empirical science, we seem unable to justify any mathematical knowledge at all.

Furthermore, accounts of our knowledge of logic typically appeal to mathematical theories in their metalanguages. We use set theory to model first-order logic and any logic strong enough to do the work for science. So, even appeals to an ontologically uncontroversial logic seem to entail commitments to mathematics.

The mathematical platonist should resist reliance on logical substitutes for mathematics. Any strong logic hides mathematical claims and requires mathematics for its interpretation. Scientists need more than first-order logical machinery, whether in the guise of mereological axioms which govern the structure of a substantivalist space-time or in the guise of traditional mathematical theories. And mathematicians explore universes which far outstrip the needs of scientific theories, in any case.

We have seen reasons to prefer autonomy platonism, to indispensability platonism, to fictionalism, to logicism and related projects. Much of the dialectic of this chapter and the foregoing ones, though, is predicated on an assumption of an acceptable epistemology for abstract objects. The challenge to provide an epistemology for abstract objects, one consistent with our epistemology for concrete and ordinary objects and one which is neither spooky nor mystical, is long standing and has appeared intractable. It is, of course, the main motivation for all views of mathematics other than platonism. The Benacerraf-Field problem I discussed in Chapter One is merely the contemporary locus for the eternal debate.

And yet I believe that the challenge is not so difficult to meet. Ironically, one key to meeting it appears in Quine's work in his view of ontology as the result of theory construction and modeling, i.e. positing.

In the next chapter, I describe two versions of autonomy platonism. The first is Mark Balaguer's FBP. The second is my preferred version.

Chapter Nine: Two Versions of Autonomy Platonism

Any justificatory account of mathematical beliefs which is both platonistic and does not appeal to the uses of mathematics in science for that justification is a version of autonomy platonism. Thus there are varieties of autonomy platonism. Plato, Descartes, Leibniz, and Gödel can all be naturally interpreted as autonomy platonists. Locke and Hume share some aspects of the autonomy platonist's view that mathematical claims retain their truth independent of their uses in science (or, less anachronistically, in accounts of our sense experiences). For the present, we can divide autonomy platonisms into two categories: those that rely on a capacity of mathematical intuition as part of their account of our knowledge of mathematics and those that do not. Among the former is the autonomy platonism of Jerrold Katz; among the latter is Mark Balaguer's FBP.

The version of autonomy platonism which I believe is most defensible relies on mathematical intuition. This version best captures commonsense beliefs about mathematics, best represents a tutored understanding of the relationship between mathematics and science, and best captures the ways in which mathematics is practiced. Because of its reliance on mathematical intuition, sometimes seen as a mystical pseudo-capacity, intuition-based autonomy platonism is controversial. In this chapter, I first discuss some problems with Balaguer's non-intuition based view. Then, I describe my favored account and defend it against charges that intuition is mysterious. This defense involves invoking some circular reasoning. In the next chapter, I defend this kind of circular reasoning more generally.

§1: Plenitudinous Platonism

Balaguer's plenitudinous platonism, or FBP, claims that every consistent set of mathematical axioms truly describes a universe of mathematical objects.[98] Certain mathematical questions, like the question of the size of the continuum, have no determined answer. Some such questions, like whether Goldbach's conjecture is true, seem clearly to have a correct answer; either there is a large even number which can not be written as the sum of two primes or there is not. Indeed, most mathematicians are convinced that Goldbach's conjecture is true even in the absence of a deductive proof of its truth.

Other mathematical questions elude consensus. Consider the axiom of projective determinacy, which says that all projective subsets of the set of functions from ω (the set of all natural numbers) to ω are determined. (The projective sets are obtainable from Borel sets by repeatedly taking complements and images.) Projective determinacy follows from the existence of infinitely many Woodin cardinals and from some other large cardinal axioms. If the axiom is true, then many open questions about projective sets which are left undecided by ZFC are settled.[99] The elegance and strength of such results motivate some set theorists to accept the axiom of projective determinacy. But the axioms of ZFC are intuitively pleasing to many mathematicians while large cardinal axioms are far less universally lauded. Moreover, one might believe that at least some of the questions left unsettled by ZFC are really open, and not just because the axioms of ZFC are too weak. Perhaps some apparently well-formed questions in mathematics are, unlike Goldbach's conjecture, neither true nor false. Also, some large cardinal axioms, in particular ones which yield the too-strong axiom of determinacy, are inconsistent with ZFC, which is good evidence against them. How large is the set theoretic universe? The answer, if there is a unique answer, eludes us.

A related and better-known case concerns the size of the continuum. Gödel famously claimed that the continuum has a unique size. Developments in recent decades, especially Cohen's model-theoretic proof of the independence of the continuum hypothesis from the standard axioms of set theory,

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

[99] See Martin and Steel 1989: 72. See also the useful discussion in Maddy 1988b: §VI.

have undermined Gödel's claim. Various different and conflicting claims about the size of the continuum are all consistent with standard set theory. One could adopt stronger axioms (e.g. the existence of Woodin cardinals) which settle the question univocally. But, it has seemed to some set theorists that no unique answer is yet warranted.

FBP is largely motivated by and accommodates the view, held by some mathematicians and philosophers in response to the openness of such questions as the size of the continuum and the truth of the axiom of projective determinacy, that there is no fact of the matter about certain mathematical questions. On FBP, ZF + CH and ZF + not-CH each truly describe real set-theoretic universes, despite their conflicting claims. Our ignorance of the size of the continuum or the truth of the axiom of projective determinacy is, on this view, no accident. It is, in particular, not the consequence of a mere lack of evidence. It is the result of there being many sizes of the continuum and there being no fact of the matter whether the axiom of projective determinacy is true.

Interestingly, plenitudinous platonism is inspired by, and closely related to, Field's fictionalism. On Field's view, "[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: 320.)

The proponent of 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, the proponent of FBP believes that the mathematician is free since all his theories are true. In either case, truth is no constraint on our estimation of mathematical theories. The fictionalist says that statements of the different sizes of the continuum do not conflict because none of them are true. The plenitudinous platonist says that there are diverse set-theoretic universes.

Both the proponent of FBP and the fictionalist, then, make claims akin to Resnik's Euclidean rescues. Just as there can be several equally-good geometrical theories, there can be several equally-good set-theoretic universes, each with their own defining set of axioms. The fact that these geometries or set theories are inconsistent when taken together is, for Field and Balaguer, no evidence against any of them either individually or taken together. We need not choose among conflicting theories because their relative inconsistency is no evidence against them.

FBP and fictionalism thus recommend distinctly different attitudes from traditional platonism and standard mathematical methods toward open questions. Mathematicians are rarely deterred from seeking a solution to an open question by philosophical speculation that the question may be open in principle. The traditional platonist, like Gödel, approaches such Euclidean rescues in mathematics warily, preferring to find a unique answer. Open mathematical questions may seem unanswerable only to be later acclaimed true or false. Some of the questions which motivate FBP will provably require adjustments to well-entrenched and intuitive axioms. But axioms have been adopted and ceded before. The traditional platonist, seeking unique answers to open questions, may see FBP as precipitously abandoning well-formed questions.

Let's put aside fictionalism to focus on the autonomy platonistic view. Insofar as FBP countenances the existence of mathematical objects and the truth of many mathematical claims, it is a platonistic view. FBP is a variety of autonomy platonism because only the consistency of the axioms, and not their applications in science, determines whether they are acceptable, whether they truly describe a mathematical structure or universe. It is not an indispensabilist view and does not suffer all of the unfortunate consequences as a result of being an indispensabilist view. Still, I have two objections to FBP.

My first worry about FBP is that it generates too many objects in too many true theories. By claiming that any consistent set of mathematical axioms truly describes a mathematical universe, it portrays questions which are really open as closed. FBP provides no satisfactory account of our focus on preferred theories and interpretations, as on the standard model of the Peano postulates. Every model of the axioms, on FBP, is equally acceptable. Focus on the standard model is explained by the proponent of FBP sociologically and by its ubiquity. As an autonomy platonist, Balaguer does not believe that the applicability of a mathematical theory grounds its truth. It merely explains why we tend to be interested in some theories rather than others. Balaguer claims that our interest in one set theoretic universe or other is no evidence of its mathematical superiority.

But mathematicians not only focus on the standard model as a mere matter of sociological pressures. They also believe it to be the correct model. Not every consistent mathematical theory is a good mathematical theory. There are gradations among different consistent theories, differences which derive from their mathematical properties. Some theories are more elegant, more unifying, or more predictive of further mathematical results than others. Consistency is a minimal requirement for mathematical goodness, but it is not a sufficient condition.

FBP only requires apprehension of consistency as a guide to the truth of a mathematical theory. This aspect of FBP makes it minimally problematic epistemically, since we obviously have a capacity to derive and recognize contradictions. If consistency were a sufficient condition for mathematical truth, then FBP would be a sufficient version of autonomy platonism. But I believe that FBP is an insufficient account of our views of mathematics. It would be nice to think that we need no capacity other than an ability to recognize contradictions to ground our estimations of various mathematical theories. Our mathematical practice belies this fantasy. Justifying our interest in a standard model, if it is not to rest on application, will require appeal to some capacity to distinguish between consistent but untrue mathematical theories and consistent theories which are also true. In describing this capacity, I will appeal to a version mathematical intuition. FBP avoids invoking any kind of contentious intuition. But it provides an unsatisfying answer to the hard but important question of why we privilege certain systems on mathematical grounds.

§2: FBP and Necessity

My second objection to FBP concerns its denial of the necessity of mathematical claims. For the indispensabilist, mathematical objects exist as contingently as the physical world; this Unfortunate Consequence pushes us toward autonomy platonism. But if we adopt FBP as our autonomy platonism, then, according to Balaguer, we still suffer from Modal Uniformity: mathematical objects do not exist necessarily; mathematical claims are true only in those worlds in which mathematical objects exist and those worlds are a proper subset of the possible worlds.

Balaguer defends the contingent existence of mathematical objects. He argues that some platonists, like Jerrold Katz and David Lewis, needlessly invoke necessity to ground their epistemology. Such philosophers argue that since mathematical objects exist necessarily, there is no need to account for the conditions of their existence. But appeals to necessity are both otiose for Balaguer and ungrounded, in part because of the obscurity of the concept.

> 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 It just doesn't seem to me that there is any interesting sense in which ‘There exists an empty set’ is necessary but ‘There exists a purple hula hoop’ is not (Balaguer 1998: 44-45; see also pp 166 et seq.).

In contrast, there are important and interesting differences in the modal status of the two claims. That there is a purple hula hoop depends on facts about the world over which we have some control. We have some interactions in events that result in the creation of hula hoops and we can put together a plan for the eradication of hula hoops. We can explain what contingent facts are contingent on. But such explanations are absent in the mathematical and logical cases: we can't say anything about what differences could yield the existence or non-existence of mathematical objects. Nothing we do or could do has any effects on the existence or non-existence of mathematical objects.

I do not know what would constitute the definition of metaphysical necessity Balaguer demands beyond the (metaphorical or literal) characterization of necessity as truth in all possible worlds. Balaguer rightly argues that mathematical necessity must be distinct from logical necessity if we take a standardly narrow view of logic. Mathematical truths might be conceptually necessary, depending on one's view of concepts. But the account in terms of possible worlds seems clear enough: mathematical objects exist in all possible worlds; true mathematical claims are true in all possible worlds and false ones are false in all possible worlds. Indeed, it is this claim that Balaguer denies by claiming that mathematical claims are not necessary because nominalistic worlds, worlds without mathematical objects, are possible.

Balaguer's claim (a), that it is epistemologically useless to claim that mathematical claims are necessary, might be right, though it is not far from Balaguer's own claim, perhaps indistinguishable from it. 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 could help the FBPist explain why consistency entails truth. So the claim of necessity can be useful.

There are at least two reasons to want an account of mathematics on which mathematical objects exist necessarily. Besides the first, the Katz-Lewis view which uses necessity to ground an epistemology, one might merely wish to account for the commonsense belief that there is a difference between what might have been different and what could not have been different. One of the goals of the autonomy platonist is to accommodate that traditional view. If one believes that mathematical objects do not exist necessarily, then FBP might be preferable to a necessitarian account. But I believe that the traditional view is, on the whole, more satisfying, as I hope to show.

Balaguer agrees that the claim 'if there are numbers, then three is prime' is necessary. His main focus is on the claim that mathematical objects exist necessarily. For Balaguer, nothing exists of necessity and there could have been nothing.

> (a) [C]orresponding to every way that the physical world could be set up, there are two different possible worlds, one containing abstract objects and the other not; and (b) if we were "presented" with a possible world, we wouldn't know whether it was a world containing abstract objects or a physically identical world without abstract objects, and what's more, we wouldn't have the foggiest idea what we could do in order to figure this out. The reason for this, if I am right, is that for any such pair of physically identical worlds, we don't know what the difference between them really amounts to (Balaguer 1998: 166)[100]

[100] Balaguer's view might be clearer here: "It's at least possible that nominalism is true. If you think nominalism isn't even possible, then you owe an explanation of WHY it's not possible. And absent such an explanation, it looks like the claim that math is necessary is just an unmotivated claim to the effect that it's just a brute fact about the space of possible worlds that there's no possible world where it seems that there should be one" (Personal correspondence, 5/16/2012).

Again, Balaguer FBP is close to Field's fictionalism. For Field, mathematical objects do not exist, but their existence is (logically) possible.

From the necessitarian point of view, it is difficult to see what kind of explanation for the necessity of mathematical claims one could provide. Causal explanations are out. Abstract objects are not governed by the laws of physics and so can have no explanations of the sort we provide for contingent non-existence claims. Purely mathematical explanations generally yield conditional claims: on the basis of certain axioms or assumptions, certain other theorems follow. We can explain why it is impossible for there to be a set such that its power set is the same size as itself, for example, only on the assumption of the existence of sets. It seems as if this is just the kind of question that doesn't have a good answer, like 'Why is there something rather than nothing?'.

But either mathematical objects exist in all (or no) possible worlds or there are no good arguments for either their existence or their non-existence. In other words, if necessitarianism is false, then there are no reliable arguments for either platonism or fictionalism. To see this, let's imagine, with Balaguer, that there are some worlds in which mathematical objects exist and others in which they do not. Consider our world and another just like ours and imagine that in one of these two worlds there are mathematical objects while the other is nominalistic; otherwise they are identical. By symmetry, we can imagine that we are in the nominalistic world. In our world, people who believe that there are perfect numbers believe something false. In the other world, they (or their counterparts) believe something true.

Persons in both worlds have mathematical beliefs, *ex hypothesi*, but they don't know whether to be fictionalists or platonists. We and our dopplegangers in the platonistic world have exactly the same reasons on both sides; any evidence is the same in both worlds. Since there's no difference in the concrete aspects of the two worlds and we are, let's presume, exhaustively concrete beings, any way of coming to know which world we are in is present in both worlds. So any argument for platonism or fictionalism is unreliable. Even if it were sound in our world, the same argument could be asserted in the doppleganger world; in at least one world it will be unsound.

Conversely, let's imagine that there are reliable arguments for either a contingent platonism like FBP or a contingent fictionalism like Field's. Such an argument would hold in any possible world, since the evidence in any world would be the same. And since the argument is reliable, we should presume its soundness in any world. If it is a platonist argument, then we should conclude platonism in any world. If it's a fictionalist argument, then we should conclude fictionalism at any world. So either there are no worlds in which mathematical objects do not exist or there are no worlds in which they do exist, contradicting our assumption. So there are no reliable arguments for contingent platonism or contingent fictionalism.

Thus every argument for either platonism or fictionalism in a universe in which there are some worlds with mathematical objects and some worlds which lack them will be unreliable. Even if an argument is sound in a particular world (i.e. it concludes fictionalism in a nominalistic world or it concludes platonism in a platonistic world), there will be other worlds in which the exact same argument can be asserted and in which it is unsound.

I've assumed, in both scenarios, that there are some reliable arguments for either platonism or fictionalism. Maybe there aren't any. But then Balaguer's request for an argument for the necessity of mathematical claims is unanswerable. In contrast, I believe that there are good arguments for a view of mathematics which includes the necessary existence of mathematical objects. Such arguments will show that our mathematical methods require appeals to intuitions and these intuitions are best captured by axiomatizations which include existential mathematical claims. It's a long-ish story, and indirect, but it's the best one can do and I will attempt to do it in the rest of this book.

FBP attempts to account for mathematical knowledge without appeals to mathematical intuition on the basis of merely our pre-theoretic apprehension of consistency. The autonomy platonist who wishes to explain our focus on the standard model will appeal to more contentious epistemic capacities.

Some philosophers are skeptical of the prospects for such a view.

> 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: 335.)

Contra Field, this is exactly the position I believe to be worth pursuing. The purportedly 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 platonist must look for accounts of mathematical intuition.

§3: Intuition-Based Autonomy Platonism

It is time, finally, to describe the intuition-based epistemology for autonomy platonism I have been promising. Two constraints on the account are, first, that it be consistent with a non-mystical, non-spooky view of humans and their belief-gathering processes and, second, that it plausibly fit with our actual practices. I'll start with a brief sketch, from Mark McEvoy, of an autonomy platonist view consistent with the one which I support.

> Our basic mathematical concepts arise from causal interaction with physical objects that approximate mathematical objects (e.g., approximately square objects, or n-membered sets of physical objects). The elementary concepts so obtained are then available for examination by reason which can establish some elementary truths involving those concepts (e.g., elementary arithmetical and geometrical truths). The development of proof and of axiomatization further extends our ability to reason about these concepts. Some time after we have begun to establish mathematical truths, we notice that propositions involving mathematical concepts are not precisely true of anything in the empirical world. This, once we begin to see the problems faced by non-platonist views in attempting to account for mathematical truth (and, more generally, for the metaphysics of mathematics), leads us to conclude that if mathematical truths are to be true at all, they must be true of something else. This is how we end up with mathematical knowledge of entities that (we theorize) are abstract and independent of us. Our knowledge is a priori, despite the empirical origins of our elementary mathematical concepts, because we establish mathematical truths solely by reason (McEvoy 2012: fn 6).

Note that on McEvoy's sketch, our belief-gathering processes are utterly uncontentious. We have sense experiences of ordinary objects. These experiences are not taken to be infallible or necessarily truth-conducive. Neither are they immediate or unmediated by other beliefs or theories. They are just particular experiences of any sort. But, combined with our ability to reason, they lead us to particular mathematical beliefs. We reflect on our experiences and our particular mathematical beliefs, developing, eventually, full-blown mathematical theories. Our construction of these theories is mediated by both considerations of theory construction and by our particular mathematical beliefs. These theories have as their subjects abstract objects.

I take McEvoy's sketch to be perfectly natural, of both the development and the justification of our mathematical beliefs. To develop it into a full-blown epistemology for autonomy platonism, though, I must perform a couple of tasks. I need to say more about the nature of reflection or, as I will call it, mathematical intuition and reasoning. Briefly, the role of intuition is to provide some fallible but *prima facie* acceptable mathematical beliefs. I also need to say more about the ways in which we reason from simple mathematical claims to substantial mathematical theories. Insofar as I take this process to be just the ordinary practice of mathematics, there is not too much to say about it. Historians and sociologists of mathematics can provide a better account of the details than I will. But, again briefly, the process is one of seeking reflective equilibrium between our intuitive mathematical beliefs and our systematizations of those beliefs in the forms of broad and abstract mathematical theories, guided throughout by our ability to recognize valid inferences and consistency. Lastly, I will say something simple about why knowledge of mathematical objects, long seen as troubling for mathematical epistemology, is not really a problem. Here, I will rely on Quine's view of ontology as the result of positing, of modeling our best theories.

The remainder of this chapter will be devoted to these three tasks. In the next chapter, I will reflect on the nature of my account, especially responding to a concern about its structure.

§4: Mathematical Intuition

Intuition-based autonomy platonism invokes an a priori faculty of mathematical intuition. I take intuition to be a belief-forming process akin to sense experience but one which yields beliefs in propositions whose content is unavailable to our senses. A full account of this faculty should include a detailed examination of all our belief-forming processes and a partition of these into a priori and empirical classes. I will not provide that complete account here. I will characterize mathematical intuition in order to show how it can function in an epistemology for autonomy platonism, how it relates to philosophical reasoning generally, and how it avoids some criticisms of other accounts of intuition and why we should take it to be reliable if fallible.

§4.1: A Fallibilist Account of Mathematical Intuition

I start my account of mathematical intuition with a brief examination its role in the work of Jerrold Katz. I will depart in significant ways from Katz's account. But I agree with Katz, and McEvoy, that mathematical intuition is connected with mathematical reasoning generally. For Katz, intuition is our capacity to grasp simple mathematical truths, ones too basic to be derived.

> We can say that reason is rationality in application to deductive structures and intuition is the same faculty in application to elements of such structures. We can think of intuition as reason in the structurally degenerate case (Katz 1990: 381).

This far, I follow Katz: a mathematical intuition is an experience or recognition which can yield a belief. When I have a sense experience, I might form a belief based on that experience. For example, on seeing an apple in my hand I may form a belief that there is an apple in my hand. I might, of course, not form that belief. I might, say, be considering whether I were in a dream state. Similarly, when I have an intuition of a basic mathematical fact, say that seven and five equal twelve, that will ordinarily lead to or confirm my belief that seven and five are twelve. But, in skeptical cases, I might withhold my belief. For example, I might wonder whether arithmetical claims are merely fictional. I can have an intuition that a mathematical claim is false, as when I think about an unrestricted axiom of comprehension given a background set of beliefs about how it leads to paradox.

Mathematical intuition, as I use the term, can lead us to recognize a mathematical proposition as true or false. More than that, it mediates our recognition of the modal character of the proposition. We have an intuition not only that a sum holds, say, but that it must. This recognition contributes to the phenomenal content of an intuition. It feels as if the content of a particular intuition has modal character. When I see an apple and develop a belief based on my sense experience that it is red and ripe, I recognize that that same apple could have been green and may not be ripe. But when I think about the sum of seven and five, I know that it can only be twelve. This modal character of my mathematical beliefs arises directly from the nature of the objects of those beliefs, both independent of sense experience and immutable.

Reflecting both on the content and modal character of our mathematical beliefs, it is natural to distinguish the method by which I arrive at those beliefs, which I am calling intuition, and the process of my justification of those beliefs, from the methods and justifications of my ordinary beliefs, like those about apples. This is just the distinction between *a priori* methods of acquiring beliefs and empirical ones. We can see that mathematical intuition is an *a priori* method of belief formation just by considering the content and modal character of the beliefs we acquire by intuition.

In contrast to the way that *'a priori'* has often and unfortunately been understood, my claim that mathematical intuition is *a priori* should not be taken as ensuring that the content of the beliefs acquired by intuition are free from error. Mathematical claims ordinarily will be necessarily true, if true, and necessarily false, if false. But we are lamentably constructed so that we sometimes take false claims for true ones. My claim that mathematical intuition is *a priori* is a recognition of the distinctness of the processes of acquisition and justification of mathematical beliefs from the processes of our acquisition and justification of beliefs about ordinary objects. It is not a guarantor of security for those beliefs.

Some philosophers believe that appeals to mathematical intuition must carry with them a commitment to the truth of any belief acquired intuitively.[101] Such a view might be held because it seems to follow from the assumption, which I hold, that many mathematical beliefs, however acquired, are necessarily true if they are true. But the inference is invalid. The modality of mathematical claims is independent of the epistemic status of our beliefs about those claims. Again, a mathematical claim is necessarily true if true; but we might be wrong about it.

In empirical cases, we have mechanisms to correct our false beliefs. Among those mechanisms are both sense perception and considerations of theory construction. We might cede the belief that there is a pool of water in the distance both by approaching the mirage and by thinking more broadly about theories of light and reflection. We might cede a mathematical belief both in response to contrary intuitions and on the basis of broader theoretical considerations. In both empirical and mathematical domains, we check our particular beliefs with further particular beliefs and systemic ones.

Despite their abstract content, there are ways in which we can acquire mathematical beliefs that are not *a priori*. I can read about the proof of a new theorem without following the proof, so that I am acquiring belief by testimony rather than mathematical intuition. As children, we learn lots of mathematical beliefs by testimony or just by looking at lists of mathematical facts like multiplication tables. These are empirical methods of belief acquisition and they can not, by themselves, justify those beliefs. It is our grasping of these mathematical facts, our understanding them and their modal character and their content, which gives them their *a priori*, if fallible, nature.[102]

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[101] Mulnix seems to hold this view: "[R]ational intuition is a non-inferential belief-forming process where the entertaining of propositions or certain *contemplations* result in true beliefs, as well as one being convinced of the truth of these propositions" (Mulnix 2008: 717-8). Katz waffles, sometimes taking intuition to be an immediate source of knowledge (see Katz 1998: 43) but sometimes recognizing the fallibility of intuition (see Katz 1998: 44).

[102] "[E]ven if one's mathematical beliefs are initially produced by a causal process, this does not render mathematical knowledge *a posteriori*" (McEvoy 2004: 437). Leibniz's view is precedental: "Although the senses are necessary for all our actual knowledge, they are not sufficient to provide it all, since they never give us anything but instances, that is particular or singular truths. But however many

§4.2: Mathematical Intuition and Philosophical Intuition

Mathematical intuition is related to what is often called philosophical intuition. Indeed, given the modal character of many beliefs which arise from philosophical intuition, the processes are quite similar. Philosophical intuition, like mathematical intuition, is often derided, but more often defended than mathematical intuition. Since I am making claims similar to many of the claims about philosophical intuition, it will be worth a brief survey of some recent relevant work.

Where scientific evidence is often experimental and may be obtained by observation, many philosophical and mathematical claims are counterfactual and so can not be acquired empirically. Ernest Sosa defines intuition to show both its connection to belief and its distance from empirical observation.

> At t, it is intuitive to S that p iff (a) if at t S were merely to understand fully enough the proposition that p (absent relevant perception, introspection, and reasoning), then S would believe that p; (b) at t, S does understand the proposition that p; and (c) the proposition that p is abstract (Sosa 1998: 259).

Clause a in Sosa's definition captures the immediacy and simplicity of an intuition. George Bealer also takes intuition, which he calls rational intuition and which he applies to both philosophical and mathematical cases, to be a simple grasping. He notes that intuition has a phenomenal character while urging that it is not a spooky faculty.

> We do not mean a magical power or inner voice or special glow or any other mysterious quality. When you have an intuition that A, it *seems* to you that A... a genuine kind of conscious episode (Bealer 1998: 207).

Bealer distinguishes intuitions from beliefs, commonsense opinions, judgments, spontaneous inclinations to belief, the raising to consciousness of nonconscious background beliefs, guesses, hunches, and merely linguistic intuitions. I'll review his arguments briefly, since they are salutary for my discussion of mathematical intuition.

We can see the distinction between belief and intuition in mathematics when we recognize that the set-theoretic comprehension axiom may seem true, we may have an intuition that it is true, even though we have over-riding beliefs that show it to be false. Indeed, we can have intuitions about both its truth and its falsity, which again shows that our intuitions are fallible. The claims that intuitions are beliefs or commonsense opinions or judgments belies a category error. They may lead to beliefs or opinions, but they are not themselves beliefs; they are cognitive experiences.

Inclinations to believe are not episodic in the way in which intuitions, which have phenomenal content, are. Intuitions can not be identified with all of my nonconscious beliefs since I have many more of them than I have intuitions, or even possible intuitions. And they can not be identified with raising nonconscious beliefs to consciousness because we often have intuitions which lead us to utterly new beliefs, as when I follow a proof of a new theorem. "If I am to have an intuition about numbers, then above and beyond a mere inclination, something else must happen - a sui generis cognitive episode must occur. Inclinations to believe are simply not episodic in this way" (Bealer 1998: 209).

Intuitions are neither guesses nor hunches. They ordinarily have different phenomenal character and we give up our guesses and hunches when presented with contrary data. If I guess that there are five

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instances confirm a general truth, they do not suffice to establish its universal necessity; for it does not follow that what has happened will always happen in the same way" (Leibniz, New Essays on Human Understanding, 49).

coins in your pocket, and you pull out six, I cede my guess. But if it seems to me that there are five coins in your pocket, and you pull out six, I can still hold that it seemed to me that there were five even though that seeming was not accurate. Guesses are often just wrong and need no account; intuitions are linked to seeming, not to guessing.

Rational or mathematical intuitions are much closer to linguistic intuitions, like the intuition of the grammaticality of a sentence of natural language. Still, it is not the case that all rational intuitions are linguistic ones. Linguistic intuitions regard words of a particular language while rational intuitions, like that 'if P then not-not-P', hold for any language.

So I follow Bealer in characterizing mathematical intuition as a seeming, an experience, one which can accompany some mathematical beliefs, but does not accompany more complex ones.

> It does not *seem* to me that $25^2=625$; this is something I learned from calculations or a table. Note how this differs, phenomenologically, from what happens when one has an intuition. After a moment's reflection on the question, it just *seems* to you that, if P or Q, then it is not the case that both not P and not Q. Likewise, upon considering [the Gettier-style case of a person mistaking poodles for sheep] it just *seems* to you that the person in the example would not know that there is a sheep in the pasture. Nothing comparable happens in the case of the proposition that $25^2=625$ (Bealer 1998: 210-1).

Again similarly, Bealer acknowledges the modal character of such intuitions. "When we have a rational intuition - say, that if P then not not P - it presents itself as necessary: it does not seem to us that things could be otherwise; it must be that if P then not not P" (Bealer 1998: 207).

For Bealer, it seems, mathematical intuition is just rational intuition applied to mathematics, no different at all from philosophical intuition. Since my focus here is on intuition in mathematics, I take no position on whether philosophical intuition is precisely mathematical intuition. But the characteristics which Bealer ascribes to intuition describe what I take as mathematical intuition.

Let's put Bealer's account of rational intuition aside to continue to characterize mathematical intuition in comparison with accounts of philosophical intuition. I take it not to be, as Kant and the mathematical intuitionists of the early twentieth century had it, a mental construction, though it is a psychological experience with phenomenal character. We are not creating the content of the belief about which we have an intuition; we are grasping that content. Still, Charles Parsons invokes a Kantian-style intuition which shares some aspects of ordinary philosophical intuition. He assimilates 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" (Parsons 1980: 155).

Again, mathematical intuition does not have a privileged epistemic position, as Jonathan Cohen explains for philosophical intuition.

> The term "intuition" here is not being used in the sense of Spinoza, Bergson, or Husserl. It does not describe a cognitive act that is somehow superior to sensory perception. Nor, on the other hand, does it refer merely to hunches that are subsequently checkable by sensory perception or by calculation. Nor does this kind of intuition entail introspection, since it may just be implicit in a spoken judgment. Its closest analogue is an intuition of grammatical well-formedness. In short, an intuition that *p* is here just an immediate and untutored inclination, without evidence or inference, to judge that *p* (Cohen 1981: 318).

Some recent work in psychology concerns a concept of intuition close to the one I am describing and makes clear that it has no privileged epistemic position. Indeed, psychologists show that our intuitions can be epistemically problematic. In psychology, intuition is often aligned with automatic systems, in contrast to reasoning, which is aligned with analytic systems. Some people believe that their analytic systems, their ability to reason, override their intuitions. Recent research has shown that our intuitions are actually in charge most of the time. Daniel Haybron, following work of Jonathan Haidt, identifies the unconscious mind, associated with intuition, with an elephant and reason, or the conscious mind, with a rider of the elephant.

> The elephant dwarfs the rider, who will have a hard time getting the elephant to do anything it doesn't want to. Still, one might think that the rider is basically in charge. Yet Haidt points out that the analytic system is a recent - and still somewhat buggy - evolutionary innovation, appended to a basically intuitive brain that previously managed pretty well without it... It's not that intuition is a tool that a rational creature often employs; it's rather, to put it crudely, that reason is a tool that a basically instinctual creature often employs to accomplish certain ends. For the most part, the intuitive system sets the agenda (Haybron 2008: 246).

The challenge to a defender of intuition as an element of mathematical epistemology is to defend the security of mathematical beliefs with the fallibility of intuition. The worries about intuition which arise in cases that Haybron considers, especially weakness of will, are not present in the mathematical case. The nature of the content of mathematical intuitions and the ways in which our theories are both formal and constrained by a clear and syntactic notion of consistency contribute to their better security than our moral or prudential intuitions.

I have gone a distance afield in discussing the concepts of rational, philosophical, and psychological intuition and I take no position on the subtle relations among them. I intend this discussion to be useful in identifying certain aspects of intuition across disciplines. Intuitions are conscious cognitive episodes in which we grasp some abstract content with modal character. These episodes ground some beliefs, but are not themselves beliefs.

§4.3: Unhelpful Characterizations of Mathematical Intuition

Let's return our focus to mathematical intuition proper. Colin Cheyne reasonably surveys five attempts to explain mathematical intuition and argues that they are not successful.

> (1) intuition as unconscious inference (inferential intuition),
> (2) intuition as direct apprehension of any state of affairs (ESP intuition),
> (3) intuition as part of the process of ordinary sensory perception (perceptual intuition),
> (4) intuition of the truth of certain propositions (cognitive intuition),
> (5) intuition as direct apprehension of platonic entities or platonic states of affairs (direct platonic intuition) (Cheyne 1997: 114).

While Cheyne finds all five interpretations unacceptable, his dismissal of the latter two is too quick. Mathematical intuitions are not unconscious inferences, (1), but conscious apprehensions of a mathematical proposition. They are nothing like ESP, so (2) is out. The faculty of intuition may be related to the faculty by which we turn our sense experiences (retinal images, olfactory stimulations, etc.) into apprehensions of individuated objects. But since mathematical intuition is a faculty of apprehending claims about objects which are essentially non-sensory, it can not be a faculty of sense perception, (3).

Cheyne divides direct platonic intuition (5) into two possible cases: intuition of objects, which he calls intuition-of, and intuitions about propositions, which he calls intuitions-that. He ascribes the belief that we have intuition in these senses to Gödel for his well-known passages about intuition, ones which we saw in §1.5. Any special faculty of intuition-of, one which is supposed to parallel our sense perception, is properly considered mystical; I agree with Cheyne that we can dismiss this route. Cheyne assimilates intuition-that to the case of cognitive intuition (4).

So Cheyne's dismissal of mathematical intuition comes down, I think, to (4), central to my account of mathematical knowledge. Cheyne provides no argument against the possibility of intuition as conceptual knowledge. Indeed, he's fine with accounts of knowledge based on analysis of concepts. His worry is about the existential import that many mathematical claims carry.

> The claim that platonic knowledge is conceptual knowledge is an alternative to the claim that platonic knowledge is intuitive knowledge, a discussion of which is beyond the scope of this paper. An account of platonic knowledge as conceptual knowledge might include some process of intuition, but so long as that intuition was no different than that involved in acquiring non-platonic conceptual knowledge, then it is conceptual knowledge which is doing the important work in such an account. The notion of a special platonic cognitive intuition is simply *ad hoc* and explains nothing" (Cheyne 1997: 118).

Cheyne here attempts to elude the hard questions about intuition by contrasting conceptual and intuitive knowledge. Concepts, taken as contents of thoughts, are most plausibly understood as abstract objects, just like mathematical ones. People have their own thoughts, but they share concepts. The autonomy platonist who invokes intuition to explain our knowledge of mathematical objects and propositions will naturally invoke linguistic intuitions to explain our knowledge of concepts. The same capacity is at stake, and nothing Cheyne says undermines the use of intuitions, in either linguistics or mathematics, in constructing effective theories.

Cheyne's worry, presumably, is that the defender of mathematical intuition who appeals to (4) is somehow smuggling in some sort of spooky capacity to perceive abstracta. That's not a worry for my account. The objects of mathematics are nothing like perceived. They are just, as I will say in §6, posits like other objects.

Despite the breadth of application of the capacity of intuition, appeals to mathematical intuition are widely derided and often with this concern about mystical capacities of perception. Field pointedly rejects appeals to intuition throughout his work. "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: 28.)[103]

Insofar as we think of intuition along the lines of Cheyne's (2) and (3), as an extra-sensory perception, such a move is indeed desperate. Of course we have no such mystical ability. Fortunately the notion of intuition on which I am relying is not like those.

The question which Field raises elsewhere and hints at here, of whether and to what degree our mathematical intuitions are reliable, is a deep and excellent one. One of the virtues of my account is that I leave the reliability of intuition as an open question since I make no presumption that mathematical intuition is infallible. Some intuitions are more reliable than others just as some sense experiences are more clear than others. As I will describe more fully in §5, we test our fallible intuitions against our systematizations and work toward a reflective equilibrium between our mathematical intuitions and our mathematical theories. In order to show that our intuitions are generally reliable, I need just need to

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[103] See also Field 1989a: 21, where he derides the sui generis solution to the Benacerraf puzzle about identifying numbers with sets; Field 1982a: 66-67, on Gödel's view, which Field misleadingly calls indispensabilist; and Field 1985b: 190. Compare Field's claim with that of Frege: "We are all too ready to invoke inner intuition, whenever we cannot produce any other ground of knowledge" (Frege 1953: 19).

show that our best mathematical theories, the results of the process of attempting to achieve equilibrium between our intuitions and our theories, is reliable. That is a much less daunting challenge.

It is conceivable that our mathematical intuitions could turn out to be highly unreliable. Since our theories of mathematics are constructed to account for those intuitions, it is doubtful that we could discover this fact. We could be just deeply wrong about much of mathematics and I do not believe that we can rule out this skeptical worry. It is of a piece with the problem of proving the consistency of our mathematical theories. We can establish some relative consistency results. For example, the Dedekind-Peano postulates are consistent if ZFC is consistent. But we do not have an immanent proof of the consistency of ZFC and indeed we know that no such proof within ZFC is possible.

More likely than discover that our mathematical intuitions are highly unreliable, we will discover that some aspects of our mathematical intuition are incorrect, some particularly intuitive claims that we now take to be true are wrong. Gödel thought that it was intuitive that the continuum hypothesis was false. Perhaps it is, but perhaps not. Some intuitions are better than others.

For another example, consider the axiom of choice. In its ordinary form, it is intuitively compelling. With the background of ZF, it is provably equivalent to the highly counter-intuitive well-ordering theorem. One or other intuition has to go; perhaps, even, the background theory of ZF is the problem. In such cases, our intuitions are of limited direct use. We have to learn more about the consequences of adopting Choice or not and we may have to accept that some of our intuitions are misleading, as we adjust them upon learning the inconsistency of naive comprehension.

§4.4: Mathematical Intuition and Mysterianism

Part of the worry about autonomy platonism which makes it seem desperate to philosophers like Field is that intuition seems to be 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: 3-4.)

Resnik's claim that *a priori* intuition is incapable of scientific investigation reveals a lack of scientific ingenuity. We can ask people about their intuitions. We can compare reports about intuitions and seek theories to explain interpersonal consistencies and conflicts. We can even use standard neuroscientific tools like fMRIs to see what people's brains are doing when they have intuitions. Resnik's derision is puzzling.

The autonomy platonist should agree with the desire to naturalize epistemology by debarring mystical and mysterious elements. Mathematical intuition must be compatible with a mature psychology. But 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.

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. Few philosophers favor a pure Millian inductive account of mathematics.

On the mysticism side, we see worries about accounts of mathematics from Plato, Descartes, 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: 503).

If naturalism is to do any work at all, it must forestall an autonomous mathematical epistemology as long as autonomy platonism 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. But neither motivation weighs at all against an account like the one I am presenting.

One reason why one might think that autonomy platonism 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 platonist 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: 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: 10).

Putnam's demands for the details of neural processes which account for our apprehension of mathematical objects is too stringent. 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.[104] Indeed, the claim that there are neural processes which provide mathematical perception would be part of an empirical account of mathematics, not an apriorist one like the one I am defending.

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.[105] The alternatives to intuition-based autonomy platonism are too unsatisfying. Lacking full accounts of hard neuroscientific issues like consciousness, let alone apparently easier ones like perception, dismissing autonomy platonism is too hasty, considering both the robustness of pure mathematics, the need for intuition to account for it, and the bare fact that intuition is one of the ordinary tools in the mathematician's kit.

§4.5: The Unsurprising Reliability of Mathematical Intuition

Part of what leads to claims of mysterianism against the proponent of mathematical intuition is that opponents of mathematical intuition often frame intuition as a cognitive faculty which leads somehow inexplicably to infallible claims. Such a concern is beside the mark for the version of intuition on which I rely. My appeals to intuition are fallible starting points in a process of reasoning to robust mathematical theories, as I will describe in the next section. Still, to the degree to which appeals to intuition are truth conducive, we must respond to Field's criticism about their reliability. Earlier, I took this challenge as skeptical. But there is a more charitable understanding of the challenge to explain why appeals to intuition would be truth-conducive, if not infallible.

Part of the explanation, unsatisfying as it may be, is just that it is a brute fact about the abilities of mature reasoners to analyze concepts *a priori* that we often get such analyses right. We consider concepts, look at their inferential relations, and derive further concepts from the ones we hold. We recognize consistency and can distinguish interesting mathematical questions from more pedestrian or dull ones. More importantly, our ability to systematize, model, and connect different mathematical theories provides a constraint on our reasoning which answers some of the complaints about intuition. Richard Creath complains that earlier appeals to intuition are desperate because we have no way of resolving conflicts when intuitions differ. "This doctrine of intuition...is a scandal. Intuitions notoriously differ, and there is no plausible way of resolving these differences" (Creath 1991: 349).

It is ironic that Creath points to the development of non-Euclidean geometries as part of his

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[104] McEvoy agrees. "[T]he platonist ought not see intuition as a separate module" (McEvoy 2004: 433).

[105] See Putnam 1994: 506.

evidence of the problems with intuition. He is right that some people found the parallel postulate intuitively compelling. But mathematicians sought support for that intuition over millennia. Even Euclid seemed uncomfortable with it despite its attractiveness and seems to have avoided invoking it early in the *Elements*. If our account of intuition were infallibilist, as Descartes's was, the variability of intuitions would indeed be a problem. But on a fallibilist notion of intuition, this variability of intuitions is utterly expected and useful. It helps us find interesting mathematical problems. Indeed, the case of Euclidean geometry is one in which we have been able to generalize and expand mathematics just because of the ways in which intuitions were regarded, as useful starting points and not as infallible guides. This is a perfect example of how we can resolve differences about intuitions, by attempting to systematize those intuitions and examine the variety of theorems provable in the different theories.

It is certainly not the case that we all are born with good and reliable mathematical intuitions. Our mathematical abilities require training and that training allows us to hone our skills. Our early perceptual and small motor skill are lousy at first; we mature. The same process holds for mathematical skills, including our intuitions about mathematical claims.

From a phenomenological point of view, at least some of us have experiences which can be called intuitions about mathematics: immediate, non-inferential graspings of mathematical concepts and relations. Such experiences are a natural and ubiquitous facet of mathematical practice despite our lack of a neuroscientific account of them.

> We do not pretend to have a theory of a mechanism which explains how we come to form intuitive notions which are so astonishingly successful...But we regard it as absurd to reject the use of this ability just because we don't have a theoretical explanation... (Kreisel and Krivine 1971: 169; see also Lavers 2009: 6).

Intuitive experiences presumably have some neural correlates and so are not un-natural or spooky. The question is whether they can play a legitimate role in mathematical epistemology.

It might be useful, in thinking about mathematical intuition and the charges of mysterianism levied against it, to contrast the case with another philosophical view charged with mysterianism. Mind-body dualism is historically defended by those who accept faculties akin to mathematical intuition, and one can see similarities in the two views. Both are posited by those who find physicalistic accounts of their respective disciplines (philosophy of mind, philosophy of mathematics) lacking. Both involve commitments to non-sensible objects. Further, the two views may be naturally aligned. If, as Putnam argues but I deny, we can not account for mathematical intuition neuro-scientifically, the dualist can always claim that intuition is a faculty of the disembodied mind. Granting dualism, who could argue?

Still, it would be a mistake for the defender of mathematical intuition to align with the mind-body dualist. The similarities of the two cases are formal and shallow. Furthermore, mind-body dualism is so contentious a view that associating one's philosophy of mathematics with it weakens the account rather than strengthens it. Also, mind-body dualism is false.

The most important dissimilarity between the two views is that in the dualist's case, a neuro-scientific account of mental activity obviates the motivation for positing disembodied souls. For earlier dualists, like Descartes or Leibniz, the difficulty (they might say 'impossibility') of conceiving how conscious experience could be aligned with the physical mechanisms of the body led to the posit of a non-corporeal seat of thought. Leibniz considers walking inside the mechanical parts of a supposedly-thinking physical substance, like a brain. We would see only moving parts: no memory, no thought. Like Ned Block's Chinese Nation case, Leibniz's thought experiment seems to show that the physicalist has difficulty explaining consciousness. "Perception, and what depends on it, is inexplicable in terms of mechanical reasons, that is, through shapes and motions...When inspecting its interior, we will only find parts that push one another, and we will never find anything to explain a perception " (Leibniz, *Monadology* §17).

In contemporary philosophy of mind, these earlier arguments for positing a non-corporeal soul are re-framed as purportedly showing that there is an explanatory gap between neuro-scientific accounts of human experience and our conscious awareness of that experience. Bridging this gap is sometimes called the hard problem of consciousness. Since there can be no thought in a mechanical body, the dualist (or idealist monist) argues, there must be some essentially active, essentially perceptive, essentially conscious component to the basic elements of the world: souls.

These arguments for dualism, both historical and contemporary, rely on a subjective inability to conceive how conscious experience can be an aspect of physical activity. The more we learn about the specifics of how neural activity correlates with our experience, the smaller the supposed explanatory gap seems to be. Advances in neuroscience move philosophers away from dualism.

In contrast, advances in neuroscience do not mitigate the motivation for positing abstract mathematical objects at all. Mathematical objects are posited in order to account for the semantics of mathematical sentences we take as among our most secure, like 'there are prime numbers'. Neuroscience can help explain how our brains work when subitizing, calculating, or inferring. But such accounts go no way to undermining the semantic analysis of mathematical sentences. Providing the neural correlates of consciousness reduces or eliminates the motivation to posit a soul. Providing the neural correlates of mathematical thought reduces or eliminates the motivation to posit a soul which is the seat of that thought, but has no effect on how we understand the subjects of such thoughts, on how we best model our mathematical theories.

A contemporary mind-body (substance) dualist persists in positing a mysterious non-corporeal substance with an active, causal-explanatory role in our experience despite the advances in neuro-scientific explanations of that experience. For seventeenth- and eighteenth-century philosophers, without the benefit of contemporary neuroscience, such a posit might seem justifiable. For contemporary philosophers, positing a soul is stubborn adherence to mysterianism in the face of powerful explanatory physical theories. But positing abstract mathematical objects remains the best account of the semantics of our mathematical sentences, whatever the neural correlates of our mathematical experiences, like counting or apprehending a proof, turn out to be.

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 about mathematical intuition has accomplished.\

§5: From Mathematical Intuition to Mathematical Theory

The intuition-based epistemology that I am proposing starts with a weak (i.e. fallibilist) version of mathematical intuition. Intuition yields some mathematical beliefs. But the account so far does not do justice to the breadth and security of our beliefs. Moreover, since even the best mathematical intuitions may lead us to false beliefs, we need some method to improve our beliefs, some way to systematize and check them.

McEvoy's sketch of intuition-based autonomy platonism includes both an apprehension of some elementary mathematical truths and the connection of those truths with other truths, presumably through the development of axiomatic systems and some logical apparatus governing deduction. We must be able to compare different systematizations for their mathematical and more-general theoretical virtues.

These processes of refining and improving on the mathematical beliefs generated by intuition are just the natural and well-refined methods of mathematics. In other areas of philosophy, the process is well known as the method of seeking reflective equilibrium. We balance our intuitive apprehension of basic mathematical facts with our evaluations of the systematizations of our mathematical knowledge. The systems tend to be organized axiomatically. The intuitions are constraints on the system-building and the systems are constraints on the intuitions. Bertrand Russell describes the process neatly.

> When pure mathematics is organized as a deductive system - i.e. as the set of all those propositions that can be deduced from an assigned set of premises - it becomes obvious that, if we are to believe in the truth of pure mathematics, it cannot be solely because we believe in the truth of the set of premises. Some of the premises are much less obvious that some of their consequences and are believed chiefly because of their consequences. This will be found to be always the case when a science is arranged as a deductive system. It is not the logically simplest propositions of the system that are the most obvious, or that provide the chief part of our reasons for believing in the system (Russell 1924: 325).

We hold some basic mathematical claims to be true, and necessarily so: simple arithmetic facts, core geometric propositions, some set-theoretic claims, maybe Hume's Principle. We seek systematizations of those particular beliefs both to see if they are consistent and to make connections with other mathematical theories. We balance our formal theories with our particular beliefs, adjusting the axioms as they fit the theorems, perhaps giving up some basic (intuitive) principles in order to achieve an elegant systematization.

In her masterful, "Believing the Axioms," Maddy hints at the role of intuition in set theory in moving from simple and vague claims to systematized axioms. She claims that the pairing and union axioms are the formal versions of some nascent intuitions.

> When justifications are given, they are based on one or the other of two rules of thumb. These are vague intuitions about the nature of sets, intuitions too vague to be expressed directly as axioms, but which can be used in plausibility arguments for more precise statements... the two in question are limitation of size and the iterative conception (Maddy 1988: 484; see also Decock 2002: 242).

As we move from rough intuitions to precise formal theories, we are guided by two three main cognitive tools: our ability to recognize consistency, our inferential powers, and our mathematical intuition. The former guides the mathematician categorically, especially where she invokes rigorous formal systems, logics which, as was Frege's goal, wear their consistency on their syntactic sleeves. Our inferential tools are both formal and intuitive. The latter guides the mathematician where the former two fail.

In assessing the balance between sets of axiomatizations and particular mathematical claims, intuition again guides us fallibly. There are two ways in which our intuitions might steer us wrong. First, as we have already seen, we might give up some basic claims which appear intuitive. An unrestricted comprehension axiom in set theory appeared intuitively correct to Cantor and Frege and others; Bealer reports that it still seems correct to him. Similarly, Euclid's parallel postulate seemed intuitively correct for millennia before the development of robust non-Euclidean geometries showed the limits of its range.

Conversely, Leibniz's work with infinitesimals and Newton's work with fluxions seemed intuitively false to many mathematicians: how could the sum of an infinite number of infinitely small quantities result in a finite quantity? Their methods were eventually justified not only by their fruition but by the epsilon-delta definition of limits which grew out of the nineteenth century work on arithmetizing analysis.

Second, we might find that certain systematizations better organize mathematical phenomena than others. A perhaps old-fashioned way to think about the virtues of formal systems is to evaluate the intuitive nature of the axioms. The old story says that theorems are justified by their derivability from axioms and axioms are judged by their obviousness. As Russell noted, this old story fails to capture the proper relationship between axioms and theorems, a relationship in which axioms depend on theorems as much as theorems depend on axioms. The lively contemporary research project of reverse mathematics is precisely an attempt to work toward axioms from theorems and serves as an supporting example for Russell's claim.

Instead of judging axioms by their foundational certainty or obviousness, then, we judge entire formal systems. Our intuitive judgments about forrmal systems may vary and err. For a simple example, consider whether arithmetic is best captured by Peano axioms, by those axioms modeled within set theory, by those axioms modeled within category theory, or within second-order logic. Relatedly, consider Benacerraf's famous question about how best to model the objects of arithmetic within set theory, using Zermelo sets or Von Neumann sets. Mere apprehension of consistency can not guide our choices among provably equivalent models or axiomatizations. Intuition guides our preferences within the constraints of consistency.

Consider a proposition such as the axiom of choice. To some, in some formulations, it seems obviously and intuitively true. For example, in a simple and natural formulation, it merely says that for any set of sets, there is a choice set, one which consists of exactly one member of each of the sets. Other formulations are far less intuitive. Indeed, given a background of ZF set theory, the axiom of choice is equivalent to the well-ordering theorem, that every set can be well-ordered, including sets like that of the reals which we have utterly no idea how to order. Intuitions about the axiom of choice thus vary; on pain of consistency, the intuitions can not all be correct. Moreover both the axiom of choice and its denial are consistent with the axioms of ZF, if ZF is consistent.

To evaluate the axiom of choice, we look at the ways in which it facilitates inferences and at what theorems can be proved with it and without it. We evaluate both the broader systems and the further theorems by their intuitiveness, balancing a range of factors, especially strength and elegance. Whether or not we should believe the axiom of choice will thus be guided by our intuitions about both axiomatizations of set theory and particular further theorems. We seek reflective equilibrium between our particular intuitions and our systematizations, again guided by consistency and our intuitions about theoretical virtues. This is all just standard mathematical practice.

Baker emphasizes the way in which axioms are inferred from theorems in response to Juha Saatsi's claim that mathematics is deductive. Saatsi rightly argues that we do not make abductive inferences from physical phenomena to mathematical objects.[106] Baker replies that our methods often are abductive.

> Consider the axioms of our preferred mathematical theories, for example the axioms of ZFC. However these are justified, it is not by deduction from other more basic claims. One idea is that what is going on here is abductive: the axioms are chosen that best systematize the basic set theoretical (or arithmetical, or geometrical) claims that we accept. And that doesn't look too far away from 'the inductive method' (Baker 2009: 629).

Baker is right that our inferences to the axioms are not deductive. They may even reflect some

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[106] Saatsi also wrongly argues that we do not make abductive inferences from mathematical phenomena, an argument I will ignore here.

aspects of induction, insofar as inductive methods are sometimes structurally similar. But the inductive base for our choices of mathematical axioms is not observational or empirical in any way. When we choose axioms, we look at the kinds of theorems we can infer from them, we see how elegant and powerful a theory we can construct with them, we seek connections to other theories. This method may be structurally like abductions in empirical science, but the similarity is merely structural.

I will say little more here about the process of achieving reflective equilibrium in mathematics, my intuition-based epistemology for autonomy platonism. It embraces ordinary and natural descriptions of mathematical methods. Those methods vary and there is much more to be said about the ways in which mathematicians do their work. There are deep and interesting questions, for instance, about the roles of computers in mathematical proofs, about inductive procedures, about the relation of truth to proof, and many more such questions. The procedure I have described briefly here as the method of seeking reflective equilibrium in mathematics is general enough to be adapted naturally to whatever gets properly called mathematics. I do not pretend to be presenting an original or controversial analysis of mathematical methods. I am claiming that ordinary mathematical practice, whatever it is, plays a central role in our mathematical epistemology.

But I have a bit more to say about two remaining open questions. The first is about mathematical ontology. The methods I have been discussing in the last two sections have described the relation between our beliefs and mathematical propositions, but I have not spoken enough of beliefs about mathematical objects. I will not say much beyond what I will say in the next section: our beliefs about mathematical objects arise from considerations of standard semantics for mathematical theories. My main concern is to say that that is all that one needs to say.

The second question is whether the process that I described as seeking reflective equilibrium in mathematics is one of justification. I began §3 by emphasizing two constraints on any mathematical epistemology, that it be consistent with a non-mystical view of humans and that it fit with our actual practices. The epistemology I am defending clearly captures our mathematical methods since it focuses on the relationship between those methods and our mathematical beliefs. The more important question is whether those methods are justifications of those beliefs. That is the subject of the next and last substantial chapter of this book.

§6: On What Mathematical Objects There Are

In the past two sections, I have described the ways in which we balance our beliefs in particular mathematical claims, ones which may or may not be intuitive, with our beliefs about how best to organize and systematize those claims. These beliefs are about propositions, not objects. But I have been moving easily in this book between object platonism, characterized by the claim that some mathematical objects exist, and sentence platonism (or truth-value platonism), characterized by the claim that some mathematical claims are non-vacuously true. That is, I am defending autonomy platonism as both a realism about mathematical claims and a realism about the existence of mathematical objects. In this section, I discuss the simple inference of object platonism from sentence platonism. My goal is not so much to defend the inference of object platonism from sentence platonism, though I believe that the inference is valid. The goal of this book is to contrast two versions of object platonism and so a full defense of object platonism is unnecessary. My central concern here is to block the strongest objection to object platonism so that reinterpretive views of mathematics, ones which deny the inference to object platonism from sentence platonism, lose their motivation.

The central challenge for object platonism has come to be known as the access problem: how can we, whose epistemic capacities seem limited to sense experience, have knowledge of the abstract objects of mathematics? The access problem arises from platonism's natural two-realm view, that the referents of mathematical singular terms inhabit a realm which is separate from us. The access problem motivates much of the common derision of mathematical intuition.

One way to try to solve the access problem for platonism involves positing a special human ability to learn about abstract objects. This approach is exemplified by Plato, Descartes, Gödel, and Katz, among many others. The central complaints about rationalism involve accusations of mysticism and desperation, and failure of parsimony, both epistemological parsimony and parsimony of the resulting ontology. If mathematical intuition is a special faculty of grasping abstract objects, the charges of mysterianism seem apt.

The solution, for my intuition-based autonomy platonist comes, perhaps ironically, from the work of Quine. While I have been critical of many aspects of Quine's work in the philosophy of mathematics, this criticism does not apply to his solution to the access problem. His solution arises not from his appeal to empirical science to justify our mathematical beliefs or from his holism. It comes from his view of ontology as the result of positing, of modeling our best theories. Quine's posits-based approach to ontology is consistent with the intuition-based autonomy platonism I have described. It replaces questions of access with justifications of theories and their corresponding posits.

To demand access is to demand that a perceiver be able to correlate the objects he or she believes exist with particular perceptions. The traditional empiricist requires lines of access from, say, the tree to my eyes, to my brain, to my beliefs. He or she draws roughly parallel lines to account for beliefs about all objects. The sense-data reductionist demands, as did Hume, a connection to sense experience for every legitimate claim.

Quine denies that a satisfactory account of piecemeal access, like that of the sense-data reductionist, is available, independently of the mathematical case. His alternative method is a response to the difficulties of describing our access both to ordinary objects and the posits of empirical theories, objects which are too small or too distant to perceive. Instead, Quine isolates evidence, on one side of a theory, and ontology on the other. Between them stands a theory which, as a whole, must be consistent with the evidence.

Central to the autonomy platonist's criticism of the indispensability argument is the rejection of Quine's holistic claim that evidence for scientific portions of our best theories extends to their mathematical theorems. My rejection of the transfer of evidence from science to mathematics does nothing to undermine the legitimacy of holism within scientific theory when applied to the objects of scientific theory.[107] More importantly, I have said nothing to undermine Quine's proper claim that ontology is the result of modeling our best theories. The difference between the autonomy platonist and the indispensabilist is on the question of which theories we should believe. The indispensabilist says that mathematical theories, on their own, having nothing to do with sense experience, do not independently compel our belief. It is only when they are invoked for the purposes of constructing theories which account for our sense experience that the cease being mere recreation. The autonomy platonist argues that mathematical theories themselves are proper objects of belief whether or not they are used in physical science.

So I am arguing that we should believe our mathematical theories and that beliefs in mathematical objects follow directly. We use mathematical intuition as part of both our acquisition and justification of our mathematical beliefs, but not as a special faculty of perception of mathematical objects. Our knowledge of mathematical objects is not the result of any kind of special extra-sensory apprehension. It is just a result of interpreting our best mathematical theories, taking mathematical objects to be posits of mathematical theories in the same way that electrons, quarks, trees, and cats are posits of scientific theories.

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[107] Sober believes that Quine's holism does falter within science, that we confirm or disconfirm small portions of a scientific theory when we test a particular hypothesis. I remain agnostic here on that claim.

Thus, if my epistemology for autonomy platonism is correct, there is no worry about access. Quine's method effectively and decisively dissolves the access problem. Some philosophers, unclear about how the access problems for mathematical objects is moot, are more comfortable with sentence platonism. The way in which I account for our knowledge of mathematics (i.e. the justification of our mathematical beliefs) begins not with apprehension of objects but intuitions about the truths of mathematical claims. So even if someone were to worry about the posit-based account of our mathematical ontology, the remainder of my account remains secure.

Such hesitance, though, is unwarranted. The access problem is a latent vestige of an old-fashioned and false view of ontology as exhaustively described by those objects which are given, immediately, in sense perception.

§7: The Yield of Intuition-Based Autonomy Platonism

So there are two excellent lessons to be learned from Quine's work on the indispensability argument. First, the posit-based view of ontology is important to explain and justify our beliefs about objects not directly available to sense experience; that method applies broadly, in science and mathematics. For Quine, those disciplines are inseparable. For the autonomy platonist, those are independent disciplines. Either way, the objection against platonism that there is a problem about access to mathematical objects is deflated.

Second, my defense of our capacity of mathematical intuition has a formal similarity to the indispensability argument. Both are kinds of inferences to the best explanation and so rely on posits. For the indispensabilist, we infer mathematical knowledge as the best explanation of the ubiquity and effectiveness of mathematics in science. Much of the preceding portion of this book is a rejection of that inference. But for my intuition-based epistemology, we infer mathematical intuition not merely from its phenomenological properties, but also from our mathematical knowledge: it is implausible to account for that mathematical knowledge without positing a capacity of mathematical intuition. Therefore, we should believe that we have such a capacity.

A significant advantage of intuition-based autonomy platonism over indispensability platonism is its ability to avoid the unfortunate consequences, both those which apply to mathematical claims and those which apply to mathematical objects. Invoking the method of seeking reflective equilibrium in mathematics does not impugn or denigrate the status of mathematical claims. The constraints on mathematical systematization are purely mathematical. The process of developing and justifying mathematical theories contains ineliminable *a priori* elements. The fact that we hold our mathematical beliefs fallibly does not detract from our apprehension of their necessity. We hold them to be true necessarily, if true, and false necessarily, if false. Our faculty of intuition and the procedures by which we justify our beliefs, working reflectively between intuition and systematization, are imperfect. But the modal status of the claims we consider are unaffected by these imperfections.

In parallel, and in contrast to the indispensabilist's platonism, there is no reason for the intuition-based autonomy platonist to worry about characteristics like temporality or ontic blur. Our best understanding of mathematics is one in which no temporal properties (other than mere Cambridge properties, like being thought of by a person at a time) apply to mathematical objects. Mathematical objects are utterly different from physical objects, arising as posits of mathematical theories which are independent of empirical science.

We have seen two different accounts of autonomy platonism which can be used as alternatives to indispensability platonism and its unfortunate consequences. Balaguer's FBP is the less contentious. But FBP does not best capture the nature of mathematical practice and fails to account both for the necessity of mathematical claims and for the ways in which we seek answers to open questions. My version of autonomy platonism takes the modal character of mathematical claims as a datum and captures the way mathematicians work, balancing their intuitions about mathematical results with their formalizations and systematizations in proofs.

I have shown that the nature of mathematical intuition, rather than being contentions and mystical, is ordinary and natural. So is our theorizing. Moreover, the ways in which we derive our ontology from modeling our theories dissolves the traditional access problem. Still, traditional platonism has been attractive to many folks for its securing of our mathematical beliefs. If our intuitions are fallible, and our theories are guided by no more secure method, and our ontology arises from interpreting our theories, it seems that the traditional security of mathematical reasoning is lost.

So be it. The Humean condition is the human condition. Even in mathematics, we have no assurances that our reasoning is free from error. Claims of traditional platonists, like Descartes, to foundational security were unfounded.

Still, a glib dismissal of old-fashioned foundationalism goes no distance to answering the resultant worry about my version of autonomy platonism. I may have done justice to the ways in which mathematics is practiced. But what shows that I have actually justified any beliefs? The method of reflective equilibrium on which my account is founded seems, like coherentist epistemologies, to be liable to be untethered to mathematical truth. On my account, we justify the beliefs based on our mathematical intuitions by appealing to the constructions of mathematical theories and we justify the mathematical theories at least partly by their consistency with our intuitions. This method threatens to undermine the justificatory role that an epistemology for mathematics is supposed to provide. Such an account seems liable to charges of circularity.

And so it is. In the next chapter, I will respond to the charge of circularity, embracing the circularity in the account but arguing that the circularity is not problematic and does not debar my account from justifying our mathematical beliefs.

Chapter Ten: Circles and Justification

My intuition-based platonism is autonomous because it takes the justification of mathematical beliefs to be independent of the uses of mathematics in scientific theory or explanation. Since such justification is independent of science, it must have another basis. I make a couple of simple claims. We are justified in believing in mathematical objects because they are the referents of singular terms in many true mathematical sentences or objects in the domain of their models. We are justified in believing many mathematical sentences to be true because they ascribe properties to mathematical objects that they truly have. 'The positive square root of seven is greater than two and less than three' is true. It is true because it ascribes properties and relations to numbers which they actually have or stand in.

I have been arguing that standard charges of mysterianism and worries about access for the platonist are unfounded. Now I must deal with a more-serious concern about the epistemology I am promoting. The claim that mathematical beliefs are autonomous is, in its barest form, just the clam that they are self-justifying, The claim that mathematics, indeed any set of claims, can be self-justifying has an air of sophistry. Indeed, others have rejected the view that I am defending just because it sounds like a viciously circular argument. I'll start this chapter by looking at the reasons others have for rejecting claims that mathematics is autonomous and thus self-justifying. Then, in the majority of this chapter, I'll argue that my account is circular, but not problematically so.

§1: Bootstrapping

In the absence of scientific justifications of mathematical beliefs, given the Unfortunate Consequences of even the strongest version of the indispensability argument, one might wonder whether mathematics can justify itself. Perhaps we should admit mathematical objects into our ontology, not because of their indispensable use in physics or biology or psychology or economics, but because they are the objects to which mathematical theories themselves refer. If we accept the legitimacy of mathematics in its own right, the indispensability argument is otiose.

Unfortunately, taking mathematical claims to be self-justifying appears to be illicit bootstrapping. If theories could justify themselves, any theory of anything would be justified. Ghosts may be required for the study of paranormal phenomena and witches may be required for the study of sorcery. The argument for the truth of the claims or the existence of the objects of a discipline can not depend solely on their indispensability within that field.

It is interesting to note that invoking purely mathematical justifications for mathematical claims is one way think about naturalizing our epistemology for mathematics on one interpretation of 'naturalism' discussed by Maddy.[108] If we take naturalism to entail a deference to the practice of scientists, the naturalist might take the claims of practicing mathematicians at face value, as we saw in §4.5 with what Baker called strong mathematical naturalism. Mathematicians say things like, "There are infinitely many primes." At face value, there is no doubt that mathematical objects exist. The defender of this mathematical-practice argument has no need to wonder whether our scientific theories or explanations or practices commit us to mathematical objects because we are already committed to them by mathematical practice.

A problem with this mathematical-practice argument, for the naturalist, is that it contradicts another compelling sense of 'naturalism', one on which only natural (i.e. concrete and not abstract) objects exist. This latter version of naturalism is really an eleaticism on which only objects in space-time or objects which are causally active exist. On this latter version of naturalism, mathematical objects are ruled out of our ontology nearly by definition.

But the central problem with the mathematical-practice argument is that we need justification for taking mathematical statements at face value. We do not know if mathematical practice itself is to be

[108] See §3.1.

taken seriously in the way that we do not take seriously the practice of purported psychics and sorcerers. Even if we accept mathematical practice generally, the references to mathematical objects by mathematicians in their work may be instrumental or unserious. The work of Geoffrey Hellman and Charles Chihara in reformulating mathematical claims as modal claims is meaningful precisely because we need not take mathematical claims at face value. As Michael Potter observes, "What mathematicians say is not always a reliable guide to what they are doing: what they mean and what they say they mean are not always the same" (Potter 2007: 18).

The question of whether mathematical claims are to be taken at face value is exactly the question of whether mathematics is self-justifying, whether bootstrapping or circular justification in mathematics is legitimate. Penelope Maddy and Hilary Putnam have each explored positions liable to such a bootstrapping criticism. Let's look at these two positions, see what they're missing, and then see how to provide that for my autonomy platonism.

Maddy's arguments pave much of the route I have traveled in this book, though from a naturalist's, rather than a platonist's, perspective. She responds to two related worries about the indispensability argument which I share. First, she is concerned that the extent of the mathematical beliefs which are justified by the indispensability argument is limited (i.e the unfortunate consequence Restriction). She notes that practicing mathematicians believe in a fuller mathematical universe than empirical science requires.[109] Second, she complains that the indispensabilist's view is inconsistent with mathematical practice (i.e. the unfortunate consequence Subordination of Practice). "[I]ndispensability theory cannot account for mathematics as it is actually done..." (Maddy 1992: 289).

Moreover, Maddy claims that mathematical practice, rather than empirical science, should determine mathematical ontology.

> If a mathematician is asked to defend a mathematical claim, she will most likely appeal first to a proof, then to intuitions, plausibility arguments, and intra-mathematical pragmatic considerations in support of the assumptions that underlie it. From the point of view of the indispensability theorist, what actually does the justifying is the role of the claim, or of the assumptions that underlie its proof, in well-confirmed physical theory. In other words, the justifications given in mathematical practice differ from those offered in the course of the indispensability defence of realism (Maddy 1997: 106).

Maddy thus presents a modified indispensability argument which first appeals, like the standard indispensability argument, to the indispensable applications of mathematics to convince us generally that there are mathematical objects. Then she modifies the argument by appealing to mathematical practice itself to determine the extent of mathematical ontology. "The compromise goes like this. Take the indispensability arguments to provide good reasons to suppose that some mathematical things (e.g. the continuum) exist. Admit, however, that the history of the subject shows the best methods for pursuing the truth about these things are mathematical ones, not those of physical science" (Maddy 1997: 108).

Maddy's approach renders the indispensability argument otiose in favor of some pure mathematical justifications. Her defense of pure mathematical justification comes via appeals to strong mathematical naturalism. "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

[109] In later work, Maddy moderates this claim, saying that mathematics itself is indifferent to ontology. "[M]athematics in application...tells us nothing about mathematical truth or ontology" (Maddy 2007: 344).

disregard the evidential relations of practice or recommend reforms on non-mathematical grounds" (Maddy 1992: 276).

Insofar as Maddy invokes a standard indispensability argument, her view may be as effective or anemic as QI in justifying beliefs about applied mathematics. Let's put concerns about those beliefs aside to focus on the extension of the argument. By itself, Maddy's modified indispensability argument provides an incomplete justification of pure mathematics. If her justification comes only from mathematical practice, then it suffers from the standard worries about bootstrapping. Appeals to psychic powers and revelation are also older than experimental natural science. While mathematical practice may be successful explaining mathematical phenomena, psychic powers may be successful in explaining psychic phenomena. The natural scientist claims that ghosts do not exist and thus we need no account of our knowledge of them. Similarly, the scientist with nominalist tendencies claims that mathematical objects do not exist, and thus we need no account of them. Maddy's account of pure mathematical justification needs more than a bare appeal to practice.[110] Unfortunately, her naturalism, which she lately calls second philosophy, debars any kind of extra-mathematical evidence.

Putnam similarly flirts with a position liable to a bootstrapping criticism. To see his argument, let's return to the success argument from §6.2. "I believe that the positive argument for realism 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 1975: 73).

Note that Putnam's words are open to at least two different interpretations. It might be taken as an ordinary indispensability argument, as I took it when I interpreting it as MS. Or it might be taken to be a hint at an autonomy platonism.

MSB MSB1. Mathematics succeeds in itself. That is, it is fruitful.
MSB2. There must be a reason for the success of mathematics.
MSB3. No positions other than realism in mathematics provide a reason.
MSBC. So, realism in mathematics must be correct.

Like Maddy's modified argument, MSB is, by itself, insufficient to justify mathematical beliefs because it appeals to illicit bootstrapping. We need antecedent criteria to determine whether a theory is successful in the sense of MSB1. If the criteria come from within the theory, then any theory can be deemed successful. The crystal ball can tell you to believe the crystal ball.

Putnam and Maddy are correct that within mathematics, mathematical criteria reign. We can justify a posit, for example, by its fruitfulness. Consider Descartes's posit at the foundation of analysis.

AG There is a one-one correspondence between the points on a line and the 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. Similar arguments can be made within a variety of special sciences. The objects of biology (e.g. DNA sequences) have indispensable uses in biology. Preference orderings have especial uses in economics. For these disciplines, there is a kind of indispensability underlying their posits. Such justifications might be called intratheoretic indispensability arguments.[111] Intratheoretic indispensability

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[110] Regarding Maddy's view, Marianna Marfori writes, "[M]athematical naturalism...ultimately fails to explain the difference between mathematics and pseudo-science" (Marfori 2012: 337).

[111] See Marcus 2010, §8.

arguments are kinds of inferences to the best explanation. They yield justifications of particular theorems in mathematics and other sciences in the same way that a theoretical posit in physics yields electrons. They demonstrate connections and entailments within theories. But when it comes time to evaluate the theory itself, when we wonder if we should believe in mathematical claims or biological claims or alchemical claims or psychic claims, or what makes it the case that we do believe them when we do, intratheoretic indispensability arguments are insufficient. We can only entertain a statement such as AG if we have prior commitments to points and lines and real numbers. Within a discipline, such arguments are often essential. But they cannot justify, by themselves, knowledge of an entire discipline like mathematics.

So the question which faces Maddy's modified argument and Putnam's MSB is how to establish the legitimacy of pure mathematics. One option is to step back to look at the discipline as a whole to see if it has properties that would render it acceptable. We know that parapsychology, unlike biology or mathematics, say, does not have the legitimacy of a proper science. If we had a method for distinguishing the good theories from the bad ones, we could apply them to know whether mathematics is a proper science. Criteria for good science would 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.

In the philosophy of science, the problem of settling on these criteria is known as the demarcation problem. I will say more about it later in this chapter. For now, note that without a solution to the demarcation problem, or at least a demonstration that any solution must pronounce mathematics legitimate, any claim for the autonomous legitimacy of mathematics appears liable to a bootstrapping criticism. Maddy's modified indispensability argument and Putnam's MSB both suffer these problems which could be solved if we find a reasonable solution to the demarcation problem.

Appeals to a solution to the demarcation problem, while welcomed by the intuition-based autonomy platonist, are debarred by some versions of naturalism, most-famously Quine's and most-relevantly Maddy's. Quine urges abandonment of any first philosophy as a consequence of his holism: there is no perspective from which to evaluate theories outside of the theories themselves. For Quine, the demarcation problem is moot since there is only one theory, properly understood, and no way to step outside of that theory to evaluate it.

Maddy's modified indispensability argument is also crippled by this naturalistic inability to evaluate theories.

> The Second Philosopher—bless her!—doesn't talk this way; just as she employs no demarcation criteria for science vs. non-science, she has no litmus test for philosophy vs. non-philosophy. Instead, she notes...that considerations of existence and truth and knowledge, of ontology and epistemology, do not in fact play an instrumental role in settling questions of mathematical method (Maddy 2007: 349, fn 12).

The intuition-based autonomy platonist holds no such prejudice about first philosophy. We need not take the dictates of our first-philosophizing as infallible. But we can formulate some defeasible criteria for evaluating theories and by doing so avoid worries about circular argumentation.

The argument of the remainder of this chapter is a defense of circular justification in philosophy and the conclusion that mathematics is indemnified against bootstrapping criticisms by a proper understanding of (if not a solution to) the demarcation problem.

§2: Two Circles in Philosophy

The problem of circular justification has a long history in philosophy. To take a well-worn example, consider this excerpt from the letter of dedication preceding Descartes's *Meditations.*

> It is of course quite true that we must believe in the existence of God because it is a doctrine of Holy Scripture, and conversely, that we must believe in Holy Scripture because it comes from God; for since faith is the gift of God, he who gives us grace to believe other things can also give us grace to believe that he exists (Descartes, CSM II: 3; AT VII: 2).

As Descartes recognizes, the circularity of the scriptural argument is problematic, perhaps formally so. One might generalize this result as showing that circular structures like this one are inherently fail to justify any claim within the circle. The consequences for the intuition-based autonomy platonism I have sketched are potentially devastating.

For a more-recent, yet no-less well-worn, argument, consider Quine's opposition to meanings in "Two Dogmas of Empiricism." Quine argues that any definition of 'meanings' used to support an analytic/synthetic distinction is circular. The identity conditions for meanings are synonymy: two terms have the same meaning if and only if they are synonymous. But in order to explain synonymy, one must appeal to concepts which presuppose meaning or synonymy. Since all attempts to characterize synonymy are inter-related, any attempt to define synonymy leads one into a circle of intensional concepts.

Quine limns three possible grounds for defining synonymy: logic, dictionary definition, and substitutivity *salva veritate*. We can introduce logical rules to govern synonymy, but only on the basis of our prior understanding of which terms are to be taken as synonymous and which sentences are to be taken as analytic. We thus define synonymy in terms of analyticity, in order to explain analyticity in terms of synonymy. We say that 'bachelor' is synonymous with 'unmarried man' because 'bachelors are unmarried men' is analytic and vice-versa. That's a circle.

Taking analytic statements to be true by definition also presupposes, rather than explains, synonymy. The lexicographer merely reports prior synonymies. Explication, which adds clarifying information to a definition, relies on other, pre-existing synonymies. There are a few exceptions of definitions by stipulation (e.g. when scientists name a planet or molecule). But these exceptions are rare and subject to devolution. Taking synonymy as dictionary definition presupposes the community's understanding of which terms are synonymous, but the community takes terms as synonymous on the basis of dictionary definitions (or other community standards). Again, we have a circle.

Lastly, we can appeal to the fact that terms are synonymous when they can be substituted for each other without changing truth values. One can substitute 'unmarried man' for 'bachelor' in 'a bachelor is not married' and related expressions. But examples like 'creature with a heart' and 'creature with a kidney', which are coextensive but not synonymous, force us to strengthen the condition for substitution. A natural attempt would insist on substitutivity salva analyticity: two terms are synonymous if they can be substituted for each other while maintaining the analytic or synthetic nature of the expression in which they are substituted. But again, we are met with a circle: defining synonymy in terms of analyticity in order to define analyticity in terms of synonymy.

Alternatively, we could make the condition for substitution modal, isolating synonymous expressions as those whose identities are necessary. It is necessary that bachelors are unmarried men and it is necessary that anything plain is unadorned. But, again, such claims are circular in that they explain one intensional idiom (synonymy/ analyticity) in terms of another (modality). "Our argument is not flatly circular, but something like it. It has the form, figuratively speaking, of a closed curve in space" (Quine 1953: 30).

While he only shows it for these three cases, Quine insists that any attempts to explain synonymy which appeal to other intensional concepts will be unacceptably circular. He demands a reduction of intensional concepts to non-intensional ones. But it seems impossible to analyze 'meaning' reductively. Quine concludes that meanings are illegitimate and should be eschewed by our best theories. Like Descartes's unbeliever, Quine rejects a philosophical view because of circularity.

§3: Pragmatic Analyses

Still, there are some reasons to resist characterizing circular reasoning as always fallacious. First, circular arguments are deductively valid; since they conclude what is already in the premises (either explicitly or implicitly), we can construct no counter-models to any circular argument. Since there is no formal error in such inferences, any error must be pragmatic.[112]

Second, there seems to be a difference between arguing in a circle and begging a question. Douglas Walton distinguishes the two by arguing that circular reasoning is not necessarily fallacious even though begging the question is.[113] Arguments are used for purposes and embedded in social contexts. When we label a particular argument circular, we often ignore its context and goals. Once those are considered, what looks like a vicious circle up close may turn out to be an enthymeme, presupposing independent evidence for some aspect of the circle. Background assumptions may always be brought to bear on any premise. An argument which appears to be circular in isolation may thus avoid begging the question.

Walter Sinnott-Armstrong provides an example of a circular argument which can be used legitimately in some contexts but which is illegitimate in others.

SA Ohio is the Buckeye State.
Mary lives in Ohio.
So, Mary lives in the Buckeye State (Sinnott-Armstrong 1999: 177).

When invoked against someone who denies the first premise (who believes, say, that Indiana is the Buckeye State), SA merely gainsays, providing no further evidence. But when presented to someone who does not know the first premise, SA is unproblematic. The argument is circular, but can be used fruitfully.

To identify the contexts of argument, to discover when reasoning is circular and whether a given circle is fallacious, two models have recently been explored: an epistemic model and a dialogical model.[114]

On the epistemic model, knowledge is ordered as in axiomatic theories and circular reasoning is that which invokes later propositions in arguments for earlier ones. Within formal systems with an ordinal ranking among theorems, defining circularity according to the epistemic model might be useful, at least as a partial definition of a fallacy. But ordinary and philosophical reasoning is unlikely to be foundational in this simplistic sense.

Mathematical reasoning, which admits of axiomatic organization, may invoke such an epistemic model to characterize circularity within particular proofs. But given the variety of competing axiomatizations available for any mathematical theory, the epistemic model can not explain mathematical circularity more generally.

On the dialogical model, circular reasoning is that which assumes a claim which is not part of the background or common assumptions of the participants in the dialogue. On this model, an argument is viciously circular depending on the context of its use. The question remains: what are the pragmatic

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[112] Or, anyway, not purely formal. Biro 1977 argues for an account of vicious circularity which he calls epistemic and which believes avoids the subjectivity of pragmatic interpretations. See Sanford 1981: appendix for a response to Biro.

[113] See Walton 1991, esp. pp 29-33.

[114] See Walton 2006.

defects of question-begging arguments which distinguish them from benign or virtuous circles?

On either model, the strategy for finding benign or virtuous circles is to seek evidence, in the earlier propositions or within the dialogue, which can support some aspect of the argument, to ground one or more premises.[115] When presented with a circular argument, one asks whether there are independent justifications for any of its premises. If so, the circular argument can be seen as pragmatically useful even if it contains no novel evidence for its conclusion. Finding such evidence is sufficient for identifying virtuous circles.

For Sinnott-Armstrong's SA, appeals to a state proclamation dubbing Ohio the Buckeye State, say, could ground the first premise, avoiding circularity. Roy Sorensen provides compelling examples of virtuous circles, in five classes of arguments, like RS, which are themselves evidence for their conclusions.

RS There are at least two tokens of an eleven-word sentence.
So, there are at least two tokens of an eleven-word sentence (Sorensen 1991: 250).

RS and similar examples are neat tricks, consistent with a pragmatic theory of circular argument. The evidence supporting such arguments, the grounding of a particular premise, is found directly in the argument.

Sorensen's examples, like those characterized as benign on either the epistemic or dialectical models, are grounded by evidence. While philosophers debate whether there are further conditions on virtuous reasoning, they all seem to agree that an ungrounded circle is vicious. Mill argued for the weakness of deduction precisely because of its ungrounded circularity.[116] The claim that ungrounded circles are vicious ones, empty and null, continues to be the standard view.[117]

The cases of §2 are of ungrounded circles, as is the case of central importance, intuition-based autonomy platonism. The recent work on virtuous circles, on dialogical and epistemic modes, does not engage the question of whether we can accept such circular arguments, ones with no support from external premises or self-supporting premises. For such justification, we have to look elsewhere.

§4: Ungrounded Circles in Philosophy

Putting aside pragmatic analyses of circular reasoning which invoke background grounding, in this section I argue that some ungrounded philosophical circles are virtuous. Nelson Goodman famously argues that our justifications of induction and deduction are both circular and not problematic.

> How do we justify a *de*duction? Plainly by showing that it conforms to the general rules of deductive inference. An argument that so conforms is justified or valid, even if its conclusion happens to be false... Principles of deductive inference are justified by their conformity with accepted deductive practice. Their validity depends upon accordance with the particular deductive inferences we actually make and sanction. If a rule yields unacceptable inferences, we drop it as invalid. Justification of general rules thus derives from judgments rejecting or

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[115] See Rips 2002: 773 for references to related work on grounding, in rhetoric.

[116] See Mill 1941: Book II, Chapter III, §1.

[117] "[T]he innocuous circularity of (all) valid arguments becomes vicious only when such arguments are used to lead us from a supposedly known truth to a supposedly unknown one, where the former is no more knowable than the latter" (Biro 1977: 264).

> accepting particular deductive inferences (Goodman: 1979: 63-4).

According to Goodman, our certainty regarding deduction does not come from *a priori* insight into the correctness of some abstract, general principles. Instead, we have simple beliefs about which inferences are acceptable. We formulate deductive principles which accord with these inferences. We accept inferences which follow the deductive principles we construct. We justify the particular inferences by the general deductive principles, and the deductive principles by their individual instances. We have reasoned in a virtuous circle.

Similar remarks hold for Goodman's views of induction. Consider how one might introduce a term like 'tree' to a language which lacks it. We perceive some similarities in our environment, say elms, maples, and oaks. We introduce 'tree' to apply broadly to these things and not to mountains or cats. Once we introduce 'tree', we look for some explanation of what makes something a tree, some essence or unifying principles. Once found, we can use these principles to determine whether borderline cases (e.g. pomegranate shrubs, azaleas, or geraniums) are, in fact, trees. In some cases, we discover that terms we have chosen do not apply to all of our original paradigms according to our general principles. 'Fish' does not apply to whales, even if it were originally introduced to apply to all sea creatures. Scientists discovered regularities among hidden properties of mammals and other fish which override their more obvious properties.

> An inductive inference, too, is justified by conformity to general rules, and a general rule by conformity to accepted inductive inferences. Predictions are justified if they conform to valid canons of induction; and the canons are valid if they accurately codify accepted inductive practice (Goodman: 1979: 64).

Goodman's account of both deductive and inductive justification is clearly, unapologetically, and ungroundedly circular. We justify particular inductive or deductive claims or beliefs in terms of general principles from which they follow. We justify our general principles in terms of the specific claims they yield.

> This looks flagrantly circular... But this circle is a virtuous circle. The point is that rules and particular inferences alike are justified by being brought into agreement with each other. *A rule is amended if it yields an inference we are unwilling to accept; an inference is rejected if it violates a rule we are unwilling to amend*. The process of justification is the delicate one of making mutual adjustments between rules and accepted inferences; and in the agreement achieved lies the only justification needed for either (Goodman: 1979: 64).

The virtuous circle that Goodman defends is exactly, in structure, the one I have been defending for intuition-based autonomy platonism. It has come to be known, in the work of John Rawls, Catherine Elgin, and others, as reflective equilibrium. In *A Theory of Justice*, Rawls develops a normative ethical theory, beginning without any presumption of either first principles of justice or stable particular claims. Instead, he begins with tentative principles and raw intuitions and works up to theories of justice and considered judgments which are adopted together. Our particular ethical judgments support our ethical theories and our ethical theories yield (and thus justify) particular ethical judgments. By eschewing appeals to reductive or grounding principles, he presents his theory of justice as an ungrounded circle.

Rawls developed his method in part by reflecting on Chomskyan linguistic methodology.

> A useful comparison here is with the problem of describing the sense of grammaticalness that we have for the sentences of our native language. In this case the aim is to characterize the ability to

> recognize well-formed sentences by formulating clearly expressed principles which make the same discriminations as the native speaker. This is a difficult undertaking which, although still unfinished, is known to require theoretical constructions that far outrun the ad hoc precepts of our explicit grammatical knowledge (Rawls, *A Theory of Justice*, 47).

For Chomsky, we construct linguistic theories on the basis of our intuitions about grammaticality. Chomsky's goals are the specifications of Universal Grammar (UG) and the transformation rules which map UG into specific natural languages. To specify the structure of UG, linguists rely on intuitions of ordinary folk regarding grammaticality. These intuitions about grammaticality form the starting points of the theory, as our intuitions about fairness and justice form the starting points of the theory of justice, as our intuitions about which deductions are acceptable form the starting points of logical theory, and, I am arguing, as our intuitions about mathematics form the starting points of mathematical theories. The theory of grammar is then tested by its conformity to our linguistic intuitions. Our final theory is constructed to accommodate our intuitions, balanced with our interests in simplicity, strength, and other theoretical virtues.

These circular philosophical methods of Goodman, Rawls, and Chomsky are also found in Jerrold Katz's response to Quine's argument against the intensions, the circularity criticism we saw in §2. Katz develops what he calls a non-reductive or autonomous theory of sense, defining sense strictly in terms of sense properties.

> (D) Sense is that aspect of the grammatical structure of sentences that is responsible for their sense properties and relations (e.g. meaningfulness, meaninglessness, ambiguity, synonymy, redundancy, and antonymy) (Katz 2004: 17).

By defining 'sense' in terms of sense properties, Katz gives up the demand for a reductive definition and adopts the family of related intensional notions together. We have a set of inter-theoretically linked intensional terms: analyticity, meaning, synonymy. We adopt the whole group by appealing to their systematic virtues for the intensional idioms themselves. His circular definition D allows substantial characterizations of the relations among various intensional properties.[118]

The autonomous theory of sense was not acceptable to Quine. But the central question facing the intuition-based autonomy platonism is whether the autonomous theory of sense is objectionable because of its structure or for some other reason. If the problem is structural, then intuition-based autonomy platonism, as well as these other philosophical circles, have to be abandoned.

But despite his argument in "Two Dogmas," Quine's real objections to meanings are not formal or logical, against the circularity of the definition. Quine's real arguments are Okhamist, concerning whether they must be introduced to account for behavior.

> 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 1990: 198).

This interpretative claim is centrally important here. Since the question of the acceptability of intensions concerns whether they play a role in a legitimate theory, the argument against circularity in

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[118] The idea of a adopting a family of related notions may be seen in Quine's adoption of the family of notions of logical truth: equivalence, implication, consistency; see Quine 1986: 48-49. We could take this family of logical notions as a fifth example of a virtuous circle.

"Two Dogmas" is otiose and there is no relevant objection (at least in Quine's argument against meanings) which can generalize to other philosophical circles (or "closed curves").

In mathematics, circular reasoning is often useful and may be essential. For example, derivations of equivalencies, as of different versions of the axiom of choice or of the parallel postulate, are inherently circular.[119] Such examples abound within mathematics. Moreover, as Russell argued, the standard account of our interest in the axioms of mathematics is insufficient without a recognition of the inherent circularity in our methods, as we saw in §9.5. On Russell's view, and my own, our adoption of mathematical theorems is not based on some kind of especial insight into fundamental axioms. We adopt a theory *en masse*, justifying the axioms by the theorems they yield and justifying the theorems by appeal to the axioms. Again, the reasoning is circular.

§5: Vicious Circles in Science

In §2, we looked at two philosophical arguments which appeared fallaciously circular: for the veridicality of scripture and for meanings. I then argued that circular reasoning is a central component of some philosophical methods and that at least one of those two arguments, the one about meanings, may be unproblematic even though it is ungroundedly circular. I do not mean to indicate that there is no problem with circular reasoning. Circular reasoning in ordinary contexts can be sophistical and irresponsible. Descartes is correct that the scriptural circle is unconvincing to many people. And circularity is related to some deep problems in science. In this section, I want to show how the problem of circularity in philosophical methods is of a piece with some problems in science proper.

In academic contexts, several recent papers allege that circular reasoning underlies some dangerous errors in science.

> Inability to detect circles in others' arguments leaves people at the mercy of inappropriate or unscrupulous attempts at persuasion. Inability to detect or to break out of circles in one's own thinking may lead to narrow-mindedness, or even delusions, in which one's beliefs about a topic are self-authenticating, sealed off from evidence that might cast doubt upon them (Rips 2002: 768).

Gerd Gigerenzer complains that psychologists errantly invoke overly simplistic (one-word) or circular explanations, ones which are, unscientifically, immune from refutation.

> Here is how circular restatements work: The observation that A influenced B is "explained" by saying that A had the propensity to influence B... The explanation is the phenomenon, merely couched in slightly different terms. Thus, when researchers observed that people are influenced both by the logical form of a syllogism and the believability of its conclusion, this belief-bias effect was explained by the operation of two reasoning systems, one logical and the other based on prior belief (Gigerenzer 2009: 2).

Gigerenzer describes ascriptions of so-called matching bias which do no more than label errors which participants make on the Wason selection task. They hint at the presence of a psychological mechanism one might find explanatory of subjects' errors. But they do not provide any evidence for that

[119] "[I]n the context of the axioms of Euclidean geometry, the parallel postulate was proved to be equivalent to the proposition that the sum of the interior angles of a triangle is 180 degrees. It is not very plausible to claim that the derivation of the theorem was fallacious on account of begging the question" (Wilson 1988: 50).

mechanism beyond the errors themselves.

Gigerenzer thus frames the old problem of vacuous explanation (e.g. morphine having the dormitive virtue) as one of circularity in scientific methods. Since the phenomenon and the explanation are equivalent, pretending that one explains the other is viciously circular. Gigerenzer extends this criticism from psychological explanation to behavioral economics, including research on inequality aversions, availability heuristics, and other cognitive biases.

In a recent paper in neuroscience, Vul, Harris, Winkielman, and Pashler raised striking worries about high correlations between activity in the brain and measures of personality and social behavior. Validity levels in several reports even exceeded the reliability of the measure. Vul et al. noticed that a wide range of researchers looking at correlations between brain activity (as measured by fMRIs) and emotional or behavioral conditions selected only the data from areas of the brain (voxels) that showed correlations in early tests. Scientists were making what Vul et al. call a nonindependence error.

> This approach amounts to selecting one or more voxels based on a functional analysis and then reporting the results of the same analysis and functional data from just the selected voxels. This analysis distorts the results by selecting noise that exhibits the effect being searched for, and any measures obtained from such a nonindependent analysis are biased and untrustworthy (Vul et al. 2009: 279).[120]

As an analogy, Vul et al. show that one can select a weather station whose temperature readings seem to predict changes in the value of a set of stocks with a high correlation. They computed the correlations between the readings of a weather station in Adak Island, AK, with each of 3,315 financial instruments available for the New York Stock Exchange between November 18 and December 3, 2008. But they averaged the correlation values of only the stocks whose correlation exceeded an arbitrarily high threshold.

> Thus, the final measure (the average correlation of a subset of stocks) was not independent of the selection criteria (how stocks were chosen): this, in essence, is the nonindependence error. The fact that random noise in previous stock fluctuations aligned with the temperature readings is no reason to suspect that future fluctuations can be predicted by the same measure, and one would be wise to keep one's money far away from us or any other such investment adviser (ibid: 280).

Vul et al. label the nonindependence error one of vicious circularity: one chooses the voxels by their correlation with the phenomenon under study and then reports a high correlation based on the study of the (blood oxygen levels in those) voxels and their relations to observable behavior. They argue that the error is pervasive throughout neuroscience. Fiedler 2011 argues that this general problem of sampling bias, of choosing samples based (viciously circularly) on their prior-known characteristics, is pervasive in psychology, too.[121]

In a 1994 paper, Harold Brown considered just these kinds of recently-observed problems in the

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[120] See also Kriegeskorte et al. 2009: "Such distortions can arise when the data are first analyzed to select a subset and then the subset is reanalyzed to obtain the results. In this context, assumptions and hypotheses determine the selection criterion and selection can, in turn, distort the results" (535).

[121] Hahn 2011 argues that some of the problems may not be circular arguments, technically, but, "[P]otentially illicit dependence" (Hahn 2011: 179). Absent a better theory of circular reasoning, the distinction matters little here.

philosophy of science. The problem Brown considers is related to the old problem of the observation sentence: if all observations are theory-laden, then no observation can support a theory without circularity; any observation is guided by the theory it is supposed to test. There appears to be no way to step out of the circle of our best theory to determine whether evidence supports it.

Brown argues that this problem is merely theoretical. In practice, observations can contravene an assumed theory. He considers using telescopes to determine the change in angle between two distant celestial objects. One assumes the theory of relativity in the process of designing the experiment. In particular, one uses a relativistic formula for the Doppler effect to determine the recession velocity of one of the objects. One then uses the recession velocity to determine the change in angle between the two objects. Using the recession velocity to confirm relativity theory would lead to a problematic circle, since we assume the relativistic version of the formula for the Doppler effect. But using the recession velocity to determine the change in angle between the two objects does not violate circularity strictures, since the resultant calculation can yield a speed (between the two objects) greater than the speed of light, which is prohibited in relativity theory. In other words, one assumes relativity theory to design the observation/ experiment, but there remain opportunities for the result to contradict the assumed theory.

> [T]he moral of this story is that the essential use of an hypothesis in the interpretation of a set of observations does not automatically prevent an empirical outcome that challenges that hypothesis. Such challenges may be impossible in some cases, but this must be shown by detailed examination of the specific case in question (Brown 1994: 409).

Brown thus distinguishes circular reasoning from defectively (or viciously) circular reasoning, even within science, even among the kinds of closed, or sealed, circles which one might consider absolutely problematic. Shogenji 2000 argues that Brown's example and others like it are properly understood, within a Bayesian framework, as not circular at all. Instead of taking a claim (e.g. the theory of relativity) as both an hypothesis and a background belief under test, which would lead to circularity, we can see it as an hypothesis twice. Similarly, but within the context of economics, Nagatsu 2010 argues that the availability of disconfirming results obviates worries about the circularity of the set-up. Whether circular reasoning in science is legitimate or not depends on the particular case, on the design of the experiment and the opportunities for a result to undermine a given hypothesis.

The problem of distinguishing between legitimate and illegitimate scientific methods in neuroscience, psychology, and empirical science broadly thus appears to be difficult and messy. I have no broad and easy solution to it. The problems must be solved by working practitioners in their respective fields, not dilettante philosophers like me. What is important to note about the discussion of this section is that the question of whether reasoning is viciously circular is really a deep question about scientific or philosophical methodology.

Still, there are two morals that might generalize to any methodology. First, with Shogenji, we can see both particular claims and theories which are to be brought into balance with them as hypotheses available for testing. For the case of central importance here, this entails seeing both our mathematical intuitions and our systematizations of them as hypotheses under consideration. Second, it is important to have criteria which can undermine hypotheses and bring particular judgments into question. All claims must subject to evaluation and refutation. Again, in the mathematical case this is the role of a solution to the demarcation problem.

§6: Virtuous Circles and the Demarcation Problem

Let's return to a more-direct evaluation of whether ungrounded circular justifications, like the ones to which I appeal in my defense of intuition-based autonomy platonism, can be legitimate. Rosanna Keefe limns three cases in which philosophers accept some circularity.

First, take accounts of response-dependent concepts. Many have thought that the essence and application conditions of various concepts must be characterised by reference to judgements involving the very notions in question. For example, they specify the conditions in which the concept of red applies to something in terms of people's judgements that it is red; similarly, perhaps being money requires being regarded as money. Second, some philosophers have proposed self-professed 'non-reductive' analyses of, for example, possibility. They deny the notion of possibility can be reduced to non-modal notions and claim to give illuminating analyses, despite presupposing, or employing, some (primitive, or unanalysed) possibility. Third, it is sometimes said that circularity can be fine if the circle of concepts is big enough. Perhaps we can analyse concepts by tracing their conceptual connections with other concepts, where, arguably, circularity is unavoidable, since you cannot escape the network of inter-connected concepts (Keefe 2002: 276-7).

Let's call these:

AC Application conditions
NRA Non-Reductive Analyses
SC The size of the circle

Taking SC first, one might think that virtuous circles are just large ones. Iacona and Marconi, for example, propose that one might discern fallacious circular reasoning by the "straightforward" entailment of a conclusion from a premise (Iacona and Marconi 2005: 30). Similarly, Mark Johnston claims that definitions are circular only when they yield trivial biconditionals; see Johnston 1989: 147. The examples of §4 are large circles. Small, ungrounded circles, as in Descartes's scriptural example, seem unproductive, both pragmatically and philosophically.

Moreover, as we increase the size of a defective circle, its viciousness seems to wane. CG1, one might plausibly think, is a worse argument than CG2.

CG1 We know that God exists because we know that God exists.
CG2 We know that God exists because the Bible says that He does.
We know that the Bible is true because it is the word of God.

Given that CG2 seems to improve the relevant argument, if only slightly, one might wonder whether large size could be a sufficient condition for virtuous circularity. The examples of useful circles of §4 all seem to gain support from exploring the mutual interrelations among various concepts: Quine's between various intensional properties; in mathematics, the relations among axioms and derived theorems and among different varieties of axiomatizations; in ethics, among particular ethical claims and theories.

Both Shogenji and Hahn argue that larger circles can help achieve the pragmatic goals of presenting an argument. By increasing the size of the circle, we increase our confidence in the inter-related aspects of the large circle and thus increase our confidence in the circular argument. Hahn's example, related to the worrisome circles in scientific methodology, is patterned after CG2.

CE Electrons exist because their signature effects can be seen in a cloud chamber (Hahn 2011: 173).

On the one hand, we can see the underlying structure of CE as just like that of CG2. On the other hand, CE seems legitimate in ways that CG2 is not.

> The self-dependent justification case is analogous: Staring at the cloud chamber, one cannot simply assume that the traces one is seeing are the signature effects of electrons, not only because the existence of electrons is the claim in question, but because even if they do exist, what the cloud chamber shows may not actually be caused by electrons in the way our theories assume. The desired interpretation of what one is seeing, however, is made more likely by our observation, and that, in turn, increases our degree of belief in the claim (Hahn 2011: 174).

One difficulty for using the size of a circle as a guide to its virtue is that some large circles are just as useless as very small circles, like CG1, and can be more defective just for obfuscating. The psychic who uses the claims of the crystal ball to confirm the claims of the crystal ball is no less defective for appealing to long and complicated psychic connections. "An MSPP [mediate standard *petitiones principii*] spanning several mediating arguments will no doubt stand a better chance of passing undetected, but will beg the question just as much as an ISPP [immediate standard petitiones principii]" (Smith 1987: 207).

Consider Walton's Bank Manager example.

> Manager: Can you give me a credit reference?
> Smith: My friend Jones will vouch for me.
> Manager: How do we know he can be trusted?
> Smith: Oh, I assure you he can (Walton 2006: 248).

We might interject a series of references, beyond Jones, for Smith. But no matter how large we make the trustworthiness circle, the circularity problem arises. Even if everyone were to think that Smith is trustworthy, it may still be the case that s/he is not. Since trustworthiness is a concept whose conditions are defined outside the given circle, there is no hope of saving the ungrounded trustworthiness circle, no matter its size.

Still, it may be that in the case of some philosophical concepts, in which external conditions for definition are absent, size is a relevant factor. What gives the examples of §4 their utility is, in part, the demonstrated interrelations. The larger such a circle, the more interrelations can be demonstrated. So SC may be a factor. It may even be a necessary condition. It is not a sufficient condition for virtue.

Similarly, cases of AC need not be problematic. Keefe shows that we can specify the application conditions of 'red', without appealing to redness itself, even if some measure of circularity is present. She considers: 'x is red iff P judges that x is red'. The recurrence of 'red' on both sides of the biconditional ensures a measure of circularity in the definition. But we can determine whether P judges a ball to be red without presuming any knowledge of whether the ball is actually red. Following Humberstone 1997, Keefe calls such cases inferentially circular, but not analytically circular.

Still, some related cases are problematic. Keefe considers 'S knows that p iff S has a true belief that p which S knows to be justified'. Though we need not assume 'S knows that p' to determine the application conditions on the right-hand side, we do need to assume a different knowledge condition: whether S knows that her belief is justified. This redundancy is an unacceptable circularity.

Keefe concludes that the difference between the two cases is one of groundedness. "[T]he circularity of an account is acceptable iff the target term appears on the righthand-side only in the context of compositionally independent operators" (Keefe 2002: 282). In the end, she rejects even this conception. But even if it were to work, it would merely return us to Walton's point about enthymemes: in cases in which there is external support for a circle, the circularity may not be question-begging. But we are looking for criteria for ungrounded circles.

Keefe's NRA is closest to the point. If we have a non-reductive analysis of the theory of sense, say, or a non-reductive theory of justice, we have to ask whether to accept the theory of sense or justice as a whole. We may not be looking for grounding of some portion of the theory. Indeed, the whole point of autonomy platonism, in contrast to the indispensabilist's platonism, is to avoid grounding our mathematical justifications empirically.

In this vein, John A. Burgess argues that circular definitions are problematic if they are either uninformative or inaccurate. Unfortunately, as we know from the old paradox of analysis, if a definition is accurate, it tends to be uninformative and if it is informative, it tends to be inaccurate, extending our concept beyond the common understanding.

Still, working with the dispositional theory of color as an exemplar, Burgess holds that some circular definitions can be both informative and accurate. He claims that the following set of circular definitions of redness is benign.

> 10. *x* is red iff *x* is disposed to look red to most red-competent observers *o* under red-suitable conditions *c*.
> 11. Conditions *c* are red-suitable iff all and only the things which are disposed to look red to red-competent observers in *c* are red.
> 13. An observer *o* is red-competent iff all and only the objects that are disposed to look red to *o* in suitable conditions are red.
>
> ....
>
> I think we do best to think of (10), (11), and (13) as a set of simultaneous equations. No concept in the family - red, red-suitable, red-competent - has any priority. None is reducible to any of the others... nor is any reducible to concepts outside the family. We ascribe all three under the constraint that we are bound to adjust our judgements to ensure that each of (10), (11), and (13) come out true. Our trio of concepts stabilize one another. The blatant circularity ensures this. The informativeness constraint is met in two ways. First, what I have just said about our trio of interdefinable concepts is informative. Showing how they interact as a family illuminates each of them. Secondly... assertability-conditions give us an entering wedge for applying the definitions in a way that is almost always inferentially non-circular. Noncircular surrogates could be at best only contingently true; at worst they would fail, even at the extensional level, to conform to the accuracy norm. What more is required to show that the circularity evident in the dispositional theory of colour is virtuous? (Burgess 2008: 225-31).

The interdependency of Burgess's family of definitions also describes the examples of §4 and the relations between theorems and various axiomatizations in mathematics. In all cases, we have circles which provide stability, informativeness, and illumination. While circles may be supposed to be problematic because they are uninformative or inaccurate, the circles we find in mathematics, in intensional theories, in Rawlsian ethics, and in Goodman's view of logic, are neither.

§7: Autonomy Platonism and the Demarcation Problem

Determining when a set of definitions, a philosophical theory, or a scientific methodology is benignly circular, then, whether it is stable, informative, and accurate, or merely empty bloviating, requires a view about when to adopt a theory and its phenomena, about the demarcation between legitimate theories and phenomena and illegitimate ones. As we saw in the beginning of this chapter, this is, or is a corollary of, the demarcation problem in the philosophy of science: are mathematical theories, ethical theories, linguistic theories (of abstract senses), induction, and deduction legitimate accounts of real phenomena? Or, are they, like the psychic's theory, not?

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, results are dubious.

Burgess and Rosen, in their *A Subject with No Object*, describe seven conditions on good science, conditions which we could invoke, as a working hypothesis, for the designation of virtuous circular reasoning.

> BR1. Correctness and accuracy of observable prediction;
> BR2. 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;
> BR3. Internal rigour and consistency or coherence;
> BR4. Minimality or economy of assumptions in various respects;
> BR5. Consistency or coherence with familiar, established theories;
> BR6. Perspicuity of the basic notions and assumptions;
> BR7. Fruitfulness, or capacity for being extended to answer new questions. (Burgess and Rosen 1997: 209).

The application of BR1-BR7 to a paradigmatically unscientific theory like astrology is weak in a way that the application to a paradigmatically legitimate one, like physics, is strong. That's some evidence that this characterization, or a refinement of it, would work for distinguishing virtuous from vicious circles in philosophy.

I am going to adopt BR1-BR7, somewhat dogmatically and with only one small refinement, as a paradigmatic solution to the demarcation problem. An extended discussion of the details seems unnecessary here. I believe that any solution will yield the same results I am about to sketch regarding mathematics, our central focus. So let's look at how BR1-BR7 apply in the mathematical case.

Merely by omitting 'observable' from BR1, 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. Fundamental mathematical theorems should be perspicuous, as in BR6, and proofs, or at least proof methods, must be available for scrutiny and receive broad acceptance, as in BR5. Also as in BR5, 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's proof increased the range of mathematical phenomena which topology predicts, as in BR2. Mathematicians often seek alternative proofs of a theorem, which helps with BR1, BR3, and BR5. A mathematical theory must be consistent, as in BR3.[122] Axioms should be fruitful, as in BR7, and few, as in BR4. The consequences of axioms should be intuitively acceptable.

The weights we ascribe to the different factors may differ from those we ascribe in any particular empirical case. Mathematicians emphasize consistency, BR3; empirical scientists prefer to emphasize economy, BR5. But empirical cases will differ amongst themselves, too. Physicists tend to weight precision, BR2, more than psychologists, whereas the reverse seems to be true of perspicuity, BR6.

The revised interpretation of observation in BR1 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 seems like magic. But, the uses of intuition on which I am relying are thin and mundane; they are ordinary aspects of everyday mathematical practice. Moreover, the other

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[122]At least, it must be globally consistent. Dialetheic systems might contain local inconsistencies, but these have to be isolated somehow from the rest of a theory.

criteria overwhelmingly favor considering mathematics as a science. We should not stack the deck in favor of one criterion alone. Weighting observable predictions over mathematical predictions, and over the other criteria, renders BR2-BR7 moot. Such a move would be acceptable only to someone with independent objections to taking mathematics to be a legitimate science. In that case, we are using the demarcation criteria, and really only BR1, just to rule out mathematics.

One should not worry that a wider interpretation of BR1 will actually grant legitimacy to pseudo-sciences. Pseudo-sciences like parapsychology fail to satisfy multiple criteria. Psychic practice, for example, 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. Psychics can say whatever they want and their results are not replicable. They fail to cohere with accepted science. The psychic's methods are suspect at all of BR1-BR7.

The application of BR1 - BR7 to mathematics varies from their application to science, but they are the same criteria, properly applied to different domains. 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.

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. Any solution to the demarcation problem with pronounce mathematics legitimate and worthy of our belief.

We look to the practice of science as having consequences for what we should believe exists. The question naturally arises about what makes physics such that we should believe in the objects to which it refers. To answer that question, we develop criteria for determining which scientific practices are legitimate. As we do so, we find that mathematics itself, independent and autonomous from empirical science, is good science, even more successfully fulfilling any set of criteria we may develop.

The use of such criteria may themselves be seen as question-begging, as putting a label on the difference rather than explaining it. The nominalist may argue that using particular mathematical claims to support our general mathematical theories while using the fact that mathematical theories can yield particularly satisfying mathematical claims is viciously circular. The platonist may argue that the circle is, following Burgess and Keefe, virtuous. On the proposal I am presenting, of seeing the question as a corollary of the problem of demarcation, we ask whether a particular set of criteria, like BR1-BR7 apply to mathematics. Predictably, the nominalist denies it and the platonist affirms it. Similar fruitless dialogues can occur concerning the theory of sense or deduction.

The nominalist can argue that the circularity in our justifications of our beliefs in these theories gives us a hint that there is a problem with them. Unless we have a non-mathematical reason for believing mathematical theorems (say in terms of the applicability of mathematics to the physical world) or for believing in senses (say in terms of the grasping of concepts in thought), we should be suspicious of such theories. But this objection just shows, once again, that the real question about ungrounded circular reasoning in philosophy is the demarcation problem. We know from the fact that circular arguments are valid that there is no formal problem with them. We now see that the problem with ungrounded circular reasoning is not so much one of putting the arguments in proper pragmatic context. Instead, it is a general problem in the philosophy of science. The defective and dangerous circles of §5 are problems with scientific methods; to see them as problems of circular reasoning is misleading. The virtues of the philosophical circles of §4 are ones which we apprehend from the same point of view of evaluating theories as we use to develop solutions to the demarcation problem.

A typical attitude toward circular arguments is that they are empty or useless, pragmatically. "One convenient feature of the sweeping rejection of circular justifications is that the objector is freed from any further need to consider the details of the argument in question" (Brown 1004: 406). In the cases of the philosophical circles I am examining, and the way we should deal with them, this criticism is inapt even though the circles are closed.

I have discussed a wide range of circular reasoning in philosophy. But my central concern is with the autonomous justification of mathematical beliefs. I invoked other circles to show that the problem facing our understanding of mathematics is a broad one, that the solution I am suggesting is closely related to solutions in other areas and that whatever objections one might have to my account of intuition-based autonomy platonism, one should not take its faults to be formal. If one rejects mathematical beliefs, as the fictionalist does, one should do so for considerations about the goodness of mathematical theories, ones which, frankly, I am at a loss to imagine.

One should not think that all of these examples of philosophical circles stand and fall together, though the form of the defense I present is in common. We believe mathematical theorems because of their intuitive appeal and their coherence with other results. We believe in the generalizations we make on the basis of induction because of their observable predictions and their coherence with other generalizations. We have circles among evidence and theory. We ask whether the whole package is legitimate and look towards meta-theoretical criteria, such as BR1-BR7, to determine our answer.

§8: Intuition-Based Autonomy Platonism and Naturalism

Epistemologists sometimes distinguish between accounts which focus on the context of discovery and ones which focus on the context of justification. The intuition-based autonomy platonist provides a robust account in the context of discovery since it essentially adopts mathematical practice as self-justifying. The question facing the intuition-based autonomy platonism which I've tried to address in this chapter is whether the method of seeking reflective equilibrium in mathematics can be justificatory. The challenge, I take it, is to show that the circularity of the account is no barrier to its being justificatory. I have shown this by arguing that accounts which rely on reflective equilibrium in the context of justification are common and useful and unproblematic.

As I mentioned earlier, Maddy has paved much of the ground over which I have trod, especially in her deference to mathematical methods, as they are practiced, as an element of mathematical methodology. But Maddy's account falls short of mine in ways which can be attributed to her naturalism.

'Naturalism' is a plastic term with a variety of senses. There are two ways in which Maddy's naturalism debars a proper solution to mathematical epistemology. The first way is in her inability to invoke mathematical intuition as a significant aspect of that epistemology, in her concern about mysticism. The second way is in her unwillingness to evaluate the various strengths of different theories, to engage the demarcation problem. Maddy's Second Philosopher refuses to pronounce on the legitimacy of mathematics because she believes that any evaluation of theories is impossible. The intuition-based autonomy platonist I have described has no scruples about either mathematical intuition, as a fallible but essential cognitive capacity for apprehending mathematical claims, or about distinguishing legitimate from illegitimate theories.

There is a sense of 'naturalism' on which my account of mathematical epistemology is utterly naturalist. It is the sense on which a description of humans as physical beings, with no extra-sensory perceptions or superstitions, is likely to be complete. On other senses of naturalism, ones on which all objects of our ontology are ordinary or physical objects, say, this account is non-naturalistic. I do not know what the proper definition of 'naturalism' is and I do not care to adopt any particular one. Indeed, 'naturalism' is not an important doctrine to me and I do not want to claim that my view is naturalistic. But I do wish to insist that there is nothing spooky or mystical about it.

Typical objections to platonism focus on the mystery of our access to abstract objects. I have argued that there is no mystery, and no access problem, once we understand a posit-based ontology properly. I do not expect that my claims will be readily accepted, given that this virtue of Quine's indispensability argument, which initially and properly adopted this posit-based view, has never been fully appreciated. The posit-based view deflects the access challenge. There is no problem about knowing mathematical objects and no special Gödel-style perception of objects required. Instead, the account I have presented faces the criticism of circularity and bootstrapping.

I hope to have shown, in this chapter, that worries about bootstrapping are endemic to a wide range of philosophical accounts: in linguistics, in ethics, in logic. They are going to arise for any theory which does not depend, either directly or indirectly (via holism), say, on physical theories. Even empirical scientific theories are liable to bootstrapping criticisms if we raise the questions of why we should believe them: we believe in our scientific theories because they account for our sense experiences but we understand our sense experiences only through a background or conceptual scheme of empirical theories. The bootstrapping concern, then, applied to empirical science, is related to the challenge for scientific realists. The problem, if it is a problem, is ubiquitous.

And in its ubiquity, I believe, we find ways to deflate it. When we reflect properly on our methods in philosophy and in science, we find that they are generally attempts to align particular observations with general claims. We grasp a variety of particular claims. We attempt to systematize those claims broadly. We are willing to abandon some of the particular claims for the benefits of elegant and broad systematizations. We look to extend our systematizations to comprehend further particular claims. There is nothing mystical or magical about this process and it does not secure any belief. But neither does its circularity impugn our beliefs in the theories, or particular claims, or the nature of the objects to which the particular and general claims refer. That we hold our claims fallibly does not entail that we are debarred from making claims about the necessity of mathematical truths, when true, or that our reasoning in proofs and in the understanding of particular claims, is *a priori*.

Chapter 11: Conclusions

In the first part of this book, we looked at the indispensability argument through the eyes of an unapologetic platonist and found it sorely wanting. In the last two chapters, I developed an alternative, autonomy platonism.

Any theory which is not strictly empirical science will seem suspect to some philosophers just for being autonomous. To such philosophers, I have little to say except to implore them to recognize how their accounts are incongruous with mathematics as it is practiced and with our ordinary views about mathematics. It is the rare person who does not recognize the robust security of our mathematical claims. The question is how best to mature our intuitions about our mathematical beliefs. Contemporary philosophers of mathematics tend either toward denying them (as fictionalists and reinterpreters do) or to a lame version of platonism on the basis of an indispensability argument. This book is an attempt to provide reasons away from the lame version of platonism and toward a more substantial one.

We started, as so many discussion of philosophy of mathematics do these days, by looking at the Benacerraf-Field dilemma. It is generally accepted that Field's version, which demands that the platonist account for reliability, is better than Benacerraf's original version, which couches the problem in terms of the causal theory of knowledge. But I think that there is more to be said in favor of Benacerraf's version than there is in favor of Field's. Field denies that the central problem Benacerraf raises is about justification. He demands an account of the reliability of beliefs about abstract objects. But an account of reliability is fairly easy. Mathematics is immanently reliable. The real question is whether the reliable and replicable methods of mathematics are justifying, whether the circular nature of the justificatory story I have been telling is legitimate.

One way to see that the problem is one of justification is to remember that the central problem facing the platonist, for a long time, is one of access. The access problem arises centrally from a naive view about ontology, one which is connected to a naive concept of causation. The naive story is that what exists are objects with which we have direct causal contact. Quine's view of ontology as a system of posits properly undermines this story and the access problem disappears with it. But the worry remains: how can our mathematical beliefs be justified in the absence of causal contact between us and the mathematical world? That is, I take it, Benacerraf's question. My answer to that question involved reflecting on our cognitive capacities, especially on our mathematical intuitions, and the practice of refining and systematizing our intuitions into theories.

Intuition is of course a controversial subject, but my uses of it are fairly thin and I think that they should be uncontroversial. We possess neither a complete account of what exists nor how we know what exists. A healthy attitude toward our speculation would be to seek an equilibrium between our best estimates of each. Indispensabilists insist that we settle what exists according to strict empiricist tenets, that all evidence is sense evidence, and that our best beliefs about what exists must conform to these strict constraints about our capacities. Mathematical beliefs surely are beliefs of spatio-temporal beings. But the best account of these beliefs may posit capacities, such as intuition, which do not fit within the empiricist's strict constraints. Such justifications may be acceptable to a broad-minded naturalist as long as they are consistent with our best scientific (including neuroscientific) accounts of our selves. Naturalism in this broad sense could account for mathematical beliefs, beliefs about abstract objects which are inaccessible to our sensory apparatus.

Given its ubiquity and enduring security, we need an account of our mathematical knowledge. Part of this account just refers to ordinary mathematical methodology. 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 to account for our experience. The autonomy platonist starts with mathematical intuitions, perhaps about the natures of mathematical objects, and constructs a 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: 3.)

Quine avoids sense-data reductionism in part due to the practical impossibility of constructing scientific theory out of sense-data, but also on principle. The logical empiricists 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 accepts our beliefs about ordinary physical objects as defeasible starting points for the construction of scientific theories. Those theories are to be judged both on the basis of their ability to account for this initial evidence and, since evidence under-determines theory, on the basis of constraints on theoretical construction. All indispensability arguments share with Quine this view that our evidence does not include our apprehensions of mathematical phenomena.

The intuition-based autonomy platonism I have defended starts with substantive claims in both epistemology and metaphysics. We start our mathematical theorizing with intuitions about a range of mathematical objects: sets, numbers, and spaces. We discover new theorems and generate new proofs which contain existential assertions and we expand our mathematical ontology. Debates over foundations, especially in set theory, have not generated universal agreement on the extent of the set-theoretic universe; we may contract our ontology. We seek a reflective equilibrium between our mathematical intuitions and our mathematical theories, guided by a recognition of the fallibility of our beliefs, both our intuitions and how it all fits together.

In its deferential appeal to the mundane aspects of mathematical practice, my account of mathematical epistemology may be disappointing. There is no magic here. Mathematical objects are not the kinds of things we access with our senses or in any other way. Critics of platonism often argue that the access problem is serious, but there's no real problem to be solved. Mathematics is on solid ground. The proper account of mathematical epistemology should be mundane and disappointing. The problems only arise from a naive view about our world, about our understanding of mathematics and our relation to its objects.

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