The Fregean Assumption

or

Does Philosophy of Mathematics Rest on a Mistake?

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Forward

1. Terminological and Notational Conventions

In this work, I will use some terms in a distinctive way.

The Contemporary Period

I shall use the phrase ‘the Contemporary Period’ to refer to the period of some sixty five years starting in the middle of the Twentieth Century (roughly 1950) and continuing to the present.

The Actual World

I use the phrase ‘the actual world’ in this work to refer to our whole universe consisting of galaxies, stars, nebulae, etc. of which the planet earth we live on is just a part, and including everything that is in, on, or is any part of anything that is part of the actual world. Thus, the actual world contains microscopic entities such as bacteria, atoms, and quarks. In addition, if abstract entities such as numbers and functions also truly exist, then such things also exist in the actual world. The word ‘actual’ is being used here to distinguish our world from the so-called “other possible worlds” that play such an important role in modal logic. (In modal logic, the actual world is represented as just one
of many possible worlds in terms of which ‘necessity’ and ‘possibility’ are defined). I frequently drop the word ‘actual’ when it is clear that I am referring to the actual world.

I also use the phrase

\[ \phi \text{ is true in the actual world} \]

to mean: in the actual world, \( \phi \) is the case.

**Propositions About the Actual World**

By ‘a proposition about the actual world’, I shall mean a proposition whose truth value will depend upon the features, contents, history, or character of the actual world. Thus, any of such a proposition’s quantifiers should be explicitly or implicitly understood to range over the contents of the actual world. For example, if the existential proposition ‘there is a prime number’ is about the actual world, then it should be understood to be asserting the existence in the actual world of a prime number. Thus, the mathematical theorem ‘There are infinitely many prime numbers’, understood to be a proposition about the actual world, should be interpreted to assert that there are infinitely many prime numbers in the actual world.

**Fregean**

\[ \text{See } \{\text{Chihara, 1998 } \#285\}, \text{ Chapter 1 for a full explanation of such usage.} \]
The term ‘Fregean’ is used in this work to denote any scholar who accepts or assumes the thesis that, in general, mathematical theorems are propositions about the actual world. Of course, it is the great logician Gotlob Frege whose name is used here eponymously because he was the first of all the philosophers I shall be discussing in this work to advance a philosophy of mathematics that satisfies the above condition.

Notice that a Fregean, as I use it here, is not necessarily someone who is a “Fregean” in the usual sense of the that term—someone who is a “follower” or advocate of Frege’s philosophy and hence someone who adheres to and accepts many or most of the principal doctrines advanced in Frege’s writings. As I shall show later in this work, there are many Fregeans who accept practically none of the chief philosophical theses advanced in Frege’s writings.

The Fregean Assumption

The phrase “the Fregean Assumption” is used throughout this work to denote the assumption or belief that mathematical theorems are propositions about the actual world. I use the word “assumption” in that phrase because the Fregean Assumption has not generally been accepted or believed by philosophers on the basis of a decisive argument, a careful study, or even a detailed investigation: it is, generally speaking, just an assumption that many philosophers make. (Why they have accepted such a view will be given some speculative explanations later).

Why Investigate the Fregean Assumption?
Why should philosophers in general, and philosophers of mathematics in particular, be interested in a work investigating the Fregean Assumption? First of all, I believe that this assumption is extremely widespread—it is an assumption that is made, I believe, by the majority of the well-known philosophers of mathematics writing in the Contemporary Period. As a result, this assumption has strongly influenced the ontological and epistemological writings of the majority of the Anglo-American philosophers of mathematics.

The philosophical implications of accepting the Fregean Assumption are vast. For example, imagine that a philosopher accepts the Fregean Assumption and as a result comes to believe that all significant mathematical theorems and mathematical truths are true propositions that hold in the actual world. Then such a philosopher should believe that Euclid’s theorem that there are infinitely many prime numbers is a true proposition about the actual world. Consequently, to be consistent, the philosopher should hold that there are, in the actual world, infinitely many prime numbers; hence the philosopher should be a Mathematical Platonist. Clearly, the ontological and epistemological implications of the Fregean Assumption are far-reaching. So there is a need for a serious investigation into the plausibility and truth of the Fregean Assumption.

Now if one examines the Fregean Assumption with a critical eye, one will find that the assumption is by no means obviously true. Why should mathematical theorems and mathematical truths be regarded as propositions about the actual world? It is certainly hard to see how one could put forward the assumption as an obvious or self-evident truth. Thus, one would think that such a widely held and fundamental belief about the nature of mathematics would be subjected to much scrutiny and discussion. Surprisingly, philosophers of mathematics have, on the whole, simply ignored the question of what grounds there are for the truth of the assumption. Why is that the case? And
why have so many philosophers of mathematics accepted the proposition? What is it about the Fregean Assumption that has made it a sort of a default view of the nature of mathematics?

Real Mathematics

Real mathematics is the mathematics used by working scientists and engineers, in everyday scientific work, in engineering and practical scientific work, as well as in highly theoretical work of the most sophisticated kind. It includes the pure mathematics used by practically all mathematicians expanding the boundaries of mathematical knowledge. This is the mathematics that is taught in universities and research institutions all over the world.²

The reader will obtain a clearer grasp of what real mathematics is by pondering the contrast I shall now draw between real mathematics on the one hand and various formal versions of (parts of) mathematics, such as Zermelo-Fraenkel set theory (or ‘ZF’) and first-order Peano Arithmetic (henceforth ‘PA’), that mathematical logicians have developed. Here are some of the ways in which real mathematics differs from such formal theories as ZF and PA:

(1) Language

Real mathematics is expressed in natural languages, in contrast to the logician’s models of mathematics, which are typically formulated in artificial formal languages such as the language of first-order logic.

² What I am calling real mathematics, G. Kreisel and J. Kriven call “intuitive” or “informal” mathematics. Their characterization of this mathematics is: “mathematics as understood by ordinary working mathematicians” ([Kreisel, 1967 #533], p. 160). For these authors, foundational studies “are concerned with describing and analyzing” real mathematics (ibid.)
Reference

In real mathematics, one can refer to practically anything—in model theory, for example, references can be made to such items as the language in which a mathematical or logical theory is expressed, as well as to the symbols and sentences of the mathematician’s language. Reference can also be made to the set theoretical models of the logician’s formal theories. By contrast, reference is severely restricted in the logician’s formal theories: for example in ZF, as standardly interpreted, one can refer only to sets. Thus, strictly speaking, one cannot do model theory if one is restricted to using only the referential resources of ZF alone. When one expands the referential devices of ZF by making use of coding devices, such as Gödel numbering and representation, one is already making use of the referential and deductive devices of metamathematics and not staying within the resources of ZF alone. Thus, a proof that ZF is “conservative” as Hartry Field defines the term does not allow one to conclude that a system in which ZF is coded to do the metamathematics of Peano arithmetic is also conservative. This difference in referential power is certainly a factor that no philosopher should ignore.

Differences in referential power are striking when one compares the difference between real group theory and first-order versions of group theory that have been formalized.³ In group theory, one finds such theorems as:

³ See, for example, {Mates, 1972 #38}, Chapter 11, Section 4.
There is no more than one identity element

which is given in first-order versions as:

\[-(\exists y)((x)(x + y \rightarrow x) \land y \neq e)\]

The theorem of real group theory can also be expressed:

In any group, there is no more than one identity element

But no theorem of first-order group theory can literally express what the above English sentence does, since the vocabulary of the formal theory is not adequate to express the idea of “all group structures”. Clearly, then, there is a difference between the expressive power of real mathematics and that of first-order mathematics.

(3) Expressive power

In mathematics, one makes use of semantic concepts such as truth and satisfaction, which are not even expressible in most formal theories used to model mathematical theories, even when such metamathematical devices as Gödel numbering and representation are used.

(4) Proofs
What function as proofs in the logician’s models of a mathematical theory are “derivations”. These are constructed according to precisely specified syntactic rules from an effectively decidable set of axioms. By contrast, the theorems of mathematics may be proved in systems without clearly specified rules of inference or even definite axioms. As the distinguished mathematician William Thurston⁴ wrote:

[W]e should recognize that the humanly understandable and humanly checkable proofs that we actually do [produce in real mathematics] are what is most important to us [practicing mathematicians], and that they are quite different from formal proofs. For the present, formal proofs are out of reach and mostly irrelevant: we have good human processes for checking mathematical validity. ({Thurston, 1994 #421}, p. 1710).

(5) Axiomatization

Much of mathematics is carried out without having a precisely delimited set of rules of inference specified or even a precisely given set of axioms. Indeed, few areas of mathematics were axiomatized until the Nineteenth and Twentieth Centuries. The logician’s models of mathematics, on the other hand, are generally supplied with a precise set of inference rules and a specific axiomatization.

(6) Definitions

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⁴ Thurston, a Berkeley Ph. D. and Princeton professor, was awarded the Field Prize in 1983 for his work in an area of geometry known as “foliation”. Of particular interest to philosophers of mathematics are his writings on proof and intuition, which have given rise to a lively discussion among professional mathematicians. For a discussion of his work on the geometry of three dimensional manifolds that played an important role in the path to Grigory Perelman’s celebrated solution of the “Poincare Conjecture”, see {O'Shea, 2007 #536}, pp. 170-4.
Many of the things talked about in mathematics are never defined or even characterized in sufficient detail or specificity so as to distinguish them from a number of other entities talked about in mathematics. Is the natural number two identical to the integer two or the rational number two or the real number two or the set \( \{\emptyset\} \)? In mathematics, such questions are generally left open. For example, was some unique abstract object ever singled out as the referent of the English word ‘two’, when we were being taught to add and to multiply? In many contexts in which arithmetic is used, it is questionable that ‘two’ is being used as the name of something. (This idea is pursued later). This contrasts with what is done in the formal models of mathematics I have been discussing, as say, in the development of classical mathematics in ZF or in the construction of arithmetic developed in Frege’s system where the natural numbers are defined to be specific entities in his ontology.

Philosophers of Mathematics

In this work, I use the term ‘philosophers of mathematics’ to denote those Anglo-American academic scholars who have published one or more substantial works on the nature of mathematics, and excluding those academics who are primarily mathematicians, continental philosophers, Wittgensteinians, or logical positivists. In so using the term ‘philosophers of mathematics’, I do not wish to suggest in any way that those kinds of philosophers listed above who write on the nature of mathematics are not worthy of being classified as ‘philosophers of mathematics’—it is just that I wish to focus my discussion to a more limited group. And I wish to do so without continually adding cumbersome and awkward explanatory and cautionary clauses and phrases.
2. A Personal View of the Nature of Philosophy

Shortly after I began my teaching career at Berkeley, I had lunch with two of my former teachers--mathematicians who were in the city to attend a conference. Intrigued by the fact that I had switched fields from mathematics to philosophy, one of these professors rather pointedly said to me: “So, you are now a philosopher of mathematics! So far as I can see, philosophy of mathematics is either logic or mysticism. Which kind do you do?” Needless to say, I do not regard my work in the philosophy of mathematics as being just logic, even though logical publications, logical results, and logical reasoning are clearly relevant to my view of the nature of mathematics. Nor can I find anything in my philosophical views that is in the slightest way mystical. How then should I have answered their question?

In this section, I shall provide a description, from my general perspective, of the sort of undertaking I take philosophy to be, and also of the goals that I, as a philosopher, am trying ultimately to attain. It will then be clear to the reader that what I do in philosophy is neither logic nor mysticism. I describe my views about the nature and goals of philosophy in this work in order to contrast it with the views of certain other philosophers of mathematics whose writings I shall be discussing: this comparison will show clearly that they have a very different conception of the goals of philosophy from those I shall describe below.

The philosopher seeks an understanding of the world. The sort of understanding sought might be called "Big Picture understanding". What one seeks in philosophy is the really "Big Picture": What, in general and in broad
outlines, is the universe like? What, in general and in broad outlines, is our (i.e. humanity's) place in the universe? How, in general and in broad outlines, do we (humans) gain an understanding of the universe?

This “Big Picture” goal explains a striking feature of philosophy: the fact that, for practically any heavily studied area X of serious intellectual work, there is a philosophy of X. There is philosophy of biology, philosophy of physics, philosophy of language, philosophy of religion, philosophy of art, philosophy of history, and so on. For each X, the philosopher seeks to fit X into this Big Picture.

In this search for the Big Picture, coherence is an essential ingredient. We seek an understanding of X that is consistent with our other beliefs about the universe and us. Take the philosophy of language, for example. Here, we seek an understanding of the nature of language and our mastery of language that is consistent with both our common sense beliefs and also our scientific views about the universe we inhabit and also about us as organisms, with the features attributed to us by science. Any account of the nature of language that conflicted with the prevailing scientific accounts of how we learn a language would be considered by most philosophers of language to be in serious trouble. We seek a coherent and comprehensive Big Picture, where all the different Xs fit together. Thus, one would expect a contemporary philosopher’s account of mathematics to be consistent with our generally accepted views of science and scientific knowledge.

One can see, then, why in philosophy there is great attention to uncovering and solving paradoxes. A paradox is an argument that starts with premises that seem to be incontestable, that proceeds according to rules of inference that are apparently incontrovertible, but that ends in a conclusion that appears to be obviously false. In many cases, a paradox ends in an outright
absurdity or even a self-contradiction. Thus, a paradox evidently shows us that either one or other of our “incontestable” premises are not true or that some “apparently incontrovertible” rule of inference that we used in our reasoning is not valid. Ultimately, it seems to show that the totality of our beliefs do not form a coherent whole and hence that there is a need to repair our beliefs—which is one reason philosophers of mathematics continue to work on the paradoxes of mathematics and set theory discovered in the late Nineteenth and early Twentieth Centuries. It is all part of the philosopher’s ongoing project of refashioning our beliefs into a coherent whole.

My overall view of philosophy can best be understood by examining how I view the specific branch of philosophy I shall be discussing in this work, namely the philosophy of mathematics.

3. A Personal View of the Philosophy of Mathematics

The philosopher of mathematics seeks to fit mathematics into the Big Picture of how we humans gain an understanding of the universe we inhabit—this, in a way that is consistent with our generally accepted views of the world, and with science and scientific knowledge in particular. What I would now like to emphasize is that, from my perspective, the mathematics that the philosopher must fit into the Big Picture mentioned above is the mathematics that is used by working scientists and engineers, in everyday scientific work, in engineering

See {Chihara, 1973 #48}, Chapter 1 for a discussion of the paradoxes and of Russell’s attempt to solve them.

The reader can find a fuller account of my view of the nature of philosophy in the Introduction to {Chihara, 2004 #468}. 
and practical scientific work, as well as in highly theoretical work of the most sophisticated kind. It includes the pure mathematics used by practically all mathematicians expanding the boundaries of mathematical knowledge. This is the mathematics that is taught in the universities and research institutions all over the world. It is what I call “real mathematics”. So the question under consideration is: Why should philosophers of mathematics seek to provide an accurate account of real mathematics? Wouldn’t it be just as useful for the philosopher to have an accurate account of the logician’s formal models of mathematics? Well, if what one wants in philosophy is a Big Picture of what it is that humans learn, study, and use when mathematics becomes a significant factor in their intellectual lives, then it is real mathematics that should be the target of the philosopher of mathematics investigations. After all, mathematics is the mathematics that ordinary human beings learn from an early age; it is the mathematics that the vast majority of practicing mathematicians know, study, do their research in, and publish results about; it is the mathematics that almost all (if not all) scientists use in their scientific work; it is the mathematics that ordinary people use in everyday life when they use any mathematics at all. If we are to understand the intellectual thoughts and achievements of ordinary human beings, we need to gain an accurate understanding of real mathematics! (Of course, an accurate understanding of the logician’s formal models of mathematics may be an important aid to understanding real mathematics, but the ultimate goal should be to understand real mathematics). Of course, much of the mathematical logician’s development of logic is itself an essential part of real mathematics and hence is something that falls within the philosopher of mathematics’s area of study, and hence should be an important part of the account of mathematics being developed.
Philosophers of mathematics should seek to give an accurate account of
the nature of real mathematics for yet other reasons. The philosopher’s
account of mathematics is only part of the Big Picture she seeks to fill in. One
must not lose sight of the other areas of research that need to be accurately
portrayed to obtain a Big Picture of the intellectual life of humans—other areas
that are intimately and importantly related to mathematics. Since mathematics
plays such an important role in the physicist’s work, we can be confident that
an accurate account of physics will require an accurate account of real
mathematics, since, as was emphasized earlier, the mathematics used by
physicists when they construct their scientific theories, make predictions, and
verify their hypotheses, is real mathematics and not the logician’s versions.
Most physicists have no interest in working with the logician’s highly
regimented, grammatically restricted, referentially primitive, models of
mathematics. The real mathematics physicists typically use is neither
axiomatized nor even expressed in the language of set theory or some
formalized version of predicate logic.

My view of the nature of the philosophy of mathematics will be
contrasted with those of the following philosophers of mathematics I shall be
discussing in this work: Hartry Field; the Princeton team of John Burgess and
Gideon Rosen (whose “minimalism” will be examined in Chapters 6 and 7); and
Penelope Maddy (whose “realism” and “naturalism” I shall be discussing in some
detail in Chapters 4 and 10). For example, much of Field’s philosophy of
science and mathematics is based upon the assumption that the mathematics
used in physics is a form of set theory formalized in the language of first-order
logic known as Zermelo-Fraenkel set theory (henceforth: ‘ZF’ for short). In fact,
at one point, Field is forced to resort to a specific form of ZF that he calls “ZFU”
(ZF with urelements), which is formulated in a way that, supposedly, allows the set theory to refer to physical entities. But not only is it the case that scientists almost always use real mathematics in their scientific work, there are also important practical reasons requiring that they use real mathematics, as opposed to the arcane formal versions of set theory that Field has in mind. Just think of how impractical it would be for working scientists to actually use ZFU as the mathematical framework for their empirical theories. Can you imagine a theoretical geneticist attempting to assess the plausibility of some complicated thesis of population genetics while actually working with the intricate version of number theory and analysis that one gets in ZFU?

Should I care whether or not my account of mathematics is consistent with the actual practices of working mathematicians? Well, since I am aiming to give an account of the nature of real mathematics, then, my account of mathematics had better be consistent with the actual practices of working mathematicians. Indeed, this sort of fitting of the account I give with the actual practice of working mathematicians can serve as a sort of criterion of the accuracy of the account. In particular, if a philosopher’s account of mathematics does not fit or agree with the actual practices of working mathematicians, who do their research with real mathematics, it will be hard to defend it as an accurate account of the mathematics that is actually being used.

One can get a better understanding of the conception and goals of “philosophy of mathematics” (in the sense I have been proposing here) by comparing and contrasting my understanding of this field with those set forth by the well known philosophers of mathematics mentioned above. As was indicated in the beginning of this introductory chapter, some mathematicians
and philosophers regard mathematics as little different from logic\textsuperscript{7}, so
philosophy of mathematics may be considered to be one and the same as
philosophy of logic. Certainly some of the big results in mathematics were
produced by logicians proving important logical theorems and developing
important logical theories. And some of the truly big results in logic were
obtained in the course of pursuing goals that arose from researches in the
philosophy of mathematics. Still, I believe one should distinguish philosophy of
logic from philosophy of mathematics. In philosophy of mathematics, as I use
the phrase, one is trying to fit one’s account of the nature of real mathematics
into the Big Picture described earlier. This attempt may be greatly aided by,
and may stimulate, much logical research, as well as important growth, in the
field, but this should not confuse us into thinking mathematics is one and the
same thing as logic (as Russell believed\textsuperscript{8}), for the principal goals of mathematics
may differ greatly from the “goals of logic”—at least as philosophical logicians
understand the phrase.

4. Mathematical Realism

A natural starting point for theorizing about the nature of mathematics is
to regard mathematics as a theory about things as numbers, functions, sets,
and spaces. Of course, this immediately raises the question of whether such
mathematical objects truly exist or are merely objects of the imagination or

\textsuperscript{7} Cf. “Mathematics and logic, historically speaking, have been entirely distinct studies. . . . But
both have developed in modern times: logic has become more mathematical and mathematics
has become more logical. The consequence is that it has now become wholly impossible to
draw a line between the two; if fact, the two are one.” (\textit{Russell, 1920 #103}, p. 194).

\textsuperscript{8} See the Introduction to the Second Edition of \textit{Principles of Mathematics}, where is it written:
“The fundamental thesis of the following pages, that mathematics and logic are identical, is one
which I have never since seen any reason to modify” (\textit{Russell, 1937 #627}, p. v)
“mathematical fictions” of some sort. Now if one were to believe that mathematical theorems are propositions about the actual world—if, in other words, one were to accept what I shall call, in the next section, “the Fregean Assumption”–then it is very likely that one would also end up believing in the existence (in the actual world) of mathematical objects. So, one would very likely end up being what I have been calling a “Mathematical Platonist” (or “Platonist” for short). I am, of course, using the term “Platonism” to denote the view that such things as numbers and sets truly exist (in the actual world). Thus, the term ‘Platonist' will be used to denote the person who believes that such things as numbers, sets, functions, vectors, matrices, spaces, etc. in fact exists in the actual world. Those philosophers who deny or reject the thesis that such entities actually exist are called “nominalists”. The dispute between Platonists and nominalists will be discussed in great detail in this work.

Now Platonism and the Fregean Assumption are related to one another in the following way: any philosopher who accepts the Fregean Assumption will probably also be a Platonist. To see why this is the case, consider the following bit of reasoning:

Euclid’s theorem that asserts the existence of infinitely many prime numbers has been accepted as true by essentially all mathematicians. Now if one accepts the Fregean Assumption, one will believe that the Euclidean theorem about prime numbers is a proposition about the actual world. Hence, most Fregeans believe that there are prime numbers in the actual world and are Platonists.

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9 Much more details on this topic are to be found in (Chihara, 2004), Chapter 5, and (Maddy, 1990).
Let us now ponder the implications of accepting the theorem that there are prime numbers greater than five? Is such an acceptance tantamount to an acceptance of the existence of prime numbers in the actual world? And should such an acceptance commit one to being a Platonist? Well, suppose that the mathematician’s acceptance amounts to no more than the belief that, among the theorems of arithmetic, there is one that can be expressed by the sentence ‘There are prime numbers greater than five’. Strictly speaking, a mathematician who believed that the sentence ‘there are prime numbers greater than five’ is a theorem of arithmetic need not conclude that there are (in the actual world) such things as prime numbers: Such a conclusion requires an additional acceptance of a substantial philosophical thesis such as the Fregean Assumption. Nevertheless, most (but not all) well-known Fregeans are also Platonists.

Actually, most Realists maintain a much more robust philosophical view of our supposed knowledge of mathematical objects than is indicated by the above simple discussion. For example, the renowned mathematical logician Kurt Gödel, whose philosophical views about the reality of sets will be discussed in more detail in Chapter 2, has expressed in his publications a much more philosophically substantial view of how we have come to know that such mathematical objects as sets truly exist.

5. A Fundamental Assumption About the Nature of Mathematical Theorems

Consider the following inference that has frequently been made by philosophers of mathematics throughout the Twentieth Century. There is a theorem that was proved over two thousand years ago by Euclid: it asserts
that there are infinitely many prime numbers. Euclid’s theorem has been accepted as true by essentially all mathematicians and philosophers of mathematics. However, the well-known Princeton team consisting of John Burgess and Gideon Rosen has inferred from Euclid’s theorem that there exist infinitely many prime numbers in the actual world—from which they have concluded that nominalism is false.10

Practically all philosophers of mathematics accept the reasoning of Euclid’s theorem and also agree that a theorem of mathematics asserts that there are infinitely many prime numbers. So how can I, a nominalist, contest the above Burgess-Rosen reasoning that nominalism is false? I believe that the many philosophers of mathematics who agree with the Burgess-Rosen reasoning described above are all making a fundamental assumption about the nature of mathematical theorems that is yet to be justified.

What is this fundamental assumption? It is what I call “the Fregean Assumption”: they all believe that mathematical theorems are propositions about the actual world. Now more than one eminent scholar has accepted the Fregean Assumption without explicitly asserting it; and had they specifically defended such a position in their writings, there might have been a sizable amount of discussion in the literature about such a thesis. It turns out that the Fregean Assumption has generally been something that philosophers tacitly assume rather than explicitly articulate. This immensely popular assumption, which I shall henceforth refer to as “the Fregean Assumption”, will be one of the central topics of this work.

6. Azzouni’s Puzzle

10 Later, I shall give the exact words they used and where they made this inference.
In trying to reconcile common sense views about the nature of mathematics with actual mathematical practices and attitudes, it is not uncommon to run into some difficult and deep puzzles. Such puzzles have been vividly described by Jodi Azzouni in his Metaphysical Myths, Mathematical Practice. What follows is one of these puzzles.

Mathematics seems to be about objects or entities of a peculiar sort: such things as numbers, points, sets, functions, and mathematical spaces are discussed and studied by mathematicians; yet the objects studied, assuming that there are such things, seem to be things that are very different from the physical objects that empirical scientists study.

A crucial part of the practice of empirical science is constructing means of access to (many of) the objects that constitute the subject matter of that science. Empirical scientists attempt to interact with most of the theoretical objects they deal with, and it is almost never a trivial matter to do so. Nothing like this seems to be involved in mathematics. ([Azzouni, 1994 #309], p. 5).

Evidently, mathematical objects do not interact causally with us; indeed, they seem to exist entirely outside our physical space and time. Thus, Azzouni characterizes mathematical objects as “metaphysically inert”, and this inertness gives rise to the following puzzle: there seems to be no epistemic role for mathematical objects to play. More specifically, mathematical objects seem to play no role in establishing mathematical truths—that is, mathematical objects play no role in the common everyday practice of proving mathematical theorems. To illustrate this point, Azzouni suggests imagining that mathematical objects ceased to exist at some time, and that the common

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11 Subtitled “The Ontological and Epistemology of the Exact Sciences”.

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mathematical work of proving theorems went on as usual. It is hard to see how that work would be disrupted by the elimination of mathematical objects. This gives rise to what Azzouni calls “the epistemic role puzzle”: What role in traditional mathematical practice do mathematical objects play? (Azzouni, 1994 #309, p. 58).

7. What Is A Number?

In mathematics, one talks of numbers of various sorts: natural numbers, integers, rational numbers, real numbers, and complex numbers. Philosophers attempting to make sense of this kind of talk typically classify numbers as “abstract entities”: things that do not exist in space and time. Some philosophers regard numbers as merely “in the mind”; others maintain that numbers are not mental creations but rather “eternal objects” that exist independently of human thoughts and concerns. Still others have suggested that numbers are merely signs or symbols that are manipulated and used according to rigid rules.

In attempting to make sense of the philosopher’s talk of numbers, some scholars have raised a variety of puzzling problems, some of them being concerned with problems of reference: As I asked earlier to make a somewhat different point, is the natural number 2 the same entity as the integer 2? Is the integer 2 the very same thing as the rational number 2? Is the rational number 2 equal to the real number 2? Is the real number 2 the very same object as the complex number 2 + 0i? Do the mathematical terms ‘the rational number 2’ and ‘the real number 2’ refer to one and the same entity?
The questions of the previous paragraph raise the following problem of reference: Do the two distinct mathematical terms in the question refer to one and the same entity? Do mathematical terms refer at all. Are they names?

These philosophical concerns having to do with what mathematical terms refer to are deepened by the fact that such questions of reference never seem to arise in actual mathematical practice. In general, mathematicians do not concern themselves with the kind of questions about reference to numbers that seem to bother philosophers of mathematics. As Azzouni has written:

The current philosophical concern with how mathematical terms pick out what they refer to is an oddity from the point of view of mathematical practice, which, in broad respects, is simply not concerned with reference. Any view that fails to explain why this is the case has not explained something crucial about mathematics. (Azzouni, 1994 #309, p. 31).

I shall take up this concern of Azzouni’s later in Chapter 8.

8. A Related Problem About Numerical Terms

Mathematical Analysis abounds with such numerical terms as ‘2’, ‘square root of 2’, ‘2/3’, ‘pi’, and ‘4 + 2i’ all of which, I shall argue, seem to be only “partially interpreted”, because none of them seems to be so defined that the term could name some specific abstract entity. As we have been taught these terms, no specific abstract entity is ever singled out to be the denotation of one of these terms. Instead, we are taught that the totality of terms of the kind in question satisfy some general mathematical laws. For example, Birkhoff and Mac Lane write:

Instead of trying to define what the integers are, we shall start by assuming that these integers, whatever they are, satisfy certain basic algebraic laws. These laws or assumptions will be called
postulates for integers; one can deduce from them all the other known properties of integers. ([Birkhoff, 1953 #380], p. 1).

The result is that the terms seem to have a sort of “incomplete” nature: each term is so characterized that it cannot denote or stand for some specific entity since no specific entity is picked out as the denotation of any mathematical term. Indeed, this feature of numerical terms has led some philosophers to attribute the “incompleteness” to the mathematical objects themselves that are supposedly denoted by these terms. Thus, Resnik has followed Charles Parsons in calling attention to what they both call “the incompleteness of mathematical objects”:

Mathematical objects are incomplete in the sense that we have no answers within or without mathematics to questions of whether the objects one mathematical theory discusses are identical to those another treats; whether, for example, geometrical points are real numbers. This springs from the way mathematics defines its terms. When defining bottoms out, that is, when characterizing its primitive concepts, mathematics is content with axiomatic characterizations . . . Mathematics also reflects the incompleteness of its objects in defining other objects in terms of those taken as primitive. For example, when taking sets as primitive, mathematics authorizes many alternative definitions for its other fundamental objects: thus we have real numbers defined as Dedekind cuts, or alternatively as infinite sequences of rational numbers . . . ([Resnik, 1997 #314], p. 90).

Consider, then, how the natural numbers are “defined” by means of the Peano axioms. Of course, the axioms do not pick out a specific totality of objects—the natural numbers. The axioms can only pick out a kind of structure, where specific terms such as ‘zero’ and ‘one’ do not denote or name particular abstract entities. This is why I suggested that the natural numbers have been
characterized by a theory that is only “partially interpreted”: I thus implied that the theory had succeeded only in specifying a certain kind of structure.\textsuperscript{12}

Looking at other kinds of numbers that analysts work with, such as the rational numbers, the real and complex numbers, and the ordinal numbers, etc., we see that these are typically “constructed” from the natural numbers (frequently within a set theory or similar framework) and hence only “partially defined” as well. The reader can easily establish that these other kinds of numbers are so defined or characterized that a specific number term does not denote a specific abstract entity and is only, in this way, “partially interpreted”.

So should we then postulate “incomplete” abstract entities that are the denotations of mathematical terms as Resnik has suggested? Is the mathematical universe riddled with such “incomplete” entities? These are just some of the questions I shall be exploring in this work.

A Question for the Reader

Consider the following list of philosophers, who have all written extensively about the nature of mathematics:

Gotlob Frege, Bertrand Russell, Alfred North Whitehead, Kurt Gödel, Willard Quine, Hilary Putnam, Hartry Field, Mark Balaguer, John Burgess, Gideon Rosen, Penelope Maddy, David Lewis, Allvin Plantinga, Michael Resnik, Stewart Shapiro, Bob Hale, Crispin Wright, Mark Steiner, Philip Kitcher, Mark Colyvan, and James Brown.

\textsuperscript{12} Of course, there have been attempts to say what specific entities in the actual world the natural numbers are, such as Frege’s attempts in \textit{Frege, 1959 #97} and \textit{Frege, 1964 #120}. But even if one accepted one of Frege’s analyses, the notion of extension of a concept in terms of which Frege defines the natural numbers is itself only partially defined.
I now pose the following question for the reader to consider straight off:

Is there some fundamental feature that each of the philosophers on the above list (but not by every philosopher of mathematics) has at one time or other either attributed to, or else tacitly assumed to be possessed by, those propositions generally regarded as mathematical truths?

This question can be rephrased in the following way:

What, if anything, do all the various views about the nature of mathematical theorems and mathematical truths, expressed at some time by each (but not all) of the philosophers on the above list, have in common?

What about the property of being true? Surely, each proposition generally regarded by mathematicians as being a mathematical truth is in fact true. So won’t it be reasonable to assume that all the philosophers on the above list would believe that each mathematical proposition that is generally accepted as a mathematical truth is, in fact, true? It turns out that such an assumption is not correct, since several philosophers of mathematics—among them, Hartry Field—have maintained that no mathematical theorem is, in fact, true.

The answer to the above displayed italicized question may not be obvious at this point, but I suggest that the reader try to come up with an answer, at
least provisionally, and I shall provide my answer to the question at the end of this work.
Chapter 1

The Clash of the Titans:  
The Fregean Assumption Verses the Hilbertian Alternative

Introduction

I shall begin my analyses of the nature of mathematical truth by discussing a dispute about the nature of geometry, involving two of the greatest mathematical logicians of all time: this dispute was between that “father of mathematical logic” Gottlob Frege and the great all around mathematician David Hilbert.13

Here is how Frege’s achievements in logic have been described by scholars:

If there is one point upon which all recent historians of logic agree, it is upon the eminent place of [Frege] among those who have contributed to the development of the subject. Alonzo Church says flatly that Frege ‘is unquestionably the greatest logician of modern times’; I. M. Bochenski calls Frege ‘undoubtedly the most distinguished thinker in the field of mathematical logic’ and says that Frege’s Begriffsschrift is comparable in importance with only one other book

13 I discussed this dispute earlier (in {Chihara, 2004 #468}, Chapter 2, Section 1). I take up the dispute again in this work in order to introduce the reader to the huge differences in how these two mathematical-logicians view the nature of mathematical theorems.
in the entire history of logic, namely, Aristotle’s Prior Analytics; and William and Martha Kneale find that ‘the deductive system or calculus which he elaborated is the greatest single achievement in the history of the subject.’ (Mates, 1972 #38, p. 227).

Regarding Frege’s importance in, more specifically, the area of philosophy that is the target of this present work, Michael Dummett wrote, in his 1991 classic book on Frege: “[H]e was the greatest philosopher of mathematics yet to have written.”¹⁴

Hilbert, on the other hand, was a working mathematician, whose importance to mathematics went far beyond the areas of mathematical logic and philosophy of mathematics. Thus, Richard Courant wrote:

David Hilbert was one of the truly great mathematicians of his time. His work and his inspiring scientific personality have profoundly influenced the development of the mathematical sciences up to the present time. His vision, his productive power and independent originality as a mathematical thinker, his versatility and breadth of interest made him a pioneer in many different mathematical fields. (The Foreword to Reid, 1970 #437).

Hermann Weyl expressed his assessment of Hilbert with the words:

A great master of mathematics passed away when David Hilbert died . . . No mathematician of equal stature has risen from our generation. (“David Hilbert and His Mathematical Work” published in Reid, 1970 #437).

And Constance Reid cites what was written about Hilbert in Nature to describe the importance of his work and influence:

At the time of Hilbert’s death, it was said in Nature that there was scarcely a mathematician in the world whose work did not derive in some way from that of Hilbert. Like some mathematical

Alexander, he had left his name written large across the map of mathematics. There was, as Nature pointed out, Hilbert space, Hilbert inequality, Hilbert transform, Hilbert invariant integral, Hilbert irreducibility theorem, Hilbert base theorem, Hilbert axiom, Hilbert sub-group, Hilbert class-field. ([Reid, 1970 #437], p. 216).

1. The Clash

In 1899, Frege wrote to Hilbert requesting clarification of some ideas appearing in the renowned mathematician’s recent Foundations of Geometry. This letter became the opening shot in what became known as the “Frege-Hilbert controversy” or the “Frege-Hilbert dispute”, which was carried on by letters in the years 1899 and 1900.

The dispute was primarily about geometry, and their differences ranged over such matters as the nature of axioms, theorems, definitions, and methods of proving independence and consistency. Not surprisingly, the controversy has received much discussion and assessment by scholars and historians of logic and mathematics. As both Frege and Hilbert are now regarded as giants in the fields of logic and the foundations of mathematics, one might refer to the dispute (borrowing a Hollywood title) as the “Clash of the Titans”.

In the introduction to his Festschrift on geometry, Hilbert wrote: "Geometry requires . . . for its consequential construction only a few simple facts. These basic facts are called axioms of geometry." Notice that, in this quotation, Hilbert is claiming that the “few simple facts” that geometry requires are called axioms of geometry--what are called “axioms” are said by Hilbert to

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15 See the Editor’s Introduction to Section IV of [Frege, 1980 #255] for a description of some of this discussion and assessment. [Blanchette, 1996 #557] contains an excellent more recent discussion of the dispute. Also relevant to the dispute is [Blanchette, 2007 #599].

16 This is quoted in ([Frege, 1971 #303], p. 25) and then criticized.
be facts. In his Foundations of Geometry, he tells his readers that the axioms express "certain related facts basic to our intuition" (Hilbert, 1971 #287, p. 23). Thus, it can be seen that Hilbert, at least some of the time, thought of the axioms of geometry as expressing true statements.

However, in section 3 of his Foundations of Geometry, Hilbert asserts that the axioms there "define the concept 'between'," and he goes on to say, in section 6, that the "axioms of this group define the concept of congruence or of motion." These characterizations elicited the following critical question from Frege: "How can axioms [that express facts basic to our intuition] define something?" (Frege, 1971 #303, p. 25). If the axioms are definitions, then for Frege they are laid down by fiat or stipulation. On the other hand, if the axioms are facts (or express facts), then they are truths—and they are truths whether or not we take them to be truths: not something that is stipulated to be so. So how can the axioms be both true statements that express facts and also definitions?17

I believe that Hilbert had adopted a significantly new approach to geometry in his book, but, unfortunately, traces of the old traditional approach (embodied in the language of geometry he was using) remained in his mind to influence how he expressed his thoughts, thus producing the somewhat misleading characterizations of his axioms described above.

We get some idea of how Hilbert regarded his axioms from one of his letters to Frege. In response to Frege's claim that, from the truth of one's axioms, it follows that the axioms do not contradict one another, Hilbert wrote back that, as long as he had been thinking about these matters, he had been saying “just the reverse”:

17 I am here focusing on only one aspect of the dispute between Frege and Hilbert. I discuss other aspects of this dispute in Chihara, 2004 #468, Chapter 2, Section 1.
If the arbitrarily given axioms do not contradict one another, then they are true and the things defined by the axioms exist.\textsuperscript{18} ([Hilbert, 1980 #306], p. 40).

By this reasoning, Hilbert thought that one could prove that the arbitrarily given set of axioms of the real numbers do define something and the things defined do exist. In other words, Hilbert thought that one could simply lay down axioms about some new kind of mathematical entities—axioms stating how these new entities are inter-related—and by proving that the set of axioms is consistent, one would prove that the axioms legitimately define something that can be said to “exist”.

We can make better sense of Hilbert’s ideas by considering them within the setting of first-order logic. Imagine that a mathematician lays down a set of first-order sentences as axioms, and the set is proved to be consistent. This would imply that the set of axioms succeeds in singling out a class of models. Any first-order structure satisfying the axioms would have to be such that the individual constants refer to specific “parts” of (or “points” in) the structure—the “parts” being related to each other in definite respects. In this way, the axioms can be regarded as “implicitly defining” the non-logical constants themselves. The axioms implicitly tell us what the individual constants refer to (and hence “mean”) in the various models of the theory. Thus, it would be natural for Hilbert to claim, as he did in a letter to Frege, that "to try to give a definition of a point in three lines is to my mind an impossibility, for the whole structure of axioms yields a complete definition" ([Hilbert, 1980 #306], p. 40). It is, of course, the whole set of axioms—and not just a single axiom—that determines what properties each model of the theory must have.

\textsuperscript{18} Needless to say, it is by no means clear just what ‘exist’ means in this quotation.
There are passages in Hilbert’s letters to Frege that suggest that Hilbert was definitely thinking in terms of models of the axioms when he called his axioms ‘definitions’. He wrote, for example:

[I]t is surely obvious that every theory is only a scaffolding or schema of concepts together with their necessary relations to one another, and that the basic elements can be thought of in any way one likes. If in speaking of my points I think of some system of things, e.g. the system: love, law, chimney sweep . . . and then assume all my axioms as [specifying] relations between these things, then my propositions, e.g. Pythagoras' theorem, are also valid for these things. In other words: any theory can always be applied to infinitely many systems of basic elements. ([Hilbert, 1980 #306], p. 40).

Paul Bernays, writing in The Encyclopedia of Philosophy, describes Hilbert's axiom system "not as a system of statements about a subject matter but as a system of conditions for what might be called a relational structure" ([Bernays, 1967 #308], p. 497). Similarly, Ian Mueller describes the content of Hilbert's geometrical axioms as "structural" and characterizes the Hilbertian geometry as "the study of structure" (Mueller, 1981), p. 9).

It should be noted that, when we view geometry in the structural way Hilbert did, then the axioms of geometry cannot be said to be literally and straightforwardly true—at least in the usual sense in which factual statements of, say, biology are said to be true. Such sentences could be taken to be “true” only in a specifically mathematical sense in which being “true of” (or “satisfied by”) a mathematical structure is counted as being true. It is for this reason that Hans Freudenthal describes this revolutionary aspect of Hilbert's geometry with the words: "[T]he bond with reality is cut. Geometry has become pure mathematics. . . . Axioms are not evident truths. They are not truths at all in the usual sense" ([Freudenthal, 1962 #310], p. 618).
The editors of {Frege, 1980 #255} have described what they took to be (at that time) the dominant view of Frege’s criticisms of Hilbert, especially among mathematicians: quoting H. Scholz, they write that, while Frege “created much that was radically new on the basis of the classical conception of science, with the result that his critical remarks, though very acute in themselves, are still worth reading today”, his criticisms “must nevertheless be regarded as essentially beside the point” (p. 31). As these scholars assess Frege’s criticisms of Hilbert, Frege was simply “no longer able to grasp Hilbert’s radical transformation of [the classical conception of science]“ (p. 31). Perhaps that assessment of Frege’s criticisms is overly harsh, but I think it is safe to say that Frege did not grasp the mathematical fruitfulness and value of the view of geometry that Hilbert was advancing in his Foundations of Geometry.

It might surprise some readers, especially avid fans of Frege’s work, to learn that Frege had maintained in his {Frege, 1959 #97} that the theorems of geometry were synthetic a priori truths--this at a time when non-Euclidean geometries was already known among geometric specialists, having been developed some fifty years earlier. Wasn’t Euclidean geometry toppled from its supreme perch by such developments? Not so, writes Morris Kline:

For thirty or so years after the publication of Lobatchevsky’s and Bolyai’s works all but a few mathematicians ignored the non-Euclidean geometries. They were regarded as a curiosity. Some mathematicians did not deny their logical coherence. Others believed that they must contain contradictions and so were worthless. Almost all mathematicians maintained that the geometry of physical space, the geometry, must be Euclidean. ({Kline, 1982 #457}, p. 88).

However, by the second half of the 19th Century, there were important mathematicians who had begun to have real doubts about the primacy of
Euclidean geometry.\textsuperscript{19} Still, it was not until the 20\textsuperscript{th} Century that the tide completely turned against this primacy: “The scientific world did not awaken to the reality of non-Euclidean geometry until the creation of the special theory of relativity in 1905” ({Kline, 1963 #559}, p. 522).

How then are we to understand Frege’s persistent and confident belief that the axioms of Euclidean geometry are synthetic a priori truths? Matthias Schirn has noted that “both his writings and his philosophical correspondence provide scarcely a clue about to what extent he kept abreast with developments in geometry in the second half of the nineteenth century” ({Schirn, 1996 #563}, p. 26). He goes on to write:

To my mind, it is astonishing that the influential work on the foundations of geometry by Riemann and Helmholtz, in particular their arguments against the a priori character of geometry, are completely passed over in silence in Frege’s writings. . . . Speculations aside, it is true that he fails to work out any solid argument for the claim that geometric truths are known a priori. By contrast, Riemann and Helmholtz in effect adduce powerful arguments in favour of the empirical nature of geometry. ({Schirn, 1996 #563}, pp. 26-7).

I suspect that Frege’s persistent conviction regarding the a priori nature of Euclidean geometry was, at least partially, due to the following two doctrines that he had accepted: (A) existential mathematical theorems are “ontologically weighty” truths about the actual world\textsuperscript{20}; and (B) there are three sources of all our knowledge: sense perception, a geometrical and temporal source of knowledge that he called “intuition”, and “the logical source of

\textsuperscript{19} Kline suggests that the publications of Gauss’s notes, which expressed his doubts about the possibility of proving the truth of Euclidean geometry and his conviction that the theory of space does not have the same epistemological standing as does pure mathematics, played an important role in this changing attitude toward the primacy of Euclidean geometry ({Kline, 1982 #457}, p. 88).

\textsuperscript{20} By ‘ontological weighty truths’, I mean true propositions that have ontological consequences that are significant and far-reaching.
knowledge”. Thus, Schirn suggests that “it seems that [Frege] never abandoned his conviction that everything geometrical must be originally intuitable and that non-Euclidean geometry leaves the basis of intuition entirely behind” ({Schirn, 1996 #563}, p. 28).

2. The Crux of the Dispute

In this section, I shall focus on one aspect of the dispute that lies just beneath the surface of the debate: Frege and Hilbert disagreed about the nature of an important ingredient of mathematics, and this particular disagreement underlies (and explains) many of their conflicting attitudes toward the nature of mathematics. Strangely, their differing views about this ingredient are never specifically singled out for analysis or investigation during their disputes by either of them. What I want to focus on is the very different positions that these important savants adopted regarding the nature of the theorems of geometry. Frege took the traditional approach to these theorems that went all the way back to Euclid: this is the approach that takes geometric theorems to be propositions (statements that are either true or false) about the world.21 In other words, Frege made the Fregean Assumption regarding geometry.22 More specifically, Frege believed, as did Euclid, that the traditional axioms of geometry were true propositions about real (or “actual”) physical space—that is, the space of the actual world in which we exist. Thus, for Frege, the theorems of geometry are all truths about the physical world we live in. This is what I shall call “the Fregean Assumption” (when restricted to the case

21 This Euclidean view of geometry will be discussed in some detail shortly in Section 3 A.

22 I shall say much more about Frege’s acceptance of the Fregean Assumption later in sections 3 and 5
of geometry); later, the term “the Fregean Assumption” will be applied more generally to theorems of mathematics in general. For example, in the following section, I shall discuss how Frege’s views about arithmetic are related to his attitude regarding Euclid’s development of geometry.

On the other hand, for Hilbert, as I noted earlier and will discuss below in detail, the theorems of his geometry are not truths at all in any straightforward sense of ‘truths’, and only partially signify what entities are being talked about or referred to. Instead, these theorems are true of (or are satisfied by) certain sorts of geometric structures, and it is in this structural way that Hilbert’s geometries are said to be true. Thus, the two disputants in this “Clash of the Titans” are theorizing about very different conceptions of geometry. The Hilbertian geometries are different in kind from what Frege has in mind when he ponders the nature of geometry.

What I shall now do is to take this distinction between Frege’s conception of geometry and that of Hilbert’s and imagine that Frege and Hilbert were disagreeing not just about the nature of geometry, but were putting forward and defending differing views about the nature of mathematics in general. Thus, I shall carry out my discussions of the general nature of mathematics in this work using the following definitions:

The term ‘Fregean’ will be used to refer to those scholars who maintain, assume, or believe that, in general, the theorems or truths of some area(s) of mathematics are propositions about the actual world. Such scholars will be said to be “Fregean with respect to the area(s) in question”. For example, one could say that Frege was Fregean with respect to geometry.
The term ‘Hilbertian’ will be used to refer to those scholars who maintain or believe that, in general, theorems or truths of some area(s) of mathematics are not propositions that are true or false in the usual sense of these words but instead are true of (or satisfied by) of the mathematical structures that are characterized, studied and developed in those areas of mathematics. Such scholars will be said to be ‘Hilbertian with respect to the area(s) in question. Thus, one could say that Hilbert was Hilbertian with respect to geometry.

Also, I may characterize a particular philosopher or mathematician as Fregean (or Hilbertian) with respect to arithmetic or analysis. I should also emphasize that my use of these terms here does not indicate that a Fregean (Hilbertian) necessarily accepts all of the key views that Frege (Hilbert) had put forward. (I may drop the phrase ‘with respect to - - -‘ if the context makes it clear to the reader how the phrase is to be completed).

Now I believe that, with respect to arithmetic and analysis, the Fregean view has become the dominant view among philosophers of mathematics of the Contemporary Period. So the question arises: Why have so many philosophers of mathematics become Fregeans with respect to the above areas of mathematics? This is a question to which I shall propose some speculative answers at some point in this work.

3. The Dispute as an Event in the History of Mathematics

(a) Frege’s Euclidean View
Most philosophers of the Contemporary Period who write about the nature of arithmetic and analysis are, I believe, Fregeans with respect to those two areas, insofar as they can be seen to assume, or in some cases explicitly adopt, the Fregean Assumption with respect to those areas. But if that is so, we can raise the question as to why have so many philosophers become Fregean with respect to those areas? I shall now sketch some preliminary answers to that question, and return to the question in Chapter 10.

One reason why so many philosophers have adopted the Fregean Assumption may be due to the fact that Frege’s view of mathematics was, in its basic structure, so much more familiar to philosophers than the Hilbertian view was. Recall that Hilbert’s view of geometry was model-theoretic. Now during the first seventy five years of the Twentieth Century, among philosophers, the most influential mathematical logicians were Frege, Russell and Whitehead, and Quine, none of whom seems to have played a truly important role in the development of model theory that was taking place among mathematical researchers working in the area of geometry; so it is not surprising that relatively few philosophers made probing studies of the model-theoretic versions of geometry being developed at that time. Furthermore, most philosophers of that period were familiar with Euclid’s Elements. The system of geometry presented in that work was, for over two thousand years, a sort of model of what a good mathematical theory should be. And it is noteworthy that Frege was quite a fan of Euclidean geometry, going so far as to express a complete dismissal of non-Euclidean geometry.\textsuperscript{23}

\textsuperscript{23} I shall provide references and grounds for this claim in Chapter 10.
One can thus regard what Frege accomplished, with his logical analysis of arithmetic put forward in his Foundations of Arithmetic and The Basic Laws of Arithmetic, as a sort of carrying out, in the field of arithmetic, what Euclid accomplished in his Elements. Thus, one might think of Euclid’s geometry as the great ancestor of Frege’s arithmetic. For this reason, it is not surprising to find that philosophers were more familiar and comfortable with Frege’s version of arithmetic than they were with the more radical (for that time) model theoretic version of mathematical theories that Hilbert and his colleagues in mathematics were promoting.

There is, however, one feature of Frege’s logical system that, it should be noted, is not a feature of Euclid’s, and this is because Frege’s system has certain ontological primitives—concepts and extensions of concepts— which are not to be found in Euclid’s geometry. There are no axioms in the Elements that assert the existence of abstract entities of the sort assumed by Frege’s fundamental laws.\(^\text{24}\) It was, of course, the axioms about the ontological entities of Frege’s system—concepts and extensions—that are responsible for the inconsistency of his system and that, ultimately, led to Frege’s complete abandonment of his whole Logicist enterprise.

\section*{(b) Hilbert’s Model Theoretic View of Geometry}

The sort of model theoretic position that Hilbert took, in his dispute with Frege, had its roots in the mathematics of non-Euclidean geometry that was developed in the 19\textsuperscript{th} Century by such renowned mathematicians as Carl Gauss,

\footnote{\textsuperscript{24} There are, however, some axioms that assert the constructibility of various geometric things.}
Georg Riemann, Johann Bolyai, and Nicolai Lobatschewsky. To clarify Frege’s dispute with Hilbert and to explain how the dispute between these two giants of logic is related to the developments in the study of non-Euclidean geometries, let us begin with a brief examination of the differences in their respective views about proving the consistency and independence of axioms.

In his [Hilbert, 1971 #287], Hilbert gave a number of consistency and independence proofs that were essentially what we would now call “model theoretic proofs”. Indeed, he began his exploration of geometry by describing the kind of structure he would be investigating: they would satisfy the five groups of axioms he specified from which the most important geometric theorems would be deduced. Then, in Chapter 3, he proved the consistency of the system by constructing a model of the axioms from the real numbers.

Frege’s reaction to Hilbert’s proofs of consistency was completely negative. The reasons he rejected Hilbert’s proofs of consistency are easy to grasp in light of the previous paragraph. For Frege, the axioms of geometry express propositions that are true. Hence, the set of axioms cannot be inconsistent. It is no wonder that Frege considered Hilbert’s way of proving the consistency of a set of axioms to be beside the point. Indeed, from Frege’s perspective, Hilbert’s model theoretic proofs of consistency are unsatisfactory because they fail to take account of the meanings of the predicates and terms occurring in the axioms. (Of course, in Hilbert’s geometry, the predicates and terms are not fully interpreted). It is no wonder, then, that Frege’s dismissed Hilbert’s proofs of consistency with the assessment “that its central methods

See [Bonola, 1955 #556] for a detailed historically rich description of this development. I shall discuss these developments in more detail later in Chapter 9.

However, I did discuss the dispute about consistency and independence in my [Chihara, 2004 #468]
were incapable of demonstrating consistency and independence, and that its usefulness in the foundations of mathematics was highly questionable” ({Blanchette, 1996 #557}, p. 318).

Obviously, the two disputants differed fundamentally on the question of the validity of Hilbert’s consistency proofs of sets of axioms because they differed about the very nature of the axioms in question. As I have been emphasizing, Frege viewed the axioms in question as expressing propositions about the actual world; Hilbert’s axioms, however, like the (uninterpreted) sentences of first-order logic— are not fully interpreted and not anything that can be true in the way Frege thinks mathematical theorems are true. Since these logicians were writing and arguing with significantly different sorts of axioms in mind, it is not surprising that they never arrived at a meeting of minds regarding the soundness of Hilbert’s method of proving consistency and independence.

4. Who Won the Dispute?

From the contemporary point of view, Hilbert’s view—especially in its use of mathematical interpretations of the axioms of his geometry to generate model theoretic consistency and independence proofs—is generally regarded by most mathematicians who have studied the dispute, as the one that is correct. This is because the model theoretic understanding of the axioms and theorems of geometry is the one that eventually won out in the fields of mathematics and logic. Thus, Patricia Blanchette writes: “Regarding the general usefulness of the method, it is clear that Frege was wrong; the last one hundred years of work in logic and mathematics gives ample evidence of the fruitfulness of those techniques which grow directly from the Hilbert-style approach” ({Blanchette,
Certainly, no respected mathematician today believes, as Frege did, that Hilbert’s model theoretic view of geometry should be “made to line up as a museum piece alongside alchemy and astrology” (({Frege, 1979 #607}, p. 169).

It should also be noted that Hilbert cannot be singled out as the sole, or even primary, inventor of the model-theoretic approach to geometry and foundational studies. For example, at about the time the “Clash of the Titans” was taking place, G. Peano and his students were working with ideas and methods that were strikingly similar to those of Hilbert’s. Furthermore, Alessandro Padoa presented, at the First International Congress of Philosophy held in Paris in 1900, an account of “deductive theories” that is clearly in the model theoretic tradition of contemporary logic, writing:

[D]uring the period of elaboration of any deductive theory we choose the ideas to be represented by the undefined symbols and the facts to be stated by the unproved propositions; but when we begin to formulate the theory, we can imagine that the undefined symbols are completely devoid of meaning and that the unproved propositions (instead of stating facts, that is, relations between the ideas represented by the undefined symbols) are simply conditions imposed upon the undefined symbols.

Then, the system of ideas that we have initially chosen is simply one interpretation of the system of undefined symbols; but from the deductive point of view this interpretation can be ignored by the reader, who is free to replace it in his mind by another interpretation that satisfies the conditions stated by the unproved propositions. . . . Logical questions thus
become completely independent of empirical and psychological questions . . . ([Padoa, 1967 #423], p. 120-1).²⁷

Although it is true that Hilbert was one of the key pioneers in the development of the model theoretic analyses of geometric axioms and theories, the seeds of the model theoretic approach are to be found much earlier in the discoveries and developments related to the rise of non-Euclidean geometries. Nicolai Lobatschewsky is regarded by some historians of mathematics as the “Copernicus of geometry” because he revolutionized the subject ([Boyer, 1985 #124], p. 586). In 1829, Lobatschewsky published an article entitled “On the Principles of Geometry” which described a geometric structure that was both incompatible with the parallel postulate of Euclidean geometry and also inherently consistent. Because of this fact, the paper is said to mark the “official birth of non-Euclidean geometry” ([Boyer, 1985 #124], p. 587).

But it was a student of Gauss’s, Georg Riemann, who made the most spectacular early contributions to the study of non-Euclidean geometries. Of special importance to the development and acceptance of non-Euclidean geometries was the way in which Riemann approached the problem of defining the properties of space. Riemann introduced the concept of a manifold, which is a geometric object that is locally Euclidean but may have a global non-Euclidean structure. This idea was revolutionary and paved the way for the development of non-Euclidean geometries.

²⁷ It has been pointed out in ([Mancosu, #558], p. 323) that Padoa’s notion of interpretation differs significantly from Hilbert’s. For Padoa:

An interpretation of a generic system is given by a concrete set of propositions with meaning. In this sense the abstract theory captures all the individual theories, just as the expression $x + y = y + x$ captures all the particular expressions of the form $2 + 3 = 3 + 2$, $5 + 7 = 7 + 5$, and so on.

Hilbert’s view, on the other hand, is described as follows:

Hilbert defines an interpretation by first specifying what the set of objects consists in. Then a set of relations among the objects is specified in such a way that consistency and independence is shown. ([Mancosu, #558], p. 325).
geometry is Riemann’s generalization of the concept of geometry so that it became the study of “manifolds of any number of dimensions in any kind of space”, thereby freeing geometry from its traditional focus of dealing with points and lines in space: Riemann’s geometry could deal instead with sets of ordered n-tuples that are specifiable according to certain sort of rules {Boyer, 1985 #124}, p. 589). Riemann also showed how one could interpret the primitive terms of geometry as denoting features on the surface of a sphere, yielding a sort of model of a non-Euclidean geometry. Thus, Boyer writes: “In showing that non-Euclidean geometry with angle-sum greater than two right angles is realized in the surface of a sphere, Riemann essentially verified the consistency of the axioms from which the geometry is derived” ({Boyer, 1985 #124}, p. 590). He continues:

In much the same sense Eugenio Beltrami . . . showed that there was at hand a corresponding model for Lobachevskian geometry. This is the surface generated though the revolution of a tractrix about its asymptote, a surface known as a pseudosphere in as much as it has constant negative curvature, as the sphere has constant positive curvature. If we define the “straight line” through two points on a pseudosphere as the geodesic through the points, the resulting geometry will have the properties resulting from the Lobachevskian postulates. ({Boyer, 1985 #124}, p. 590).

Beltrami’s researches showed, in effect, that the fifth postulate was not provable from the other postulates of Euclid, since the first four postulates were true in the model he devised for Lobachevskian geometry and the fifth was not.28

28 Beltrami specified a model that realized a portion of a Lobachevskian plane. Hilbert showed, in his {Hilbert, 1971 #287}, Appendix V, that the entire Lobachevskian plane cannot be realized by a regular analytic surface in space by Beltrami’s method.
One can see from the developments described above that model theoretic concepts and methods were being introduced and fruitfully exploited by these pioneering researchers, long before they were used in the sort of view put forward by Hilbert and the Italian mathematicians. Other important factors in the development of Hilbert’s views about geometry were the disputes regarding the question of what geometry best represented our physical space—a question that could not be answered a priori, and hence was not a question for mathematicians to solve. For this reason, mathematicians began to regard the axioms of geometry in the way Hilbert did in his dispute with Frege and not as propositions about the actual world. As Einstein said: “In my opinion the answer to this question is, briefly, this: – As far as the laws of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality.”  

Thus, we have the development of the sort of geometry that Hilbert published in his Foundations of Geometry: an axiomatic system that has many different possible interpretations and models but no single “intended interpretation”—a view of geometry that has been adopted today by most mathematicians.

5. Frege Made the Fregean Assumption

I wish now to emphasize a specific feature of Frege’s analysis of arithmetic, the noting of which I regard as crucial for understanding how he, and a great many other philosophers of mathematics, went seriously off track: this is his acceptance of the Fregean Assumption. That Frege made the Fregean Assumption can be inferred from how he presented number theory in his

\[^{29}\{\text{Kline, 1963 #559}, \text{p. 521}.\]
Foundations of Arithmetic and The Basic Laws of Arithmetic. What Frege did in those writings was to present a version (or model) of number theory as a formalized axiomatized theory expressed in the language of his new predicate logic, with an effectively decidable set of axioms and rules of inference. The axioms of the system (the “fundamental laws”) were supposed to be propositions about the actual world that neither require proof (to be known to be true) nor have proofs, and are of a general logical nature (to use the terminology of {Frege, 1959 #97}). The theorems of the system, which were proven using only the fundamental laws and definitions, were regarded as analytic. The Basic Laws of Arithmetic was evidently thought by Frege to contain a sufficient number of derivations of theorems to convince readers that all the “truths of number theory” are derivable in the system. This was supposed to convince the reader that all the truths of number theory are analytic. Of course, Frege had argued earlier in his Foundations of Arithmetic that the propositions of arithmetic are analytic (even though he admitted that the considerations he used to support the thesis in his book only showed that the thesis was probable). Thus, we have substantial grounds for thinking that Frege believed that what he called the “propositions of arithmetic” were propositions about the actual world. Since Frege maintained, in his Foundations of Arithmetic, that the theorems of geometry were synthetic a priori truths (and hence propositions about the world), one can infer that Frege believed that the theorems of both areas of mathematics were propositions about the

30 I use the terminology of {Frege, 1959 #97}, p. 4⁺, where ‘analytic’ is defined.

31 Of course, we now know from Gödel’s Incompleteness Theorems that, if we mean by ‘truth of number theory’ a sentence of Frege’s system of number theory that is true, then it is not possible for any such consistent “formal system” of number theory to have all the true sentences as theorems, since the term ‘formal system’ is so defined that it must have an effectively decidable set of axioms. See {Chihara, 1999 #415} for more details.

32 See {Frege, 1959 #97}, p. 102.
world. Since these are the two areas of mathematics that Frege discussed in most detail in his publications, and since there are no grounds to believe that he ever questioned the Fregean Assumption or expressed any doubts about it, I shall classify Frege as a Fregean in the discussions to follow and attribute to him the acceptance of the Fregean Assumption. Given the context of these discussions, no serious confusions should arise.

6. Frege’s Victory

Although Hilbert can be considered to have been on the winning side regarding the way the theorems of geometry should be understood, by a striking irony of history, Frege (in effect) won over a great many (if not the majority) of philosophers of mathematics when it came to the question of how non-geometric mathematics in general should be regarded: it turns out that a large number (evidently, the majority) of philosophers of mathematics of the Contemporary Period can be classified as Fregeans because they explicitly maintain or assume that (non-geometric) mathematical theorems are propositions about the actual world. For example, here is a brief list of some of the well-known philosophers of the Twentieth Century who, on the basis of what they have written at some point in their careers, I classify as having been Fregeans:

Gotlob Frege, Bertrand Russell, Alfred North Whitehead, Kurt Gödel, Willard Quine, Hilary Putnam, David Lewis, Hartry Field, Mark Balaguer, John Burgess, Gideon Rosen, Bob Hale, Crispin Wright, Michael Resnik, Stewart Shapiro, Mark Steiner, Penelope Maddy, Philip Kitcher, Mark Colyvan, and James Brown.
In other words, all the philosophers I listed at the beginning of the Introduction were, I claim, Fregeans. Thus, it is tempting to borrow some terminology from the philosophy of science and call the Fregean view “the Received View” of the nature of mathematics.\textsuperscript{33}

Now some scholars might question my inclusion of Resnik and Shapiro among the Fregeans, on the grounds that Resnik and Shapiro were well-known Mathematical Structuralists. “Surely, no Structuralist could be a Fregean” it might be thought. So I again emphasize that Fregeans are not necessarily Fregeans (in the usual sense of ‘Fregean’). A Fregean merely holds that mathematical theorems are propositions about the actual world. Since Resnik and Shapiro held that structures are things in the actual world,\textsuperscript{34} their view that mathematical theorems are propositions about structures amounts to the view that mathematical theorems are propositions about things in the actual world and hence about the actual world. That is why I classify them as Fregeans.

Why did so many philosophers—especially philosophers of mathematics in the Anglo-American academic world—become Fregeans and not Hilbertians? The answer to this question is not at all obvious, since there are some grounds for thinking that mathematicians as a whole have not adopted the Fregean Assumption. Thus, according to the eminent mathematician Paul Cohen\textsuperscript{35},

\begin{itemize}
  \item[\textsuperscript{33}] See \{Suppe, 1974 #469\}, pp. 3-5.
  \item[\textsuperscript{34}] See \{Chihara, 2004 #468\}, Chapter 4, where the Structuralism of both Shapiro and Resnik is discussed in detail.
  \item[\textsuperscript{35}] Winner of the Bôcher Memorial Prize in \textit{mathematical analysis}. Among philosophers of mathematics, Cohen is best known for his proof of the independence of the Continuum Hypothesis and the Axiom of Choice from the standard axioms of set theory (such as the those of ZF)—work for which he was awarded the Fields Medal and also the National Medal of Science. See his \{Cohen, 1966 #378\} and \{Cohen, 1971 #297\} for more details.
\end{itemize}
“probably most of the famous mathematicians who have expressed themselves on the [Realism-nominalism] question have in one form or another rejected the Realist position” ({Cohen, 1971 #297}, p. 13). Now if the Fregean Assumption were true, one would expect most mathematicians to realize that their theorems should be classified as propositions about the actual world. This is because, if mathematical theorems were propositions about the actual world, then one would expect most famous mathematicians to realize that mathematical theorems were propositions about the actual world. After all, who better to realize that mathematical theorems are propositions about the actual world than the famous mathematicians (who are famous because of their achievements in mathematics)? Hence, if the Fregean Assumption was assumed to be true by most mathematicians, most of the famous mathematicians could reasonably be expected to regard the Euclidean theorem about the infinity of primes to be a proposition about the actual world and hence as asserting that there exists infinitely many prime numbers in the actual world. In that case, one would expect most of the famous mathematicians of the world to believe that there are prime numbers in the actual world and hence one would expect most of these mathematicians to be Realists. Since such an expectation conflicts with Cohen’s observations about how most of the famous mathematicians have expressed themselves on the Realism-nominalism question, we have some grounds for thinking that the Fregean Assumption has not been adopted or assumed by most mathematicians—and certainly not by most famous mathematicians.

In that case, I ask again: Why have so many philosophers of the Contemporary Period become Fregeans (and not Hilbertians)? I suspect that one reason may be due to the fact that Frege’s view of mathematics was, in its
basic structure, so much more familiar to philosophers than the Hilbertian view was. Recall that Hilbert’s view of geometry was model-theoretic. Now none of the early pioneers of logic who were primarily philosophers (I have in mind Frege, Russell, and Quine), seems to have been especially concerned with the developments in model theory taking place during the early part of the 20th Century. More specifically, of the three mentioned above, only Quine seems to have even been aware of any of the truly significant developments and achievements in model theory that were taking place when they were theorizing about the nature of logic; and Quine’s own research seems to have incorporated little of what was taking place in model theory. From the following discussion, it will be seen that Quine was marching to his own (Frege-Russell) drum and not to that of the mathematical logicians who were independently developing the model-theoretic brand of logic.

On the other hand, most (if not all) philosophers of mathematics of the Contemporary Period were familiar with Euclid’s Elements. The system of geometry presented in that work was, as I mentioned earlier, a sort of model of what a good mathematical theory should be like. It is not surprising, then, that philosophers—especially philosophers of mathematics—would have been more inclined to accept the sort of analysis of number theory provided by Frege, which was close to the Euclidean model, than they were to accept the Hilbertian rival that presupposed a radically different version of geometry. (I shall develop this explanation in more detail in Chapter 10).

There is another reason why philosophers of the Contemporary Period could be expected to be more inclined to be Fregeans rather than Hilbertians: the Fregean view allows a much more straight-forward and initially simple

\[\text{[Heath, 1956 #288].}\]
explanation to be given of why and how mathematics can be applied to the physical world. According to the Fregean Assumption, mathematical theorems are propositions about the world, just as the laws and empirically justified generalizations of physics are propositions about the world—they both are analyzed to be propositions about the world—indeed, true propositions about the world. So, given the Fregean Assumption, it is plausible to conclude that mathematical theorems apply to the physical world, just as the laws and empirically justified generalizations of physics apply to the world. To explain why and how mathematical theorems can be applied to the world, when mathematical theorems are understood in the way Hilbertians analyze mathematical theorems, seems to require more complicated and indirect analyses of applications.
Chapter 2

Three Influential Fregeans

Introduction

Despite Hilbert’s victory over Frege in the dispute about geometry, when the focus shifts to the other areas of mathematics such as number theory and analysis, we see that the majority of Anglo-American philosophers became not Hilbertians, but Fregeans. Why? In the previous chapter, I provided one possible reason for the wide acceptance of the Fregean Assumption among philosophers. But there are other possible reasons which I shall explore in this chapter.

To explain why I believe the majority of philosophers of mathematics of the Contemporary Period accepted the Fregean Assumption, I need to discuss the following three philosophers who played central roles in spreading the Fregean view to the philosophical public: Bertrand Russell, Willard Quine, and Kurt Gödel. (I remind the reader that I am writing here about the Fregean (in my special sense) view and not about a general Fregean view).

1. Russell’s Acceptance of the Fregean Assumption
I suspect that most contemporary philosophers of mathematics, who were aware of the fact that Frege was a Fregean, would expect Russell to have also been a Fregean. After all, Russell made many of the same fundamental assumptions about the nature of mathematics that Frege made and Russell’s analyses of number theory was very similar to Frege’s analyses. This agreement is clearly illustrated in the Russell-Whitehead three volume work Principia Mathematica (henceforth PM), where the development of arithmetic from the axioms of Russell’s system is very close to Frege’s development of arithmetic from the axioms of his Grundgesetze. Indeed, it is well-known that Russell was in substantial agreement with much of Frege’s work in logic and the foundations of arithmetic. The following passage of a letter Russell wrote to Frege indicates how much:

For a year and a half I have been acquainted with your Grundgesetze der Arithmetik, but it is only now that I have been able to find the time for the thorough study I intended to make of your work. I find myself in complete agreement with you in all essentials. . . . With regard to many particular questions, I find in your work discussions, distinctions, and definitions that one seeks in vain in the works of other logicians. Especially so far as functions is concerned . . . I have been led on my own to views that are the same even in the details. ([Russell, 1967 #606], p. 124).

That the axioms of PM were understood by Russell in basically the Fregean way (as propositions about the actual world) receives strong confirmation from his discussion of the axiom of infinity, which was one of the axioms of PM:

[N]othing can be known a priori as to whether the number of things in the world is finite or infinite. The conclusion is, therefore, to adopt a Leibnizian phraseology, that some of the possible worlds are finite, some infinite, and we have no means of knowing to which of these two kinds our actual world belongs. . . . The axiom of infinity will be true in some possible and
false in others; whether it is true or false in this world, we cannot tell. ([Russell, 1920 #103], p. 141).

It can be seen from the above quotation (where Russell uses the phrase ‘this world’ to refer to what modal logicians call “the actual world”) that the axiom of infinity was being taken by Russell to be a postulate that may or may not hold in the actual world. Notice, however, when that axiom was listed among the basic logical principles of the mathematics developed in PM, Russell was taking the axiom to be a true proposition about the actual world: it was an assertion that the actual world contained infinitely many “individuals”. All of this implies that the theorems of mathematics of PM were being put forward by Russell in basically the way the theorems of Frege’s system were put forward: that is, as assertions about the actual world. Thus we have strong reasons for maintaining that Russell accepted the Fregean Assumption.

When Frege published his important works on logic, they were neither well understood nor widely read. It seems to me very likely that the tremendous influence of PM—especially concerning matters relating to the nature of logic and mathematics—contributed significantly to the widespread acceptance of many of Frege’s fundamental views about logic and mathematics. However, PM was by no means a best seller when it was first published. Logic was not a subject that intrigued or inspired many philosophers in the early decades of the 20th Century. And I suspect that a great many philosophers of that period had only a vague and sketchy understanding of what was claimed and accomplished

37 The authors of PM felt driven to accept such an axiom, but they had a hard time justifying listing the axiom as a truth of logic (see [Kneale, 1962 #102], p. 669). Furthermore, as the quotation above from [Russell, 1920 #103] indicates, Russell came to question its inclusion among the axioms of logic.

38 According to Russell, Whitehead and he each made a negative profit of fifty pounds from the publication of PM ([Russell, 1968 #609], p. 152).
in PM.\textsuperscript{39} For this reason, I suspect that scores of Oxford and Cambridge dons writing in the early part of the 20\textsuperscript{th} Century simply accepted PM as authoritative in matters of logic and foundations of mathematics.\textsuperscript{40} Since PM assumes many aspects of the Fregean view—in particular, the Fregean Assumption— it is not surprising that numerous English philosophers ended up adopting fundamental views about logic and mathematics that were compatible with those of Frege and Russell. Furthermore, it is easy to see how such an acceptance of views about the nature of logic and mathematics put forward in PM would eventually spread to the United States, especially in view of the fact that both Russell and Whitehead visited and spent time there teaching and giving lectures at a number of academic institutions of learning.\textsuperscript{41}

2. Quine’s Acceptance of the Fregean Assumption

Quine is one of the most influential figures in the history of Anglo-American philosophy of the 20\textsuperscript{th} Century.\textsuperscript{42} In the area of philosophy of

\textsuperscript{39} Part of the reason so few philosophers had a clear understanding of the logical and philosophical details of PM is to be found in the work itself: the philosophical justification, as well as the intricate details, of the logical formalism of PM, were far from clear. The well-known mathematical logician Hao Wang once wrote that he could “remember puzzling over [the pages 37-59 of PM which contains much of its philosophical material] as a freshman in 1940, hour after hour, with little success” ([Wang, 1965 #336], p. 19). It was reported (to me) that Tarski once said (probably jokingly) that it was a good thing the antinomies of set theory and class theory were implicit in Frege’s system, since if we had to find such antinomies in the system of PM, no one would have found them.

\textsuperscript{40} I doubt that very many of the British philosophers of that period really worked hard at understanding the logical details of PM or got their “hands dirty” working with the logical system of that work.

\textsuperscript{41} For a description of Russell’s life in the United States in the years 1938-44, see his [Russell, 1968 #609], Chapter 6. See also [Cooke, 1956 #624], Chapter 5, for Alistair Cooke’s amusing descriptions of his meeting with Russell in the U. S. and of what he learned about Russell’s “need of women” and his “lechery”. Cooke quotes Russell as saying “Chastity: I gave it a good try once, but never again” (p. 200).

\textsuperscript{42} The reader unfamiliar with Quine’s accomplishments and honors can find much more about them in the Stanford Encyclopedia of Philosophy.
mathematics, he is most famous for his Indispensability Argument for the Existence of Mathematical Objects, which was described by Hartry Field to be the only non-question begging argument he knew of for believing that mathematical theorems are true. This argument, which I have discussed in much detail in all of my previous books on the philosophy of mathematics, is certainly the most widely known and discussed argument for Mathematical Platonism of the Contemporary Period. Mark Colyvan, for example, has published a whole book, The Indispensability of Mathematics, presenting the merits and implications of basically Quine’s Indispensability argument and defending it against its detractors. (I shall comment on Colyvan’s defenses later in Chapter 9, Section 6). In this section, I shall discuss this most famous and influential of philosophical arguments for the existence of mathematical objects to illustrate the important role that the Fregean Assumption has played in the philosophy of mathematics of the Contemporary Period.

I begin by discussing briefly a suggestion put forward by Charles Parsons in a recent book (which was somewhat misleadingly entitled “Philosophy of Mathematics in the Twentieth Century”) that Quine was a nominalist—a suggestion that appears to imply that Quine could not have seriously put

43 One can consult my [Chihara, 1973 #48], Chapter 3, for some background reading of this topic.

44 This idea of Field’s will be discussed in Chapter 3.

45 [Chihara, 1973 #48], [Chihara, 1990 #34], and [Chihara, 2004 #468].

46 [Colyvan, 2001 #452]. I shall discuss some parts of this work in Chapter 8.

47 [Parsons, 2014 #622]. I say “misleadingly” because Parsons discusses only a few of the philosophical views about the nature of mathematics that were put forward by philosophers in the Twentieth Century. By comparison, William Asprey and Philip Kitcher qualify their introduction [Kitcher, 1988 #442] to their edited work History and Philosophy of Modern Mathematics, by labeling it “An Opinionated Introduction”, even though they provide a much fuller coverage of the philosophy of mathematics in the twentieth century than is provided by Parsons in his book.
forward an argument for the existence of mathematical objects as I have claimed in a number of my books. Parson’s suggestion, however, concerns a sense of the term ‘nominalism’ that is apt to mislead many philosophers of mathematics: what he meant by the term ‘nominalism’ is very different from the sense of the term that is my concern in this chapter. Thus, Parsons notes that the opening sentence of the early paper {Goodman, 1974 #585}, which Quine wrote jointly with Nelson Goodman, is: “We do not believe in abstract entities”. And Parsons allows that if that is what is meant by ‘nominalism’, Quine’s nominalism was relatively “short-lived”. But Parsons continues:

I will argue that there is another sense in which Quine is a nominalist or at least has a strong nominalist tendency, which appears in his earliest writings and was to all appearances held for the remainder of his career. . . . It can be called nominalism about meaning and in particular about predication. (Parsons, 2014 #622), p. 200).

From this passage, it is clear that Parsons was by no means contesting or undermining my contention that Quine was a Mathematical Platonist.

That Quine accepted the Fregean Assumption is strongly suggested by his assertion that mathematics is an integral part of the empirical sciences—indeed, he says that mathematics is “on a par with” the physical and social sciences, in which it is said to receive its applications (Quine, 1966 #286), p. 231). Such a view implies that the assertions of mathematics are propositions about the world, as are the assertions of the empirical sciences (with which it is “on a par”). Thus, the many theorems of mathematics that are existential in nature are taken by Quine to be asserting that there exist in the actual world

48 Quine, of course, later adopted a brand of “Platonism” which I have discussed in great detail, especially in {Chihara, 1973 #626}, Chapter 2; {Chihara, 1990 #34}, Chapter 1, Section 2; and {Chihara, 2004 #468}, Chapter 5, Sections 2 and 3.
abstract mathematical objects of various sorts—a position that is suggested by the following oft-quoted passage from Word and Object in which Quine says of the nominalist:

He [the nominalist] is going to have to accommodate his natural science unaided by mathematics; for mathematics, except for some trivial portions such as very elementary arithmetic, is irredeemably committed to quantification over abstract objects. ([Quine, 1960 #78], p. 269).

According to Quine, the nominalist cannot accept the consequences of using mathematics in her scientific theories in a way that is consistent with the rejection of abstract objects, because such a use commits one to the thesis that there are, in the actual world, mathematical objects (mathematics is “irredeemably committed to” such objects).49

I now shall give some reasons for thinking that Quine had this Fregean view pretty much at the very start of his philosophical career. We should note that Quine studied, and wrote his Ph. D. dissertation, under the supervision of Alfred North Whitehead. The dissertation, entitled “The Logic of Sequences: A Generalization of Principia Mathematica”, was completed in 1932, and a revised version of the thesis was published in 1934 under the title “A System of Logistic”. It is clear that Quine followed closely, and greatly admired, the Russell-Whitehead line of research from the very beginning of his academic research in logic.50 Thus, given the description at the beginning of this chapter of Russell’s Fregean view of mathematics, it is not surprising that Quine would

49 The reader can find a much more detailed presentation of Quine’s Platonism in [Chihara, 2004 #468], Chapter 5.

50 Quine dedicated his [Quine, 1963 #368] to Russell writing: “To BERTRAND RUSSELL whose ideas have long loomed large in this subject and whose writings inspired my interest in it”.
also adopt the Fregean way of viewing the axioms of PM and of understanding them to be propositions about the world. Of course, we cannot conclude that Quine ever consciously thought that he was siding with Frege and against Hilbert in their famous dispute. I certainly have no evidence that Quine ever seriously studied the Frege-Hilbert correspondence, and I know of nothing that shows he ever considered, or seriously explored, any alternatives to the Fregean position on logic and mathematical theorems which he adopted. Thus, he may have simply regarded the Fregean position as the obvious one. Furthermore, I cannot find any evidence that Quine spent a great deal of time pondering and investigating the ontological implications of Hilbert’s view of logic and mathematics during his early period of research, when he was working on various aspects of PM.

Although Quine seems to have sided with Frege very early in his academic career in holding that the theorems of mathematics are propositions about the actual world, he did not immediately accept Frege’s view that the theorems of mathematics are true. To see why he was reluctant to accept straight off the Fregean view of mathematical theorems, it is enlightening to consider a lecture he delivered at the Harvard Philosophical Colloquium in March of 1946 in which he argued that “the nominalistic issue [whether there are universals or abstract entities] is a real one”.51 It is evident from the lecture that Quine considered the nominalism-Platonism controversy in the philosophy of mathematics to be a serious topic worthy of philosophical research. It was during this period that Quine was striving to be clear about the considerations that support or undermine the various options in the nominalism-Platonism controversy.

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51 p. 6.
Here’s how Quine framed the “thesis of nominalism” in his lecture: “Discourse adequate to the whole of science can be so framed that nothing but particulars need be admitted as values of the variables” (Quine, 2008 #621, p. 6). The question of whether nominalism might be a correct metaphysical view reduces, according to Quine’s analysis of the situation, to the question of whether classical mathematics is absolutely indispensable for science. Since Quine thinks that classical mathematics—even classical arithmetic—is “incompatible with nominalism”, he came to the conclusion that the nominalist is faced with the task of showing “how the service of classical mathematics as an auxiliary to the natural sciences could be performed, adequately though more clumsily, by those fragments of mathematics or logic which are still constructible from a nominalistic point of view” (Quine, 2008 #621, pp. 14-15). Of course, this reasoning depends upon his assumption that the theorems of classical mathematics that are existential in nature are propositions that assert the existence of mathematical entities in the actual world. In other words, the Quinean view that mathematics is “incompatible with nominalism” depends upon his view that the existential theorems of mathematics are propositions that assert, presuppose, or imply the existence of mathematical entities in the actual world.52

When Quine published (with Goodman) his “Steps Toward a Constructive Nominalism”, he seemed to be espousing the nominalistic position. But in many other papers that Quine published in the forties, he seems to be concerned primarily with setting out, and exploring, the options open to the disputants of the nominalism-Platonism controversy, while taking a sort of neutral stance on

52 For more details on Quine’s lectures from my perspective, see Chihara, 2008 #543.
the options. For example, in “On What There Is”—a 1948 paper in which Quine set out his general views on ontological commitment—he wrote:

The variables of quantification, 'something', ‘nothing’, ‘everything’, range over our whole ontology, whatever it may be; and we are convicted of a particular ontological presupposition if, and only if, the alleged presupposition has to be reckoned among the entities over which our variables range in order to render one of our affirmations true. ({Quine, 1961 #344}, p. 13).

He then continued, again making clear his own commitment to the “ontologically weighty” view of classical mathematics:

   Classical mathematics . . . is up to its neck in commitments to an ontology of abstract entities. . . . The issue is clearer now than of old, because we now have a more explicit standard whereby to decide what ontology a given theory or form of discourse is committed to . . . ({Quine, 1961 #344}, p. 13).

However, his conclusion is not the advocacy of Platonism or of nominalism. Rather:

[T]he question of what ontology actually to adopt still stands open, and the obvious counsel is tolerance and an experimental spirit. . . . Let us see how, or to what degree, natural science may be rendered independent of platonistic mathematics; but let us also pursue mathematics and delve into its platonistic foundations. ({Quine, 1961 #344}, p. 19).

Thus, so far as I can see, Quine never wavered, during this period, from his acceptance of the Fregean view that the existential theorems of classical mathematics assert the existence of mathematical entities in the actual world. To make the Fregean underpinnings of this Quinean thesis explicit, I shall now restate the thesis as:

[***] The existential theorems of classical mathematics assert the existence in the actual world of mathematical entities.
Because he accepted [***], he always required the nominalist to develop science without mathematics: nominalists were given the task of developing a nominalistic version of science that utilized some kind of replacement for mathematics that did not commit one to the existence of abstract entities.

Consider the following description that appears in Goodman-Quine “Steps Toward a Constructive Nominalism”:

By renouncing abstract entities, we of course exclude all predicates that are not predicates of concrete individuals or explained in terms of predicates of concrete individuals. . . . We shall, then, face problems of reducing predicates of abstract entities to predicates of concrete individuals, and also problems of constructing certain predicates of concrete individuals in terms either of certain others or of any others that satisfy some more or less well-defined criteria. . . . Devices like recursive definitions and the notion of ancestral must be excluded until they themselves have been satisfactorily explained. ([Goodman, 1974 #585], pp. 175-6).

The nominalist is not allowed, by this account, to use any area of mathematics by understanding it, or interpreting it, to be in some way not ontologically committed to mathematical objects. Neither Goodman nor Quine seemed to have imagined that there could be a way of construing the theorems of mathematics along the lines suggested by Hilbert in his dispute with Frege. No wonder, then, that Quine would end up concluding, in Word and Object, that the nominalist “is going to have to accommodate his natural science unaided by mathematics” ([Quine, 1960 #78], p. 269). This thesis is the core of his Indispensability Argument (an argument I shall be discussing in more detail in Chapter 4). Indeed, the Indispensability Argument rests essentially upon [***]. Of course, [***] rests upon the Fregean Assumption, so Quine’s Indispensability Argument rests ultimately upon the Fregean Assumption. Thus, my position is that:
Quine’s Indispensability Argument presupposes the truth of the Fregean Assumption

Since Quine eventually came to believe that the costs of accepting nominalism were simply too high and that the “thesis of nominalism” could never be carried out in a satisfactory way, he opted for a full-blown Platonic position, according to which mathematical objects are assumed to exist (in the actual world). In this way, he ended up believing both that mathematical theorems are assertions about the world and also that they are true. In other words, Quine ended up adopting a standard form of the Fregean Assumption. I suspect that the assumption that mathematical theorems are propositions about the world was not something that Quine ever seriously doubted.

3. Quine’s Influence

The large influence of Quine’s research and teaching at Harvard in logic and set theory, among philosophers of mathematics in the United States (especially those from the East Coast\(^{53}\)), should also be mentioned in connection with the reason the Fregean view of mathematics became so widespread.\(^{54}\) Since Quine came upon the philosophical scene after having been strongly influenced by the ideas of Russell and Whitehead, having written his Ph.

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\(^{53}\) I suspect that Quine’s influence at Berkeley was not so strong during this period because logic at Berkeley was so strongly influenced by the *Group in Logic and Methodology of Science*, especially given the powerful influence in the group that Alfred Tarski had.

\(^{54}\) Hilary Putnam’s influence in the philosophical community should also be mentioned in this connection—in particular his brief, but clearly argued, [*Putnam, 1971 #390*] supporting the Quinean argument for belief in mathematical objects. I shall discuss Putnam’s paper in detail in Chapters 4 (Section 2) and 9 (Section 3).
D. dissertation on PM, under the supervision of Whitehead (all of which no doubt played an important role in his adoption of the Fregean view of mathematics), many philosophers of mathematics could be expected to regard mathematical theorems in basically the way Quine did (and hence in the way Frege, Russell and Whitehead, as well as Gödel did). Thus, it seems reasonable to suppose that it was primarily due to the influence of these giants in the field that led to the widespread adoption by Anglo-American philosophers of the Fregean Assumption.

There is one more reason why so many philosophers writing on mathematics may have accepted the Fregean Assumption. Only Gödel of the five logicians mentioned in the previous paragraph seems to have seriously worked in the developing area of model theory, and his philosophical views about the nature of mathematics were not widely studied by Anglo-American philosophers during the first half of the Twentieth Century. Although Quine seems to have been aware of some of the significant developments in model theory that were taking place, when the above five were theorizing about the nature of logic, and as I mentioned earlier, he seems not to have incorporated much of what was taking place in model theory into his own philosophical writings about mathematics. Thus, it is not surprising that most philosophers would also attach little importance to these model theoretic developments, when advancing their views about the nature of mathematics.

4. Some Key Elements of Quine’s Philosophy of Mathematics

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55 Thus, John Passmore writes: “After the publication of Princpia Mathematica symbolic logic was little cultivated in England; the leadership passed to Germany, Holland, Poland and the United States—and even, then, to mathematicians rather than to philosophers.” (Passmore, 1957 #616, p. 394)
I shall now recount and reexamine some of the chief elements of Quine’s philosophy of mathematics, with the lessons of the previous section in mind. I shall start with some implications of his acceptance of the Fregean Assumption. This fundamental view of mathematics gave rise to his doctrine that:

(1) Mathematics is irredeemably committed to abstract objects.

From (1), we get his thesis that:

(2) Nominalists are forced to develop a view of the world that substitutes for current scientific theories a version of science lacking mathematics (except for some trivial versions of elementary arithmetic).

Now there is another thesis of Quine’s, to which I alluded earlier, that needs to be noted. The thesis I have in mind is expressed by Quine in the following way:

(3) Mathematics—not uninterpreted mathematics, but genuine set theory, logic, number theory, algebra of real and complex numbers, differential and integral calculus, and so on—is best looked upon as an integral part of science, on a par with the physics, economics, etc., in which mathematics is said to receive its application. (Quine, 1966 #286, p. 231).

I should emphasize that (3) is not the conclusion of some explicit philosophical or logical argument: it is, like so many of Quine’s theses, just asserted in the course of developing his philosophical view of the world.

Another point to be mentioned is that the quotation requires a certain amount of analysis and thought to be properly understood. Thus, when Quine mentions “uninterpreted mathematics”, he undoubtedly has in mind the kind of formal theory that first-order PA is when it is left uninterpreted. On the other
hand, when he speaks of “genuine set theory, logic, number theory, algebra of
real and complex numbers, differential and integral calculus, and so on”, what he
has in mind is evidently what I have called (in the Preface) “real mathematics”.

The reason I claim that Quine must have had in mind real mathematics
when he make the assertion (3) above is because none of the formal versions
of mathematics, such as PA, are “integral parts of science” or are said to
receive their application in physics, economics, etc. Practicing scientists do not
use PA in their daily work or when they are theorizing about their experiments
or the data they have accumulated, and Quine certainly knew this. Evidently,
then, it is real mathematics that he had in mind when he asserted (3) above.
So, for Quine, real mathematics was “an integral part of science”.

5. Quine’s Challenge to the Nominalist

From the above discussion, one can see why Quine has been understood
to have advanced the kind of “Indispensability Argument” that I have
characterized as a “challenge to the nominalist”\(^{56}\): Quine can be understood to
have challenged the nominalist to show how he or she can espouse anything like
an adequate version of science, since real mathematics is, from (1),
“irredeemably committed to abstract objects”. Evidently, the nominalist’s
science would have to be lacking in real mathematics, and how can such a
science be anything like our contemporary version of science? It was in
response to this “challenge to the nominalist” that I put forward my
Constructibility Theory (to be discussed in Chapter 4).

\(^{56}\) See [Chihara, 2004 #468], Chapter 5, Section 2.
6. Gödel’s Acceptance of the Fregean Assumption

In discussing Cantor’s Continuum Hypothesis or the Continuum Hypothesis (henceforth ‘the CH’), Gödel rejected the belief of a number of researchers that, if the CH were proven to be independent of the standard axioms of set theory, the question of its truth or falsity would simply lose its meaning, just as the question of the truth or falsity of the Fifth Postulate of Euclidean geometry was thought to have lost its meaning with the discovery of its independence from the other postulates. Gödel was convinced that the CH had a truth value that was independent of whether or not it was formally decidable from the axioms of standard versions of set theory. This conviction was tied to his belief that sets truly exist in the actual world. Thus, he argued in his paper (Gödel, 1964b) that such an independence result in set theory would render the question of the truth or falsity of the CH meaningless only if set theory were regarded as a hypothetico-deductive system in which the meanings of the primitives of set theory were left undetermined. But, Gödel argued, set theory is not that sort of system. According to Gödel:

(1) the objects of set theory "exist independently of our constructions”;

57 This is Cantor’s conjecture that any infinite subset of the continuum is equinumerous with either the set of natural numbers or the whole continuum.

58 Such a position is suggested by the following quote: “Probably we shall have in the future essentially different intuitive notions of sets just as we have different notions of space, and will base our discussions of sets on axioms which correspond to the kind of sets we wish to study . . . everything in the recent work on foundations of set theory points toward the situation which I just described” ((Mostowski, 1967), p. 94).

59 It should be noted that Gödel did not believe that the question of the truth or falsity of the CH rested solely upon the belief in the existence of sets. He argued that the “mere psychological fact of the existence of an intuition which is sufficiently clear to produce the axioms of set theory and an open series of extensions of them suffices to give meaning to the question of the truth or falsity of propositions like Cantor’s continuum hypothesis” ((Gödel, 1964b), p.272).
(2) we have "an intuition of them individually" (the term “intuition” here is being used by Gödel to refer to something like a "perception" of individual sets); and

(3) the general mathematical concepts we employ in set theory are "sufficiently clear for us to be able to recognize their soundness and the truth of the axioms concerning" these objects.

He concluded that "the set-theoretical concepts and theorems describe some well-determined reality, in which Cantor's conjecture must either be true or false", even if the conjecture is independent of the other axioms.61

Now in saying that the objects of set theory "exist independently of our constructions", it is not being suggested that sets exist in the physical world: it is frequently thought by Platonists that there is a non-physical realm of the actual world in which sets exists. Indeed, Gödel himself once wrote that the objects of transfinite set theory “clearly do not belong to the physical world” (Gödel, 1964 #72), p. 271), implying that there is a non-physical part of the actual world.

Gödel’s view of set theory is that the standard axioms of set theory, such as those of ZF, are literally true statements—they correctly describe objects

60 (Gödel, 1964b), p. 271.

61 (Gödel, 1964b), p. 262. It is now well-known, as a result of Cohen’s proof (see (Cohen, 1966)), that CH is indeed independent of the axioms of standard versions of set theory.
that in fact exist (in the actual world) and that in fact are related (by the
membership relation) in the way described by the axioms. According to such
Realists, the axioms are not statements that the set theorist merely postulates
or arbitrarily lays down. They are supposed to be truths that the
mathematician has, in some way, discovered.\textsuperscript{62} Since mathematical entities are
not supposed to be things that can be seen, touched, heard, smelled, tasted, or
even detected by our most advanced scientific instruments, we seem to have,
according to the picture of mathematics advocated by these thinkers, two
causally isolated worlds. There is the mathematical world of sets, numbers,
functions, etc. from which we are excluded, and the physical world of which we
humans are members—with apparently no causal links between any member of
one of these worlds and any member of the other.\textsuperscript{63} How such a view of our
knowledge of sets is consistent what science teaches us about how we humans
are able to obtain knowledge of objects in the actual world is, for me, a
mystery. I find it impossible to reconcile the above Realistic account with
generally accepted scientific views of how humans are able to obtain knowledge
of things “outside” their minds. The idea that set theorists, just sitting at their
desks, are somehow able to discover the truths that mathematicians enshrine
as “the axioms of set theory”—truths about entities that are supposed to be

\textsuperscript{62} Gödel went so far as to claim that “we 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, 1964 #72, p. 271). Cf. G. H. Hardy’s Platonic assertion: “I believe that mathematical reality lies outside us, and that
our function is to discover or observe it, and that the theorems which we prove, and which we describe
grandiloquently as our ‘creations’ are simply our notes of our observations” (Hardy, 1941, pp. 63-4). Cf.
also (Hardy, 2002), p. 182. A more recent work defending a robust Realist view similar to Gödel’s is
(Brown, 1999).

\textsuperscript{63} It should be noted that Gödel explicitly asserted that the objects of transfinite set theory “clearly do not
belong to the physical world” (Gödel, 1964b), p. 271.)
completely undetectable by us and also independent of our thoughts and intentions—strikes me as bizarre or even unintelligible.⁶⁴

To deal with this difficulty of accounting for the mathematician’s supposed knowledge of the existence and properties of mathematical entities, Gödel postulated that we have something like a “perception” of the objects of set theory. Then he argued:

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.⁶⁵

The idea here seems to be that mathematical intuition plays a role in mathematics analogous to the role that sense perception plays in the empirical sciences. In both cases, we are pictured as constructing theories that have implications about future "perceptions", so that, in favorable instances, the theory is confirmed by "perceptions".⁶⁶ Such a view of set theory suggested to Gödel that:

There might exist axioms so abundant in their verifiable consequences, shedding so much light upon a whole field, and yielding such powerful methods for solving problems . . . that, no matter

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⁶⁴ It should be noted that not all Realists accept all the doctrines being attributed here to Gödel. There are many different brands of Mathematical Realism. See, for example, {Balaguer, 2008 #615} for a discussion of them.

⁶⁵ (Gödel, 1964b), p. 271. Of course, not all versions of Realisms, even “robust” forms, appeal to a kind of “perception” of mathematical objects being characterized here by Gödel.

⁶⁶ For additional insights into Gödel’s epistemological views about our supposed knowledge of sets, the reader should study (Gödel, 1964a), {Chihara, 1982 #168}, and [Parsons, 2014 #622], Part 1, Essays 4-5. For another Realist’s view of how mathematicians obtain knowledge of mathematical objects, see (Brown, 1999), Chapter 3. For criticisms of Brown’s view, see (Chihara, 2004), Chapter 10, Section 2.
whether or not they are intrinsically necessary, they would have to be accepted at least in the same sense as any well-established physical theory.\textsuperscript{67}

Gödel’s Realistic views about set theory I find to be paradox-ridden and unscientific.\textsuperscript{68} For example, I find it hard to make sense of his picture of the set theorist somehow picking out and referring to specific mathematical entities (such as the empty set). If we accept Gödel’s Realist’s assumption that sets are entities that are completely cut off from us and our scientific instruments (existing in a completely different world from our physical world), then how can this picking out and referring take place?

One might ponder the question: “Can’t we pick out and refer to specific things that we have never seen, touched, or experienced in any way?” No scientists has ever perceived a dinosaur, but no one doubts that scientists are able to pick out and to refer specific ones. How is this done? By way of the traces dinosaurs have left. For example, we have fossil remains of specific dinosaurs by means of which we can refer to, say, “that specific Tyrannosaur whose fossilized bones are in drawer # 7 at the Smithsonian museum”.

But suppose that dinosaurs left no causal traces. Suppose that they were things in a completely separate world from which we were totally isolated. How then could we pick out and refer to specific dinosaurs? That is the sort of situation we are in vis-à-vis the sets postulated in Gödel’s Realistic view of set theory.

\textsuperscript{67} (Gödel, 1964b), p. 271. In one of his Alfred Tarski lectures given in April, 2001, Ronald Jensen described the view expressed in the above quote as being the most influential (in the community of set theorists) of all of Gödel’s philosophical views.

\textsuperscript{68} For a detailed criticism of Gödel’s views about his postulation of a kind of perception of the objects of transfinite set theory (“mathematical intuition”), see (Chihara, 1982) and (Chihara, 1990), Chapter 1, Section 3. See also the much more recent [Chihara, 2005 #477], pp. 496-499.
Still, Gödel might argue that we can pick out and refer to a particular set by saying, for example, that “it is that particular set that has no members”. A set theorist working in ZF might imagine that she is theorizing about the “pure sets” built up from the empty set using power sets and unions. But if one supposes, with Gödel, that there really is such an entity as the empty set and that we have somehow singled it out for study, then one may well wonder how this “picking out” has been accomplished. After all, the axioms of standard formalized set theories such as ZF fail to pick out a specific relation of membership: ZF has many different first-order models with many different relations serving as the membership relation. So how can set theorists pick out, from the world of mathematical entities, that unique entity that is supposedly denoted by the phrase “that set of which no entity is a member”?

Gödel could, of course, appeal to his theory of “intuition” to attempt to explain how this picking out takes place. But any such postulation of “intuition” to achieve such an explanation would be met by much skepticism among contemporary analytic philosophers of mathematics and, I suspect, also among significant numbers of empirical scientists and mathematicians. Would Gödel’s explanation of how set theorists have learned the truth of the axioms of set theory be regarded by my ex-teacher mentioned in the Introduction as a case of “mysticism”?69 Given my view of the philosophy of mathematics described in the Forward, the reader will appreciate my general skepticism about Gödel’s Platonic view of sets and also understand my attempt to provide a quite different approach to understanding the existential theorems of classical mathematics.

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69 There are, of course, other attempts to answer the anti-Realist’s skeptical doubts, and many responses to responses. See for example [Brown, 1999 #401]. See also (Chihara, 2004), pp. 15-6, for references to works discussing the topic.
In summary, without going into any more details of Gödel’s Realistic view of sets, there can be little doubt that Gödel accepted the Fregean Assumption and that many of his controversial ontological and epistemological views about the nature of sets and our knowledge of them was based upon his acceptance of the Fregean Assumption.
Chapter 3

A Fictionalist

Introduction

In this chapter, I shall be discussing a particular view about the nature of mathematics according to which mathematical objects are taken to be like the persons and things discussed in works of fiction: mathematical objects, according to these views, will not be considered to be things that actually exist, but rather like the fictional characters in literary works. The question upon which I shall be focusing is this: Does Hartry Field make the Fregean Assumption

1. Field’s Fictionalism

Much has been written on Field’s views of mathematics. A great deal of this literature is concerned with whether, and to what extent, Field had shown that “the mathematics needed for application to the physical world does not include anything which even prima facie contains references to (or
quantification over) abstract entities”.

In contrast to this large body of philosophical writings, I shall be concerned primarily in this chapter with the significance of Field’s acceptance of the Fregean Assumption.

That Field accepted the Fregean Assumption is evident from his work Science Without Numbers. On the very first page of that work, he characterizes nominalism as the doctrine that there are no abstract objects. Although he allows that the notion of “abstract object” may not be entirely clear, he thinks it is clear that “such things as numbers, functions, and sets are abstract—that is, they would be abstract if they existed” (Field, 1980 #206, p. 1). He then asserts that, in defending nominalism, he is denying that numbers, functions, sets, or any similar entities exist. Here, it is absolutely clear that he is denying that, as I would put it, numbers, functions, sets or any similar entities exist in the actual world. Since “in developing physical theories, one needs to use mathematics”, and since “mathematics is full of reference to and quantification over numbers, functions, sets, and the like” (Field, 1980 #206, p. 1), it would appear that nominalism cannot be maintained. But this appearance is dependent upon the belief that the theorems of mathematics are true. To maintain his nominalism, Field takes the radical position that mathematical theorems are in fact false.

Now in the reasoning of the previous paragraph, “mathematics” is interpreted to be a theory whose assertions are the theorems of mathematics. It is the theorems of mathematics that make reference to numbers, functions,

70 Field, 1980 #206, pp. 1, 2. Steiner comments: “Field’s book occasioned a great deal of comment and criticism, much of it centering around his major example of classical gravity: whether his replacement for it was truly nominalist . . .; whether he had truly demonstrated that it was a replacement; whether he had demonstrated that mathematics is really not deductively necessary in physics; whether he could reproduce his success in other areas of physics, particularly quantum mechanics . . .” (Steiner, 2005 #614, p. 644).

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sets, and the like, and it is the theorems that play a crucial role in the scientists theorizing about the physical world. Thus, Field took the position that accepting the theorems of mathematics as true assertions is incompatible with maintaining a nominalistic view of mathematics. This is why he thought that, if he adopted the thesis that the theorems of mathematics are, in fact, not true, he could maintain a nominalistic view of mathematics.

Nominalism is a philosophical thesis about what exists in the actual world: it holds that abstract entities such as numbers do not exist in the actual world. Field also believes that the theorems of mathematics assert (or presuppose) the existence of abstract entities in the actual world. Thus, Field came to believe that he cannot, as a nominalist, accept the almost universally held thesis that the theorems of mathematics are true.

What is clear from the above is that Field regards the theorems of mathematics as propositions about the actual world. More specifically, he takes the mathematical theorems that assert the existence of mathematical objects to be assertions about what exists in the actual world—all of which justifies classifying Field as a Fregean. But it needs to be emphasized that his acceptance of the Fregean Assumption is not in any way supported in his book by reasoned argumentation: it is simply assumed.

I emphasize that Field does not provide any sort of argument or analysis of mathematical theorems that establishes that mathematical theorems are propositions about the world. The Fregean Assumption is simply a sort of starting position for Field’s philosophy of mathematics. However, he is a most interesting sort of Fregean, in so far as he maintains that all mathematical theorems are false. Thus, for Field, there are no mathematical truths in the
sense that none of the statements that most mathematicians take to be true are, in fact, true. For Field, the so-called “mathematical theorems” are false propositions about the world.

Why might one question the truth of mathematical theorems? Well, if standard theorems, such as Euclid’s Theorem asserting the infinity of primes, are true, then from the perspective of a Fregean, we could conclude that there exist mathematical objects in the actual world and hence that Platonism is true and nominalism is false. These are conclusions that Field was reluctant to accept. Why? Well, for one thing, accepting Platonism brings on epistemological problems that are devilishly difficult, if not impossible, to solve.

In any case, unlike most philosophers of mathematics, Field was willing to consider seriously the possibility that mathematical theorems, as a whole, are simply false. And when he considered the question ‘What grounds do we have for believing that mathematical theorems are true?’ , the only thing he could come up with were grounds relating to applications of mathematics in science and everyday life. But such grounds ultimately reduced, for Field, to some sort of Quinean “Indispensability Argument” discussed in the previous chapter. So the question became: could he find a way to evade the force of the “Indispensability Argument” or somehow circumvent that argument? This question led him to prove a theorem, “the conservation theorem”, which he regarded as showing that mathematics is “conservative over nominalistic theories” (in a sense to be explained in some detail shortly). This is the main theorem that Field used to justify his claim that he had a refutation of Quine’s “Indispensability Argument” (in {Field, 1980 #206}). Believing that he could overcome what he considered to be the only non-question begging reason for
thinking mathematical theorems are true, he felt free to maintain that mathematical theorems are false and hence that Platonism is false.

The above, in a nutshell, shows why I classify Field as a Fregean: he assumed from the start that mathematical theorems were propositions about the world. What at least some of these theorems assert, he thought, was that mathematical objects exist in the actual world. To avoid accepting the existence of mathematical objects, Field took the position that mathematical theorems are false propositions about the world. In other words, for Field, mathematical theorems are propositions about the world that are false.

Now if mathematical theorems are all false, as Field asserted in his book, how can he reconcile his belief in the falsity of mathematics with the fact that the countless bridges engineered using mathematics do not regularly collapse? More generally, how is it to be explained why the use of what he considers to be a false theory (that is, mathematics) has proved to be so useful and successful in engineering and science? In other words, how is the spectacular success of the use of mathematics to be explained, if one believes, as Field does, that the theorems of mathematics are false? It is hard to imagine how scientists could have calculated the trajectory of the rockets they have sent to Mars if they were convinced that the theorems of mathematics were all false.

Clearly, I need to explore, in more detail, the precise way in which Field attempted to refute the Indispensability Argument. Now the earlier description I gave of Field’s conservation theorem is only a very rough statement of the principle he used, and much in the statement of the theorem needs to be explained. What does ‘conservative’ mean? What is a ‘nominalistic theory’, as
Field understands that phrase? Even the term ‘mathematics’ needs to be explicated. We need to know, for one thing, if the principle theorem described above is about what I have been calling ‘real mathematics’ or about only some formal model of mathematics such as ZF?

Perhaps we can obtain a better idea of Field’s conservation theorem by seeing what it was intended to do. So let us consider how the theorem was supposed to refute the Indispensability Argument. Now, there is at least one form of Quine's Indispensability Argument that Field may be trying to undermine in his book. It is a version I call “Quine’s Challenge to the Nominalist”. This form of the argument challenges the nominalist to come up with a type of mathematics that is, on the one hand, adequate to the needs of the empirical sciences, and on the other hand, is nominalistically acceptable. The inability of nominalists to meet this challenge would be taken as evidence that the realistic view of mathematics is the only reasonable position to take. (I shall discuss this argument in much more detail in the following chapter). Now in so far as Field is only seeking to meet Quine’s challenge, there would be no need for Field to worry about whether or not his account of mathematics fits actual mathematical practices—any account of mathematics will do so long as it is acceptable to the nominalist and is also adequate for the needs of the empirical sciences.

Here is one passage that indicates that Field was pursuing this more limited goal:

[P]art of mathematics . . . is the systematic deduction of consequences from [nominalistic] axiom systems. . . . Very little of ordinary mathematics consists merely of the systematic deduction of consequences from such axiom systems: my claim however is that ordinary

[71 See {Chihara, 2004 #468}, Chapter 5, Section 2, where this version of Quine’s argument is discussed in some detail.}
mathematics can be replaced in application by a new mathematics which does consist only of this. ({Field, 1980 #206}, p. 107, n. 1, italics mine).

If Field is only interested in finding a new kind of mathematics that “can replace” ordinary mathematics in applications, then this new kind of mathematics does not have to accord with actual mathematical practices. However, as will be shown below, there is ample evidence that Field claimed to be doing much more than just replying to the Quinean challenge.

Field seems to assert, or at least strongly suggest, in a number of places, that “real mathematics” is conservative over nominalistic theories. In the following, I shall indicate why one might think Field does indeed make such an assertion. I begin with an explanation of why I believe that Field gives the impression that he is providing us with an account of real mathematics when he writes about mathematics. First consider his discussion of how his account of mathematics differs from that of the Logical Positivists. He notes that the Positivists were never able to clearly explain their frequently made claim that mathematics is “lacking in factual content”. They based that intriguing claim on the further claim that any conclusion arrived at through the use of mathematics was always “implicitly contained in” the premises. Now Field’s own account was supposed to provide “a clear and precise sense to the idea that mathematics doesn’t yield genuinely new conclusions”: it supposedly showed that mathematics can be applied to nominalistic theories “without yielding any genuinely new conclusions about non-mathematical entities” ({Field, 1980 #206}, p. 16). These passages are relevant to my concerns here because the Positivists regarded themselves as having put forth an account of mathematics—that is, real mathematics—since they clearly weren’t claiming only that first-order versions of mathematics were empty of factual content. So it is
reasonable to conclude that Field too regarded himself as putting forward an account of real mathematics.

As further evidence supporting this interpretive conclusion, consider the opening sentences of his preface to Science Without Numbers. There, he tells us that most of the current literature in the philosophy of mathematics focuses on the following three questions:

(a) How much of standard mathematics is true? For example, are conclusions arrived at using impredicative set theory true?

(b) What entities do we have to postulate to account for the truth of (this part of) mathematics?

(c) What sort of account can we give of our knowledge of these truths?

A fourth question, Field notes, is sometimes considered:

(d) What sort of account is possible of how mathematics is applied to the physical world?

It is this fourth question that is, for Field, the “really fundamental one” (Field, 1980 #206, p. vii).

It would certainly be reasonable to infer, from such passages, that Field is not writing about some special kind of formalized model of mathematics, but rather is writing about real mathematics in my sense of the term. This is because much of the current literature about mathematics to which he refers above concerns real mathematics and not mere formal models of mathematics.
Besides, his use of the term ‘standard mathematics’ in question (a) certainly suggests that he is writing about the ordinary mathematics that most mathematicians actually study and use (and not a formalized model of real mathematics).

For additional evidence supporting this understanding of Field’s views, ponder the following passage from his book:

I believe that what I do here gives an attractive account of how mathematics is applied to the physical world. This is I think in sharp contrast to many other nominalistic doctrines, e.g. doctrines which reinterpret mathematical statements as statements about linguistic entities or about mental constructions. Such nominalistic doctrines do nothing toward illuminating the way in which mathematics is applied to the physical world. ([Field, 1980 #206], p. 6).

Notice that Field writes of “how mathematics is applied”—not how mathematics could have been applied. There is, here, no suggestion that he is thinking of how science could have been carried out in first-order logic, using some such mathematical system as ZFU (in place of real mathematics) and some first-order axiomatized formal version of science (in place of science). No, he evidently thinks he is providing an account of how real mathematics is actually applied in representing events and processes in the physical world. Thus, two pages later, he writes:

The explanation of why mathematical entities are useful involves a feature of mathematics that is not shared by physical theories that postulate unobservables. To put it a bit vaguely for the moment: if you take any body of nominalistically stated assertions N, and supplement it with a mathematical theory S, you don’t get any nominalistically-statable conclusions that you wouldn’t get from N alone.\textsuperscript{72}

\textsuperscript{72}([Field, 1980 #206], pp. 8-9). What follows, shortly after, is Field’s description of his Conservation Principle that I discuss in detail in this chapter. The discussion of the Conservation Principle is intended to state clearly what was put “a bit vaguely” in the quotation.
Here again, Field seems to be writing about real mathematics—not a specialized version devised by logicians, which very few practicing mathematicians actually use and develop in their research and teaching. Thus, in writing of supplementing N with “a mathematical theory S”, he specifies no qualifications on the kind of theory S has to be, again suggesting that the theory can be any mathematical theory, presumably even a theory of real mathematics.

There are many passages in his book (especially in the Preface, the Preliminary Remarks, and Chapter 1, where he sometimes writes about “ordinary mathematics”) that provide additional confirmation of the interpretation described above, and I believe that is how most philosophers of mathematics have understood him.

In this section, it should be understood that the languages and theories to be discussed are to be taken to be the languages and theories of first-order logic.\textsuperscript{73} For the most part, Field takes mathematics to be ZF set theory, since it is widely accepted that any mathematical theory needed in the empirical sciences could be formalized in that set theory. Since he wants to allow the chosen mathematical theory to “speak” of things that the scientific theory discusses, he chooses Zermelo-Fraenkel set theory with urelements (henceforth ‘ZFU’). Now as a first approximation to what he wants his theorem (henceforth ‘Conservation Theorem’)\textsuperscript{74} to be, consider:

\textsuperscript{73} This is not meant to suggest that Field always restricts his Conservation Principle to first-order logic. Indeed, he sometimes explicitly makes clear that he wishes to include second-order logic into his discussion of his claims. However, for simplicity of exposition and argumentation, I shall restrict my discussion of his controversial theses to those about first-order logic.

\textsuperscript{74} This is the theorem that Field calls “Principle C” in Chapter 1 of \{Field, 1980 #206\}. 
If N is a nominalistic theory, then ZFU + N is a conservative extension of N (or as I shall sometimes put it: “ZFU is conservative over N”)

where:

(1) ZFU + N is the theory obtained by conjoining the two theories ZFU and N, taking the vocabulary of the new theory to be the union of the vocabularies of the two theories and taking the assertions of the new theory to be the union of the assertions of the two theories;

and where:

(2) $T^#$ is a conservative extension of $T$ if, every theorem of $T^#$ that is a sentence of $T$ is also a theorem of $T$.

Thus, the first approximation formulated above tells us that any sentence of the nominalistic theory N that is derivable in the joint theory ZFU + N is derivable in N.

One problem with the above formulation of the Conservation Principle is that N might in some way contradict ZFU or, in effect, attribute all sorts of strange properties to sets when N is conjoined with ZFU. For example, N might say “Everything satisfies Newton’s Laws”, thus implying that sets satisfy Newton’s Laws. Of course, N is not really talking about sets when it asserts that everything satisfies Newton’s Laws. So Field suggests that the quantifiers of N should be “relativized” so as to be explicitly about nonmathematical objects. This can be done as follows: introduce into the joint language, a

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75 The “assertions of a theory” is a set that is closed under the consequence relation.
monadic predicate \( M \) meaning ‘is a mathematical object’ and relativize the quantifiers of \( N \) to the nonmathematical objects in the following way: ‘\((x)(Fx \rightarrow Gx)\)’ becomes ‘\((x)(\neg Mx \rightarrow (Fx \rightarrow Gx))\)’ and ‘\((\exists x)Fx\)’ becomes ‘\((\exists x)(\neg Mx \& Fx)\)’.

As for \( ZFU \), it already talks about nonsets, so we need only add another monadic predicate to the joint vocabulary meaning ‘is a set’ and then add to its axioms a sentence that says “Every set is a mathematical object”, i.e. \((x)(Sx \rightarrow Mx)\).

The above details are generally omitted in discussions of Field’s Conservation Principle because it is seen as a rather minor point and because Field decides in his book not to introduce a special notation for the modified versions of \( ZFU + N \) (he just assumes that \( ZFU + N \) is written that way from the start (p. 12)). I provide these details because I wish to highlight a feature of Field’s account that is frequently overlooked in discussions of his view: as Field formulates \( ZFU \), set theory appears to be a kind of metaphysical theory. The theorems of \( ZFU \) are thought to be propositions about the actual world. And many of these propositions assert the existence of mathematical objects. Thus, the metaphysical nature of its assertions is made explicit by the fact that, among its theorems, there are assertions of the form ‘\((\exists x)Mx\)'\(^{76}\) (Remember that, because Field believes mathematical objects do not exist, he is convinced that \( ZFU \) cannot be a true theory). These details add further evidence that Field regards the theorems of set theory as propositions about the actual world and that Field is a Fregean.

Field never justifies his assumption that the theorems of set theory are propositions about the actual world: he just makes the assumption. This is an

\(^{76}\) The reason I suggest that \( ZFU \) is regarded as a sort of metaphysical theory is not because it has existence assertions, but rather because it asserts (at least when understood straightforwardly) the existence of mathematical objects--something that metaphysicians typically assert, deny, or argue about.
assumption that is by no means obviously true, and as I shall argue later is, in fact, highly questionable. So his view of mathematics is based upon an assumption that I shall argue is dubious.

It should be noted that treating mathematical theorems as metaphysical in nature is not new with Field. The two most eminent of the Platonist philosophers of mathematics, namely Kurt Gödel and Willard Quine, believed that the assertions of mathematics are metaphysical in nature. Of course, not all philosophers of mathematics adopt such a position.\footnote{See, for example, my own position on this question in \textit{Chihara, 2004 #468}.}

The version of mathematics presented above as ZFU + N is of little interest to one concerned with applications of mathematics, since the vocabularies of the two conjoined theories have essentially nothing in common: there is practically no way that the mathematical theory can “interact” logically with the nominalistic part, so it is hard to see how such a theory can have much use in science. For this reason, Field expands the vocabulary of the mathematical part to include the vocabulary of N so that the nominalistic vocabulary can appear in such axioms as Separation and Replacement. The expanded mathematical system, ZFU\textsuperscript{®}, can then map nominalistic objects to both pure and “mixed” mathematical objects. (See \textit{Field, 1980 #206}, pp. 9-10).

The Conservation Theorem can now be stated as follows:

\[ \text{[CP]} \] If N is a nominalistic theory, then ZFU\textsuperscript{®} + N is a conservative extension of N.

\footnote{See, for example, my own position on this question in \textit{Chihara, 2004 #468}.}
What is one supposed to conclude from the above theorem, especially regarding the Indispensability Argument and his attempt to refute it? Evidently, Field believes that [CP] provides the grounds for refuting the Indispensability Argument, even though it is far from clear just how Field inferred the unsoundness of the Indispensability Argument from [CP]. What is supposed to underlie his unsoundness proof is the following bit of reasoning: Field uses the Conservation Theorem to justify one of the more controversial of his theses about mathematics:

[NT] real mathematics is not a “body of truths”--indeed, “no part of mathematics is true” ({Field, 1980 #206}, p. viii).

Field’s justification for adopting this remarkable thesis is tied to his attitude toward the Indispensability Argument. Since he believes that the “only non-question-begging arguments [he has] ever heard for the view that mathematics is a body of truths all rest ultimately on the applicability of mathematics to the physical world . . .” ({Field, 1980 #206}, p. viii), and since he is convinced that his conservation strategy refutes those “non-question-begging arguments”, Field concludes that “there is no reason to regard any part of mathematics as true”.78

Now why did Field believe that his theorem about the conservative nature of mathematics refutes Quine’s Indispensability Argument? He never spells out his reasoning on this matter in any precise way, but I suspect that it went something like the following:

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78 {Field, 1980 #206}, p. viii, italics mine.
The Indispensability Argument is supposed to show that we need to assume the existence of mathematical objects (because we need to use mathematics) to accept the findings of modern science; and Field’s theorems on the conservative nature of mathematics is thought to undercut this reasoning by showing that we need not assume the existence of mathematical objects in order to use mathematics in science.

I shall return to this “Big Picture” expression of Field’s reasoning later in this chapter.

2. Field’s “No Reinterpretation” Account of Mathematics

It has been thought by some that Field’s account of mathematics does not require that mathematical sentences be given any special interpretation in order to be applied in science; nor does it require that a special kind of logic be used in such applications. The nominalist can even employ, for example, ZFU-- a first-order theory as standardly understood--in applying mathematics to science. For Field has written:

The way that has proved most popular among nominalistically inclined philosophers is to try to reinterpret mathematics—reinterpret it so that its terms and quantifiers don’t make reference to abstract entities . . . My approach is different: I do not propose to reinterpret any part of classical mathematics . . . ([Field, 1980 #206], p. 2).

This feature of Field’s account has been thought (for example by such philosophers as Mark Balaguer) to lead to the conclusion that the account fits actual mathematical practice perfectly. Evidently, Balaguer believes that, since the nominalist can use the standard classical systems of mathematics to draw
nominalistic conclusions from nominalistic theories, there is no place for Field’s account to conflict with any mathematical practice! This is a highly dubious belief that has not been sufficiently discussed in the literature on Field’s writings, and later in this chapter, I shall give grounds for rejecting it.

3. Field’s Deflationist View of Mathematical Knowledge

Since the assertions of mathematics are held by Field to be false, and since Field believes that the entities apparently talked about in mathematics, such as numbers and sets, do not in fact exist, he came to maintain that one can regard mathematics as being very much like a work of fiction. Interestingly, Mary Leng has accepted this idea of Field’s and developed it into a full-fledged philosophy of mathematics (which I don’t propose to discuss here). In mathematics, by Field’s account, one theorizes about things that do not exist and produces sentences about these non-existent objects that are not true—as one does in works of fiction. Yet it is very useful in science and everyday life because mathematics is, according to Field, conservative. What a neat philosophical view of mathematics it would be, if only it could be truly justified.

Field’s doctrine that the sentences of mathematics are, in certain respects, like those in works of fiction is an important feature of his overall view of mathematics. It is this doctrine that motivates his “deflationist” position about mathematical knowledge—a position that attempts to account for “mathematical knowledge” without requiring the possessor of mathematical knowledge to know that any specific mathematical theorem is true. This is because mathematical theorems are held to be false. Thus, if mathematical theorems are false, then they could not be known to be true. For this reason,
Field feels the need to investigate the question of just what it is that the mathematician knows that non-mathematicians do not know.

In his article “Is Mathematical Knowledge Just Logical Knowledge?”, Field claims that “what separates someone who knows lots of mathematics from one who knows only a little mathematics is not that the former knows many and the latter knows few of such claims as those that mathematicians commonly provide proofs of . . .” (p. 81). What separates them, according to Field, is “empirical knowledge (e.g., about what other mathematicians accept and what they use as axioms)” and, more importantly, knowledge of a purely logical sort (p. 82). Thus, it is claimed that what chiefly distinguishes someone with lots of mathematical knowledge from someone with little is that the former, but not the latter, has lots of knowledge of truths of the form:

(i) It is logically necessary that if A, then B

and

(ii) It is logically possible that A.\(^79\)

But what does it mean to say of a proposition that ‘it is logically necessary or logically possible’? Field tells us that “the modal knowledge which deflationism allows is knowledge of purely logical possibility—deflationism does not allow knowledge of mathematical possibility in an interesting sense” (1989, p. 85, n. 7). As Field uses the modal operator, \(^{79}\) {Field, 1984 #434}, p. 85.
It is logically possible that \((\exists x)(x \text{ is a bachelor} \& x \text{ is married})\)
is true. In Field’s modal logic, there are no “meaning postulates” that specify
“logical” relations among the predicates. Thus, it is logically possible that there
are married bachelors (in Field’s sense of the operator). In another work, he
emphasizes the restricted sense he gives to his modal operator by limiting the
logical truths to sentences “true by logical form alone” ((Field 1992), pp.
114-5), noting that the logical truths he has in minds are “purely logical”.

Let us, then, distinguish two quite different theses being promulgated by
Field. There is first, the thesis about mathematical truth:

\[ [\text{NT}] \text{ No part of mathematics is true.} \]

Then there is the deflationist thesis about mathematical knowledge that is in
essence:

\[ [\text{MK}] \text{ What the mathematician knows, that the non-mathematician does }
\text{not, are modal facts of the form (i) and (ii).} \]

Of course, the two views are related. It is hard to conceive of a philosopher
espousing [MK] if she did not already believe [NT]. A philosopher who accepted
[NT] could not believe that the proofs mathematicians produce, in the course of
doing what is called “proving a theorem”, are proofs of the truth of the theorem
proved. Thus, [NT] pushes one to espouse a view according to which the
theorems of set theory are not true but only logical consequences of the
axioms of the set theory being assumed; and the knowledge obtained as a
result of the proof is not knowledge of the truth of the theorem proved but only knowledge of something like:

It is logically necessary that (if A, then T)

where A is some finite set of axioms and T is the theorem proved. Consequently, [NT] pushes one toward some such proposition as [MK] and also provides [MK] with what plausibility it has. I shall have much more to say about these remarkable views of Field’s later in this chapter.

4. How Field Arrived at his View that No Part of Mathematics is True

The assessment to be provided here will not attempt to repeat or assess the many serious and technically solid objections to Field's overall view of how mathematics is applied in science that have already been published. Instead, I shall concentrate on one aspect of his basic position on mathematics: namely his radical view that “no part of mathematics is true”. I do this because this view is intimately connected to Field’s espousal of the Fregean Assumption.

How did Field arrive at this radical view? Recall that Field’s justification for adopting this remarkable thesis is tied to his following three beliefs:

(A) His Fregean belief that the theorems of mathematics are propositions about the actual world;

80 Burgess and Rosen provide a nice list of critical assessments of Field’s work in {Burgess, 1997 #227}, III.B.I.b. I also give a critical assessment in {Chihara, 2004 #468}, Chapter 11.
(B) His belief that the only non-question-begging arguments for the view that mathematics is a body of truths all rest ultimately on the Indispensability Argument.

(C) His nominalistic belief that no mathematical objects exist.

Now if, as most philosophers believe, mathematics is a body of truths, then at least one of the above three beliefs must be false. Thus, Field came to consider the possibility that mathematics was not, in fact, a body of truths. (C) suggested to him that, if he could refute the Indispensability Argument, then all three of his beliefs could be saved. Since he was convinced that his conservation account of applications of mathematics refutes the Indispensability Argument, he concluded that there is no reason to believe that any part of mathematics is true.

The reason for being skeptical of one of the beliefs of the above triad that plays a central role in Field’s argument that no part of mathematics is true—that belief is (B). Many, if not most, mathematicians have no knowledge or acquaintance with Quine’s Indispensability Argument; yet most mathematicians believe that much of mathematics is true. Thus, they must have some reasons for their beliefs in the truth of much mathematics—reasons that are not undermined by Field’s objections to the Indispensability Argument. I shall indicate some of these reasons in the following sections, starting with the examination of Group Theory.

5. Group Theory
Group theory is, as Nathan Jacobson puts it, “one of the oldest and richest branches of abstract algebra”: in particular, transformation groups play important roles in geometry, and finite groups are “fundamental in Galois’ discoveries in the theory of equations”. Jacobson tells us that it was these two areas of mathematics, geometry and the theory of equations, that provided the “original impetus” to the development of group theory (Jacobson, 1951 #520), p. 15).

What is a group? Samuel Eilenberg asks us to consider the surface of a large table and think of various “moves” we might make such as shifting its position and turning it around its center as a pivot. Two such moves might be combined to yield another such move. Thus, move M and then move N would yield the composition move MN. Call I the “move” that does not displace the table at all. Then, for any move M, MI = M = IM. Also, for each move M, there is a move M\(^{-1}\), the inverse M, that undoes what M does. Thus, we have \(M M^{-1} = I\), Finally, we have an associative law of composition: for all moves M, N, and O, \((MNO) = (MN)O\).

A group is any set of elements with a composition law that has the formal properties described above. (Eilenberg 1969), p. 153).

A formal version of group theory is presented in (Mates, 1972 #38), Chapter 11, Section 4) as an axiomatized first-order theory with identity and operation symbols. The axioms of the theory are presented below in a form that freely uses abbreviations to render the sentences more easily grasped:

1. \((x)(y)(z)((x + (y + z)) = ((x + y) = z))\)
2. \((x)((x + e) = x)\)
3. \((x)((x + x^{-1}) = e)\)
where ‘+’ stands for the operation of group addition, and ‘e’ stands for the identity element. Thus, a model of this theory must consist of a set of elements forming the domain of the model and an operation on the domain that is closed and that satisfies the above axioms.

One can also state informally what a group is as follows:

A group consists of a set S and an operation + on S that is closed and which satisfies the following three conditions: (i) the operation + is associative, (ii) there is an identity element, and (iii) every element of S has an inverse.

An example of a group that is frequently given in text books is the integers (the domain), coupled with the operation on the domain that is ordinary arithmetical addition. Another example: the rational numbers, excluding zero, coupled with the operation of multiplication.

A group that satisfies the commutative law

\[(x)(y)((x + y) = (y + x))\]

is called an Abelian group. An infinite group is a group with a domain that is infinite. Thus, the above two examples of groups turn out to be both Abelian and infinite.

Here is how Mark Steiner introduces group theory in his article “Mathematics—Application and Applicability”:
Group theory is known today as the theory of symmetries—we define a symmetry as a property which is invariant under a group of transformations. The classic symmetries, which are visual symmetries, involve transformations of space, such as rotations, translations, and reflections. ([Steiner, 2005 #614], p. 636).

The group structures are just one of a variety of algebraic structures the study of which has come to dominate the field of algebra. To explain the usefulness of the study of algebraic structures, Samuel Eilenberg gives the following example of how the study of algebraic structures has been fruitfully applied within mathematics:

In the mathematical theory of fluid dynamics, one represents an exceedingly complicated and many-sided phenomenon, the motion of a fluid, . . . with a mathematical model that consists of a system of functions obeying certain partial differential equations. . . . It is a drastically simplified and ridiculously skimpy picture of reality. But because it is so oversimplified, we can treat it by mathematical methods, and because it mirrors correctly some physical reality, it permits us to draw important conclusions. Similarly, the algebraization of a part of mathematics involves a drastic oversimplification of the mathematical concepts we study, but by concentrating on only some aspects of mathematical reality, we are able to use our knowledge of algebra to study other parts of mathematics. ((Eilenberg 1969), pp. 157-8).

For example, if a system of functions obeying certain partial differential equations satisfies the conditions required to be a group, then everything the algebraists have discovered about groups can be directly applied to the system of functions in question. Since what is now known by mathematicians about groups is enormous, to succeed in showing that a system of functions is a group generally turns out to be quite significant. In such ways, group theory has been fruitfully used to mathematically characterize and algebraically investigate features of the mathematical devices used by scientists to represent, analyze, and investigate processes and events in the physical world. So group theory has been used to produce a sort of “meta-analysis” of the
mathematics used more directly in the scientific representations of physical reality.

However, this is not how group theory came to be developed. As Steiner has observed, group theory was introduced by Évariste Galois to be applied to problems in the theory of equations. More specifically:

By studying the group of an equation, mathematicians are able to extract information about the equation itself—in fact, the application of group theory in mathematics has precisely this form: we find that a certain group characterizes a much more complicated structure, and we can get needed information about that structure by studying the properties of the group.81

Although the concept of a group is very simple, it has turned out to be amazingly useful in both pure mathematics and in science. The fruitfulness of the study of group theory for science was once amusingly illustrated by the eminent theoretical physicist Freeman Dyson:

In 1910 the mathematician Oswald Veblen and the physicist James Jeans were discussing the reform of the mathematical curriculum at Princeton University. “We may as well cut out group theory,” said Jeans. “That is a subject which will never be of any use in physics.” . . . By an irony of fate group theory later grew into one of the central themes of physics, and it now

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81 (Steiner, 2005 #614), p. 636. Galois made use of group theory to show, among other things, the impossibility of the solution by radicals of the quantic equations.
dominates the thinking of all of us who are struggling to understand the fundamental particles of nature.\textsuperscript{82}

Stanislaw Ulam once explained how the use of the concept of the group of transformations, under which physical laws have to be invariant, played a crucial role in applications of the special theory of relativity; and he concluded that the Lorentz group is “one of the most important ideas in all of mathematical physics” (\{Ulam, 1969 \#610\}, p. 4).

Earlier, I noted that most mathematicians believe that much of mathematics is true, despite the fact that they have no knowledge or acquaintance with Quine’s Indispensability Argument. Obviously, many mathematicians have reasons for their beliefs in the truth of mathematics that are not undermined by the objections to the Indispensability Argument that philosophers have developed. I shall now sketch some reasons for thinking that mathematical theorems are indeed, in a sense, true. Thus, in contrast to Field and, I shall maintain that mathematical theorems are, in a very significant sense, true (and I shall also maintain that mathematical theorems are not true propositions about the actual world in the way that propositions of science are widely thought to be).

\textsuperscript{82} (Dyson 1969), p. 97. In the cited work, the reader can find a detailed discussion of how group theory has been applied with surprising fruitfulness and importance in high energy physics. There, Dyson writes:

The immense power of group theory in physics derives from two facts. First, the laws of quantum mechanics decree that, whenever a physical object has a symmetry, there is a well-defined group (G) of operations that preserves the symmetry, and the possible quantum states of the object are then in exact correspondence with the representations of G. Second, the enumeration and classification of all well-behaved groups and of their representations have been done by the mathematicians, once and for all, independently of the physical situation to which the groups may be applied. (p. 108).
I shall begin by discussing some elementary mathematical concepts that will be used in giving some reason for thinking that mathematical theorems are true—true in a sense to be explained later.

6. Permutations

Imagine that eight wooden blocks have been lined up to produce a linear ordering. It will be useful to regard each 1-1 transformation of the set of blocks onto itself as a permutation of the above ordering of the blocks, as indicated by the arrows in the following diagram:

In effect, such a transformation permutes the top ordering to the second one, so such transformations are frequently called “permutations”. When speaking Platonically, I shall call the set of all these particular permutations $K\#$. Permutations are studied by mathematicians in a number of areas of study—for example in probability and statistics, and also in algebra, where an operation involving permutations (“permutation multiplication”) is used to define a kind of group. One kind of permutation group in particular, called “the symmetric group
on n symbols”, plays an important role in many applications of group theory ([Weiss, 1949 #554], p. 44).

A Fictionalist might argue however:

Since permutations are a kind of 1-1 function of a certain sort and, since functions are mathematical objects, they are, according to my account, mere fictions. Thus the mathematical theorems about permutations are fictional sentences and hence lacking in truth value.

In responding to the above imagined reasoning, I should like first to note that mathematicians frequently use two different ways of describing results and theorems of mathematics. Take the theorem of set theory called the “well-ordering theorem”: this theorem is sometimes expressed existentially as:

For every set x, there exist a set y that well-orders x.

This theorem is also sometimes expressed modally as:

Every set can be well-ordered

Hilary Putnam once described the sort of situation described above in the following way:

[T]alk of existence in mathematics is fungible with talk of possibility—not “possibility” in some metaphysical sense, but mathematical possibility, possibility in a sense we understand from mathematics itself. Every statement about the “existence” of any mathematical entities is equivalent (equivalent mathematically, and equivalent from the point of view of application as
well) with a statement that doesn’t assert the actual existence of any mathematical objects at all, but only asserts the mathematical possibility of certain structures. . . . Everything about the success of mathematics, and the deep dependence of much contemporary science, including physics, but not only physics, on mathematics, supports taking mathematical theorems as objective truths.\textsuperscript{83}

In what follows, I shall give some direct reasons for concluding that mathematical theorems are indeed true statements.

I shall carry out much of my discussion of permutations, not in terms of the existence of mathematical objects, but rather in terms of the possibility of carrying out certain types of operations (in this case, the operation of permuting sequences of concrete items by simply moving them around). To be more specific, where a Platonist would speak of the existence of some kind of permutation (regarded as a function), I shall generally speak of the possibility of performing some kind of permutation by simply moving the relevant items.

Let us, for specificity, reconsider the set of permutations $K#$ (which was described earlier in this section). When I wish to avoid talk of sets of permutations, I will, instead, speak of the possibility of performing permutations of kind K. Thus, when the Platonist talks of the product of two of the members of $K#$, say $P_1 \times P_2$, I shall speak of the possibility of first performing permutation $P_1$ and then performing permutation $P_2$. It can be seen that the result of performing $P_1 \times P_2$ will be an ordering that is also produced by performing one of the possible permutations of the original ordering $O_1$. It thus follows that the product operation $\times$ is closed in $K#$. It can also be easily shown that it is

\textsuperscript{83} \{Putnam, 2004 #623\}, p. 67. My Constructibility Theory, to be discussed in detail in Chapter 5, makes use of this “equivalence”.

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possible to specify an identity element for $< K#, x>$ and that, for every permutation $P$ of $< K#, x>$, one can specify a permutation that will be the inverse of $P$. Thus, with very little additional work, one will be able to show (in this “possible to specify” manner) that:

$$< K#, x> \text{ is a group.}$$

Indeed, $< K#, x>$ is what is frequently called a “permutation group”. Now the product operation $x$ is obviously not commutative, so we now have an example of non-commutative group. Groups of permutations can be seen to be important in algebra because of a fundamental theorem (due to the algebraist Arthur Cayley), one form of which is:

Every group is isomorphic to a group of permutations.\(^{84}\)

For any group with operation $x$, the following theorem can be proven:

[###] The inverse of $(P_1 x P_2) = (\text{the inverse of } P_2 x \text{ the inverse of } P_1)$

This theorem can be regarded as elliptical for the statement:

In any group, where the group operation is represented by ‘$x$’, and $P_1$ and $P_2$ are variables ranging over the group elements, then

for every $P_1$ and $P_2$, 

\(^{84}\) See {Weiss, 1949 #554}, p. 56-7 for a simple proof of version of Cayley’s theorem. Later, in Chapter 5, I shall show how set theory can be interpreted as a theory of the constructibility of open-sentences.
Obviously one can restate this statement of the theorem with a version that does not assert the existence of mathematical entities but instead asserts what it possible to do.

Now what has all this to do with the thesis that mathematical theorems are fictional sentences and not truths? Well, the above theorem of group theory [###], expressed modally about what it is possible to do, can be easily tested and confirmed to be true. To confirm [###], one could start with a linearly ordered class of objects, say four wooden cubes, each with a letter of the alphabet carved on one of its sides. Let us suppose that the cubes are: A, B, C, and D. We can also suppose that the initial ordering O₁ of the four cubes is the alphabetical ordering listed in the previous sentence. Clearly, one can perform permutations of this ordered set by physically moving the cubes in an appropriate manner. For example, one could perform some permutation P₁ on O₁ and then permutation P₂ on the ordering that results from performing P₁ on O₁, which would give us the ordering that results from performing the product permutation (P₁ x P₂) on O₁. Then we could find the inverse permutation of the result of performing (P₁ x P₂) on O₁. Proceeding in this way, one can concretely test theorem [###], which will provide one with convincing grounds for concluding that [###] is a true statement, and not a fictional sentence that is neither true nor false.⁸⁵

Since the task of actually carrying out such a confirmation may appear confusing to some readers, I shall provide a way of making the task more perspicuous and manageable. To simplify matters, let us concern ourselves with

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⁸⁵ How such a confirmation of the theorem would be carried out is discussed in more detail in {Chihara, 2010 #597}, Section 10.
permutations of linear arrangements of Arabic numerals. I shall call all such linear arrangements: “arrangements”. Hence, we shall now be concerned with permutations of arrangements. To make things even simpler, let us concern ourselves with arrangements of just the numerals 1, 2, and 3.

I shall make use of the following notation for arrangements:

\[
  r = \begin{pmatrix}
    1 & 2 & 3 \\
    j_1 & j_2 & j_3
  \end{pmatrix}
\]

is to mean: replace 1 by \(j_1\), replace 2 by \(j_2\), and replace 3 by \(j_3\). Thus, for:

\[
  p = \begin{pmatrix}
    1 & 2 & 3 \\
    3 & 2 & 1
  \end{pmatrix} \quad q = \begin{pmatrix}
    1 & 2 & 3 \\
    1 & 3 & 2
  \end{pmatrix}
\]

\[
  p \times q = \begin{pmatrix}
    1 & 2 & 3 \\
    2 & 1 & 3
  \end{pmatrix}
\]

It is easy to see that the product operation is associative and that the order of the columns in the above symbolization is immaterial.

One can now state a little “theorem”:

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\[86\] I make use of the notion introduced by Marie Weiss in her book \{Weiss, 1949 #554\}, Chapter 3, Section 3.
[***] The arrangements of the three numerals form a group with respect to permutation multiplication.

To prove this theorem, it should be noted that the identity element of this group is the following:

\[
i = \begin{pmatrix} 1 & 2 & 3 \\ 1 & 2 & 3 \end{pmatrix}
\]

And the inverse of \( r \) (defined above) is

\[
r^{-1} = \begin{pmatrix} j_1 & j_2 & j_3 \\ 1 & 2 & 3 \end{pmatrix}
\]

Given these two facts, I trust that the reader can easily confirm the truth of the above little “theorem”, by carrying out the confirming permutations—where none of the confirmations require assuming the existence (in the actual world) of any numbers or functions. Such confirmations can be carried out, with no need to assume the existence in the actual world of any mathematical objects; one can, for example, simply move little magnetic plastic numerals on the refrigerator of the sort frequently used to teach children basic properties of the number system. Notice that by proceeding in the above way, one would arrive at a refutation of Field’s claim that mathematical theorems are all false—this in a way that does not involve “quantifying over” mathematical objects or abstract entities of any sort to which Field objects.
7. Verification of the Truth of Another Mathematical Theorem

Let us now stipulate (for the purpose of simplifying the formulation of a "law" to be discussed below) that the identity transformation (the transformation that leaves each block in exactly the same position) is to be considered a permutation of the ordering.\(^{87}\) The question arises: How many possible permutations of a linear ordering of, say three of the wooden blocks described in the previous Section are there? It can be easily determined that there are exactly 3! or six different permutation of the above ordering. Let us call this little "theorem" (that there are six different permutations of the above ordering) \([P]\). According to the fictionalist Field, \([P]\) is not true (since it is a mathematical theorem). However, \([P]\) has, in effect, been established to be true countless times by many individuals working with permutations. Indeed, concrete tests of this sort have been carried out around the world yielding abundant verifications of \([P]\), without anyone ever finding any significant problems with the theorem. And as I have emphasized several times already, this theorem can be stated in a way that does not involve asserting that some mathematical object exists in the actual world.\(^{88}\)

Here is another example that is closely related to the above. Let \(C(n/k)\) be the number of combinations (or unordered arrangements) of \(k\) things taken from a collection of \(n\) objects. Then there is a mathematical theorem that:

\(^{87}\) It might be argued that the identity transformation is not a genuine permutation, since it does not "reorder" the blocks. But mathematicians simply regard such deviations from ordinary usage as slight and hardly worth mentioning.

\(^{88}\) Obviously, one can prove a more general (and less trivial) theorem using mathematical induction. For anyone having doubts about the truth of the theorem, he or she should attempt to envisage a natural number \(n\) such that the theorems holds for all natural numbers less than \(n\), but not for \(n\) itself.
C(n, k) = n!/k! (n – k)!

This theorem about combinations has been proven and verified many times by students countless times. Let \([P\#]\) be the mathematical statement:

\[C(3, 1) = 3\]

If no part of mathematics is true, then \([P\#]\) must be not true. But surely, the fictionalist Field would be foolish to deny the truth of \([P\#]\) since it is so easy to verify. Well, might they not claim that \([P\#]\) asserts the existence of mathematical objects? It certainly makes a cardinality statement, but surely fictionalists do not wish to claim that even simple cardinality statements are fictional and lacking in truth values: they do not wish to commit themselves to denying that my statement that I have two granddaughters has a truth value.

I have selected above only a few specific mathematical theorems that have been straightforwardly proven, tested, and verified to be true, but that do not assert the existence of mathematical objects. There are countless such theorems. So we have abundant examples that refute the fictionalist’s thesis that no mathematical theorem is true. Reviewing the grounds that these fictionalists have provided for concluding that no mathematical theorem is true, one can that see that they have focused primarily on a certain kind of mathematical theorem: the kind that asserts the existence of sets. Field treats mathematics as if it were simply set theory: Field regards mathematics as if it were just ZFU.

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89 See, for example, (Neyman, 1950 #600), Section 2.5.5.
Thus, I suspect that Field does not address the truth value of theorems about permutations and combinations because they have restricted his investigations to only general set theoretical theorems—and hence theorems that, they assume, are only about sets—mathematical objects that they claim do not, in fact, exist in the actual world.

Thus, as a philosopher, I am willing to contemplate seriously the fictionalists’ thesis that all of the theorems of mathematics are untruths. But before I accept such a radical thesis, I would have to be presented with justifications more forceful and sensible than the kind that Field set forth to support his radical fictionalist thesis.

Besides, even apart from the various arguments and considerations I have already provided for rejecting the fictionalist view, let us ask: “What is more probable: (a) the fictionalists’ thesis that all mathematical theorems are not true or (b) the truth of [P]?” Recalling that if [P] is true, then the fictionalists’ thesis is false, and the answer to this question is evident.

Later, I shall also show (as I have already done elsewhere\(^90\)) that one need not adopt the fictionalist’s view that mathematics is either false or fictional in order to show how well-known versions of classical mathematics can be applied in science and engineering.


As I pointed out in the Introduction to this chapter, Field claims to be giving, in his book, a correct account of how “mathematics” is applied in science. I take it that, by “mathematics”, he means the mathematics that is, in

\(^{90}\) See, for example, [Chihara, 2004 #468], especially Chapter 9.
fact, used by real (genuine) scientists, applying real mathematics, and not some formalized model of real mathematics, such as ZF. That is, I take it that he means by “mathematics” what I call “real mathematics”. In any case, giving a correct account of how real mathematics is applied in science is surely one of the principal tasks of the philosopher of mathematics, whereas merely showing (as Field does) how ZF might be applied in an imagined world in which the science used in this world is axiomatized and formalized in a first or second-order logical language would, at best, be only a step in showing how real mathematics is, in fact, applied in real science.

So how, according to Field, is mathematics applied in science? The only general account he gives in his book of applications of mathematics is in terms of the conservative nature of ZF— but not in terms of the conservative nature of real mathematics. He seems to believe that if ZF is conservative, then “standard mathematics” must also be conservative. Of course, nothing remotely like a reasonable justification is provided for such a belief. I say “of course” because it is not clear what it would even mean to assert that real mathematics is conservative—given that Field only explains what it would be for a formal mathematical theory, with a precisely limited vocabulary like ZF, to be conservative. After all, real mathematics is expressed in a natural language, whose vocabulary is not restricted as is the vocabulary of the language of ZF (recall the discussion of the difference between real mathematics and formalized versions of mathematics in the Introduction). Indeed, although one can regard ZF as a model of real set theory, there are sufficiently many important differences between the model and what it models that one cannot use the fact that ZF is conservative over certain nominalistic first-order theories to infer that real mathematics is conservative over any scientific theory. In
short, one cannot use what Field provides in his book to draw any significant
insights into how real mathematics is actually applied in real science.

One might try to argue that Field shows how a formalized and axiomatized
version of real mathematics might have been applied to a specific formalized
and axiomatized version of science, but Field’s book simply does not do what
he claims it does when he writes that the book “gives an attractive account of
how mathematics is applied to the physical world” ({Field, 1980 #206}, p. 6).
The book certainly does not give an attractive account of how real mathematics
is applied in science.

9. Field’s Deflationist Thesis Assessed

I should now like to reconsider Field’s deflationist thesis about
mathematical knowledge (discussed in Section 3). Recall that central to Field’s
thesis is the claim that what chiefly distinguishes someone with lots of
mathematical knowledge from someone with little is that the former, but not
the latter, has lots of knowledge of truths of the form:

(i) It is logically necessary that if A, then B

and

(ii) It is logically possible that A.\(^{91}\)

It should be noted that neither the theorem about permutations, nor the above
theorem about groups that I discussed earlier, is the assertion of a modal fact

\(^{91}\) {Field, 1984 #434}, p. 85.
of the form (i) or (ii) above. So clearly, mathematicians know things that non-
mathematicians do not—and they know theorems that are not of the form (i) or
(ii). But this, to me, is not surprising, since I do not consider Field’s deflationist
thesis to be at all intuitively plausible.

Let me summarize, briefly, how I assess Field’s view of how mathematics is
applied in science. The only general account he gives in his book of applications
of mathematics is in terms of the conservative nature of ZF. He thus seems to
believe that if ZF is conservative, then “standard mathematics” (real
mathematics?) must also be conservative. But nothing even close to a
reasonable justification is provided for such a belief. Thus, although one can
regard ZF as a model of real set theory, there are sufficiently many important
differences between the model and what it models that one cannot use the fact
that ZF is conservative over the kind of nominalistic first-order theories he
considers in his book to infer that real mathematics is conservative over any
real scientific theory. Hence, one cannot use what Field provides in his book to
draw any significant insights into how real mathematics is applied in real
science. Ergo, Field’s book simply does not do what he claims it does when he
writes that it “gives an attractive account of how mathematics is applied to the
physical world” (Field, 1980 #206, p. 6).92 (In a later chapter, I shall present
a very different picture of how mathematics is applied in science).

10. The Fregean Assumption Reconsidered

   I begin this section by noting that many of the fundamental claims of
Field’s philosophy of mathematics, which I have been attacking, are based on his

92 Cf. {Peressini, 1997 #389}.
thesis that no part of mathematics is true. What I want to emphasize here is
that, in opposition to Field’s thesis that no part of mathematics is true, I believe
that mathematical theorems are, indeed, “true” in some appropriate sense of
‘true’. The many examples I gave in this chapter of mathematical theorems
that can be, and have been, convincingly verified to be true show that it would
be reckless to deny that these theorems are, in some appropriate sense, “true”.
So rather than concluding from the trio of premises described earlier in Section
4 that no part of mathematics is true, as Field does, I would infer that it is
probably the Fregean Assumption, which underlies Field’s reasoning, that is
false. (This is the first of many reasons I shall give in this work for maintaining
that the Fregean Assumption is false).

It may be somewhat surprising to non-philosophers that one needs to
argue in defense of the thesis that mathematical theorems are, in some sense,
true, but in philosophy practically nothing can be taken for granted and almost
anything can be challenged. (Recall my description of philosophy and
philosophy of mathematics in the Introduction). However, the reader can be
assured that I, for one, will develop my view of mathematics without rejecting
the truth of mathematics. Thus, what follows in this work will be based upon
the thesis that mathematical theorems are in some sense true. Of course, the
difficulty here is to spell out in some detail in just what sense mathematical
theorems are true. But I need to make clear that my view here is that any
reasonable philosophical view of the nature of mathematics should be
consistent with the view that mathematics is, in an appropriate sense of ‘true’,
true.
The qualification of the previous paragraph was emphasized in italics because in some of my earlier publications\textsuperscript{93} I attacked what I called “the Central Assumption” of classical mathematics. This was the assumption that I expressed in the following way:

The theorems of classical mathematics are true.\textsuperscript{94}

Now most of the philosophers of mathematics I shall be investigating in the following chapters have regarded the theorems of mathematics as true propositions about the actual world. We saw in the previous chapter that Quine arrived at such a position chiefly on the basis of his adoption of the Fregean views of Frege and Russell. Then, to a great extent due to the acceptance of the Fregean Assumption by the “big four”, Frege, Russell, Quine, and Gödel, most philosophers of mathematics of the Contemporary Period have also accepted the Fregean Assumption. Whether such a widespread acceptance of this metaphysical principle has been a beneficial feature of much of the philosophy of mathematics of the Contemporary Period is something I shall be investigating in this work.

\textsuperscript{93} See, for example, [Chihara, 2008 #543], Section 4.1.

\textsuperscript{94} [Chihara, 2008 #543], p. 138.
Chapter 4

The Role of the Fregean Assumption in Various Indispensability Arguments

Introduction

As I mentioned in Chapter 2, in the area of philosophy of mathematics, Quine is best known for his Indispensability Argument for the Existence of Mathematical Objects, the principal idea of which being that reference to, or quantification over, mathematical objects such as numbers and functions, is indispensable for serious theorizing in science. Quine’s Indispensability Argument is widely regarded as the most influential anti-nominalism argument that has been published yet. Many versions of Quine’s Indispensability Argument have appeared in the philosophical literature since Quine first advanced his version, and much has been written about them. But what has not been discussed or even noticed about these arguments is the crucial role that the Fregean Assumption plays in them—a role that will be the subject of this chapter.

I first responded to Quine’s Indispensability Argument some forty years ago in Ontology and the Vicious-Circle Principle, and followed it up in 1990 with
a different way of undermining it. In 2004, I came up with still another way of responding to the argument. In Chapter 10, I shall provide yet one more way of undermining the argument that differs markedly from my earlier ones by attacking the Fregean basis of Mathematical Realism.

1. Quine’s Indispensability Argument for the Existence of Mathematical Objects

I believe Quine is the first philosopher to have put forward an argument for the existence of mathematical objects by making use of the idea that the existence of mathematical objects is required by the needs of the empirical sciences. However, Quine never explicitly published such an argument, with clearly stated premises and the specific conclusion that mathematical objects exist in the actual world. The following quotation from Quine’s writings (already discussed in Chapter 2) is one of two that strongly suggest such an argument:

[The nominalist] is going to have to accommodate his natural sciences unaided by mathematics; for mathematics, except for some trivial portions such as very elementary arithmetic, is irredeemably committed to quantification over abstract objects. (Quine, 1960 #78), p. 269).

Another is:

Classical mathematics . . . is up to its neck in commitments to an ontology of abstract entities. . . . The issue is clearer now than of old, because we now have a more explicit standard whereby to decide what ontology a given theory or form of discourse is committed to . . .

95 In {Chihara, 1990 #34}, Chapter 1, Section 2.

96 (Quine, 1961 #344), p. 13. The “explicit standard” to which Quine refers in the above quotation is, I believe, his “criterion of ontological commitment”. See {Chihara, 1973 #48}, Chapter III, Section 3, for details.
On the bases of such statements, several philosophers\textsuperscript{97} have constructed explicit Indispensability Arguments for the existence of abstract objects. So it is not surprising that a variety of statements of the argument, based upon the above quotation, have been put forward as “Indispensability Arguments”.

It was noted earlier in Chapter 2 that one could interpret Quine to be, in effect, challenging the nominalist to produce a satisfactory version of the natural sciences using a nominalistically acceptable version of mathematics. Such a way of interpreting the Indispensability Argument resulted in my producing the Constructibility version of set theory, discussed in Chapter 5, which I argued could theoretically provide a nominalist with the mathematical tools needed in science. However, in this chapter, I shall be interpreting Quine’s “Indispensability Argument” in a more standard way—more specifically, in the way that one of the top philosophers of the United States, Hilary Putnam,\textsuperscript{98} has interpreted it.

2. Putnam’s Version of the Indispensability Argument

\textsuperscript{97} For example, Putnam, Maddy, and Resnik have produced such Indispensability Arguments (to be discussed shortly).

\textsuperscript{98} I first met Putnam during the 1960-1 academic year at Oxford, at which I was carrying out studies of Wittgenstein’s writings under the tutelage of Elizabeth Anscombe. Oxford was then widely regarded as the Mecca of analytic philosophy, attracting philosophers from all over the world and, in particular, from the United States. For example, Hilary Putnam and Jerry Fodor were both at Oxford when I was there. I can remember being greatly impressed by Putnam, not only for his brilliance in philosophical discussion, but also for his detailed knowledge of science, mathematics, and logic. In this work, I discuss the version of the Indispensability Argument he defended in \textsuperscript{Putnam, 1971 #390}, even though there are grounds for thinking that he no longer accepts it (see, for example, \textsuperscript{Putnam, 2004 #625}, especially Part 1, Lecture 4). I discuss Putnam’s version here because it is so clearly stated and because it presents a version of the Indispensability Argument that many philosophers take to be “the Indispensability Argument”. For the sake of simplicity of exposition, I shall discuss Putnam’s reasoning in this early work, as if he still accepts the argument. I trust that readers of my work will not be misled about Putnam’s present views on the Indispensability Argument by my way of discussing Putnam’s early book.
In his short monograph Philosophy of Logic, Putnam presents his version of Quine’s argument in the following way:

(1) Quantification over mathematical entities is indispensable for science, both formal and physical.

(2) Therefore we should accept such quantification.

(3) This commits us to accepting the existence of the mathematical entities in question.

He then adds:

This type of argument stems, of course, from Quine who 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 #390], p. 57).

What is striking about Putnam’s formulation of his version of the argument is his terminology: Quine’s original formulation of the argument in Word and Object was in terms of mathematics: what was thought to be indispensable for science was mathematics; and Quine seemed to be willing to consider, at least, the possibility that some adequate version of science might be formulated in such a way as to contain no mathematics. Of course, as I mentioned earlier, after exploring such a possibility in some depth and detail, he seems to have concluded that such development was not a reasonable possibility.

The question then arises: How is the necessity of quantifying over mathematical entities in science (as Putnam put it) connected to the necessity of having mathematics in science (as Quine put it)? Well, could you have mathematics in science if there were no quantification over mathematical
entities in science? Putnam is suggesting in his argument that the answer is: No. And Quine seems to have a similar view when he wrote: “mathematics . . . is irredeemably committed to quantification over abstract objects” ([Quine, 1960 #78], p. 269). So if mathematics is necessary for science, then both Putnam and Quine seem to have agreed that quantifying over mathematical entities would also be necessary for science.

On the other hand, suppose that quantifying over mathematical entities were necessary for science. Could we conclude that mathematics is also necessary for science? Well, if one’s theory had quantification over mathematical entities, then one would have to have some mathematics to make sense of that quantification—mathematical entities are not things like stones that one might just pick up on a walk in the park—and such quantification would presuppose a certain amount of mathematical theory to make sense of the mathematical entities being quantified over. In other words, quantifying over mathematical entities would require some mathematics. So one could make a case for holding that the Putnam version of the Indispensability Argument is essentially equivalent to the Quinean version.

Interestingly, Putnam’s version of Quine’s argument has been regarded by some philosophers as clearer and more persuasive than Quine’s own version—clearer because Putnam’s version is stated specifically as an argument, with definite premises and a definite conclusion (unlike Quine’s implicit “challenge to the nominalist”); and more persuasive because Putnam takes up, and responds vigorously to, a number of imagined criticisms of the Indispensability Argument that Quine never considered. Thus, Field has written: “The most thorough
presentation of the Quinean argument is actually not by Quine but by Hilary Putnam: cf. The Philosophy of Logic . . .” ({Field, 1980 #206}, p. 107).

Another question worth raising is: Why did Putnam choose to discuss indispensability in terms of quantification rather than mathematics? I think that Putnam found that the quantificational expression of indispensability allowed him more flexibility in his discussion of Quine’s position. For example, in a chapter of the book entitled “How Much Set Theory Is Really Indispensable for Science?”, Putnam considers the question: Does science require the full impredicative set theory of, say ZF, or could science make do with a predicative set theory (such as Wang’s $\Sigma_\omega$)? This was, for Putnam, not a question of realism versus nominalism, for in either case, science would require quantification over mathematical entities, namely sets. Thus, using the quantificational expression of indispensability, one could argue that the answer to the above question would not affect the validity of the Indispensability Argument. Putnam concludes:

Insofar, then, as the indispensability of quantification over sets is any argument for their existence . . . we may say that it is a strong argument for the existence of at least predicative sets, and a pretty strong, but not as strong argument for the existence of impredicative sets. ([Putnam, 1971 #390], p. 56).

The Indispensability Argument that begins this section shows that Putnam thinks that mathematical theorems are propositions about the actual world.

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99 Also, some philosophers (such as Maddy) have referred to the “Quine/Putnam Indispensability Argument” (see {Maddy, 1990 #207}, Chapter 1, Section 4). I should note that I am not always clear what she had in mind, in attributing to Putnam a sort of co-authorship of the argument: is she is thinking of his version of Quine’s argument as presented in The Philosophy of Logic; or the somewhat different argument for the existence of mathematical entities found in {Putnam, 1979 #313}, where he appeals to “quasi-empirical” justifications of various principles and theories of mathematics; or something else entirely?
The reason I make this claim is because he infers from (1) and (2) that we are thereby committed to “accepting the existence of the mathematical entities in question”. What this means is that Putnam is holding is that from the truth of (1) and (2), one can infer that mathematical entities exist in the actual world. In other words, Putnam was making the Fregean Assumption.

So the next question to consider is whether Putnam also believes that mathematical theorems are true. It is clear that Putnam believes that mathematics is indispensable for science, but might he not also believe that mathematics is not true? No, he argues strenuously against such a position, writing:

[i]t 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”. Such a judgment could only be made if one accepted a transcendent method as superior to the scientific method; but this philosopher, at least, has no interest in doing that. ([Putnam, 1971 #390], pp. 73-4).\footnote{101}

So at this point, at least, Putnam was accepting a standard form of the Fregean Assumption. He is accepting the thesis that mathematical theorems are true propositions about the actual world.

3. Lewis’ Variations on a Theme of Quine’s

\footnote{100} I use the present tense in the above sentence to indicate that that Putnam was a Fregean when he wrote the book being quoted. I do not know whether he is a Fregean at the present time.

\footnote{101} Recall Field’s claim that “The most thorough presentation of the Quinean argument is actually not by Quine but by Hilary Putnam: cf. The Philosophy of Logic . . . ”.
There are two principal theses underlying Putnam’s version of Quine’s Indispensability Argument:

(a) The Fregean Assumption that the theorems of mathematics are propositions about the actual world;

and

(b) The thesis that we have good reasons to believe that mathematics is true.

Quine, of course, used these two theses in his Indispensability Argument. Now David Lewis also accepted these two theses. Since nominalists believe that there are no mathematical objects in the actual world, from Lewis’s perspective (which assumes the above two theses (a) and (b)), nominalists must believe that the theorems of mathematics, at least as standardly construed, are not true. It is for this reason that Lewis came to attribute to the nominalist the belief that mathematicians are seriously in error and in need of correction. Thus, one can appreciate what is the basis for Lewis’s anti-nominalist rhetorical argument which he stated as follows:

Mathematics is an established, going concern. Philosophy is as shaky as can be. To reject mathematics for philosophical reasons would be absurd. ([Lewis, 1991 #63], p. 58).

He then added the rhetorical flourish:
I’m moved to laughter at the thought of how presumptuous it would be to reject mathematics for philosophical reasons. How would you like the job of telling the mathematicians that they must change their ways, and abjure countless errors, now that philosophy has discovered that there are no classes? Can you tell them, with a straight face, to follow philosophical argument wherever it may lead? . . . Not me! ([Lewis, 1991 #63], p. 59).

Why have I listed Lewis’s rhetorical argument here as kind of Indispensability Argument? It is because of its premise that mathematics “is an established, going concern” which cannot be rejected on philosophical grounds—that is, he is taking mathematics to be an indispensable part of our world view that cannot be rejected for merely philosophical reasons.

4. Resnik’s Holism-Naturalism Indispensability Argument

Here is how Resnik formulated his version of the Indispensability Argument, which he called the “Holism-Naturalism Indispensability argument”¹⁰²:

(1) Mathematics is an indispensable component of natural science.

(2) 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.

(3) Hence, 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.

¹⁰² Resnik cautions his readers that this argument “is clearly based upon principles that Quine accepts, although it is not clear that it accurately paraphrases his or Putnam’s arguments” ([Resnik, 2005 #613], p. 430).
This version of the Indispensability Argument was heavily criticized, in particular by Penelope Maddy and Elliot Sober, who took aim especially at premise (2). In reply, Resnik has put forward a new Indispensability Argument that he hoped would evade their objections. I shall take up Resnik’s slightly revised version of the argument after discussing some other versions of the Indispensability Argument.

5. Maddy’s Changing Views of the Indispensability Argument

Maddy’s views about the soundness of Quine’s Indispensability Argument have changed radically over time. Consider first the position she took in her book Realism in Mathematics.

(a) Maddy’s Set Theoretical Realism

The “set theoretical realism” that Penelope Maddy developed in her book Realism in Mathematics was an eclectic production. From Quine and Putnam, Maddy accepted their version of the Indispensability Argument; from Gödel, she took the “recognition of purely mathematical forms of evidence and the responsibility for explaining them” (Maddy, 1990 #207, p. 35); and from her study of George Pitcher’s A Theory of Perception, she came up with an explanation for how we are able to know that sets exist and have the properties that set theorists have postulated about them. According to Maddy’s book, we know that there are sets because we can actually perceive sets. Adopting Pitcher’s approach to perception, she analyzed the perception of sets in such a

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103 See Resnik, 2005 #613, p. 431, for a discussion of these objections and references to the relevant articles.

104 Pitcher, 1971 #619.
way that Steve perceives a set of eggs in a carton if the following three
conditions are satisfied: a) there is a set of eggs in the carton; b) Steve gains
the perceptual belief that this set is three membered; and c) the set of eggs
participates, in an appropriate causal way, in the generation of Steve’s belief
({Maddy, 1990 #207}, p.63).

Thus, Maddy’s early acceptance of the Quine-Putnam Indispensability
Argument involved accepting the thesis of the argument that mathematical
objects exist in the actual world; but she went much further than either Quine
or Putnam in attributing some non-mathematical properties and features to sets
than were not attributed to sets by either the Quine or Putnam versions. For
example, she believed that the set of eggs in my refrigerator exists in a specific
place in the actual world: that is in the very place that the eggs inhabit. Thus,
the set of eggs was maintained by her to inhabit the actual physical world; her
view was that some sets have existence in specific locations in physical space,
and can be perceived, held, and moved about in space. In other words, some
sets were held by Maddy to be very much like physical objects.\textsuperscript{105} Thus, it is
abundantly clear that, at least in this period of Maddy’s philosophical career,
Maddy accepted the Fregean Assumption.

In what follows, I shall focus on Maddy’s original acceptance of the
Indispensability Argument, even though she came to reject the argument in
later works. In her book, she wrote: “We are committed to the existence of
mathematical objects because they are indispensable to our best theory of the
world and we accept that theory” ({Maddy, 1990 #207}, p. 30). This thesis

\textsuperscript{105} In earlier publications, I raised a number of objections to the above views of Maddy. For
example, I gave critiques of Maddy’s views that we humans can literally perceive sets in
{Chihara, 1982 #168} and {Chihara, 1990 #34}, Chapter 10, Section 4, which I shall not repeat
here
resulted from her conclusion that mathematical objects are indispensable to our best theory of the world because mathematics is indispensable for our best theory of the world—namely contemporary science. She also concluded that mathematics is at least approximately true, writing:

[S]uccessful applications of mathematics give us reason to think that mathematics is a science, that much of it at least approximates truth. Thus successful applications justify, in a general way, the practice of mathematics. ([Maddy, 1990 #207], p. 34).

Thus, she can be understood to have accepted a version of the Fregean Assumption and advanced a sort of Indispensability Argument for the existence of mathematical objects of the following sort:

(M-1) Mathematics is indispensable to our best theory of the world  
(namely, contemporary science)

Because the existence of mathematical objects is indispensable to mathematics,

(M-2) The existence of mathematical objects is indispensable to our best theory of the world.

So:

(M-3) “We are committed to the existence of mathematical objects because they are indispensable to our best theory of the world and we accept that theory” ([Maddy, 1990 #207], p. 30).
Interestingly, Maddy came later to abandon completely the soundness of her set theoretical realism. The reason she gave for this rejection of her earlier position was expressed during the question-and-answer period of her presentation at a Berkeley Logic Colloquium: there, she was directly asked by the mathematical logician John Addison why she had abandoned her set theoretical realism. Her answer, as I remember it, was that she no longer thought the Indispensability Argument was valid. In the following section, I shall give some of the principal reasons she has published for rejecting the Indispensability Argument.\(^\text{106}\)

6. Maddy’s Rejection of the Indispensability Argument

(a) The Argument in “Indispensability and Practice”

In her 1992 paper, “Indispensability and Practice”\(^\text{107}\), Maddy draws inspiration from the controversy over the reality of molecules and atoms that took place in the 19\(^{\text{th}}\) Century to fashion an argument for rejecting the Indispensability Argument.\(^\text{108}\) To facilitate an appreciation of Maddy’s objection to the Indispensability argument, let us consider the following example that the historian of science Mary Jo Nye described to show how some of the strongest arguments for the “absolute reality of atoms” arose in organic chemistry:

\(^{106}\) The reasons she gave for abandoning the Indispensability Argument are discussed in detail in \{Chihara, 2004 #468\}, Chapter 5, Section 9. I analyze in this section just two of the reasons she gave for giving up the Indispensability Argument.

\(^{107}\) \{Maddy, 1995 #236\}.

\(^{108}\) \{Maddy, 1992 #211\}.
In 1826 Gay-Lussac and Liebig published their discovery of the identity in composition of silver fulminate and silver cyanate (AgCNO), and this was followed by Wöhler’s transformation of ammonium cyanate into isomeric urea (1928) . . . This isomerism seemed inexplicable except on the grounds of real, structural molecules. . . . Yet there was still a great deal of hesitation about the apparent submicroscopic world of discontinuous matter.\(^{109}\)

What this example seems to show is that, even though the best explanation of an established phenomenon (the transformation of ammonium cyanate into isomeric urea) implied the existence of “real, structural molecules”, there still was a great deal of hesitation among experts to conclude that molecules in fact exist. The lessons Maddy extracted from such historical examples prompted her to draw a distinction between those parts of a scientific theory that are deemed to be true and those parts that are regarded as useful—even indispensable—but not true, writing:

We must even allow that the merely useful parts might in fact be indispensable, in the sense that no equally good theory of the same phenomena does without them. Granting all this, the indispensability of mathematics in a well-confirmed scientific theory no longer serves to establish its truth. (\{Maddy, 1992 #211\}, p. 281, italics mine).

When Maddy wrote the above words, she evidently concluded that, in the light of such examples as the one above, the Indispensability Argument of Quine and Putnam was not valid.\(^{110}\)

(b) Maddy’s 1997 Rejection of the Indispensability Argument

In her book Naturalism in Mathematics, Maddy cites the role of continuous spacetime in contemporary scientific theory as an example of an entity that is

\(^{109}\) \{Nye, 1972 #453\}, p. 4, italics mine.

\(^{110}\) I give a fuller discussion of these grounds of Maddy’s given in for rejecting the Indispensability Argument in \{Chihara, 2004 #468\}, Chapter 5, Section 9.
now indispensable for our best scientific theory, even though it is not clear that a responsible use of indispensability can warrant an ontological commitment to the reality of continuous spacetime. Without going into the details of her examples, here is the lessons she learns from such examples:

First, we’ve seen that scientists do not take the indispensability appearance of an entity in our best scientific theory to warrant the ontological conclusion that it is real; for this conclusion, the appearance must be in a hypothesis that is not legitimately judged a “useful fiction” . . .
Second, we’ve seen that the status of some applications . . . are as yet unsettled . . . ({Maddy, 1997 #317}, p. 152).

She concludes that a responsible indispensability argument “seems unlikely to support the existence of more than a few (if any) mathematical entities” ({Maddy, 1997 #317}, p. 153).

Elaborating the basic idea of the above objection to the Indispensability Argument, Maddy writes:

[M]athematical existence assumptions in science, and their accompanying assumptions about the structure of physical reality, are not treated on an epistemic par with ordinary physical assumptions: the standard for their introduction are weaker, and their role in successful theory lacks confirmatory force; they are at once favoured and trivialized. The trouble is that this epistemic disanalogy undermines the ground work of the original Quinean argument. ({Maddy, 1990 #207}, p. 156).

This claim about how empirical scientists treat mathematical existence assumptions in their work is surely sound: as far as most empirical scientists are concerned, mathematical statements in general, and existential mathematical statements in particular, that occur in their scientific papers and reports are not regarded as being on “an epistemic par” (to use Maddy’s terminology) with ordinary physical assumptions. Maddy concludes from the above claim that
“science seems not to be done as it would have to be done if it were in the
Quinean business of assessing mathematical ontology” ([Maddy, 1990 #207],
p. 157).\footnote{111}

I certainly agree with Maddy on these points. Yes, empirical scientists do
not concern themselves with what Quine calls the “ontological commitments” of
mathematical theorems. We can all agree with Maddy’s assertion that
“physicists seem happy to use any mathematics that is convenient and
effective, without concern for the mathematical existence assumptions involved
. . .” ([Maddy, 1997 #317]. P. 155). However, I question the last assertion of
the set-off sentences of Maddy’s book quoted above. As a simple matter of
logic, it does not follow from what I have accepted above (about how scientists
regard the existential theorems of mathematics) that “the ground work of the
original Quinean argument” is undermined: Quine’s original Indispensability
Argument was certainly not an argument about how scientists reason about the
ontological implications of the mathematical theorems they use. The
Indispensability Argument was a philosophical argument put forward by a
philosopher, and the fact that scientists do not give arguments of that kind is
no good reason to infer that the Indispensability Argument should be judged to
be invalid or unsound. (I should note, however, that my criticisms of Maddy’s
assessment of Quine’s argument should not be taken to imply that I believe
that the Indispensability Argument is in fact sound—far from it, as will be
become obvious in Chapter 10).

7. Colyvan’s Versions of the Indispensability Argument

\footnote{111 It should noted that, in the period being discussed, Maddy seems to have continued
to accept the Fregean Assumption.}
In his book *The Indispensability of Mathematics*, Mark Colyvan advances the following argument, which he calls “the Quine/Putnam Indispensability Argument”:

(CV-1) We ought to have ontological commitment to all and only those entities that are indispensable to our best scientific theories;

(CV-2) Mathematical entities are indispensable to our best scientific theories;

Therefore:

(CV-3) We ought to have ontological commitment to mathematical entities.\(^{112}\)

Notice first that Colyvan’s “Quine/Putnam argument” has both a normative premise and a normative conclusion, despite the fact neither the Quine nor the Putnam version of the “Indispensability Argument” has any such feature. Also, for some reason, which is by no means clear to me, Colyvan seems to think that the Indispensability Argument is best seen as normative.\(^{113}\) But, despite what he calls the above argument, I shall refer to it as “Colyvan’s Indispensability Argument”.

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\(^{112}\) [Colyvan, 2001 #452], p. 11.

\(^{113}\) See [Colyvan, 2001 #452], pp. 11-2.
I shall not examine here each of the many defenses of this version of the Indispensability Argument that Colyvan presents in his book,\textsuperscript{114} but focus only on one: this is his reply to Maddy’s objection analyzed in the previous section.

Since Colyvan believed that the Indispensability Argument was sound, his reaction to Maddy’s objection of her 1992 paper “Indispensability and Practice” was to attempt to refute it, or at least to blunt it. Colyvan’s principal rebuttal consists in agreeing with Maddy’s account that the mid-Nineteenth-Century atom was indispensable to the best available theory and also that many scientists treated the atom instrumentally; but he disagreed with Maddy’s view that these scientists were correct in so treating the atom. In other words, Colyvan simply rejected Maddy’s historical counter-example to the argument. For Colyvan, the scientists who rejected the reality of the atom and interpreted them instrumentally were simply wrong, going so far as to suggest that such scientists were guilty of “intellectual dishonesty” in using atoms in their best chemical theories, while denying their actual existence.\textsuperscript{115}

Now I have some doubts about the reasonableness of Colyvan’s assessment described in the previous paragraph—some of which I sketched in a previous work.\textsuperscript{116} I shall here only indicate why I doubt that Colyvan was in a very strong position to conclude that the scientists in question were guilty of “intellectual dishonesty”. Let us keep in mind that we are concerned here with events that took place over two hundred years ago—events many important features of which we, in the present-day world, have no knowledge. In order to

\textsuperscript{114} I do this in \{Chihara, 2004 #468\}, Chapter 5, Section 9.

\textsuperscript{115} \{Colyvan, 2001 #452\}, p. 100. Colyvan admits that we have in this case a difference of intuitions and that many may not share his intuitions.

\textsuperscript{116} Some of my doubts were set forth in \{Chihara, 2004 #468\}, pp., 138-140. The objections I give in this chapter are quite different.
obtain significant historical evidence supporting his assessment of the scientists in question, we would be forced to rely upon primarily notes, letters, and publications of various sorts which have survived to the present day. But what, in fact, do we really know about the states of mind of the authors of such “writings” when they were preparing such things? What do we know of their doubts, worries, questions, pains, aches, and even their motives, desires and fears, which may have impinged upon their thoughts and inferences? Thus, in trying to calculate accurately the states of mind (including the relevant degrees of belief) of the relevant scientists who lived two centuries ago, and who were hesitant to draw the conclusion that real, structural molecules truly existed, would seem to be an extremely difficult, if not impossible, task. So I doubt very much that Colyvan had truly convincing grounds for concluding that the scientists in question were guilty of “intellectual dishonesty”.

On the other hand, should we allow the above Maddy example to convince us that the Indispensability Argument is unsound? I must confess that, here too, I have some doubts that the Maddy examples decisively show that the Indispensability Argument is unsound, especially on the basis of examples such

117 J. L. Austin once gave a lecture at the Philosophy Colloquium of the University of Washington. During a discussion which took place later at a faculty member’s home, Austin expressed doubts about historical knowledge in general. He gave the example of what happened after the end of World War II when a number of British intellectuals had asked why the Allies had not accepted a conditional surrender from Germany, when it was clear that Germany was going to lose—so many lives, they argued, could have been saved had they accepted such a surrender. Austin used the example to show how, in a relatively short period of time after the war, the intellectual climate had changed so much that a number of scholars of England had a completely different understanding of what was politically possible than that possessed by those who had experienced the war. According to Austin, accepting such a surrender was absolutely unthinkable to those working in the government of Great Britain at that time. The fact that scholars could have thought such a surrender was possible suggests the folly of historians now trying to infer how people thought and reasoned some two hundred years ago on the basis of the small bits and pieces of material that have somehow survived those many years. (I should note that the above account of Austin’s visit is largely based upon what one of my teachers, David Keyt, related to me about the visit).
as the one discussed above in which a relatively few scientists many years ago were hesitant to infer the truth of a theory that was thought to be, in some sense, “indispensable”.

8. Resnik’s Pragmatic Indispensability Argument

In contrast to Colyvan’s way of responding to Maddy’s objection, Resnik has attempted to devise another version of the Indispensability Argument which he hoped would evade her doubts that “we can count on science to provide evidence for the truth of mathematics”.118 A central idea of the revised version is expressed by the thought that, when Newton calculated the orbits of the planets, “he presumably took for granted the mathematical principles he used” ({Resnik, 2005 #613}, p. 431). From a consideration of such examples, Resnik put forward his “Pragmatic Indispensability Argument” as follows:

(R-1) In stating its laws and conducting its derivations, science assumes the existence of many mathematical objects and the truth of much mathematics.

(R-2) These assumptions are indispensable to the pursuit of science, moreover, many of the important conclusions drawn from and within science could not be drawn without taking mathematical claims to be true.

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118 The reader may wish to consider other replies that Colyvan has suggested a defender of the Indispensability Argument might give in response to Maddy’s objection ({Colyvan, 2001 #452}, p. 100). These replies are discussed in my earlier work {Chihara, 2004 #468}, pp. 138-140.
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.\(^{119}\)

Since we are confident that we are “justified in drawing conclusions from and within science”, we must be justified in taking the mathematics used in science to be true, and hence that mathematical objects exist.

Now there are two unstated assumptions that Resnik makes in arguing as he does: His taking the mathematics used in science to be true amounts to taking the theorems of mathematics used in science to be true propositions about the actual world. This is because he is taking the existential theorems of mathematics to be asserting that mathematical objects exist in the actual world (which is why Resnik considers himself to be a Mathematical Realist). In effect then, Resnik is making use of the Fregean Assumption.

Resnik emphasizes that this version of the Indispensability Argument does not assume or presuppose that even our best scientific theories are true. Nor does this pragmatic version of the argument assume that the evidence for our scientific theories is also evidence for the truth of mathematics. However, as Resnik reasons:

\[
\text{[G]iven that we are justified in doing science, we are justified in using (and thus assuming the truth of) the mathematics in science, because we}
\]

\(^{119}\) {Resnik, 2005 #613}, p. 431).
known of no other way of obtaining the explanatory, predictive, and technological fruits of science.\textsuperscript{120}

In Chapter 10, I shall analyze Resnik’s pragmatic argument in a way that is very different from the way I analyzed a similar argument that he gave earlier.\textsuperscript{121} Indeed, I shall provide an analysis of all the Indispensability Arguments discussed in this chapter that differs significantly from those I gave in my earlier publications.

Of course, the Indispensability Arguments are by no means the only anti-nominalist arguments to be found in the philosophy of mathematics literature, as I shall show in later chapters, when I discuss the Burgess-Rosen war on nominalism.

9. Other Philosophers Who Accepted the Fregean Assumption

Obviously, the philosophers in this chapter who were mentioned to have accepted the Indispensability Argument can be inferred to have also accepted the Fregean Assumption. Thus, one can add the following four names to the list of philosophers who accept (or at least, at one time, accepted) the Fregean Assumption\textsuperscript{122}:

Putnam, Lewis, Resnik, and Colyvan

\textsuperscript{120}[Resnik, 2005 #613], p.432. Cf. earlier versions of the “the Pragmatic Indispensability Argument” that are to be found in [Resnik, 1997 #314], Chapter 3, Section 3

\textsuperscript{121} See [Chihara, 2004 #468], Chapter 5, Section 7.

\textsuperscript{122} I do not include Maddy’s name in this list since she explicitly rejected the Indispensability Argument some time after she had maintained the soundness of the argument.
Chapter 5

Mathematics Without Existence Assumptions: Dispensing with the Fregean Assumption

Introduction

As I mentioned earlier, my initial reaction to Quine’s Indispensability Argument was one of skepticism: it seemed to me then that the apparent need to quantify over mathematical objects might be only a sort of artifact of the typical way scientists and engineers represent motions and happenings in physical space. Science, as presently done, does use a kind of mathematics that seems to involve quantification over mathematical objects. But is that particular brand of mathematics absolutely necessary for use in science? I pondered these questions because the Realist’s interpretations of scientific statements in which mathematical objects were referred to (or “quantified over”) simply did not sit well with my general skepticism about views that postulated the existence (in the actual world) of various kinds of non-physical entities (such as mathematical objects and universals) advanced since the time of classical Greek philosophy.

In Chapter 3, I noted how, in response to the Quine-Putnam Indispensability Argument, Field advanced the view that the theorems of
mathematics are all false, because of his conviction that such things as mathematical objects do not really exist. In contrast to Field’s ontological views, I believed that one need not adopt such a radical thesis in order to avoid the implications of Quine’s Indispensability Argument. In this chapter, I shall first describe briefly my early published responses (expressed in Chihara, 1973 #48) to Quine’s Indispensability Argument. There, I put forward the view that the apparent need to quantify over mathematical objects in order to have an adequate version of science is only apparent: that in fact one could develop a kind of science that utilized a version of mathematics none of whose theorems contained quantification over mathematical entities.

The main idea of this initial approach was to develop a nominalistic version of mathematics within the framework of a predicative version of set theory. The key feature of this initial response was the appeal to a modal system of mathematics that would not require a commitment to the existence of abstract objects. Since this early response was succeeded by what I consider a more powerful way of answering Quine’s challenge, I will spare the reader the task of having to sort through all the messy details of the early version, by providing only a sort of “Big Picture” view of that version. Thus, most of this chapter will be devoted to my later response to Quine’s argument.

123 I remember purchasing my copy of Word and Object in Oxford during the 1960-1 academic year. In the margin of p. 269 my copy of that work, where appear the words “mathematics, except for some trivial portions such as very elementary arithmetic, is irredeemably committed to quantification over abstract objects”, I had written:

This philosophical doctrine should be soundly refuted.

124 Russell diagnosed the various mathematical paradoxes or antinomies that were discovered early in the 20th Century as always violating what he called the “vicious-circle principle”. Any set theory that conformed to the “vicious-circle principle” was called a predicative set theory. The reader can find a detailed discussion of Russell’s diagnoses of the various vicious-circle paradoxes and the kind of set theory he developed in PM in Chihara, 1973 #48, Chapter 1.
it will be seen that neither of these two ways of answering Quine presupposes the Fregean Assumption. The later response to Quine’s challenge makes use of what I call “constructibility quantifiers”, which not only is a “tool” in my anti-Platonism arguments, but also is meant to shed light on the nature of real mathematics.

1. A Nominalistic Version of a Predicative Set Theory

In an early work, I put forward a version of set theory that was intended to be a response to Quine’s claim that nominalists are forced to develop a view of the world that substitutes for current scientific theories a version of science that is completely lacking in mathematics (except for some trivial versions of elementary arithmetic). What I aimed to show was that nominalists could develop a version of set theory that is both “nominalistic” and “predicative”.

What does it mean to say that a version of set theory is nominalistic and predicative? Let me give a quick rough-and-ready explication of these terms. A nominalist is someone who does not believe in or accept the existence of abstract entities. A theory or philosophical view is nominalistic if it does not assume or presuppose the existence of abstract entities.

Now some philosophers may find the above explanation of ‘nominalist’ to be unhelpful because of the vagueness of the term ‘abstract entities’. I shall not attempt here to define what an abstract entity is, because in the philosophy of mathematics one generally knows pretty clearly when a philosophical view is not nominalistic. It is easy to see that a theory asserting the existence of mathematical objects such as numbers, sets, functions, matrices, and spaces, is not nominalistic. Thus, I shall follow Field (discussed in Chapter 3) in taking the
term ‘nominalistic’ to be intuitively understood among philosophers of mathematics.\footnote{For a fuller explanation of what nominalism is, see my \cite{Chihara, 2005 #477}).}{\footnote{What I am calling predicative set theory should be distinguished from what such researchers as Sol Feferman and Geoffrey Hellman call predicative set theory (or predicative mathematics) in their article \cite{Feferman, 1995 #603}: “Predicative mathematics in the sense originating with Poincare and Weyl begins by taking the natural number system for granted, proceeding immediately to real analysis and related fields” (p. 1). For a more recent discussion of predicativity and an overall picture of what has been done on the topic in recent years, see \cite{Feferman, 2005 #501}.}}

What about the term “predicative set theory”—at least as I am using the phrase in this chapter.\footnote{See \cite{Chihara, 1973 #48}, Chapter 1, especially Sections 2 and 4.}{\footnote{See \cite{Chihara, 1973 #48}, Chapter 1, Section 2, for a presentation of the various formulations of the principle that are to be found in Russell’s writings, as well as a commentary of these different formulations. See also \cite{Gödel, 1964 #155}, p. 18, and \cite{Fraenkel, 1958 #513}, p. 175.}} A predicative set theory is one that satisfies (or is consistent with) the Russell-Whitehead “vicious-circle principle”—a principle that the authors of PM came up with as a means of avoiding the various paradoxes and antinomies that plagued early researchers of the foundations of mathematics and logic during the late 19\textsuperscript{th} and early 20\textsuperscript{th} centuries.\footnote{See \cite{Chihara, 1973 #48}, p. 3) for a discussion of Russell’s statement of this principle and for a citation of the reference.}{\footnote{See \cite{Gödel, 1964 #155}, p. 18, and \cite{Fraenkel, 1958 #513}, p. 175.}} The principle was supposed to prevent or block the paradoxes from ever arising in the logical system or set theories that incorporated it. Unfortunately, what Russell meant by ‘the vicious-circle principle’, however, turned out to be quite vague, especially since the principle was expressed in a number of different ways—ways that do not seem at all to be equivalent.\footnote{See \cite{Chihara, 1973 #48}, p. 3) for a discussion of Russell’s statement of this principle and for a citation of the reference.}{\footnote{See \cite{Gödel, 1964 #155}, p. 18, and \cite{Fraenkel, 1958 #513}, p. 175.}} Here are just two of the many statements of the principle that were expressed by Russell:

Whatever involves all of a collection must not be one of the collection.\footnote{See \cite{Chihara, 1973 #48}, p. 3) for a discussion of Russell’s statement of this principle and for a citation of the reference.}{\footnote{See \cite{Gödel, 1964 #155}, p. 18, and \cite{Fraenkel, 1958 #513}, p. 175.}}
Whatever contains an apparent variable must not be a possible value of that variable.\textsuperscript{130}

Needless to say, most scholars found it difficult to say with confidence precisely what the principle asserts. But Hao Wang developed a set theory, which, he was confident, would conform to what the principle required: this is Wang’s set theory $\Sigma$, which is a kind of union of formal systems $\Sigma_\alpha$, where $\alpha$ is any “constructive ordinal”. (Wang described this system as a “recipe for making formal systems wherever an ordinal $\alpha$ is given”). Now the notion of a “constructive ordinal” raises some problems of explanation, which I wished to avoid; but since omega is the first transfinite ordinal number, I opted to work with $\Sigma_\omega$ (just as Russell and Whitehead kept the system of orders in PM to omega levels): it was clear that that $\Sigma_\omega$ was one of the formal set theories of $\Sigma$.\textsuperscript{131}

As I stated in Section 2 of Chapter 5 of \cite{Chihara, 2004 #468}, my own philosophical study of predicative mathematics was brought about by my search for a way of responding to the version of Quine’s Indispensability Argument that I called, “Quine’s Challenge to the Nominalist”. I regarded Quine as, in effect, arguing as follows:

Let’s see you nominalists accommodate science without committing your self to mathematical objects. Let’s see you produce a system of

\textsuperscript{130}See (\cite{Chihara, 1973 #48}, p. 4) for a discussion of this version of the principle and for a citation of the reference.

\textsuperscript{131} The reader can find the details of Sigma Alpha given in \cite{Wang, 1962 #532} and \cite{Wang, 1962 #604}.
mathematics which would be adequate for the needs of the natural scientist, but which would not require its quantifiers to range over abstract objects.

A number of features of Hao Wang's predicative set theory $\Sigma_\omega^{132}$ suggested to me that a predicative set theory might be interpretable in such a way that one could produce a system that was both nominalistically acceptable and also strong enough to yield a system adequate for the needs of the empirical sciences.

Although $\Sigma_\omega$ is a formal theory, I shall here discuss the set theory informally, giving a sort of “Big Picture” view of the theory. The sets of Wang’s $\Sigma_\omega$ were sorted into different levels, and at each level, each set of that level was definable by an open-sentence in such a way that the defining condition could not violate the vicious-circle principle. Wang followed a procedure suggested by certain ideas puts forward in PM: at level n, the defining condition of a set would be given by an open-sentence whose bound variables ranged only over things of level n-1, thus guaranteeing that the condition did not presuppose the existence of some set that had been defined by the very condition itself—a condition that was not in violation of one version of the vicious-circle principle.

Wang’s procedure suggested to me that we didn’t really need the sets at all: we could make do in a predicative set theory by using the defining open-sentence instead. For example when, in the set theory, something $x$ was said to be a member of a set defined by the open-sentence $\phi$, the nominalist could say instead that $\phi$ was true of $x$ or was satisfied by $x$. More generally, in an open-

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$^{132}$ Given in {Wang, 1962 #532}. See {Chihara, 1973 #48}, Chapter V, Section 1, for a description of this set theory.
sentences version of set theory in which open-sentences played the role of sets, the satisfaction relation would replace the membership relation, resulting in a kind of theory of the constructibility of open-sentences instead of a standard set theory.\textsuperscript{133}

My aim in setting out this constructivistic interpretation of predicative mathematics was to provide the nominalist with a kind of mathematics far more versatile than the “trivial portions such as very elementary arithmetic” that Quine had allowed the nominalist. Since a particular formal version of the predicative system $\Sigma_\omega$, nominalistically interpreted in the above way, had been produced,\textsuperscript{134} I argued that the nominalist had a reasonable response to Quine’s argument.

Why I thought something like what I was proposing might be adequate for the needs of the nominalist attempting to develop a version of mathematics that would cast doubt on Quine’s challenge is illustrated by the following points in my reasoning. Where predicative analysis most strikingly differs from standard classical analysis is in the properties of the respective real number systems. As became apparent from Hermann Weyl’s work regarding the need for the axiom of reducibility in PM, the least upper bound theorem\textsuperscript{135} is not obtainable in predicative analysis.\textsuperscript{136} However, one can prove, in $\Sigma_\omega$, that for every bounded set $x$ of real numbers in the universe of the set theory, there is a real number in this universe that is the least upper bound of $x$. Furthermore,

\textsuperscript{133} Although the sentences in this paragraph give the basic idea of my “nominalistic interpretation of $\Sigma_\omega$”, the details of the interpretation were somewhat more complicated. (These details need not detain us here, since I came to feel that this way of responding to Quine was not satisfactory).

\textsuperscript{134} See [Chihara, 1975 #601], where a nominalistic version of $\Sigma_\omega$ is formalized and presented as a response to Quine’s Indispensability Argument.

\textsuperscript{135} This is the theorem that asserts: Every bounded set of real numbers has a least upper bound.

\textsuperscript{136} See [Weyl, 1987 #231], pp. 47-8.
although there are only denumerably many sets in the universe of $\Sigma_\omega$, one can develop a theory of “relative indenumerability”, in which a set will be classified as indenumerable in level n if there is no function of level n that enumerates it. Using this notion of relative indenumerability, a nontrivial version of measure theory can be developed, since, as Wang put it: “the notion of relative indenumerability is sufficient to provide us with sets of nonzero measure for measures defined on each given level” ({Wang, 1962 #532}, p, 569). As for the real numbers themselves, Wang notes that:

It is quite easy to prove that all real numbers which can be obtained by ordinary procedures of classical analysis can be obtained in the system Sigma omega (indeed, in a partial system Sigma five).\textsuperscript{137}

Of course, such ideas are by no means sufficient to show that a predicative system of the sort that can be developed in $\Sigma_\omega$ would indeed be theoretically adequate for science. And actually showing that such a predicative system of mathematics would indeed be adequate for the needs of science is by no means a trivial task.\textsuperscript{138} Still, I felt that the burden of proof rested with the defenders of the Indispensability Argument and my predicative responses were directed at showing that this burden had not been met.

At this point, let us track what these developments suggested to me. Remember, the force of the Quinean argument for the existence of mathematical entities rested upon the premise that the nominalist would have to develop his scientific theories unaided by any significant mathematics. Quine

\textsuperscript{137} {Wang, 1962 #532}, p. 574. The quotation given above differs slightly from Wang’s printed words in so far as I have replaced his symbolic names of the set theories in question with the spelled out English versions.

\textsuperscript{138} Cf. {Chihara, 1975 #601}. 
believed that the nominalist would have to “accommodate his natural science unaided by mathematics”, because he believed that, “except for some trivial portions such as very elementary arithmetic, [mathematics] is irredeemably committed to quantification over abstract objects.” And what I was arguing was that the nominalist would have much more at his disposal than just “very elementary arithmetic”—he could make use of a version of predicative mathematics, as is developed in $\Sigma_\omega$ —a system of set theory which is more powerful and flexible than just the very elementary arithmetic that Quine was allowing the nominalist.

As indicated above, for example, the version of analysis developable in $\Sigma_\omega$ can serve as the framework for the construction of a non-trivial version of measure theory (which is not something one could do in elementary arithmetic). Furthermore, the version of analysis obtainable in $\Sigma_\omega$ has other advantages over the standard impredicative classical analysis. For example, it can be proved to be consistent and it is clearly simpler than impredicative set theories such as ZF.\footnote{See \{Wang, 1962 #532\}, p. 584.} All of the work described above refuted Quine’s claim that the mathematical tools that the nominalist could use to formulate and develop his science would be restricted to only very elementary arithmetic. Of course, I had not shown in my book or in later articles that the nominalist could, in fact, develop an adequate version of science, even if she could make use of a predicative version of mathematics—something that I came to feel was a significant problem.\footnote{See, for example, \{Chihara, 1975 #601\}.}

2. The Impredicative Approach
Although the predicative approach I took in my 1973 book provided the nominalist with an initial response to Quine’s “challenge to the nominalist”,¹⁴¹ I decided a few years after its publication to explore the possibility of devising a response that would avoid the thorny problem that the predicative approach ran into of showing that a reasonable version of science could be developed using only predicative mathematics. My explorations led me to develop what I called the “Constructibility Theory of Types” (or “Constructibility Theory” for short). This theory, first presented in {Chihara, 1990 #34}, differed in many important respects from the predicative system described in Section 1. Although both systems made use of a modal operator (“it is possible that”), the later version required a much stronger operator in the sense that any thing possible in the earlier version would be possible in the later version, but not vice versa. This change would allow one to develop a constructibility version of simple type theory. Such a change led John Burgess and Gideon Rosen to write, in their survey of the various nominalistic reconstructions of mathematics:

[A]fter the publication of his first book [Chihara] became convinced that the restrictions of predicativism are unnecessary. He thereby became, for purposes of this survey, another philosopher. ([Burgess, 1997 #227], p. 197).

The quoted passage, however, is in one small respect a bit misleading. The quote is misleading in the way the following entry that appeared in The Philosophical Lexicon¹⁴² is misleading:

chihara-kiri, n. The death of aleph-nought cuts.

¹⁴¹ See [Chihara, 1975 #601].

¹⁴² The Philosophical Lexicon was edited by Daniel Dennett and Karel Lambert and first circulated in mimeograph form. The quotation is from the Seventh Edition, copyrighted in 1978.
It suggests that the nominalistic reconstruction of mathematics of the earlier book [Chihara, 1973 #48] involved accepting the “restrictions of predicativism”, thereby restricting the nominalist’s mathematics to only countably many (or aleph-nought) totalities. Now what I wished to show in that early work was that nominalists could respond to Quine’s challenge by making use of a kind of mathematics far more scientifically useful and flexible than the “very elementary arithmetic” that Quine had allowed the nominalist. In other words, I was arguing that the nominalist had more mathematical resources available than very elementary arithmetic which Quine was allowing and that it was by no means obvious that such resources would be inadequate for having an acceptable version of science. I was arguing, in still other words, that the nominalistic scientist could at least make use of a kind of predicative mathematics in her scientific work and that that mathematics might be sufficiently strong to have an adequate science. I certainly had not put forward any arguments or theoretical considerations to conclude that the nominalist should be restricted to such a mathematics. It seemed to me even then that alternative nominalistic reconstructions more powerful than the predicative mathematics were quite feasible.

The Constructibility Theory was developed in a very different philosophical environment than the one I faced when I put forward my predicative version of nominalistic mathematics. The earlier predicative version was published in 1973 to meet the Quinean challenge to the nominalist. When I began working on my new book, there were many new versions of the Indispensability Argument that needed to be discussed. By that time, Quine was claiming that there actually was “evidence” supporting the postulation of
mathematical objects—indeed evidence of a scientific nature.\textsuperscript{143} Hilary Putnam was also supporting what one might regard as a kind of Indispensability Argument that made use of his idea of “quasi-empirical” justifications.\textsuperscript{144} In addition, Putnam had published his monograph Philosophy of Logic,\textsuperscript{145} which defended Quine’s Indispensability Argument by putting forth a clearer version of the argument and defended it against various objections.

Penelope Maddy, too, was supporting a version of the Indispensability Argument in connection with her own radical version of mathematical realism, according to which we humans are able to see, hear, taste, smell, and feel sets of physical objects.\textsuperscript{146} Both Michael Resnik and Stewart Shapiro were defending Mathematical Structuralism and advocating a belief in the existence of mathematical objects they called “structures” or “patterns”. Hartry Field had published his Science Without Numbers, and Burgess had published a paper \{Burgess, 1983 #292\} directed at undermining my \{Chihara, 1973 #48\}. Needless to say, the reconstruction of mathematics I would need to produce in this new environment encountered more difficult challenges than the earlier one I faced in devising the predicative reconstruction. In the meantime, I had begun to devise a way of interpreting the formalism of simple type theory without presupposing that it have Platonic domains—that is, I was devising an interpretation of the theory that was nominalistically acceptable. In a 1984 publication, I put forward a nominalistic version of simple type theory, which

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\textsuperscript{143} See \{Chihara, 2004 #468\}, pp. 115-6, for a discussion of Quine’s position on this matter.

\textsuperscript{144} In \{Putnam, 1979 #313\}, pp. 64-9.

\textsuperscript{145} \{Putnam, 1971 #390\}.

\textsuperscript{146} See her \{Maddy, 1980 #167\} and also \{Maddy, 1990 #207\}.
would not assert the existence of sets, but instead would only assert that it is possible to construct various kinds of open-sentence tokens. Thus, where an existential predicate quantifier, say ‘(EF)’, occurs in a formal sentence of the standard type theory, one would instead find, in this nominalistic version, what I called a “constructive-quantifier”, which should be understood to say: “It is possible to construct an F such that . . .” ([Chihara, 1984 #125], p. 259).

How does my Constructibility Theory differ from my earlier nominalistic version of predicative mathematics? Clearly, the Constructibility Theory provides a more powerful version of classical analysis than can be obtained in predicative mathematics. However, I wasn’t becoming another (or a different) philosopher in trying to meet the new challenges, but instead was the same philosopher making use of more mathematical resources than I had earlier, in order to overcome the new challenges that had arisen.

What are the main differences between the nominalistic reconstruction of the predicative mathematics of my earlier work [Chihara, 1973 #48] and the reconstruction of the impredicative mathematics of my later works [Chihara, 1990 #34] and [Chihara, 2004 #468]? In both cases, the theorems of the reconstructive systems involved assertions of the constructibility of open sentences. However, in the former case, but not in the latter, one knows the form of the open-sentence, as well as the specific language of the open-sentence that is asserted to be constructible. In asserting a theorem of the predicative system, one can always specify an effective procedure for generating a sequence of open-sentences, one of which would have to be the open-sentence asserted to be constructible. Not so, of course, for the open-sentence asserted to be constructible in the latter impredicative case. Such
differences point to the advantages of more clarity, specificity, and certainty that the predicative system possesses over the impredicative system. However, the mathematical system generated by the latter system has the advantages of increased simplicity and greater mathematical power not possessed by the mathematics of the former. My position was that the nominalist could use either one or both of these reconstructions of mathematics in order to develop a nominalistic version of science.

I have been giving a Big Picture account of my Constructibility mathematics because the details of a formalized version were presented earlier in Chapter 9 of {Chihara, 2004 #468}; there, I also showed how the Constructibility version of number theory could applied to the physical world in a way that mimicked the way Frege showed how his version of number theory could be used to produce physical world applications.

3. A Widespread Misunderstanding

Let us now consider in more detail the kind of development of mathematics that I was setting forth, since some philosophers have seriously misunderstood my goals in producing the Constructibility Theory. Consider, in particular, how Mark Balaguer supported his claim that fictionalism is the best version of anti-platonism: he argued that “fictionalism interprets our mathematical theories in a very standard, straightforward, face-value way,” whereas other versions of anti-realism “advocate controversial, non-standard, non-face-value interpretations of mathematics that seem to fly in the face of actual mathematical practice.”147 Balaguer was even more explicit in his

147 {Balaguer, 1998 #425}, p. 102, italics mine.
objection to my version of anti-platonism, suggesting that the above objection applies to all non-fictionalist versions of anti-realism:

This is simply because all such views involve non-standard, non-face-value interpretations of mathematical theory. Chihara and Wittgenstein, for example, do not think that sentences like ‘There exists a prime number between 2 and 4’ express truths about numbers. Chihara takes such sentences as making assertions about what open-sentence tokens it is possible to construct, and Wittgenstein seems to think that such sentences express rules. These are surely non-standard interpretations. ([Balaguer, 1998 #425], p. 103).

Here, Balaguer is basing his objection on a complete misunderstanding of the Constructibility Theory.¹⁴⁸ He takes me to be maintaining that the Constructibility Theory provides an open-sentence account (or analysis) of real mathematical statements and theories. That is, he understands me to be maintaining that the assertions or theorems of real mathematics are actually assertions of the constructibility of open-sentences! Thus, Balaguer believed that the Constructibility Theory set forth what Burgess calls a hermeneutical account of the assertions of real mathematics.

Now I never claimed, nor do I hold, that real mathematics—the mathematics that contemporary working mathematicians actually use and develop—is a theory about the constructibility of open-sentences. Had Balaguer really studied my book [Chihara, 1990 #34] (instead, evidently, of merely skimming it), he would have seen that I was not putting forth any such view of mathematics. He would have understood that my Constructibility Theory was not developed to provide an analysis of the meaning of such English statements as ‘There exists a prime number between 2 and 4’. Thus, my first characterization of the Constructibility Theory is given with the following words:

¹⁴⁸ I should note that, although I do not respond to Balaguer’s criticisms of Wittgenstein (leaving such a task to other philosophers), I do consider them to be based upon a very distorted reading of Wittgenstein’s writings.
[M]y own approach to these problems [having to do with mathematical existence] is similar, at least in one respect, to what Heyting advocated. If we are puzzled about certain aspects of classical mathematics, why not construct another kind of mathematics that will avoid those features of the original system that gave rise to the puzzles? A study of these alternative mathematical theories will give us a new perspective from which to view classical mathematics, which could prove to be extremely enlightening. This strategy may not give us a direct solution to the problem described [in Chapter 1 of {Chihara, 1990 #34}], but it may enable us to gain insights into the role classical mathematics plays when it is applied. ( {Chihara, 1990 #34}, p. 23, italics not in the original).

It should have been clear to Balaguer, from this passage, that my aim was not to supply an interpretive analysis of real classical mathematics, but rather was to provide us philosophers with a “another kind of mathematics” which would give us a “new perspective from which to view” classical mathematics.

In other places, I give an even more explicit rejection of the kind of interpretation of my Constructibility Theory that Balaguer is assuming. For example, I write:

The Constructibility Theory is not a theory about how to analyse actual mathematics. I have not been claiming that the existential quantifier in ordinary mathematics should be treated as a constructibility quantifier. ( {Chihara, 1990 #34}, p. 174).

In another place, I write:

In developing the constructibility version of cardinality theory and analysis, I did not claim to be giving an analysis of what ordinary mathematical sentences mean. It is not crucial, for my purposes, that one determine the literal meaning of mathematical sentences. ( {Chihara, 1990 #34}, p. 250).

In short, Balaguer’s complaint is that my approach involves a non face-value interpretation of standard mathematics. However, my response was not trying to give a hermeneutic interpretation of standard mathematics, but only
to give an alternative interpretation that would provide a new perspective from which to view classical mathematics.

What then were the purposes for which the Constructibility Theory was developed? I have already indicated, in the introduction to this chapter, what I wanted the theory to achieve. Keeping in mind these goals, one can think of the theory as a kind of nominalistic model (a kind of “theoretical instrument”) to facilitate carrying out and assessing logical, mathematical and philosophical reasoning. Thus, if an adequate model of mathematical reasoning can be constructed which does not “quantify over” or presuppose the existence of mathematical objects, then one would have reasons for being skeptical of the claim that mathematical objects must be presupposed in order to explain why mathematics has proved to be so useful for scientific reasoning. Thus, to argue that such a model is not an accurate “interpretation” of actual mathematics would be to miss completely the point of the Constructibility Theory.149

4. A General Description of the Constructibility Theory

The Constructibility Theory (an overall account of which follows) was put forward in 1990 primarily for two reasons:

(a) to respond to Quine’s “challenge to the nominalist”;

and

149 The nature of Balaguer’s misunderstanding about the Constructibility Theory can better be understood by studying my response to the Burgess objection of [Burgess, 1983 #292] that will be discussed in the next chapter.
(b) to facilitate, from the nominalistic point of view, the production of an analysis and explanation of the role mathematics plays in everyday life and in science.

Regarding (b), the Constructibility Theory was used in the explanation of how a nominalist can apply ordinary finite cardinality theory in the way we all do without assuming that there are mathematical objects ([Chihara, 2004 #468], Chapter 7, Sections 2, 3). It was also used to explain how the nominalist can have a “structural account of mathematics” without assuming the existence of structures ([Chihara, 2004 #468], Chapter 8).

Can one have a theory of Xs even though there do not exist any Xs? How can one theorize about Xs if there are no Xs? In general, of course, there is no problem theorizing about Xs, even though there are no Xs, since one can theorize about what properties and features Xs would have if there were such things. For example, one would have no problems theorizing about, say buildings on the moon, even though there are no such buildings in existence. One can certainly theorize about what buildings on the moon would have to be like to be useful to humans living on the moon. So initially, there does not seem to be any inconsistencies involved in a nominalist having a theory involving structures. Furthermore, it may appear that a researcher would not require the actual existence of structures in order to theorize about them, since a sufficiently detailed description of a structure—a description specifying what the objects of the structure are and how they are related to one another—would allow the researcher to theorize about the structure just as effectively as having the actual structure would.

However, I needed to provide a detailed account of structures that would be adequate for explaining such things as how one could reason about the
structures needed in, say, Abraham Robinson’s development of nonstandard analysis, and that task is more formidable than simply showing, in a general way, how a nominalist could theorize about structures. For what I wanted to accomplish, I needed to develop a general framework for talking about structures, which would be sufficiently powerful for the kind of model theory Robinson was presupposing when he produced nonstandard analysis. What I needed was something that could play the role that set theory plays in Platonic model theory. The Constructibility Theory was devised to address this problem.

I first presented the Constructibility Theory in 1990 as a stand-alone theory. In 2004, I developed finite cardinality theory within the framework of a formalized axiomatic version of the Constructibility Theory, which followed closely Frege’s development of finite cardinality theory in his major works. This finite cardinality theory clearly illustrated how a version of arithmetic could be developed without “quantifying over” numbers, sets, extensions of concepts, or any of the other Platonic entities that had been used to develop formalized versions of number theory.

In order to show in some detail why I do not consider the Constructibility Theory to be a direct competitor of real mathematics, I shall now present the basic elements of the theory in the course of putting forward a Constructibility way of interpreting a formal version of a standard set theory. Thus, in the following exposition, I shall depart somewhat from the way I first discussed the theory in 1990. The idea is to take the formalism of a well-known set theory—Simple Type Theory (henceforth: STT)—and then fashion a constructibility interpretation of the formalism in a way that is consistent with my nominalistic

\footnote{150 In \{Chihara, 1990 \#34\}.}

\footnote{151 In \{Chihara, 2004 \#468\}, Chapter 7.}
position. I shall then show how a nominalist could make use of such an interpretation to formulate her scientific theories and to apply those theories, while in effect using the mathematics of a standard set theory, thus rebutting Quine’s “challenge to the nominalist”. Ironically, I shall make use of basically Quine’s own formal version of set theory to undermine his Platonic argument.

I shall begin by laying out, in a “Big Picture” way, the principal ideas underlying the Constructibility Theory. Set theory is generally applied to the physical world by appealing to sets of physical objects, but in most such cases, one can replace such appeals by making use instead of open-sentences. The set of dogs is the set of all x such that x is a dog. Notice that ‘x is a dog’ is an open-sentence that can do much of what the Platonist wants the set of dogs to do. More generally, open-sentences do much of the work that sets do in the applications. What more is needed? Why do we need the set?

Wherever in STT a set of a certain sort is asserted to exist, in the Constructibility Theory, an open-sentence of a corresponding sort is asserted to be constructible. Thus, whereas the main logical operators of STT are the existential quantifier (∃x) and the universal quantifier (∀x), the main logical operators of the Constructibility Theory are the “constructibility quantifiers” (Cφ) and (Aφ). The sentence ‘(Cφ)ψφ’ asserts the constructibility of an open-sentence φ such that ψφ and ‘(Aφ)ψφ’ asserts that every open-sentence φ that it is possible to construct is such that ψφ. And just as ‘(∃x)Fx’ is equivalent to ‘¬(x)-Fx’, and ‘(x)Fx’ is equivalent to ‘¬(∃x)-Fx’, we have ‘(Cφ)ψφ’ is equivalent to ‘¬(Aφ)-ψφ’ and ‘(Aφ)ψφ’ is equivalent to ‘¬(Cφ)-ψφ’.

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152 The details of the theory can be discovered in [Chihara, 1990 #34], Chapters 2 – 5, and in [Chihara, 2004 #468], Chapters 7 & 8.
Whereas the main non-logical constant of STT is the binary membership symbol $\in$, the main non-logical constant of the Constructibility Theory is the binary satisfaction symbol $S^2$. Also, whereas the basic relationship among the entities of STT under the standard interpretation is membership ("$x \in y$"), the basic relationship among the entities of the Constructibility Theory under the standard interpretation is satisfaction ("$\phi S^2 \psi$").

In STT, the sets are ordered into a hierarchy of types: there are sets of objects (these are sets of type 1); sets of sets of type 1 (these are sets of type 2); sets of sets of type 2 (these are sets of type 3); etc. In the Constructibility Theory, the open-sentences asserted to be constructible are ordered into a hierarchy of levels: there are open-sentences satisfiable by objects (these are open-sentences of level 1); open-sentences satisfiable by open-sentences of level 1 (these are open-sentences of level 2); open-sentences satisfiable by open-sentences of level 2 (these are open-sentences of level 3); etc.

It needs to be emphasized that what are said to be constructible by means of the constructibility quantifiers are open-sentence tokens as opposed to open-sentence types. In the actual world, typically, an open-sentence token consists of particular marks on surfaces, such as sheets of paper, which exist at a particular place and at a particular time. To say that an open-sentence of a certain sort is constructible is not to imply that any such open-sentence actually exists—it only asserts what could exist. Constructibility quantifiers do not carry ontological commitments in the way the quantifiers of standard extensional logic do.

The constructibility quantifiers were introduced in {Chihara, 1990 #34} to be used in the constructibility assertions of the Constructibility Theory, and
hence these quantifiers can be regarded as a sort of linguistic device used for writing or talking about the constructibility of open-sentences. The open-sentences that are said to be constructible by the Constructibility Theory, however, should not be assumed to contain occurrences of constructibility quantifiers: the open-sentences might be expressed in English, without any quantifiers or logical constants from a formal language.

Finally, something needs to be said about the notion of possibility involved in the phrase ‘it is possible to construct’. Of course, many notions of possibility have been researched and characterized by modal logicians. The kind of possibility that concerns the Constructibility Theory (and hence the constructibility quantifiers of the interpretation in question) is what is called “conceptual” or “broadly logical” possibility. As a rough guide, Graeme Forbes suggests that ‘it is possible that P’ be taken to mean: “There are ways things might have gone, no matter how improbable that may be, as a result of which it would have come about that P”.\(^{153}\)

Now in setting out to show that the above constructibility interpretation of the formalism of STT is useful and intuitively acceptable, I claimed, on the basis of modal reasoning, that the following two conditions are true:

(i) The axioms of the theory come out intuitively true when the formal sentences are interpreted in the above way;

and

(ii) the inference rules of the derivational system used in the theory preserves intuitive truth.

\(^{153}\) See Chapter 1, Section 1, for examples and discussion of this notion of possibility.
From (i) and (ii), it can be easily concluded that all the theorems of the theory are intuitively true.

The argument for the satisfaction of condition (i) was given in Section 2, Chapter 4, of {Chihara, 1990 #34}. That condition (ii) is fulfilled can be shown to be the case by relatively obvious and straightforward modal reasoning. I shall reinvestigate, later in this work, the question of what is meant by “the truth of these axioms”.

The Constructibility Theory was formulated as an axiomatized theory of many-sorted first-order logic. That it provides the nominalist with an obvious response to “Quine’s Challenge to the Nominalist”, can be seen from the fact that a version of classical analysis can be developed within STT—one that is widely acknowledged by mathematical logicians as being powerful enough for essentially all scientific purposes that working scientists require today. Thus, it was argued, the nominalist is not condemned to work with a completely non-mathematical version of science, as was believed by Quine. I will soon show that a nominalist can make use of a standard version of set theory in formulating scientific theories and even interpret the set theory in a standard way when applying the set theory.

Now I am not suggesting that the practicing scientist could use the Constructibility Theory in her scientific work just as easily and comfortably as she uses real mathematics. Actually, I do not believe that the practicing scientist could use, in her scientific work, any formalized version of mathematics (such as ZF, PM, or PA) just as easily and comfortably as she uses real
mathematics or real arithmetic. The idea is not to replace real mathematics with the Constructibility Theory, but rather to use the theory as a model to understand, analyze, and explain the role mathematics plays when it is used in everyday life and in science. Thus, the Constructibility Theory should be regarded as a model designed to aid us to understand the logical workings of real mathematics and to appreciate what ontological presuppositions of real mathematics are and are not required.

In the following, I shall make use of the basic elements of my Constructibility Theory in order to devise a constructibility interpretation of a {Quine, 1963 #368}. However, in presenting the basic features of this formal version of set theory, I shall not follow Quine’s way of presenting his formal theories nor his tendency to ignore model theoretic matters.

5. Quine’s Modern Theory of Types

Some readers may find the above section title misleading because the formal theory I shall be discussing, although based upon Quine’s version, differs significantly from the version of simple type theory specified in his Set Theory and Its Logic. The version I shall be presenting below is a purely formal syntactic theory, whereas Quine’s “modern theory of types” is a fully interpreted set theory. I trust that, with the above words of warning, what I present will not be confused with the set theory Quine presented in his book.

STT was devised as a simplified version of the ramified type-theoretical version of set theory implicit in PM. Quine attributes the first presentation of

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154 See [Quine, 1963 #368], Section 36, for a general discussion of how this set theory differs from the set theory developed in PM.
simple type theory to Tarski and Gödel. This section will be concerned with a formal version of STT that I shall call “Q-STT”. I shall first present the language of Q-STT—a (relatively) standard many-sorted variation on the first-order language found in Mates’s Elementary Logic, with the following small additions: in this language, there will be Arabic numeral superscripts on the individual symbols (variables and individual constants) and ordered triple superscripts on the predicates of the language. The superscripts on the individual symbols give the level of the symbol. The superscripts on the binary predicates will consist of ordered triples of Arabic numerals, where the first member of the ordered triple gives the arity of the predicate, the second gives the level of the first member in the relation, and the third gives the level of the second member in the relation. (For simplicity, I have limited the predicates of the formal theory to only binary predicates, since these are the only predicates needed in the formal theory to be discussed).

The vocabulary of the theory Q-STT will contain just one kind of nonlogical constant, the binary predicates

\[ \text{E}^{<2, n, n+1}> \]

understood to be the membership relations of the various levels. Besides the standard logical constants of Mates’s language of first-order logic, there is only one additional kind of logical constant of the language of Q-STT: the identity predicates,

\[ \text{I}^{<2, n, n}> \]

\[ ^{155} \text{P. 261.} \]
where \( n = 0, 1, 2, \ldots \).

For ease of expression and discussion, I will use a kind of short-hand to refer to the formal sentences of the theory in the more natural way Quine writes the sentences of his version of simple type theory in {Quine, 1963 #368}. (I do not, however, use the logical connectives of PM as Quine does.)

The reader should keep in mind that Q-STT differs importantly from the set theory Quine specifies as “the modern theory of types”. Remember, in particular, that for Quine, mathematical theories are like theories of biology and physics: the theorems or assertions of the theory are supposed to be propositions about the actual world—and hence are what I called “ontologically weighty” propositions. Quine’s modern theory of types is thus not like my Q-STT, which is an uninterpreted theory whose assertions (or theorems) are not held to be either true or false. Instead, Quine’s modern theory of types was put forward as a fully interpreted theory, “on a par with” the theories of astronomy and genetics to which it could be applied, and whose sentences can be said to be “true of the world”.

I shall now specify (using the short-hand mentioned above) the type-theoretical rules that lay out the formulas and sentences of the language of Q-STT. The type restrictions of the theory will be reflected in the rules for determining what counts as an atomic formula: they “are built up by using the individual constant ‘\( \in \)’ and the variables of consecutive types, in the fashion of ‘\( x^n \in y^{n+1} \)’. The rest of the formulas are built from these atomic ones by quantification and truth functions” {Quine, 1963 #368}, p. 259).

There are two principal axioms of the theory:

\[ \text{See fn 6 of Chapter 1.} \]
A. The axiom schema of comprehension:

$$(\exists y^{n+1})(x^n)(x^n \in y^{n+1} \leftrightarrow --- x^n---)$$

where ‘--- $x^n$---’ is any formula of the theory that expresses a condition on $x^n$. The above schema should be so understood that any universal closure of the above displayed formula is to be an axiom.

B. The axiom of extensionality:

$$(z^n+1)(y^n+1)((x^n)(x^n \in y^n+1 \leftrightarrow x^n \in z^n+1) \rightarrow y^n+1 = z^n+1)$$

In addition to the above two axioms, Quine lists what he calls an “annex” of two additional principles that, at times, will be used: choice and infinity. However, the annex principles are treated differently from the two principal axioms. For example, here is how Quine explains his treatment of the principle of choice:

Intuitively the principle seems reasonable, even obvious. The trouble is just that it is not as simple and elementary a statement as could be desired as a starting point, and no way is known of deducing it strictly from anything simpler. For this reason it is common practice to distinguish between results that depend on the axiom of choice and those that do not. Accordingly I shall not adopt the principle as axiom or axiom schema, but shall merely use it as a premiss where needed.$^{157}$

$^{157}$ ([Quine, 1963 #368], pp. 217-8). Russell and Whitehead treated choice in a similar way in PM.
Note that Quine was either unaware of, or chose to ignore, the practice of set theorists to justify axioms of set theory in terms of the model of set theory that is called the “iterative conception of set”—indeed, Shoenfield gives such a justification of the Axiom of Choice (in {Shoenfield, 1967 #375}, p. 253).

Regarding the Axiom of Infinity (which asserts the existence of infinitely many entities of the lowest level), I prefer to regard it as a hypothesis (calling it “The Hypothesis of Infinity”) that is true for certain kinds of interpretations of the formalism.\footnote{158}

6. Towards a Constructibility Interpretation of Q-STT

A rough sketch of how the Constructibility Theory can yield an interpretation of Q-STT was, in effect, explained earlier in Section 3 of this chapter. The reader will recall that that the fundamental idea of the Constructibility interpretation is to understand the existential quantifier of Q-STT as expressing what the constructibility quantifier does: thus, ‘(\exists x)F^1x’ is to be understood, not as asserting that there is an object x such that x is an F^1, but rather as asserting that it is possible to construct an x such that x satisfies F^1.

To clarify the above rough explanation and to forestall misinterpretations of what is meant by ‘it is possible to construct’, I shall provide, in what follows, an explication or characterization of the constructibility quantifier in terms of the well-known system of modal logic known as S5.

\footnote{158}{See {Chihara, 2004 #468}, pp. 226-7.}
7. The S5 Characterization of the Constructibility Quantifier

The constructibility quantifier

$$(CF)(\ldots F \ldots)$$

can be characterized as follows:

$$[C^\#] (CF)(\ldots F \ldots) \text{ iff } \Diamond(\exists F)(KF & \ldots F \ldots)$$

where '◊' is the modal operator 'it is possible that', '(\exists F)' is the existential quantifier 'there exists an F such that', and 'KF' holds if and only if 'F' is an open-sentence token that has been constructed and F satisfies K.

$[C^\#]$ would allow one to “translate” any constructibility statement into an assertion of possibility in standard S5 modal logic. Thus, the constructibility Theory could be formalized in a more or less standard modal logical language. The above equivalence would also provide a sort of modal logical characterization of constructibility assertions. For this reason, $[C^\#]$ will henceforth be called the ‘S5 characterization’ of the constructibility quantifier on the left.

As I mentioned earlier (and in several places in my published writings), the sort of possibility I had in mind for the constructibility quantifiers is what is characterized as “the broadly logical” concept of possibility—just the kind of possibility that is widely thought to be formalized by S5 versions of modal
logic. Thus, it seems appropriate to provide the above characterization within the framework of an S5 version of quantificational modal logic.

Why should one accept the above S5 characterization of (CF)(. . . F . . .)? Here’s a bit of plausible reasoning (using the possible worlds framework of S5 modal logic) that supports such an acceptance:

Suppose that (CF)(. . . F . . .) is the case. Suppose in other words that it is possible to construct an open-sentence F such that . . . F . . . . This implies that the world could have been such that such an open-sentence token was constructed. To express it in terms of possible worlds, we can conclude: “There exists a possible world in which an open-sentence token of the required sort existed”. So we have ◊(∃F)(KF & . . . F . . .).

Suppose, this time, that ◊(∃F)(KF & . . . F . . .) is the case. Then, using the above possible worlds semantics, we can conclude that, in some possible world, an open-sentence token F of the sort asserted has been constructed. It follows that the world could have been such that such an open-sentence token was constructed. Hence, (CF)(. . . F . . .).

8. How the S5 Characterization is Theoretically Useful

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The above S5 characterization yields some useful theoretical benefits. First of all, modal logic has been the subject of intense study and research for over a hundred years\(^{160}\): it has been developed, explored, and analyzed, as well as defended, by a great many important philosophical and mathematical logicians.\(^{161}\) Since the S5 characterization given above allows the presentation of the central ideas and theorems of the Constructibility Theory within the framework of such a well-studied area of logic, it certainly adds to the logical respectability and accessibility of the Constructibility Theory.

There are other benefits that accrue to using the kind of S5 characterization being discussed here. Such an account of the Constructibility Theory could be expected to prevent many misunderstandings. For example, Michael Resnik once raised an objection to my view that was based upon the mistaken assumption that what sentences like \((CF)(\ldots F \ldots)\) assert is that it is possible for some human to construct an open-sentence such that \ldots .  The characterizing sentence of \((CF)(\ldots F \ldots)\) makes it clear that--to use the possible worlds understanding of the modal operator '◊'--what \((CF)(\ldots F \ldots)\) asserts is that, in some possible world, an open-sentence token F of the required sort exists or has been constructed. Thus, \((CF)(\ldots F \ldots)\) is like the well-ordering theorem of set theory that asserts that, for every set S, it is possible to construct a well-ordering of S. So \((CF)(\ldots F \ldots)\) clearly does not imply, or even suggest, that the open-sentence token F is supposed to have been constructed by a human. Thus, the analysis makes it clear that the supposition that Resnik made was unwarranted and gratuitous.


\(^{161}\) Just a few of the important researchers who have produced important work in this area are: Kurt Gödel, Saul Kripke, George Boolos, Robert Solovay, David Lewis, Kit Fine, Jaakko Hintikka, Arthur Prior, Ruth Barcan, Alvin Plantinga, and, of course, C. I. Lewis.
John Burgess has commented on my response to Resnik’s objection, writing that “one would like to be told something more about the nature of the non-human intelligent being Chihara has in mind”, and then speculating that I must have in mind something like “extraterrestrials with super powers”. Thus, he was suggesting that \((\text{CF})(\ldots F\ldots)\) should be understood to be the assertion that an extraterrestrial with super powers can construct an open-sentence such that \ldots . As I see it, Burgess’s suggestion involves an even more egregious misrepresentation of my views than the one Resnik made. Burgess’s way of interpreting my constructibility assertions can be likened to understanding a classical mathematician’s assertion “For every set \(S\), it is possible to construct a well-ordering of \(S\)” as the claim that for every set \(S\), it is possible for an extraterrestrial with super powers to construct a well-ordering of \(S\)—not the sort of interpretation that any reasonable person, without an irresistible desire to liken mathematics to science fiction, would be tempted to make. Of course, the S5 characterization clearly brings out the nature of Burgess’s misrepresentation of my views, since the statement ‘There exists a possible world in which an open-sentence token of the required sort has been constructed’ does not imply or presuppose that there is a type of being—perhaps an extraterrestrial with super powers—such that, in some possible world, an open-sentence token of the required sort has been constructed by a being of that type. In other words, \((\text{CF})(\ldots F\ldots)\) does not require or presuppose the specification of some particular type of being that is doing the constructing in all cases (as Burgess assumes).

9. Why I Refrained From Giving the S5 Characterization

\[162\text{This objection was raised in his review \cite{Burgess2005} of my book \textit{A Structural Account of Mathematics}.}\]
In view of the above benefits, the reader may well wonder why I did not set forth this S5 characterization of the constructibility quantifiers when I first published the Constructibility Theory in 1990? Unfortunately, had I done so, I would then have been faced with certain problems involving S5 modal quantificational logic--problems that would have required substantial research on my part. At that stage in the development of my investigation into the nature of mathematics, I did not want to get distracted by the need for such foundational research in an area of modal logic that I had not yet researched sufficiently.

As an example of one such problem, consider the Barcan formula:

$$\Diamond (\exists x) Fx \rightarrow (\exists x) \Diamond \neg Fx$$

which is thought by some philosophers to be a theorem of S5 quantificational modal logic. Presenting the above S5 explication of the constructibility quantifier would raise the possibility that a simple assertion of constructibility of the form $(CF)(\ldots F\ldots)$ would amount to asserting $\Diamond (\exists F)(KF & \ldots F\ldots)$, which is logically equivalent to $\Diamond (\exists F)- (KF & \ldots F\ldots)$, from which, in virtue of the Barcan formula, we could conclude $(\exists F)\Diamond (KF & \ldots F\ldots)$. Thus, the simple constructibility assertion $(CF)(\ldots F\ldots)$ would allow us to infer that there actually exists an open-sentence token that could have certain features. Such a result would be unwelcome because of my conviction that asserting $(CF)(\ldots F\ldots)$ should commit one to the existence of nothing actual. At the time I was

163 A nice introduction to the controversy can be found in Konyndyk, K. (1986). *Introduction to Modal Logic*. Notre Dame, Ind., University of Notre Dame Press., Section 4.10. Unfortunately, there are some significant errors in the text that mar its presentation of the material.
writing (Chihara 1990), I did not want to spend the amount of time required to
deal with such complications, so despite some mild urging from some
philosophers, I chose to forgo the kind of S5 characterization sketched
above.

After the publication of my 1990 book, however, I began a research
project which led to the publication of (Chihara 1998). In that work, I
developed and defended a version of S5 quantificational modal logic (the
system M*), within which the Barcan formula is not provable, thus in effect
showing that the above problem does not arise within my version of S5
quantificational modal logic.165

10. An Objection Considered

The above sections may suggest to the reader the following objection.
Accepting the S5 characterization of the constructibility quantifier seems to
imply that one is also accepting the semantics of S5 modal quantification
theory. And this suggests that the Constructibility Theory is committed to the
existence of possible worlds—the existence of which is far more ontologically
extreme that the existence of mathematical objects.166

My response to this objection is to reject a presupposition of the
suggestion that gives rise to the objection. The modal quantificational logic
appealed to in the S5 characterization is only to be regarded as a mathematical
model of modal reasoning, in the way Enderton regarded other formal logical

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164 Vann McGee was in favor of my giving such an explication when he commented on some early
versions of my work on this topic.

165 See [Chihara, 1998 #285], Chapter 7.

166 This was argued by Donald Gillies and Jan Wolenski (see [Chihara, 1998 #285], p. 5.
systems such as PA.\textsuperscript{167} Indeed, I provided an alternative, non-possible worlds semantical account of the system $M^\#$ in (Chihara 1998).\textsuperscript{168} Thus, the possible worlds semantical discussions of $M^\#$ in (Chihara 1998) should not be regarded as an account of what actually exists.

11. The Restrictions on the Constructibility Quantifiers

The constructibility quantifiers place certain restrictions on the kind of open-sentences that will be said to be constructible. In other words, the variables of the constructibility quantifiers can be regarded as “ranging over” only certain sorts of open-sentences (to be determined by the restrictions to follow). These restrictions are tailor made to make the system fit the simple theory of types:

At level 0, there are “objects” or “individual things that can be regarded as urelements” of the system.

At every other level, only open-sentences will be said to be constructible.

At level 1, the open-sentences said to be constructible must be such that only things of level 0 can satisfy them;

\textsuperscript{167} See [Enderton, 1972 #75], Introduction.

\textsuperscript{168} See [Chihara, 1998 #285], Chapter 7. I actually used the expression ‘$M^\ast$’ rather than ‘$M^\#$’, in the work cited; here I didn’t want to use ‘$M^\ast$’ since the asterisk was perhaps going to be used in this work to denote a specific function.
At level 2, the open-sentences said to be constructible must be such that only open-sentences of level 1 can satisfy them;

In general, the restrictions are given by the schema:

At level \( n \), the open-sentences said to be constructible must be such that only open-sentences of level \( n - 1 \) can satisfy them

where ‘\( n \)’ above can be replaced by any Arabic numeral greater than ‘0’.

There are other restrictions that govern the kind of open-sentences that are said to be constructible in this theory:

Each open-sentence \( \varphi \) of this hierarchy that is said to be constructible must be “well-defined” over the previous level in the following sense:

Each open-sentence \( \theta \) of the previous level must either satisfy \( \varphi \) or not satisfy \( \varphi \).\(^{169}\)

Furthermore, each open-sentence \( \varphi \) of level \( n \) of this hierarchy that is said to be constructible must be extensional over the level \( n - 1 \) open-sentences in the following sense:

For all open-sentences \( \alpha \) and \( \beta \) of level \( n - 1 \), if \( \alpha \) satisfies \( \varphi \) and \( \beta \) is extensionally identical to \( \alpha \), then \( \beta \) must also satisfies \( \varphi \).

\(^{169}\) For more details of the restriction being specified, see pp. 49-50 of \{Chihara, 1990 #34\}. 

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Now some may suspect that the restriction being given above will lead to problems, if not downright contradictions or paradoxes, because the open-sentences of level \( n \) can contain instances of bound variables of level higher than \( n \) and it might be suspected that such higher level open-sentences will produce circularities of some sort. If the reader has such worries, she should examine pp. 62-5 of {Chihara, 1990 #34}, where the restriction to open-sentences extensional over the previous level, as well as the above presumed problem, is discussed in detail.

With these restrictions now made explicit, we can give the Constructibility interpretation of Q-STT.

12. The Constructibility Interpretation of Q-STT

There is only one kind of non-logical constant in the vocabulary of Q-STT, the epsilon relations of the various levels: under this interpretation, epsilon is specified to be the satisfaction relation.

The non-logical identity predicates, \( I^{t^{2}, m, m} \) of level \( m \), for all \( m > 0 \), are specified to mean:

\[
\text{is extensionally identical to}
\]

where \( F \) and \( G \) (of level \( m \)) are said to be extensionally identical iff the following holds:

\[
\text{For all } \alpha \text{ of level } m - 1, \\
\alpha \text{ satisfies } F \text{ iff } \alpha \text{ satisfies } G.
\]
The identity predicate \( l^{\langle 2, 0, 0 \rangle} \) of level 0 is to mean identity. In other words, at level 0 the identity predicate is to signify identity between objects. At all other levels, the identity predicate means extensional identity.

As I noted earlier, the existential quantifier is to be interpreted to be the constructibility quantifier, with the variables occurring in the constructibility quantifiers restricted to the ranges described above in the previous section.

I turn now to the task of showing that the axioms come out true under this interpretation.

The axiom schema of comprehension:

\[
(\exists y^{n+1})(x^n)(x^n \in y^{n+1} \leftrightarrow --- x^n ---)
\]

That all the specific instances of this axiom schema come out true under this interpretation can be shown very much as it was shown on pages 65-8 of {Chihara, 1990 #34}. The main idea is this: take the case in which \( --- x^n --- \) is a formula of Q-STT within which only \( x^n \) occurs free. Under the Constructibility interpretation, the axiom asserts that it is possible to construct open-sentence \( \varphi \) of level \( n + 1 \) such that every open-sentence of level \( n \) that it is possible to construct satisfies \( \varphi \) if and only if it satisfies \( --- x^n --- \). But any token of \( --- x^n --- \) would be an open-sentence of level \( n + 1 \) that it is possible to construct. So we can conclude that the axiom holds for the simplest case.

Since some objections have been raised to my reasoning regarding one of the cases in which one or more variables occur free in \( --- x^n --- \), I shall provide a
reformulation of my original reasoning for the disputed case—this time using the S5 characterization

\[ [C^#] \quad (CF)(\ldots F\ldots) \text{ iff } \Box(\exists F)(KF \& \ldots F\ldots). \]

Here is how I originally argued in {Chihara, 1990 #34}:

Consider next the case in which the formula expressing the condition [--- x^n---] contains free occurrences of just one variable, δ, other than x^n and that this variable is of level 0. The question is: no matter what value δ may take, will it be possible to construct an open-sentence of the appropriate sort that is satisfied by just those objects that satisfy the resulting conditions? Suppose that δ takes as value the object k. Then it is reasonable to maintain that there is some possible world in which the language is extended to include a name of the object k. Then the formula expressing the condition in question can be converted into the required open-sentence by replacing all free occurrences of δ in this formula by the name of k. And surely it would be possible to construct such a formula.

To better understand this reasoning, it should be emphasized that, in applying the theory, the level 0 variables will not be specified to range over just “objects” since I doubt that the word ‘object’ is sufficiently determinate to pick out a well-defined totality of things in the actual world over which the variable is to range: I believe that one must give some such specification as ‘persons’, ‘molecules’, or ‘bacteria’ in order to definitely pick out a relatively well-defined totality. Of course, there are indefinitely many such specifications. Now if the variable is supposed to range over a totality that is simply too roughly specified or too vaguely articulated to allow such a specification, then the axiom schema of comprehension, under the constructibility interpretation, may not be maintained to hold for such a “condition”—but I do not consider such a limitation to be a serious defect of the system.
At this point, it might be remarked that, if the range of the level 0 variables is specified to range over, say, the totality of bacteria existing in the world, then there will not be infinitely many objects at level 0. In that case, we will not have a model of the system, as ordinarily conceived. It is allowed that some interpretations of the system will have only finitely many objects of level 0. It is also allowed that there could be interpretations of the system in which there are infinitely many objects of level 0—thus, there is no “Axiom of Infinity” in the system, but only a “Hypothesis of Infinity”.

Let us return now to the above case of the Comprehension Axiom. Suppose for specificity that the variable $\delta$ takes as value, say, some bacterium $k$. An individual bacterium is not visible, and although bacteria are abundant everywhere we humans live, they are also scattered about where there are no humans. Clearly, few, if any, individual bacteria have ever actually been named. But equally clearly, $k$ could have been named (in the appropriate S5 sense of ‘could’)–it is easy to imagine a possible world in which $k$ is named. So there could be a world in which the language of the Constructibility Theory is expanded to include a name of $k$. Thus, by the kind of modal reasoning that is routinely given and accepted by S5 modal logicians, we would be justified in concluding that, in some possible world, the language of the Constructibility Theory is expanded to include a name of $k$. So in some possible world, the formula expressing the condition in question can be converted into the required open-sentence by replacing all free occurrences of $\delta$ in this formula by the name of $k$. Hence, it is possible to construct an open-sentence that is true of an “object” if and only if that object satisfies the condition in question when $\delta$ takes as value $k$.  

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I trust that the reader will be able to verify, in the above way, the “plausibility” or “reasonableness” (at least within the framework of standard S5 systems of logic) of all of the justifications for accepting the axiom schema of comprehension that I gave in (Chihara 1990), pp. 65-8—by making use of the S5 characterization wherever the reasoning involving the constructibility quantifier may have appeared obscure.

The axiom of extensionality:

\[(z^{n+1})(y^{n+1})(x^n \in y^{n+1} \leftrightarrow x^n \in z^{n+1}) \rightarrow y^{n+1} = z^{n+1})\]

Given the fact that all the open-sentences said to be constructible in the hierarchy must be extensional over their arguments, and given the fact that the identity sign is taken to mean extensional identity, the truth of the axiom of extensionality under this interpretation is trivial (obviously true).

13. The Constructibility Model is not Fregean

What can we conclude about the constructability model of Q-STT from what has been established above? Since classical mathematics has been developed within Q-STT, we can infer that a constructibility version of classical mathematics will be reproducible within the framework of Q-STT—understood, however, in the constructibility way—a version of mathematics that nominalists will prefer to use (for certain philosophical purposes) to the Platonic version. Furthermore, we can also conclude that a simple-type theoretical version of
model theory, interpreted in terms of constructibility quantifiers, can be reproduced within this framework. Also, since the Platonic model of Q-STT has been examined and heavily studied over many years by mathematicians and philosophers without the discovery of inconsistencies or logical paradoxes, we have grounds for believing that the Platonic model is coherent. These grounds can then provide additional reasons for believing that the constructibility model of Q-STT is also coherent. Interestingly, each of these two models of Q-STT can reinforce our confidence in the other, so that each of these models ends up mutually supporting the other.

Finally, I wish to emphasize what was mentioned earlier, namely that the constructibility version of Q-STT is not Fregean: its assertions are not assertions about the actual world, but rather are modal assertions of the constructibility of open-sentences (in possible worlds semantics, they are assertions about what holds in at least one possible world). The theory is not Hilbertian either. Nor should it be regarded as a philosophical view of the nature of mathematics. The Constructibility Theory was put forward as a model—a model to show that one could have a mathematical theory that allows the development of a version of classical analysis and yet does not require quantification over mathematical entities. Such a model undercuts the Indispensability Argument for the philosophical view that there must exist mathematical objects that exist independent of human concerns and theories—something that will be discussed in more detail later in this work.

14. The Two Interpretations of Q-STT Compared

We thus have two independent interpretations of Q-STT, the Platonic one of the sort that Quine had in mind when he put forward his “modern theory of
types” and the above Constructibility version. How should these two interpretations be compared and evaluated? I suggest that we should not regard them as adversaries in a contest, where only one can come out the victor (in the way Burgess and Rosen seem to understand all nominalistic “reconstructions of mathematics” in their {Burgess, 1997 #227}). The truth of Q-STT under one of the interpretations is not logically incompatible with the truth of Q-STT under the other interpretation. Better to regard these interpretations as simply alternative ways of thinking about and utilizing the formal theory. It will soon become evident that each interpretation has its distinct advantages, and each can throw light on the other—which is in line with how mathematical models function in science and engineering. One should not think that there is just one right model of Q-STT: there are different models for different purposes and different goals.

(A) The Platonic Interpretation

The interpretation Quine had in mind, in setting up his formal theory, was one that treated all the entities in the levels greater than zero as classes or sets. The version of set theory that results from this interpretation has a distinguished pedigree: it is a close descendent of the mathematics of PM. Of course, PM was developed to do much more than to provide logicians and philosophers with a formal model of classical mathematics that can be derived from a relatively small set of axioms: Russell and Whitehead were trying to reform the defective system of logic that Frege had developed in a way that would “solve” the various paradoxes of logic and mathematics that they characterized as “vicious-circle paradoxes” and yet allow the development of
classical mathematics from its axioms.\textsuperscript{170} To achieve these goals, many complications in the system were introduced. For example, PM’s very complex system of “propositional functions” (things of the system that correspond to the “concepts” of Frege’s system) ended up with an intricate structure, far more complicated than the structure formed by Frege’s system of concepts.\textsuperscript{171} PM’s complex system of propositional functions, called “the ramified theory of types”, was the logical framework within which all the terms of classical mathematics, including classes, were defined.\textsuperscript{172} Now Quine has observed that, once classes were introduced in PM, and the development of classical mathematics was undertaken, propositional functions were hardly mentioned again.\textsuperscript{173} So it was natural for mathematicians, intent on producing a formal system of mathematics in which classical mathematics could be developed from a consistent group of axioms, would find a way of pruning the system of PM of its many complications that are not needed in the development of classical mathematics from axioms. Mathematicians, by concentrating on just the mathematical part of PM, were able to simplify the system drastically, to produce what is now called “the simple theory of types” or what Quine called “the modern theory of types”.\textsuperscript{174} The formal uninterpreted version of Quine’s modern theory of types is essentially what I have been calling ‘Q-STT’.

\textsuperscript{170} See my \cite{Chihara, 1973 #48}, Chapter 1.

\textsuperscript{171} The many attempts to make sense of PM’s system of “propositional functions”, and of what the authors have said about the fundamental entities of their system, have resulted in a large variety of amazingly different interpretations of what “propositional functions” are supposed to be. See for example \cite{Wahl, 2011 #588}.

\textsuperscript{172} See, for details, Chapter 1 of \cite{Chihara, 1973 #48}.

\textsuperscript{173} \cite{Quine, 1966 #326}, p. 22.

\textsuperscript{174} Quine credits Tarski and Gödel with developing the modern theory of types (\cite{Quine, 1963 #368}, p. 261).
There is now widespread agreement that the version of classical mathematics that had been developed in PM can be developed within the STT. However, it should be mentioned that STT does have some counter-intuitive, if not downright paradoxical, features. Quine specifies some of these features in the following quotation:

One especially unnatural and awkward effect of the type theory is the infinite reduplication of each logically definable class. There is no longer one universal class V to which everything belongs, for the theory of types demands that the members of a class be alike in type . . . This reduplication is particularly strange in the case of the null class. One feels that classes should differ only with respect to their members, and this is obviously not true of the various null classes. ([Quine, 1938 #363], p. 131).

Quine’s observation about the null class seems especially apt. After all, what distinguishes the null class of level 2 from the null class of level 3? How do they differ? Well, the null class of level 2 has no members of level 1, whereas the null class of level 3 has no members of level 2. The two null classes seem to be distinguished from one another by what they don’t have as elements. This brings to mind the old joke about the person who walks into a café and asks for coffee without cream and is told: “I’m sorry, we’ve run out of cream, but you can have coffee without milk instead.”

The fact that the members of every class must be of the same level is also quite counter-intuitive. Why cannot there be a class whose members include both Michelle Obama and the class whose only member is Sarah Palin? Furthermore, it would seem that the standard way of defining binary relations as sets of ordered pairs, runs into difficulty if one uses the usual definition of ordered pairs--\(<a, b> = \{\{a\}, \{a, b\}\}. This is because, if we used these definitions in Q-STT, we would not be able to obtain binary relations--sets of ordered pairs--that obtain between things of different levels (which we
obviously would need). Quine has a simple way of obviating this problem however. Thus, to obtain a binary relation that holds between things of level n and things of level n + 1, simply use the slightly modified definition of ordered pair:

$$\text{For every } a \text{ of level } n, \text{ and } b \text{ of level } n + 1, \langle a, b \rangle = \{\langle a \rangle, b \rangle\}.$$ 

Although this way of dealing with the problem will work, there is no question that, for some relations that one may wish to express in Q-STT, quite a few new definitions would be required--which could be messy.

(B) The Constructibility Interpretation

It should not be assumed that the value of the Constructibility interpretation of Q-STT described earlier is simply restricted to its philosophical uses. The idea of creating two quite different mathematical models of a single formal theory is, of course, not a new idea or something that has been used only very rarely. As Lucienne Felix has noted about a similar case in which a researcher produces two models, taken from different areas of research, of a single structure or theory: “the experience acquired in one of the areas serves in the exploration of the other” ((Felix 1960), p. 175). Felix goes on to write:

Each model carries with it not only the structure which is being studied and which it has in common with the other model, but also other structures which are characteristic of itself; therefore, not every property of a model is usable in the investigation. (Ibid.)

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[Quine, 1963 #368], p. 261. Notice that [a] and b will be of the same level.
Keeping in mind that the case I have been discussing involves two different interpretations of a single formal theory, it should be noted that the different interpretations are radically different from each other. In the standard case of “different structures”, we do not change what the existential quantifier (‘there is an x such that’) means, whereas, in the case I have been presenting, the Constructibility Theory replaces the existential quantifier with the constructibility quantifier. They are not different models in the way that two different first-order structures model some first-order theory, but still they are different interpretations of the theory.

Each of these two quite different interpretations of Q-STT can be usefully employed, as Felix has suggested, to explore and to enhance our understanding of the other interpretation. For example, each model of Q-STT can boost one’s confidence in the coherence and the consistency of the other. To see how the constructibility interpretation can be used for such a purpose, let us first note that, from the perspective of the nominalist, there is no way that a proof can be given of the truth of the axioms of Q-STT, when it is interpreted Platonically. The reason is this: the nominalist holds that none of the entities that the Platonic interpretation postulates actually exists. But a proof of the truth of the axioms of Q-STT, interpreted Platonically, would if truly genuine establish the existence of many such entities. So the nominalist is convinced that no such genuine proof will ever be constructed.

Of course, in general, mathematicians do not attempt to construct such a proof. What they do attempt to do is to prove that the axioms are true, given that the universe of set theory has a certain structure (e.g., the iterative structures discussed earlier). Of course, such a proof does not establish that sets exist (in the actual world). The closest thing to the sort of proof I was discussing above that any contemporary scholar has seriously advanced is the
sort of argument for the existence of mathematical objects that was put forward by the Burgess-Rosen team, which they claimed has only premises that are “scarcely deniable”—an argument that will be discussed in the next chapter.

The fact that one cannot construct a genuine proof of the truth of the axioms of Q-STT, when the system is interpreted Platonically, does not preclude other kinds of justifications or plausible arguments, nor does it preclude various kinds of support for the coherence and consistency of the formalized theory. Thus, any such plausible arguments or support should be of some interest to foundationalists, and this is something that nominalists can supply. The reason is: as was shown earlier, the Constructibility interpretation of Q-STT enables us to give justifications, using S5 modal logic, for maintaining that the axioms of the theory are true. This is because one can give a kind of proof of the truth of the axioms of the constructibility model of Q-STT (using the S5 characterization of the constructibility quantifier), which provides us with a kind of (relative) consistency proof of Q-STT. In other words, the modal logical proof of the axioms amounts to a kind of consistency proof of Q-STT. So the nominalist is able to generate significant grounds for concluding that the formal theory, Platonically interpreted, is consistent and coherent.

Now, consider how the Constructibility model can provide another sort of support for the coherence and consistency of the Platonically interpreted theory. It is easy to show that we can specify, within the Q-STT framework of the Constructibility Theory, what “structures” are. As a result, we will have two quite different ways of specifying what “structures” are: the usual set theoretical version and the constructibility version. Of course, in one respect, the Constructibility version is in a somewhat better epistemological position than the Platonic one. This is because, in many cases, one can actually construct open-sentence tokens that are the “structures” asserted to be
constructible in the Constructibility model. Confidence in the appropriateness of the constructibility model is bolstered by the fact that one can actually construct open-sentence tokens that can be seen to have the logical features that something must have to be a “structure” according to the standard analysis. Actually constructing such an open-sentence can convince one that there are objects that serve as adequate nominalistic models of what one wants structures to be. In other words, one can do for simple type theory what one does for group theory: one can devise models of the formal theory that involve constructing and working with common ordinary objects.\textsuperscript{176}

The fact that many of the entities talked about in mathematics can be actually constructed and observed when one interprets Q-STT in the fashion implied by the constructibility interpretation is relevant to another feature of mathematics that has puzzled some people. Solomon Bochner has singled out what he considers to be a perennial question, which is “asked sometimes shyly and sometimes challengingly”, of why it is:

that mathematics is so peculiarly “abstract,’ in a manner which is discouraging to many who try learn it, and whether mathematics cannot be made more “intuitive,” and more accessible to larger audiences, without forfeiting its substance and its mission, whatever these might be. ((Bochner 1966), p. 30).

Now I do not claim that the Constructibility model significantly alleviates the problem of abstractness that Bochner had in mind, but I believe that the assertions of the constructibility of open-sentence tokens, which characterize the theorems of the Constructibility model, do make at least one aspect of mathematics less mysterious than do the theorems of the Platonic model of Q-STT according to which mathematicians are theorizing about relationships that

\textsuperscript{176} See Chapter 2, Section 5, for specific examples, as well as a reference to where additional examples can be found.
are supposed to obtain among entities that do not exist in the physical world. And to that extent, some people may find that the Constructibility model can alleviate some of their puzzlement about the nature of mathematics.

More generally (not restricting the focus only to “structures”), a big advantage of the constructibility model of Q-STT—one that even mathematicians should appreciate—is that, since the theorems of the Constructibility model are all understood to be assertions about the constructibility of open-sentence tokens, a great many of its theorems can be directly and straightforwardly verified—for example, by simply constructing open-sentences and determining that they have the features that the theorems asserts that they should have. This is one feature of the nominalist’s interpretation of the formal theory that is simply not available to the Platonist’s alternative model of the formal theory, according to which the axioms make assertions about the existence of things that are not observable or even detectable by the most sophisticated scientific instruments. For example, take the theorem of Q-STT that asserts the existence of an empty set of level 1. Since one cannot literally construct or detect any sets, there is no way for the Platonist to directly and straightforwardly verify the existence of the empty set. On the other hand, according to the constructibility interpretation, what is asserted by the above theorem is the constructibility of an open-sentence of level 1 that is true of no object. But that assertion can be easily verified by actually constructing such an open-sentence (which I have done) and seeing that no object can satisfy it. Such simple verifications of theorems of the Constructibility model of Q-STT can be seen to be similar to the type of confirmations of theorems of group theory achieved by using the models involving wooden blocks described in Chapter 2.
The indefinitely many such confirmations of theorems of the formalized theory that the Constructibility model of Q-STT can provide (and has provided) yields grounds that increase the probability that the formal theory Q-STT is a coherent theory—and this in turn should increase our confidence that the Platonic model of Q-STT is also coherent. Since a coherent theory must be consistent, we obtain some added measure of confidence that the Platonic model of Q-STT is also consistent.

We can now see how, specifically, the reconstruction of mathematics achieved in the Constructibility interpretation can make a contribution to foundational studies (despite certain questionable arguments of Burgess and Rosen in their {Burgess, 1997 #227}—to be discussed in the following chapter). By providing us with a completely different way of understanding the mathematical formalism—by giving us an entirely new perspective from which to view classical mathematics—we have been able to generate a new way of supporting conclusions about the coherence and consistency of the formalized theory. This suggests that we might be to produce entirely new proofs of theorems of mathematics that are completely independent of set theoretical proofs because they rest upon features of the constructibility of open-sentences. Can we not, for example, give a sort of justification of the theorem that, on the Platonic interpretation, asserts the existence of the empty set of level 3 in the way suggested above by constructing an appropriate open-sentence, without in any way making use of the axioms of Q-STT? All of this suggests that the frontiers of mathematical research might possibly be advanced using the Constructibility interpretation.
15. How the Constructibility Theory can be Usefully Applied in the Philosophy of Mathematics

To see, in particular, how the Constructibility Theory can aid us in the philosophy of mathematics, consider the following argument that Burgess put forward in his article {Burgess, 1990 #402} and which I shall classify in Chapter 6 as the third of the Burgess-Rosen “campaigns against nominalism”. This is the argument that proceeds from the following three premises:

(Premise 1) The statement

[1] Avogadro’s number is greater than $6 \times 10^{23}$.

is asserted by scientists to be not only true but known to be true ({Burgess, 1990 #402}, p. 6).

(Premise 2) Scientists have backed claim [1] by producing a convincing body of empirical evidence.

(Premise 3) [1] implies that there are numbers.

This argument, which I call “the scientific argument ¹⁷⁷ that numbers exist”, constitutes what I shall classify in Chapter 7 as the third of the Burgess-Rosen anti-nominalist campaigns. It proceeds by inferring that scientists know that [1] is true, based upon the relevant empirical evidence they have gathered, and since [1] implies that there are mathematical objects, we can infer from

¹⁷⁷ See in this regard {Nye, 1972 #453}, p. 97, for an indication of how features of Brownian motion were used to calculate Avogadro’s number.
something that is known to be true that there are mathematical objects. In this way, it would seem, we can conclude that we have good grounds for affirming that there are numbers. Thus, one might conclude that such beliefs obtained in this “scientific” way can be regarded as an advancement of our understanding of mathematics (and hence of science).

The kind of “scientific” argument that Burgess has put forward above can be reproduced in ways that do not require complicated discussions of recondite physical phenomena such as Brownian motion in order to understand the sort of experimental data used to confirm [1].

First, I give a definition:

[Def] Let us use the expression ‘the inner planet number’ to stand for the cardinal number of the “inner planets” (planets in our solar system whose orbits are interior to the orbit of the earth).

Then we can say that astronomers have come to know that

[2] The inner planet number is two.

As in the previous case, these astronomers can also provide us with strong empirical grounds for claiming to know that [2] is true. Should we reason as Burgess did above and conclude that, on the basis of strong scientific evidence, we have good grounds for affirming that there are numbers?

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178 See in this regard [Nye, 1972 #453], p. 97, for an indication of how features of Brownian motion were used to calculate Avogadro’s number.
Before so concluding, let us consider, for argument’s sake, the following imaginable situation: Imagine that there is a hypothesis H for which there exists considerable supporting empirical data. Indeed, each year the confirming support continues to grow. Yet, one implication of the hypothesis remains questionable. How can this be? Suppose that WHO (the World Health Organization) has collected data which happens to support the conjunction

\[3\] No living human is more than 145 years old and there is life on Mars.

However, all this data supporting \[3\] equally supports the hypothesis

\[4\] No living human is more than 145 years old.

Clearly, this data then does not support the hypothesis that there is life on Mars. Furthermore, as WHO collects more and more data of this sort supporting \[3\], no additional support is generated for the hypothesis that there is life on Mars. Thus, despite the growing empirical support for \[3\], the hypothesis that there is life on Mars remains questionable.

Now in investigating the question of whether the astronomer’s evidence for \[2\] is sufficient grounds for believing in mathematical objects, the Constructibility Theory can play a significant role. This is because there is a statement expressed in the language of the Constructibility Theory which has, so far as one can see, all the empirical implications of \[2\], namely,

\[5\] ‘x is an inner planet’ satisfies a two-attribute.\(^{179}\)

\(^{179}\) The cardinality predicate ‘is a two attribute’ is defined within the framework of the Constructibility Theory in (Chihara, 2004 #468), p. 179.
[5] however does not imply that there are mathematical objects. Just as the data collected by WHO supports [4] but gives us no good reason to believe that there is life on Mars, so also it can be seen that all the experimental data gathered by astronomers in support of [2] equally supports [5], but gives us no good reason for believing that there are mathematical objects, thus undercutting the reasoning sketched above for thinking that we have solid empirical evidence for believing in mathematical objects. In this way, then, the Constructibility Theory can serve to advance our understanding of mathematics by opening up new possibilities, especially to those convinced by the Burgess kind of reasoning.

Of course, this is just one of many ways in which the Constructibility Theory can provide scholars with a useful instrument for undermining arguments that are purported to show either that mathematical objects exist or else that we should believe that mathematical objects exist. The version of Quine’s indispensability argument that I called “Quine’s challenge to the nominalist” was widely thought to show that the belief in mathematical objects was required by science; and, as I noted earlier, my reconstruction was designed specifically to refute this challenge.

To produce another example of how the Constructibility Theory can be useful in undercutting philosophical claims, I shall examine Mark Steiner’s theses that the existence of mathematical objects is required to solve (or dissolve) certain philosophical enigmas or conundrums, and that this fact strongly counts in favor of belief in the existence of mathematical objects. For example, Steiner claims that Frege completely solved the “metaphysical problem of
applicability”\textsuperscript{180}. Frege’s solution is supposed to consist in showing how mathematical objects relate, not directly to physical objects but rather through concepts. Commenting on this solution, Steiner writes: “That physical objects may fall under concepts and be members of sets is a problem only for those who do not believe in the existence of sets” (\textsuperscript{Steiner, 1995 #391}). Since Frege’s solution presupposes the existence of mathematical objects, Steiner concludes that the success of Frege’s solution amounts to a strong reason for believing in the existence of mathematical objects.

The Constructibility Theory, however, provides nominalists with a way of undercutting this way of supporting belief in the existence of mathematical objects, since it provides nominalists with an alternative solution to this “metaphysical problem of applicability”—a solution that does not presuppose the existence of mathematical objects.\textsuperscript{181} In other words, the “metaphysical problem of applicability” does not constitute a problem for the nominalist, even though she does not believe in the existence of sets.

The Constructibility Theory can also be used to validate the use of arithmetical theorems to draw common inferences in everyday life, thus participating in Frege’s project of casting light on “what mankind has done by instinct [in using arithmetic]”.\textsuperscript{182} That such uses of the Constructibility Theory should count as advancing science proper is not something I wish to argue here, but they do suggest that the Constructibility Theory may indeed produce, through detailed and systematic development, what can be reasonably classified

\textsuperscript{180} \textsuperscript{[Steiner, 1995 #391], p. 132. Steiner attributes the formulation of this problem to Carl Posy.}

\textsuperscript{181} I leave it to the reader to come up with a solution making use of the Constructibility Theory. Such a solution is given in \textsuperscript{[Chihara, 2004 #468], Chapter 9.}

\textsuperscript{182} This claim, in particular, was criticized by Burgess (\textsuperscript{[Burgess, 2005 #487], pp. 88-9}). A defense of the claim, in response to Burgess’s criticism, will be given in the next chapter.
as an advance in our understanding of mathematics and, to some extent, also science.

16. The Platonic Model of Q-STT Viewed From a Nominalistic Perspective

How should a nominalist view Q-STT, when it is interpreted Platonically? Can the Platonic way of understanding Q-STT be of continuing value for the nominalist? I remember that Tarski once remarked that it was valuable for the field of mathematical logic that Gödel was such a devoted and dyed-in-the-wool Platonist. Tarski’s idea was that Gödel’s realistic views about the nature of set theory played a significant role in motivating, and providing a clear framework for, his logical researches, including, in particular, his researches on incompleteness and decidability.\textsuperscript{183} Similarly, the Platonic interpretation can be appreciated as valuable to nominalist mathematicians because interpreting mathematics in the particular realistic way Q-STT does, undoubtedly facilitates mathematical research and the proving of theorems. Thus, reconsider the procedure followed by the authors of PM: once the no-class definitions needed for developing their version of class theory were in place, they proceeded to develop mathematics in terms of classes and, essentially, dropped all talk of propositional functions. For purposes of “doing mathematics”—for arriving at useful and scientifically important theorems of mathematics—it is much neater and simpler to think in terms of classes than to have to deal with all the detailed restrictions and complicated notational features of the propositional functions of PM. Thinking of mathematics in terms of sets (instead of open-sentences) would seem to be a simpler and more fruitful way to proceed when one is

\textsuperscript{183}Unfortunately, I cannot remember if Tarki made these comments during a public meeting, such as the Berkeley Logic Colloquium, or in private at some party or tea.
attempting to prove mathematical theorems (to extend the frontiers of known mathematics) or to apply mathematics to scientific and technological problems. The nominalist can certainly appreciate the advantages of thinking of mathematics within a Platonic framework, without thereby believing in the existence of mathematical objects. (Recall that Tarski was a nominalist).

The Platonic understanding of set theory lends itself to a Fregean understanding of the theorems of set theory, under which set theory is taken to be a theory about what exists in the actual world. Such a Fregean view of set theory has both heuristic and didactic values, but should not be regarded as being seriously advanced here as a philosophically defensible position. From my nominalistic position, such a Fregean view of mathematics is seen as simply mistaken.

17. Another Way of Modeling real mathematics

There is another way of modeling real mathematics that should now be considered. Ponder Q-STT uninterpreted: that is the purely formal system Q-STT. That would give us three versions of Q-STT to be compared: (1) the Platonic version that was given the realistic interpretation based on Quine’s “modern simple type theory”, (2) the Constructibility version of simple type theory, and (3) the uninterpreted formal theory Q-STT. Can any of these versions be considered a good model of real mathematics? Initially at least, the Platonic version might seem to some (especially to those philosophers who are Realistically inclined) to be the most promising of the three. But investigation of this version has convinced me that there are just too many deep problems underlying this metaphysical view of mathematics for it to be a serious contender. I believe that the Realist’s model is so fundamentally misleading as
to create more problems than it solves. In the following, I shall discuss just a few of these problems.

To see some of the ways in which the realistic model is problematic, first consider the realistic model of a part of real mathematics that has almost universally received favorable reviews: Frege’s logical model of real arithmetic. Recall that Frege believed that such terms of arithmetic as ‘seven’, ‘the first prime number’ and ‘3 + 8’ referred to (and were proper names of) specific abstract logical objects—his extensions of concepts. One serious problem with this view was raised by Charles Parsons in {Parsons, 1983 #140} and by Paul Benacerraf in {Benacerraf, 1965 #113}. Parsons noted that:

If we admit enough extensions, on some grounds or other, then there are too many possible ways of identifying them with numbers. In fact, any reasonably well-behaved sequence of classes can be chosen to represent the natural numbers. . . . The reader will be reminded of a well-known passage in the Philosophical Investigations (I, 293). If it makes no difference in mathematics to which class a term of number refers, what is the relevance to the thesis that numbers are objects of the possibility of an identification of numbers and classes? ({Parsons, 1983 #140}, pp. 184-5).

But why should we suppose that natural number terms refer to specific objects in the first place? Frege provides no good argument for accepting such a thesis. Parsons raises the skeptical question: “It is odd that we should have to identify numbers with extensions, but that we should be able to choose this reference in almost any way we like” ({Parsons, 1983 #140}, p. 185). Not surprisingly, a number of philosophers have simply abandoned this aspect of Frege’s view of number terms.

Perhaps some Platonically inclined philosopher will suggest that Frege ran into the Parsons-Benacerraf problem because he tried to identify numbers with extensions. One could maintain the view that number terms refer to, or are
names of, specific abstract objects without thereby committing oneself to the view that numbers are extensions. One might argue that number terms refer to specific abstract objects that are not another kind of thing, such as sets or extensions. But this view is plagued by the problem of reference that I discussed at some length earlier: recall the deep problem that arises from the Realist’s thesis (that mathematical terms are proper names of abstract objects) when it is asked how we have been able to pick out and refer to the specific objects that is named by some number term. The problem arises when the Realist tries to explain how we have been able to pick out, from the enormously complex and rich realm of mathematical abstract objects that they postulate, a specific entity as the referent of some mathematical term such as the word ‘three’. How is it, one wonders, that the word ‘three’ has come to refer to that particular object in this vast realm? It surely can’t be a matter of luck or mere happenstance. Yet no Platonist has come up with a satisfactory answer to these questions. This puzzle is heightened by the fact noted by Azzouni that I mentioned in the Forward, namely that the “current philosophical concern with how mathematical terms pick out what they refer to is an oddity from the point of view of mathematical practice, which, in broad respects, is simply not concerned with reference” (Azzouni, 1994 #309, p. 31).

Let us reconsider the related problem about reference to mathematical problems I discussed in the Forward: consider the fact that, in real mathematics, mathematicians frequently make assertions that relate numbers of different sorts, as does the statement that three is less than pi. But such assertions raise the following kind of questions (which I discussed briefly in the Forward), especially for the Realist’s view of mathematics: Is the natural number three the very same entity as the real number three? And is the real number
three identical with the rational number three? These, and similar, questions seem significant and even extremely important to some philosophers of mathematics—especially those who are Realists. Here again, no Realist seems to have come up with a truly acceptable answer to such questions. (I shall return to this problem later).

Rejecting, then, the Realist’s model of real mathematics that treats number terms as proper names of abstract objects, let us consider the second alternative: my constructibility model. This time, I find that the fundamentals of the constructibility version are simply too radically different from those of real mathematics to provide us with insights throwing light on the problem at hand. So neither of the two versions of simple type theory considered seem to offer a path to a solution to the puzzle. Suppose then that we consider a third alternative, namely Q-STT (the purely formal uninterpreted system).

Since Q-STT is uninterpreted, one might think that, until it is given an interpretation, it cannot possibly assert anything. Like the sentence ‘(∃x)F^1x’ of first-order logic, the theorems of Q-STT are not straightforwardly true or false—they are only satisfied by certain sorts of structures. Thus, it seems, we must conclude that the theory does not assert anything and cannot provide us with any information. Hence, it appears to be the sort of theory that cannot be usefully applied in science and engineering. So how can Q-STT be a good model of real mathematics—which clearly can be so applied?

The above reasoning is based on some hasty assumptions. The theorems of Q-STT are thought not to be applicable in science primarily because, it is thought, they cannot provide any useful information—they are thought to be meaningless since Q-STT is not an interpreted theory. In fact, the theorems of Q-STT are not lacking in all meaning, despite the fact that the theory lacks an interpretation. After all, the sentences of Q-STT are logical sentences.
containing logical constants—quantifiers, logical connectives, and identity predicates—which are meaningful elements of these sentences. Furthermore, the non-logical constants also have some meaning due to fact that they are all binary predicates.

Consider, in this light, the example of group theory again. Take the case of a first-order version of group theory that is uninterpreted. Although lacking a complete interpretation, the axioms of group theory all have what I have called “structural content” in virtue of which only certain first-order structures are models of the theory. In other words, despite the lack of an interpretation, the theory still does determine a kind of structure—the first order structures that satisfy the axioms. And the theorems of group theory are all true of these models of the theory—they tell us what must hold in all first-order group structures. Thus, we can certainly obtain mathematically informative and useful information from the theory. And as I noted earlier, all of this mathematical information about structures can be used in scientific and practical situations to draw important information about the behavior of things in the physical world. Now Q-STT can be used in a similar fashion to draw important conclusions about the physical world. Each theorem of Q-STT is a sentence of quantificational logic, and regardless of whether or not that sentence is interpreted, it will have what I called above a “structural content”—something in virtue of which the sentence can be applied in science and engineering to draw significant conclusions about the physical world. (The notion of structural content will be discussed in more detail in Section 14 of the next chapter). In short, the theorems of Q-STT can indeed impart

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184 See, for example, (Chihara, 2004 #468), pp. 65-6, 247-8.
information—some of which might be useful at times and which can, in theory, be applied in science and engineering.

18. Does the Constructibility Version of Q-STT Presuppose the Fregean Assumption?

I end this chapter by noting that the Constructibility Version of Q-STT is not Fregean. This is because the theorems of the Constructibility Version of Q-STT all assert the constructibility of various kinds of open-sentences. Such modal assertions are not regarded as assertions about the actual world by modal logicians, because, making use of the possible worlds model that modal logicians use, such assertions are regarded as assertions about all the possible worlds and hence not about any particular world such as the actual world.
Chapter 6

The Burgess-Rosen Campaign to Refute Nominalism

Introduction

It was not just the philosophical giants of mathematical logic, Frege, Russell, Godel and Quine, who made the Fregean Assumption. As I mentioned earlier, many well-known contemporary philosophers of mathematics have also accepted the Assumption whether expressly or not. In this work (an investigation into the nature and significance of the Fregean Assumption), I have chosen as the primary example of such contemporary Fregeans the Princeton team consisting of John Burgess and Gideon Rosen: this is because they have assiduously attacked nominalism, for a span of over thirty years, from the perspective of their acceptance of the Fregean Assumption. Over these many years, the Burgess-Rosen team has waged what amounts to a war against nominalism—indeed, it might be more appropriate to call their war a “crusade”, in virtue of the persistence and ardor with which it was carried out. This crusade against nominalism can be analyzed into five “campaigns”:

The first campaign was begun by Burgess in {Burgess, 1983 #292} and continued and amplified by Burgess and Rosen in {Burgess, 1997 #227}; it had
the goal of producing negative arguments to undermine all nominalistic attempts to reconstruct versions of mathematics or science that do not presuppose the existence of mathematical entities. Their arguments were aimed at showing, in particular, that such nominalistic versions of mathematics, as the Constructibility Theory of the previous chapter, are complete failures.

The second campaign was started by Burgess in {Burgess, 1983 #292} and continued in both {Burgess, 1992 #218} and {Burgess, 2005 #487}: it consisted of the formulation of several dilemmas that supposedly proved that the nominalist’s position is untenable.

The third campaign, published in {Burgess, 1990 #402}, consists of what I called in the previous chapter “the scientific argument that numbers exist”—this argument was refuted in Section 15 of that chapter.

The fourth campaign consists of what I call the “look and see” argument of Burgess’s review of my A Structural Account of Mathematics.\textsuperscript{185} Burgess basically argued that to see that nominalism is false, one need only look and see how mathematicians have come to accept such mathematical theorems as:

\[ \text{There are numbers greater than } 10^{10} \text{ that are prime}. \]

From this, Burgess concluded: “That’s how one can come justifiably to believe something implying that [there are numbers]”.\textsuperscript{186}

\textsuperscript{185} Published in {Burgess, 2005 #487}.

\textsuperscript{186} In {Burgess, 2005 #487}, Section 6.
The fifth campaign was carried out by the Burgess-Rosen team in their {Burgess, 2005 #483}, where they produce a lengthy argument to show that mathematical objects exist in the actual world (and hence that nominalism is false). Since this argument is distinguished by its premises that were described by the authors as “scarcely deniable”, I shall refer to it as “the scarcely deniable argument”.

In a previous work,\textsuperscript{187} I responded to several of these anti-nominalist campaigns, but I shall reconsider these arguments in this work in order to investigate the question of whether there is some single common erroneous assumption underlying every one of these campaigns or whether there are several different such errors. In this chapter, I shall take up the first of these campaigns, which Burgess and Rosen believed had completely demolished various attempts to construct a nominalistic version of classical analysis. The third campaign was rebutted in Section 15 of the previous chapter. I shall take up the remaining three campaigns in the next chapter.

1. The First Campaign: Against Nominalistic Reconstructions of Mathematics

In 1983, Burgess published the paper “Why I Am Not a Nominalist”, which attempted to undermine the various nominalistic reconstructions of mathematics and science that had been put forward by philosophers to answer Quine’s “challenge to the nominalist”.\textsuperscript{188} Burgess’s paper was directed at showing that the "reconstructivists" way of answering Quine’s challenge fails

\textsuperscript{187} Chihara, 2004 #468], Chapter 6.

\textsuperscript{188} See [Burgess, 1997 #227], Part II. It should be noted that the “reconstruction” attributed to me is the predicative version discussed in the previous chapter.
utterly. The 1983 paper was just the first of many papers Burgess was to publish on the topic of nominalism. With the doggedness of Inspector Javert pursuing Jean Valjean, Burgess has been seeking, over a span of more than thirty years, to give a decisive refutation of nominalism.

As the above title of his 1983 paper suggests, Burgess seems to be giving his own personal reasons why he is not a nominalist. However, in reading the paper, it becomes evident that Burgess intended the arguments in his paper to be taken as providing convincing reasons why all philosophers should reject nominalism.

Burgess’s argument proceeds by first presenting the following definitions, which explicate the crucial terms of his case against the nominalist.

An instrumentalist maintains that "science is just useful mythology, and no sort of approximation to or idealization of the truth".

A hermeneutic nominalist holds that when the language of mathematics is properly analyzed, one will see that the scientist, in asserting mathematical propositions, is not really asserting the existence of any abstract mathematical objects.

A revolutionary nominalist is a nominalist who is proposing a new version of science--a nominalistic version--in which there are no assertions of the existence of abstract mathematical objects. The nominalist’s version of science is intended to be a theory that supersedes or replaces present day Platonic versions of science (hence the term ‘revolutionary’).
Just one premise underlies Burgess’s argument:

[5-*] A nominalist is either an instrumentalist, or a hermeneutic nominalist, or a revolutionary nominalist.

Now since none of the nominalists that the argument targets (that is, the nominalists who advance their nominalistic reconstructions of mathematics) is an “instrumentalist” according to the above definition, one can conclude from [5-*] that:

[5-1] Each nominalist targeted must be either a hermeneutic nominalist or else is a revolutionary nominalist.

Let us call [5-1] “Burgess’s fork”.

Suppose, Burgess continues, that a targeted nominalist is a hermeneutic nominalist. In that case, the nominalist’s reconstruction of mathematics is being put forward as an analysis of actual mathematical language. On this hypothesis in particular, my own nominalistic version of Wang’s Sigma Omega is being advanced as an actual analysis of the language of real mathematics—an absurd position for a nominalist to maintain. Since none of the targeted philosophers have provided any evidence for the soundness of any such analysis, we can conclude that none of them can be reasonably considered a successful hermeneutic nominalist.

Could the nominalist then be a revolutionary nominalist? That is, could the targeted nominalist’s version of science be a new theory that should supersede or supplant present day Platonic versions of science? There are
strong practical reasons for not attempting to replace contemporary scientific theories with such nominalistic reconstructions, say by replacing the mathematics now used in science with a nominalistic version of predicative mathematics. For example, Burgess writes:

[A]ny major revolution involves transition costs: the rewriting of text books, redesign of programs of instructions and so forth. . . . [I]t would involve reworking the physics curriculum [say, to allow the student to take courses to learn the logical and modal concepts required to understand predicative set theory]. ([Burgess, 1983 #292], p. 98).

Thus, Burgess concludes:

Unless he is content to lapse into a mere instrumentalist or "as if" philosophy of science, the philosopher who wishes to argue for nominalism faces a dilemma. He must search for evidence for an implausible hypothesis in linguistics, or else for motivation for a costly revolution in physics. Neither horn seems very promising, and that is why I am not a nominalist. ([Burgess, 1983 #292], p. 101).

The trouble with Burgess’s dilemma argument against the nominalists is Burgess’s fork--that is, premise [5-1]. Why should we accept it? No real argument has been given to support it. It is certainly not a self-evidently true proposition. Indeed, it does not strike me as being even slightly plausible. Not surprisingly, the targeted nominalists have simply rejected Burgess’s fork.189

In the light of such responses, Burgess seems to have reassessed his own reasoning. He has not, however, given up on the basic strategy of the original argument. Instead, he has teamed up with his former Ph. D. student Gideon Rosen190 to produce a new criticism of the nominalists’ reconstructions of

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189 See, for example, [Chihara, 1990 #34], Chapter 9, Section 2, and [Hellman, 1998 #489], especially pp. 342-3.

190 Rosen is now a faculty member at Princeton.
mathematics, but one that uses the basic elements of the earlier 1983 work. This Burgess-Rosen argument will be the principal subject of this chapter.

2. The New Burgess-Rosen Objection to Nominalistic Reconstructions of Mathematics

In *A Subject With No Object*, a work published roughly fourteen years after the publication of “Why I Am Not a Nominalist”, Burgess and Rosen focus (in the concluding chapter of the book) on nominalistic reconstructions of mathematics and science, taking account of the more recently published reconstructions, such as those of Geoffrey Hellman and myself. They do this by taking up the question ‘What value is there to be found in this body of nominalistic work?’. Their overall answer to this question is suggested by the very title of the book. Nominalism is “a subject with no object”—the various nominalistic reconstructions, it is suggested, have no significant object or purpose. Such a suggestion is reinforced by their finding essentially nothing of genuine value in these reconstructions (except as providing, perhaps, an aid to the imagination both in describing what the science of an alien intelligence could be like and also in showing how it might have shaped our theories of the world).

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191 See [Burgess, 1997 #227], Part II.

192 Thus, Stewart Shapiro writes in his review of the book:

> [T]he main sections of the [concluding] chapter contain sharp and penetrating criticisms of the nominalistic projects and of the whole point of nominalism. It is about as “conclusive” as polite, professional philosophy gets nowadays. ([Shapiro, 1998 #364], p. 600, italics mine).

I shall now examine the Burgess-Rosen critique in detail. To the question, “What are the nominalistic reconstructions good for?”, Burgess and Rosen sketch two possible replies:

(A) The reconstruction provides us with a hermeneutical “analysis of the ordinary meaning of scientific language” ([Burgess, 1997 #227], p. 208). This is the response of the “hermeneutical nominalist”.

and

(B) It provides us with an alternative version of science, which is better than, and to be preferred to, our present day versions of science. This is the response of the “revolutionary nominalist”.

The reader will easily recognize the two principal alternatives of the dilemma posed to the nominalist in Burgess’s 1983 paper: (A) is essentially the hermeneutical alternative, and (B) is the revolutionary alternative. That is, in answer to the question ‘What are the reconstructions good for?’, (A) is the answer of the hermeneutical nominalist, and (B) is the answer of the revolutionary nominalist. And it turns out that the Burgess-Rosen concluding inference is fundamentally the same as that given in the earlier paper: it relies upon Burgess’s fork. I shall not discuss their evaluations of the hermeneutical nominalist, since I do not consider the hermeneutical nominalist’s reply to be at all plausible. I shall, however, give some consideration to their evaluations of the response of the revolutionary nominalist, because it makes the meaning of response (B) clearer than what one finds in the bare statement of (B).
It is clear from their evaluation of response (B) that these authors are comparing contemporary science with an imagined scientific theory in which the mathematics of the theory is regarded as being carried out entirely in a nominalistic reconstructed version of mathematics. Thus, their assessment relies heavily on such methodological criteria as:

Consistency or coherence with familiar, established theories, or where these must be amended, minimality of the amendment.

They go on to note that such factors are very important in scientific work, because they “make for ease of use of the theory” in many ways (p. 210). Not surprisingly, they end up concluding that response (B) is unsatisfactory. Then, having judged that neither of the two responses (A) and (B) is at all acceptable, they conclude: “Since anti-nominalists reject all hermeneutic and revolutionary claims, from their viewpoint the various reconstruals are all distinct from and inferior to current theories ([Burgess, 1997 #227], p. 238)”. They then go on to infer that the nominalist’s reconstructions certainly cannot produce any advancement of science! Thus the reader is left with the impression that the Burgess-Rosen investigations into the plausibility of replies (A) and (B) had shown that the nominalist’s reconstructions could produce no such advancement. This impression is reinforced by the fact that the very next section of the chapter is entitled: “ENVOI: RECONSTRUAL WITHOUT NOMINALISM” — a section in which these authors explore the question of whether any value at all can be salvaged for the reconstructions. The section ends with the suggestion that, perhaps, these reconstructions “can contribute to enlarging our understanding of the character of our science by showing what the science of other intelligent creatures might be like” (p. 243). Evidently,
these reconstructions have been found to be good for little more than what a science fiction novel might accomplish.

3. The Subsidiary Considerations: The “More Direct Test”

Finally, there is what Burgess and Rosen call in their book “a more direct test” [of whether a nominalistic reconstruction is scientifically so superior to current mathematical theories that it ought to be credited in preference to the current ones]. This test is to consist in submitting a paper presenting a nominalistic reconstruction of mathematics to, say, the Physical Review, and then in assessing the reactions of the journal. Burgess and Rosen note that the nominalist’s paper would undoubtedly be rejected. They then ask the reader to imagine how a practicing scientist reviewing the submission might react to the nominalist’s version of mathematics in which all reference to mathematical objects is avoided. They ask: would the scientist regard such a mathematical innovation as a genuine scientific advancement? Of course not, they seem to imply, writing:

Would economics profit by its own lights from the demonstration that reference to choice functions and indifference curves could be replaced by quantification over material objects of some unobvious sort? (Burgess, 1997 #227, p. 210).

And to cement their negative evaluation of all nominalistic reconstructions, they add the rhetorical conclusion:

Certainly it would be madness to suggest that applied physicists or economists interested in predicting the perturbations of Mars before a space-shot or the fluctuations of the peso before an intervention by the Central Bank should carry out their reasoning in the language of synthetic geometry or modal logic rather than of mathematical analysis. (Burgess, 1997 #227, p. 211).
Evidently, Burgess and Rosen regard such rhetorical musings as providing powerful grounds for thinking that nominalistic reconstructions of mathematics achieve nothing that can be judged to be an advancement of science. I shall return to an assessment of this “more direct test” and its implications shortly.

4. The Link Between the 1983 Paper and the 1997 Book

Obviously, there is a close connection between {Burgess, 1983 #292} and the last section of {Burgess, 1997 #227}. However, we get further insights into this connection from Burgess himself. In his collection of previously published papers {Burgess, 2008 #544}, there is an Introduction written for his 1983 paper asserting that this early paper “first introduced a distinction between hermeneutic and revolutionary nominalism” (p. 3). He then continues:

The formulations a decade and a half later in A Subject With No Object . . . are, largely owning to my co-author Gideon Rosen, who among other things elaborated and refined the hermeneutic/revolutionary distinction, more careful on many points than those in this early paper. This piece [the 1983 paper], however, seemed to me to have the advantage of providing a more concise, if less precise, expression of key thought underlying that later book than can be found in any one place in the book itself. ({Burgess, 2008 #544}, p. 3).

In “Mathematics and Bleak House”, which is reprinted in {Burgess, 2008 #544}, Burgess expresses what he thinks the book A Subject With No Object accomplished:

Today, a couple of years after publication, it is beginning to seem that the main achievement of our book will have been to provide a decent burial for the hard-working, laborious variety of nominalism. For almost everything that has come forth since from the nominalist camp has
represented a light-fingered, larcenous variety, which helps itself to the utility of mathematics, while refusing to pay the price either of acknowledging what mathematics appears to say is true, or of providing any reconstrual or reconstruction that would make it true. ([Burgess, 2008 #544], p. 46).

I disagree with Burgess's assessment of what their book achieved. One of the goals of this chapter is to show that Burgess’s announcement of the death and burial of nominalistic reconstructions of mathematics was decidedly premature and much overblown.

5. Frege’s Reconstruction and Burgess’ Dilemma Argument

As an indication that something is drastically amiss with the Burgess-Rosen argument, let us consider one pioneering work in the foundations of mathematics that is widely considered to be of great importance for foundational studies: namely Frege’s logicist version of arithmetic, which I discussed in Chapter 1, Section 3. In his Fundamental Laws of Arithmetic, Frege produced just the sort of thing that Burgess and Rosen call “a reconstruction of mathematics”—in this case, a reconstruction of arithmetic.\textsuperscript{194} Hence, it is natural and reasonable, at least to anyone convinced by the above Burgess-Rosen argument, to run the Fregean reconstruction through the dilemma hurdle that Burgess and Rosen have raised for nominalist’s reconstructions of mathematics. In other words, it is appropriate to determine if Frege’s logical reconstruction succeeds either as an accurate “hermeneutical analysis of the ordinary meaning of” the language (or languages) of real arithmetic or else as

\textsuperscript{194} Cf. Michael Resnik’s observation:

Frege’s critical discussion in the \textit{Grundlagen} makes it quite clear that he thought that prior to his analysis, the concept of number appeared “as through a haze.” Thus, from the standpoint of his 1914 essay [the essay is discussed by Resnik in ([Resnik, 1980 #111], pp. 182-4] he would regard his definitions as providing a \textit{reconstruction of arithmetic} ([Resnik, 1980 #111], p. 185, italics mine).
an alternative version of arithmetic that is better than, and preferred to, the real version.

Interestingly, even though Burgess has written a whole book surveying the recent "Frege-inspired reconstructions of mathematics"—a book that, he tells us, can be considered as a sort of "companion to the survey of various nominalist strategies" of his A Subject With No Object and one that obviously required a substantial amount of research time and effort to write--he never connected the dots and subjected Frege’s own reconstruction of arithmetic to the dilemma with which he, Burgess, confronted the nominalist’s reconstructions. Why did he not? Was he simply blinded by the great reputation of Frege’s foundational work?

Well, the proponents of the Burgess-Rosen argument might reply that their dilemma reasoning was directed at reconstructions of mathematics and not reconstructions of a relatively small part of mathematics, such as arithmetic. True enough. I will grant, for the sake of argument, that such a reply has some initial plausibility, but still, I will show that an examination of the Burgess-Rosen reasoning as it applies to reconstruction of arithmetic will prove to be illuminating. For example, if Frege’s reconstruction of arithmetic fails the test, it should lead to some insights into why the arguments fails in the case of arithmetic, which may lead to some insight into what assumptions (that are not acknowledged) underlie the Burgess-Rosen argument against nominalistic reconstructions of mathematics.

So let us apply the Burgess-Rosen dilemma strategy to Frege’s reconstruction of arithmetic to see what results. In particular, let us ask: Does Frege’s reconstruction provide an accurate hermeneutical analysis of the ordinary meaning of the terms and expressions of real arithmetic? Do the
millions of factory workers, accountants, researchers, and scientists, who use arithmetic in their many daily professional pursuits, use such terms as ‘zero’, ‘one’, ‘plus’, ‘times’, and ‘number’ with the senses attributed to them by Frege’s definitions? Here, Burgess’s argument against nominalistic reconstructions [when applied to Frege’s reconstruction of arithmetic] should be noted:

As a thesis about the language of science, [Frege’s analysis of arithmetical terms] is, I presume, subject to evaluation by the science of language, linguistics. . . . [readers will be] familiar with the kinds of evidence cited in responsible linguistic investigations to support such hypotheses.195

Does Frege provide any expert and well devised linguistic evidence in support of the thesis that his definitions accurately capture the ordinary senses of these arithmetical terms? Not at all. The kinds of linguistic evidence required by Burgess to support the thesis in question was supplied neither by Frege nor by anyone else. And intuitively speaking, the thesis in question is highly implausible—especially given that even Russell came to feel that he, himself, did not understand one of the principal primitive concepts of Frege’s logical system used to analyze the concepts of arithmetic, namely the concept of “the extension of a concept”.196 Can we suppose that ordinary English speakers—even children—use arithmetical terms with Frege’s senses attributed to them by the logical definitions of the Fundamental Laws? And since Frege provides no serious evidence supporting the thesis in question, there are ample grounds for concluding that Frege’s reconstruction of arithmetic fails as an accurate

195 This is a quote from [Burgess, 1983 #292], p. 97, with my insertions indicated by the square brackets.

196 Thus, Russell wrote to Frege: “Every day I understand less and less what is really meant by ‘extension of a concept’.” ([Frege, 1980 #255], p. 139).
hermeneutical analysis of the ordinary meaning of the terms and expressions of real arithmetic.

Then does Frege’s reconstruction of arithmetic provide us with a worthy replacement of real arithmetic—one that is better than, and to be preferred to, our presently used version of arithmetic? It might be said that the reconstruction is clearly inferior to real arithmetic on the grounds that Frege’s system has been proven to be logically inconsistent. But we might consider this revolutionary alternative in the light of more recent work showing how Frege’s set of fundamental laws can be modified to yield a consistent system from which the usual derived laws of arithmetic can be proved.\textsuperscript{197} Still, Frege’s reconstruction is simply much too logically complicated and mathematically sophisticated to serve as a reasonable replacement for real arithmetic. Even below average IQ people can, at an early age, learn and master elementary arithmetic, but not many people with below average IQ could, at an early age, learn and truly master Frege’s reconstruction of arithmetic. All the practical difficulties of teaching enough of Frege’s logic to elementary school students to enable them to understand and to learn the arithmetic of the Fundamental Laws would seem to doom the revolutionary alternative. Also bringing in the pragmatic considerations of the sorts discussed earlier would count heavily in favor of rejecting the revolutionary alternative.

So should we conclude, in accordance with the Burgess-Rosen reasoning, that Frege’s reconstruction of arithmetic makes no scientific, mathematical, or logical advances and is only good for helping us to imagine what an alien science could be like? The ‘neo-Fregeans’ would certainly reject such an inference,\textsuperscript{197} See, for example, [Hale, 2001 #511].
since they believe that Frege’s reconstruction of arithmetic in his Fundamental Laws promises, with only some small adjustments, “defensible and attractive answers to some of the most fundamental ontological and epistemological questions which any serious philosophy of mathematics must confront” ([Hale, 2001 #511], p. vii). Would it not be more sensible to question the reasoning of the Burgess-Rosen dilemma?

I bring up Frege’s reconstruction of arithmetic because Burgess remains convinced that a nominalist’s reconstruction of mathematics must be either hermeneutic or revolutionary if it is to be of any significant value in foundational studies of mathematics or other area of mathematics, despite all that nominalists have written against the necessity of having to accept one of the horns of his dilemma. Thus, in his introduction to the collection [Burgess, 2008 #544], Burgess comments on his 1983 paper, where he first published his dilemma argument:

On two points my view has not changed at all over the past years. First, while nominalists would wish to blur what for Rosen and myself is a key distinction, and avoid taking a stand on whether they are giving a description of the mathematics we already have (hermeneutic) or a prescription for a new mathematics to replace it (revolutionary), gesturing towards a notion of “rational reconstruction” that would somehow manage to be neither the one nor the other, I did not think this notion had been adequately articulated when I first took up the issue of nominalism, and I have not found it adequately articulated in nominalist literature of the succeeding decades. ([Burgess, 2008 #544], pp. 4-5).

Here, I should emphasize that I, at least, did not, in [Chihara, 1990 #34], “avoid taking a stand on whether” I was giving a description of the mathematics we already have. I was explicit and clear that I was not giving any such description nor a prescription for a new mathematics to replace this mathematics. This is why I stated that my Constructibility Theory was devised to be an instrument or
tool for philosophical and logical analysis and assessment and showed, by way of various examples, how the theory could function as a useful instrument for analyzing philosophical arguments. It was abundantly clear that my theory was not intended to be a “rational reconstruction” of anything, in the sense in which Burgess uses the expression. (I shall say much more about my responses to the Burgess-Rosen argument shortly).

6. A Diagnosis of the Burgess-Rosen Critique

In his book The Origins of Financial Crises, George Cooper discusses the following argument of the eminent economist Paul Samuelson:

Everybody receives money for what he sells and uses this money to buy what he wishes. If more is wanted of any one good, say shoes, a flood of new orders will be given for it. This will cause its price to rise and more to be produced. Similarly, if more is available of a good like tea than people want, its price will be marked down as a result of competition. At the lower price people will drink more tea, and producers will no longer produce so much. Thus equilibrium of supply and demand will be restored.

What is true of the markets for consumers’ goods is also true of markets for factors of production such as labor, land, and capital inputs. (Cooper, 2008 #534), pp. 4-5).

Allowing that Samuelson gives a convincing explanation of how equilibrium is established in the marketplace for goods, Cooper takes issue with the last sentence in the above quotation, writing: “but when it comes to the markets for labour, land and capital inputs, there is no explanation of the mechanisms though which equilibrium is established” (Cooper, 2008 #534), p. 6). For those markets, Cooper tells us, Samuelson offers us, by a kind of slight of hand, only “proof by assertion”.

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198 Samuelson received the Nobel Prize in Economics in 1970.
Something similar is to be found in the Burgess-Rosen argument that is purported to show that all the nominalistic reconstructions of mathematics accomplish nothing of significant value. There is convincing argumentation to show that these nominalistic reconstructions succeed neither as hermeneutical analyses, nor as revolutionary replacements, of current mathematical theories, but, by what seems to be a kind of slight of hand, with no argumentation or explanation, they conclude that these reconstructions do not succeed in making any advancement in science (evidently relying upon Burgess's fork). And they then go on to wonder what, if anything, the nominalistic versions of mathematics or science do accomplish. It seems that Burgess and Rosen are also guilty of what Cooper calls “proof by assertion”.

The Burgess-Rosen team may have thought that they had produced a importantly improved version of the anti-nominalism argument appearing in the 1983 paper, but as I analyze the reasoning, it is pretty much the same basic argument with essentially the same questionable assumptions. Thus, in a way that brings to mind the earlier paper, Burgess and Rosen tacitly assume that there are only two distinct alternative ways in which the nominalistic-reconstructivist can reasonably respond to the question ‘What are the reconstructions good for?’—respond, that is, in a way that allows the reconstruction to be of some scientific or mathematical value. And by rejecting these two possible responses that the nominalist might make, Burgess and Rosen infer that no reasonable response can be given to show how the nominalist’s reconstruction can have any significant value for science or mathematics.

Why did these authors assume that (A) and (B) are the only two reasonable answers to the question ‘What are the reconstructions good for?’.
In other words, why did Burgess and Rosen accept Burgess’s fork? It was, ultimately, because they had accepted the Fregean Assumption.\(^{199}\)

Consider again answer (B). Why should one suppose that, if the reconstruction is not hermeneutic, then the only reasonable response remaining to the question above is to answer that the reconstruction provides us with an alternative version of science, which is better than, and to be preferred to, our present day versions of science? Why could not the nominalist’s reconstruction of mathematics contribute something significant to our understanding of mathematics or logic without justifying a complete “revolution” of the sort demanded by (B)?

7. Burgess and Rosen Made the Fregean Assumption

To understand why I attribute an acceptance of Burgess’s fork by the Burgess-Rosen team, it is important to see, first of all, that they made the Fregean Assumption: they believed that the theorems of mathematics are propositions about the actual world—indeed, that these theorems are true propositions about the world. Why do I attribute this Fregean view to these philosophers? Consider the following quotation:

\(^{199}\) Cf. Shapiro’s comment:

I suspect that the nominalist will accuse Burgess and Rosen of proposing a false dilemma, claiming that the revolutionary approach and the hermeneutic approach do not exhaust the options for the reconstructive problem. I do not have a third orientation to propose on behalf of nominalism, and so we will leave the dialectic with a challenge for the nominalist to articulate just what he or she claims on behalf of the detailed reconstructive system. ([Shapiro, 1998 #364], p. 609).

Of course, Shapiro could not come up with a third option because he, too, had accepted the Fregean Assumption.
Before we come to philosophy, we have a fairly uncritical attitude towards, for instance, standard results of mathematics, or such of them as we have learned about. Having studied Euclid’s Theorem, we are prepared to say that there exist infinitely many prime numbers. Moreover, when we say so, we say so without consciously mental reservations or purpose of evasion. . . . Why not just acquiesce in the minimal non- or un-nominalism many of us find ourselves coming to philosophy with? . . . [W]hile our positive conception of the nature of the numbers in whose existence we thus acquiesce may be of the haziest, we at least understand that numbers are not supposed to be like ordinary concrete things like rocks or trees or people. . . . In this sense, we acquiesce not only in their existence, but also in their abstractness. ([Burgess, 1997 #227], pp. 10-1).

From the quotation, it can be seen that Burgess and Rosen think that it is reasonable to infer, from Euclid’s infinity-of-primes theorem, that there exist in the actual world infinitely many prime numbers. In other words, Euclid’s theorem is taken to be asserting that there are infinitely many prime numbers in the actual world. This is because they are taking mathematical theorems to be propositions about the actual world. They are, in short, making use of the Fregean Assumption. (That they are indeed Fregeans will become even clearer later in this chapter).

Thus, from their Fregean perspective, nominalism can be seen to be incompatible with the truth of the theorems of real mathematics. Such a Fregean way of regarding nominalism was clearly illustrated by the decision of Field (who, you will recall, was a Fregean), not to reject nominalism, but rather to maintain that no part of mathematics is true (as I explained in Chapter 3). Clearly then, from the Fregean perspective of Burgess and Rosen, the philosopher who develops a nominalistic reconstruction of mathematics will appear to be putting forth a version of mathematics that is incompatible with classical mathematics and hence one that is supposed to replace classical mathematics. In other words, from the Fregean perspective, any philosopher who puts forward a nominalistic reconstruction of mathematics will appear to be
a “revolutionary” nominalist. Thus, one can see why Burgess and Rosen adopted the simple “either (A) or (B)” dichotomy.\textsuperscript{200}

On the other hand, from my perspective—one in which the Fregean Assumption is rejected (on the basis of arguments to be provided in Chapter 8)—the Burgess-Rosen acceptance of the simple dichotomy seemed to me to be completely arbitrary and unreasonable, especially when one is trying to determine and assess the value of a particular reconstruction of mathematics. Thus, the Fregean Assumption is a crucial unstated presupposition of the Burgess-Rosen anti-nominalistic-reconstructions-of-mathematics argument. And, it will become obvious later that:

The Fregean Assumption is an unstated premise of practically all of the Burgess and the Burgess-Rosen anti-nominalism arguments!

8. The Reconstruction of Mathematics in PM

Before examining the above Burgess-Rosen argument in detail, let us recall the well-known way of interpreting (or modeling) the sentential calculus in

\textsuperscript{200} Cf. Shapiro’s comment:

I suspect that the nominalist will accuse Burgess and Rosen of proposing a false dilemma, claiming that the revolutionary approach and the hermeneutic approach do not exhaust the options for the reconstructive problem. I do not have a third orientation to propose on behalf of nominalism, and so we will leave the dialectic with a challenge for the nominalist to articulate just what he or she claims on behalf of the detailed reconstructive system. (\{Shapiro, 1998 #364\}, p. 609).

Of course, Shapiro could not come up with a third option because he, too, had accepted the Fregean Assumption.
terms of switching circuits. Now no one would suppose that, to be of any scientific or mathematical value, such an interpretation would have to be either a hermeneutic or a revolutionary interpretation of the sentential calculus. So why should we suppose that, to be of any scientific or mathematical value, a nominalistic interpretation of a formal version of classical mathematics would have to be either hermeneutic or revolutionary? What could be motivating the Burgess-Rosen argument?

To see more clearly that the Burgess-Rosen argument is fundamentally unsound, consider one of the reconstructions of mathematics that is widely regarded as having great importance, especially in logic and the foundations of mathematics: this is the mathematical system constructed in the historically important three volume work Principia Mathematica. The eminent authors of this work, Bertrand Russell and Alfred North Whitehead, tell us on the very first page of their epic work that the system they are presenting was framed to “solve the paradoxes” of logic and mathematics that were so troubling to scholars in the early part of the twentieth century. Their strategy was to develop a paradoxes-free logical system within which a version of classical analysis would be constructed. Thus, the logical system in PM was designed to allow the derivation of mathematics that we would now classify as a ramified type theoretical version of set theory, while at the same time preventing the construction of all the known paradoxes. Within this set theory, the authors developed enough classical mathematics to convince most mathematicians and

201 See, for example, [Enderton, 1972 #75], Chapter 1, Section 1.6.
philosophers who have studied the system that essentially all of classical mathematics can indeed be reproduced in PM.\textsuperscript{202}

Of course, the set theory of the system was disguised by the “no-class” way in which set theory was developed,\textsuperscript{203} so one might be inclined to think that the mathematics of PM is not Platonic—especially since Russell sometimes said that classes were “logical fictions”.\textsuperscript{204} But Russell’s no-class theory of classes consisted in treating class terms as “incomplete symbols”, so that statements about classes turn out to be short-hand for statements about what he called “propositional functions”.\textsuperscript{205} However, as Quine has emphasized in several places, propositional functions are little different from classes and can be classified as “attributes”—entities that are clearly Platonic.\textsuperscript{206} Hence, the no-class theory of PM can be reasonably regarded as only yielding a reduction of sets to attributes, which is not a nominalistic reduction.\textsuperscript{207}

\begin{footnotesize}
\footnote{It is noteworthy that in the historic paper in which Gödel presented his Incompleteness Theorems, the STT version of PM was taken as the system that was explicitly proven to be incomplete. The reason for taking PM as this target system was explained as follows: \textquote{The most comprehensive formal systems that have been set up hitherto are the systems of Principia mathematica . . . on the one hand and the Zermelo-Fraenkel axiom system of set theory (further developed by J. von Neumann) on the other. These two systems are so comprehensive that in them all methods of proof today used in mathematics are formalized, that is reduced to a few axioms and rules of inference.}}
\end{footnotesize}

\textsuperscript{203} See [Chihara, 1973 #48], Chapter 1, Section 4, for a detailed discussion of the “no-class” theory of PM.

\textsuperscript{204} This view led Russell to some strange metaphysical theses. For example, at one time, Russell analyzed ordinary things like tables and chairs to be classes of “appearances”, and since classes were taken by him to be “logical fictions”, tables and chairs were also regarded as “logical fictions” ([Russell, 1956 #466], pp. 274-5).

\textsuperscript{205} For an explanation of the key definitions in PM in terms of which set theoretical sentences were to be translated into sentences about propositional functions, see [Chihara, 1973 #48], p. 15.

\textsuperscript{206} See, for example, [Quine, 1963 #368], p. 251.

\textsuperscript{207} Ibid.
PM’s logical reconstruction of mathematics will now be subjected to the Burgess-Rosen form of argumentation. The Burgess-Rosen team considers only two ways in which a reconstruction of mathematics can have scientific or mathematical merit: a) the reconstruction may provide an analysis of the meanings of the terms and sentences of the mathematical system or theory being reconstructed; or b) the reconstruction may be sufficiently superior, on scientific grounds, to the current mathematical system or theory being reconstructed, to the extent that it is worthy of supplanting or replacing the current system.

Let us now ask: How does PM’s reconstruction fare when regarded as providing hermeutical analyses of the meanings of mathematical terms and sentences? Very poorly. First of all, the authors of PM did not even intend their reconstructed version of mathematics to provide an analysis of the mathematics that actual practicing mathematicians were using: they were attempting to put forward a new version of mathematics in order “solve” the paradoxes. Their ramified type theory was certainly not a feature of the actual mathematics being practiced: it was invented by Russell to block certain sorts of paradoxes from appearing in the mathematics. Besides, the “no-class” method of reconstructing set theory was certainly not regarded by anyone as giving an accurate representation of the meanings of such terms as ‘set’ and ‘number’ in real mathematics.

Consider then the only alternative way that, according to Burgess and Rosen, the reconstruction could have scientific merit. Would the acceptance of PM’s reconstruction provide us with a version of science that is so scientifically superior to the real mathematics of their day that it would deserve to supplant it? Certainly Russell, at times, expressed some ideas to suggest that he
considered himself to be advocating a sort of revolution in logic and mathematics. But the question is not whether the authors of PM considered themselves to be revolutionary philosophers, but rather whether, in fact, their reconstruction deserved to supplant the real mathematics being practiced.

Burgess and Rosen suggest that one way a philosopher might judge whether a reconstruction is scientifically superior to real mathematics would be “to examine the features of the [reconstruction] and compare them to a list of recognized scientific merits” ([Burgess, 1997 #227], p. 209). The list, as the authors give it, includes:

(v) consistency or coherence with familiar, established theories, or where these must be amended, minimality of the amendment.

and

(vi) perspicuity of the basic notions and assumptions.

Using these two criteria, we see that the unfamiliarity of the ramified theory of types, the strangeness of the no-class theory, the use of higher order predicate logic with its unusual syntactic and logical notation, the unusually expressed axioms of PM, all count heavily against judging this reconstruction to be superior to the real mathematics. Focusing on criterion (vi), PM’s very complicated and difficult to grasp notion of propositional function and the intricate way they are classified in “types” (yielding what became known as the

208 Thus, they claim that “the reconstructive nominalist seems to be giving far less weight to factors (v) and (vi), familiarity and perspicuity.” And they assert that those factors “are very important in scientific work” in so far as they make for ease of use of the theory, by facilitating communicating and testing the theory, and in revising and extending it. ([Burgess, 1997 #227], pp. 210-11).
“ramified theory of types”)\textsuperscript{209}-- certainly undermines any serious attempt to replace real mathematics with the system proposed by Russell and Whitehead.

Furthermore, we can also cite, in evaluating the merits of replacing real mathematics with the mathematics of PM, the considerations that were used by Burgess in his 1983 paper: the substantial cost of rewriting text books, redesigning programs of instructions so that physics students would be able to learn enough logic to understand the complexities of [the ramified theory of types], etc. certainly point to the conclusion that “it is virtually certain that discarding the orthodox versions, leaving only [the PM] version available would have much disutility” ({Burgess, 1997 #227}, p. 211).

The historical fact that at no time was there a serious and widespread movement to replace orthodox mathematics with the reconstructed mathematics of PM also leads to the conclusion that the reconstruction did not have sufficient scientific superiority to warrant the kind of revolution under consideration required by Burgess.

We now can see that, if we reason the way Burgess and Rosen did in analyzing nominalistic reconstructions of mathematics, we shall have to conclude that the reconstructed mathematics of PM is distinct from and inferior to standard real mathematics and is therefore of no significant value to science or mathematics. Such a conclusion would be shocking--would it not?--given

\textsuperscript{209} Consider the following comments from the well-known mathematical logician Hao Wang: “So far we have largely refrained from discussing the philosophical passages in \textit{Principia}, especially pp. 37-59 [in which the notion of ‘proposition function’ appears throughout in important ways]. The writer can still remembering puzzling over these pages as a freshman in 1940, hour after hour, with little success.” ([Wang, 1965 #570], p. 19).
that PM is considered by experts in the field to be one of the truly great works in the history of foundational studies of mathematics.\textsuperscript{210}

9. A Mathematical Reason for Doubting Burgess’s Fork: Nonstandard Analysis

What I have been calling “Burgess’s Fork”—the thesis that any alternative version of mathematics must be either a hermeneutic version or a revolutionary version to be of any significant mathematical or scientific value—is also undermined by examples that are purely mathematical. The development of nonstandard analysis was first reported by Abraham Robinson in a seminar at Princeton in November of 1960. Robinson tells us that he used the concepts and methods of contemporary mathematical logic to provide a “framework for the development of the Differential and Integral Calculus by means of infinitely

\textsuperscript{210} An indication from the writings of experts of the importance of PM for the development of mathematical logic and the foundations of mathematics is to be found in the 761 page work The Development of Logic, authored by the important Oxford logician and historian of logic William Kneale and his wife Martha, where they write: “our primary purpose has been to record the first appearances of those ideas which seem to us most important in the logic of our day” (\cite{Kneale, 1962 #102}, p. v). Since they devote a whole section to the theory of types of PM, one can infer that these experts regard PM as having advanced some truly important ideas for logic.

More evidence for the importance of PM can be found in the work of the distinguished historian of mathematics I. Grattan-Guinness: The Search for Mathematical Roots, 1870 – 1940: Logics, Set Theories and the Foundations of Mathematics from Cantor Through Russell to Gödel. The scope and limits of this work is described with the words:

The story told here . . . begins with the emergence of set theory in the 1870s under the inspiration of Georg Cantor, and the contemporary development of mathematical logic by Gottlob Frege, and (especially) Giuseppe Peano. A cumulation of these and some related movements are achieved in the 1900s with the philosophy of mathematics proposed by Alfred North Whitehead and Bertrand Russell. . . . Their position was given a definitive presentation in the three volumes of Principia Mathematica (1910-1913). (\cite{Grattan-Guinness, 2000 #424}, p. 3).

Grattan-Guinness then devotes three chapters of meticulous research to the development, content, and influence of PM—which clearly shows that this scholar judges the work to have made significant advances in the field.

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small and infinitely large numbers” ([Robinson, 1996 #492], p. xiii). Basically, under the inspiration of Skolem’s construction of nonstandard models of arithmetic, Robinson constructed nonstandard models of analysis ([Robinson, 1996 #492], pp. viii-ix). These constructions were carried out by extending the totality of real numbers to a set that includes infinitely small and infinitely large numbers. The infinitely small numbers were then used to provide Robinson with “infinitesimals” which were in turn used in the definition of such concepts of the calculus as limit, differentials, continuity, and integrals, thus following in the tradition of such pioneers in analysis as Newton and Leibniz.

A few salient features of Robinson’s method of developing the calculus within this framework will be sketched below in order to facilitate the making of certain points relevant to the Burgess-Rosen dismissals. The extension of the real numbers mentioned above can be carried out in standard set theories, using the Axiom of Choice. After defining the real numbers as equivalence classes of Cauchy sequences of rational numbers, an equivalence relation over arbitrary sequences of real numbers is introduced which allows a new totality of numbers (henceforth the “hyperreal numbers”) to be defined to be equivalence classes of sequences of real numbers. An ordering relation $<$ is then defined over the hyperreals, which is then proved to be a linear ordering. Addition and multiplication of the hyperreals is defined so as to satisfy the usual algebraic laws. The resulting algebraic structure is a non-Archimedean ordered field.

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211 The exposition I give will follow the developments in {Keisler, 1971 #498} and {Keisler, 1976 #497}, both of which were based upon Robinson’s works.

212 The equivalence relation is defined using the notion of “free ultrafilter”.

213 Sums and products are defined as for Cauchy sequences.

In the following discussion, some striking features of the hyperreal numbers will be specified. Suppose that \( r \) is an equivalence class of Cauchy sequences of rationals which was defined earlier to be a real number. The hyperreal number containing the sequence \(<r, r, r, \ldots>\) will now be said to be a “standard real number”. A hyperreal number \( \varepsilon \) will be said to be infinitely small (or an “infinitesimal”) iff for every positive standard real number \( r \), \(-r < \varepsilon < r\).

It turns out that there are positive infinitesimals less than every positive standard real number. The only standard real number that is infinitesimal is the hyperreal number containing \(<0, 0, 0, \ldots>\), i.e. zero. If \( \alpha \) and \( \beta \) are hyperreal numbers whose difference \( \alpha - \beta \) is an infinitesimal, then \( \alpha \) will be said to be “infinitely close” to \( \beta \). It can be proved that each standard real number has a cluster of hyperreal numbers (its “monads”) that are infinitely close to it.

Clearly, the totality of hyperreal numbers does not fit our usual conception of the continuum. For example, our ordinary spatial intuitions do not allow there to be numbers greater than zero but less than all positive real numbers. To some mathematicians, the nonstandard model of the continuum is so counter-intuitive as to be repugnant. Not surprisingly, there has been what one might call a “backlash” against the development of the calculus within the framework of nonstandard analysis. For example, the mathematician Errett Bishop, in his review of H. Jerome Keisler’s nonstandard version of Elementary Calculus, wrote:

The technical complications introduced by Keisler’s approach are of minor importance. The real damage lies in his obfuscation and devitalization of those wonderful ideas. . . . Now we have a

\[\text{215 One mathematician with whom I corresponded about nonstandard analysis called the nonstandard numbers “bizarre” and thought the nonstandard method of referring to infinitesimals “ugly”}\]
calculus text that can be used to confirm [student's] experience of mathematics as an esoteric and meaningless exercise in technique. ([Bishop, 1977 #493], p. 2008).

Why might one believe that there are genuine advantages to teaching the calculus from the perspective of nonstandard analysis, despite the added complications and counter-intuitive aspects of the nonstandard numbers? For one thing, the basic concepts of the calculus can be given simpler and more intuitive definitions. For example, compare the standard $\varepsilon - \delta$ definition of continuity at a point with the following definition from nonstandard analysis:

A function $f$ is said to be continuous at $c$ iff:

(i) $f$ is defined at $c$; and
(ii) whenever $x$ is infinitely close to $c$, $f(x)$ is infinitely close to $f(c)$.

When one recalls that the standard definition of continuity of a function at a point contains a universal quantifier, followed by an existential quantifier, in a context in which the order of the quantifiers is crucial—something that many beginning students find confusing—one may be led to conclude that the above definition is definitely simpler and intuitively easier to grasp than the standard definition of continuity usually given.

That the basic definitions are indeed simpler and more intuitive receives some independent support from the experimental study of Kathleen Sullivan’s Ph. D. dissertation\(^{216}\). Consider the following problem:

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\(^{216}\) This was her doctoral dissertation of 1974, University of Wisconsin, Madison. I first learned of Dr. Sullivan’s dissertation from Joseph W Dauben’s {Dauben, 1988 #495}, which gives a fuller account of Sullivan’s work than I will give.
A function $f$ is defined as follows:

$$f(x) = \begin{cases} x^2 & \text{for } x \neq 2; \\ 0 & \text{for } x = 2. \end{cases}$$

The problem is to prove, using the definition of limit, that the limit of $f(x)$, as $x$ approaches 2, is 4.

Students from an experimental group of five calculus classes from four small private colleges and one large public high school, all learning calculus from the perspective of nonstandard analysis, were asked to solve the above problem (as well as many others). An equal number of students from a control group of students, comparable in ability, but who were learning the material in the standard way, were also asked to solve the problem. Here is how ‘limit’ is defined in Keisler’s book:

$L$ is the limit of $f(x)$ as $x$ approaches $c$ if, whenever $x$ is infinitely close, but not equal, to $c$, $f(x)$ is infinitely close to $L$.

Again, compared to the standard $\varepsilon$ - $\delta$ definition of ‘limit’, the nonstandard definition seems to be both much simpler and intuitively clearer. The results from this test were striking: Out of the 68 students from each group who were

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217 The results of Sullivan’s doctoral research are summarized in her {Sullivan, 1976 #494}.

218 The students in the control group were all students from the five institutions mentioned above.

219 That the students in the two groups were comparable in ability was confirmed by SAT Math Ability Scores. See {Sullivan, 1976 #494}, p. 372).

220 One instructor of one of the experimental classes commented: “When my most recent class were presented with the epsilon-delta definition of limit, they were outraged by its obscurity compared with what they had learned” ({Sullivan, 1976 #494}, p. 373).
given the problem, only two from the control group gave satisfactory proofs, whereas 14 from the experimental group were able to give satisfactory proofs. Furthermore, in the questionnaires filled out by the instructors of the classes, there was almost complete agreement among the teachers of the experimental classes that the proofs of nonstandard calculus are easier to explain and closer to intuition than those of standard calculus (Sullivan, 1976 #494, p. 373). Dr. Sullivan ends her paper on her “experiment” on the teaching of nonstandard calculus with the following conclusions:

[T]here does seem to be considerable evidence to support the thesis that this is indeed a viable alternate approach to teaching calculus. Any fears on the part of a would-be experimenter that students who learn calculus by way of infinitesimals will achieve less mastery of basic skills have surely been allayed. And it even appears highly probable that using the infinitesimal approach will make the calculus course a lot more fun both for the teachers and for students. (Sullivan, 1976 #494, p. 375).

Let us now ask the Burgess-Rosen question of value: “What is nonstandard analysis good for?” Evidently, there are only two principal answers that should be considered:

(A) Nonstandard analysis provides us with a hermeneutical “analysis of the ordinary meaning of scientific language”. This is the response of the hermeneutical nonstandard analyst.

and

(B) It provides us with an alternative version of science, which is better than, and to be preferred to, our present day versions of science. This is the response of the revolutionary nonstandard analyst.
Response (A) seems to be very questionable for the kind of reasons Burgess and Rosen gave for rejecting hermeneutical nominalism: as an analysis and exegesis of the ‘sense’ or ‘meaning’ of current mathematical language, the account is very implausible. And no significant linguistic evidence has been provided for accepting such an account of the meaning of this language.

As for response (B), one can make the same sort of reply to it that Burgess and Rosen made to the revolutionary nominalist. There are serious pragmatic reasons for not replacing our current versions of analysis (in mathematics and science) with nonstandard analysis: “it is virtually certain that discarding the orthodox versions, leaving only [the nonstandard] version available would have much disutility”. First of all, one can reply pretty much as Burgess did to the revolutionary nominalist in his dilemma argument:

> [A]ny major revolution involves transition costs: the rewriting of text books, redesign of programs of instructions and so forth. . . . [I]t would involve reworking the physics curriculum [to allow the student to take enough model theory and mathematical logic to understand various fundamentals of the nonstandard approach].

One could also make use of the “recognized scientific merits” which descriptive methodologists ”have largely agreed upon”. One could cite, in particular, the merit

(v) consistency or coherence with familiar, established theories, or where these must be amended, minimality of the amendment

to downgrade the revolutionary nonstandard analyst’s position. In particular, there is this enormous body of mathematical and scientific knowledge gathered as a result of research using standard analysis that has already been
accumulated in the mathematical and scientific archives, which would require of any would-be researcher in either mathematics or science to learn standard analysis in order to understand this past work. Practically speaking, one couldn’t just replace standard analysis with nonstandard analysis in the way the early versions of analysis were replaced with the standard $\varepsilon - \delta$ version: a competent researcher would still have to learn both versions to make use of past work. Furthermore, it would be decidedly impractical for mathematics departments to replace the teaching of standard analysis with nonstandard analysis, for their students would be at a serious disadvantage when they took any advanced scientific course in which analysis was used. Unless all the scientific and engineering departments also made such a switch, all sorts of practical problems could arise for students learning analysis in the nonstandard way.\footnote{So far as I know, no mathematics department in the country teaches their calculus classes from just the nonstandard perspective. It is possible that there are special classes or seminars in which nonstandard analysis is being taught or studied, but the disadvantages of making a complete switch to the nonstandard system are, I suspect, just too great.}

But what should we conclude from this rejection of the answers provided by both the hermeneutical and the revolutionary nonstandard analysts? Perhaps, we should reason as did Burgess and Rosen:

Since anti-nonstandard analysts reject all the hermeneutic and revolutionary claims of the nonstandard analysts, from their viewpoint, nonstandard analysis is distinct from and inferior to standard analysis. What is accomplished by producing a series of such distinct and inferior theories? No advancement of science proper, certainly, . . .

The Fly in the Ointment

\footnote{So far as I know, no mathematics department in the country teaches their calculus classes from just the nonstandard perspective. It is possible that there are special classes or seminars in which nonstandard analysis is being taught or studied, but the disadvantages of making a complete switch to the nonstandard system are, I suspect, just too great.}
The trouble with this reasoning is that it seems tacitly to assume that proponents of nonstandard analysis are restricted to a kind of all or nothing choice: either science expressed using nonstandard analysis is better than, and to be preferred to, our present day versions of science which are expressed using standard analysis (in which case, current versions of science should be completely replaced by the nonstandard versions) or nonstandard analysis is inferior to standard analysis and can accomplish no advancement of science or mathematics.

It is neither practical nor reasonable to replace current versions of science by versions using nonstandard analysis, but that should not lead us to deny that nonstandard analysis can be fruitfully employed to advance mathematics and science—especially since there are many striking examples in which nonstandard analysis was actually used in the advancement of mathematics and science. Martin Davis discusses just such a case (in his introduction to his book {Davis, 2005 #496}):

Aside from theorems that tell us that nonstandard notions are equivalent to corresponding standard notions, all the results we obtain can be proved by standard methods. Therefore, the subject can only be claimed to be of importance insofar as it leads to simpler, more accessible expositions, or (more important) to mathematical discoveries. . . . The best evidence for the [latter] is the Bernstein-Robinson theory of invariant subspaces of infinite dimensional linear spaces, which settled a question that had remained open for many years. ({Davis, 2005 #496}, p. 1, italics mine).

That nonstandard analysis has been usefully applied in the sciences has been emphasized by Joseph W. Dauben, who cites impressive results obtained both in physics, “especially quantum theory and thermodynamics”, and also in

\[222\] Two instructors of experimental classes in Sullivan’s study expressed the opinion that “the nonstandard method of learning calculus has special merit for students planning to major in engineering or physics, fields in which infinitesimals have always been considered a useful tool” ({Sullivan, 1976 #494}, p. 375).
economics, “where study of exchange economics has been particularly amenable to nonstandard interpretation” ({Dauben, 1988 #495}, pp. 192-3). Another area to which nonstandard analysis has been applied with great success, according to Wilhelmus Luxemburg, is “probability theory, notably in the theory of stochastic processes.” In short, to take the position that nonstandard analysis does not advance mathematics or science on the basis of the sort of considerations given by Burgess and Rosen (described above) for rejecting the positions of the hermeneutic or the revolutionary nonstandard analyst would be nothing short of grotesque.

10. Some Comments on the “More Direct Test”

Let us now consider the rhetorical reasoning of “the more direct test” to see if we should conclude that the mathematics of PM achieves nothing that can be considered an advancement in science or mathematics. Let us imagine that a paper presenting the mathematics of PM is submitted to the Physical Review. Undoubtedly, such a paper would be rejected. No doubt, a practicing physicist who refereed the paper would fail to see that the logical and mathematical innovations of the paper constitute genuine scientific advancements. Indeed, it seems quite conceivable that such a researcher might fail to see any logical innovations in the paper. More likely, I suspect that many

223 See p. x of his forward to {Robinson, 1996 #492}.

224 It should also be noted that nonstandard analysis has illuminated the history of mathematics in an important way. After various inconsistencies were found in the accounts of infinitesimals given by the early developers of the calculus, many mathematicians came to believe that there was an inherent danger in the very use of the notion of infinitesimals and that all talk of such notions must be eliminated from mathematics. Nonstandard analysis has shown how the ideas of the early developers of the calculus regarding infinitesimals could be made consistent and fruitful. See, in this regard, {Keisler, 1976 #497}, pp. 873-75,
physicists would even be unable to grasp the theorems of its mathematical theory, written as they are in the unusual symbolism of PM, with its many notational abbreviations and its “typical ambiguity”. And one would not expect the practicing physicist to completely understand the significance of the vicious-circle principle and the particular way in which the ramification of types is developed, especially given that so many trained logicians have missed so many of its features. This lack of understanding would be no reason for thinking that the mathematics of PM fails to make any advances in the foundations of logic and mathematics, but would be rather a reason for thinking that the referee is not really competent to judge the merits of the material being sent to her. (She would probably return it with a note suggesting that the authors try another journal). Why Burgess and Rosen think that such a strange submission would be an accurate test of the scientific merits of the material in the paper is, to me, puzzling. What in the world were they thinking?

What about their rhetorical comment that it would be madness to suggest that physicists interested in predicting the perturbations of Mars before a space-shot should carry out their reasoning in the language of synthetic geometry [Field] or modal logic [Hellman] rather than of mathematical analysis. True enough. It would be madness to make such a suggestion. But what does that prove? No nominalist that I know of ever made such a suggestion. It would also be madness to suggest that the physicist interested in predicting the perturbations of Mars before a space-shot should carry out his reasoning in the language of PM. It would even be madness to suggest that the physicist should carry out his reasoning in ZF. Does that show that the mathematics of PM or ZF achieves nothing that advances our knowledge of the nature of mathematics or its foundations?
11. “What Good is it?” Questions

I find it ironical that the mathematician Burgess has raised the question of what nominalistic reconstructions of mathematics are good for. I suggest that it is ironical because it is the sort of question that is frequently raised by non-mathematicians about the professional mathematician’s work. Questions like “What good are quaternions?”, “What is the point of studying infinite dimensional vector spaces? Aren’t there only three dimensions in space (or four if one regards time as a dimension)?”, and “Why is Fermat’s Last Theorem so important? What can you do with it?” have been raised at some time or other to more than one advanced student of mathematics. Sometimes, questions of this sort are reflections of an overly pragmatic or extreme utilitarian view of mathematical research—which many mathematicians would regard as crass or unworthy of a serious and considered reply.

I am reminded of the time when, during my graduate studies, I received a notice requiring me to undergo a physical examination for entry into the U. S. Army. I immediately applied for a draft deferment, requesting that I be allowed to complete my studies in philosophy of mathematics before serving. Shortly thereafter, I was instructed to come before the Seattle Draft Board. The very first question I was asked was: “How will your studies help the war effort?”225 “In particular, how will such studies help Boeing develop better war planes?”226

225This took place in the fifties during the Korean war.

226This brings to mind the following quotation from the artist Alexander Melamid to be found in the May 25, 2011 edition of the New York Times: “I was always told that art was good for me, but until recently I didn’t know what it was good for. What is good? What is good in the U.S.A. is health and health products.”
I certainly do not object to the Burgess-Rosen team asking the “What good is it?” question of nominalistic reconstructions of mathematics. But what I find strange—even grotesque—is their belief that they can prove somehow that such reconstructions cannot be good for science or mathematics in any truly significant way. It is as if some non-mathematician claimed to be able to prove that quaternions could never be of any real value for science. (Recall Jeans’s comment that group theory will never be of any use in physics).

Consider the Japanese art of paper folding called “origami”, which is generally regarded as little more than a kind of entertaining play. The Burgess-Rosen question of value might be raised regarding the study of origami: “What is the study of such an entertainment good for?” And one could draw the same conclusions about such a study that the Burgess-Rosen team did for nominalistic reconstructions of mathematics: “no advance of science proper certainly”. But in an article entitled “Things Are Seldom What They Seem, and Other Joys” (in The New York Times, April 3, 2006), Edward Rothstein writes: “Once restricted to domestic decoration, origami has also become useful for designing everything from foldable tourist maps to expandable heart stents. Mr. Lang [a laser physicist] has worked on the folding of air bags in a car, and on a design for a collapsible telescope-mirror more than 300 feet wide that might unfold in space” (p. B3). Actually, there is an enormous literature on “origami mathematics” and “origami science”, as well as on the use of origami as a teaching tool. There are also international conferences devoted to the mathematical and scientific uses of origami, in which papers are delivered on such topics as “the folding of uniform plane tessellations” and the “molecular
modeling of fullerenes with modular origami”. Should we conclude, using the Burgess-Rosen reasoning, that the study of origami cannot lead to any significant advances in science? Such a conclusion would be risky indeed, since origami is being used to design such things as promising origami models of molecules. 

As arguments to show that nominalistic reconstructions of mathematics are valueless go, both the Burgess 1983 argument and the Burgess-Rosen 1997 argument seem to me to be utter failures, based as they are upon unjustified assumptions (such as the Fregean Assumption) and questionable inferences. But those arguments can also be regarded, more positively, as raising interesting and, perhaps, philosophically valuable questions, such as: In what specific way are the various nominalistic reconstructions of any value? How could the mathematical system implicit in PM be regarded as contributing significantly to the human understanding of the nature of mathematics, if it has features that render it an inappropriate replacement for current real mathematics? To explore such matters, let us first single out for special consideration various senses of the term ‘model’, one of which figures prominently in discussions involving applied mathematics.

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227 For example, The Second International Meeting of Origami Science and Scientific Origami was held in Otsu, Japan from November 29-December 2, 1994. The reader might find it enlightening to examine the topics discussed at the meeting.

228 Cf. the article “Origami Inspires Rise of Self-Folding Robot” by Kenneth Chang, that was published in the New York Times National of August 8, 2014. The article describes a robot, whose creation by a graduate student at Harvard, Samual Felton, can transform itself from a flat sheet to a four-limbed device. The device is described in the article as “the first robot that can fold itself and start working without any intervention from the operator”. I repeat: Who’s to say, with certainty, that the study of origami cannot lead to any advances in science?
12. Models: a Preliminary Discussion

Let us, first of all, distinguish the following three different kinds of models. There are the mathematical structures that form the subject matter of model theory—something with which all logicians can be assumed to be well acquainted. Let us call these structures “s-models”. And there is a second kind of model—the kind of model constructed by applied mathematicians for use in analyzing and understanding processes and events that take place in the physical world. Let us call these models “am-models” (applied mathematical models, or models typically devised in applied mathematics). Of course s-models frequently enter into the constructions and descriptions of models of this second kind. For example, a model that a physicist constructs of some physical phenomenon may involve reference to mathematical structures (say, group structures or vector spaces). The term ‘model’, in both senses, is frequently used in discussions about the nature of mathematical logic.

Then there is a third sense of the term ‘model’ that should also be noted: this is the sense that fits, in a general way, one of Thomas Kuhn’s uses of the term ‘paradigm’. We use the term ‘model’ sometimes to refer to a kind of ideal or paradigm, as in the sentence “She is a model of politeness”. In a postscript that appears in his influential book (Kuhn 1970), Kuhn notes that he used the term ‘paradigm’ in two different ways in the first edition of the work—an ambiguity that has led to serious misunderstandings of claims he had made using the term ‘paradigm’:

On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solutions of the remaining puzzles of normal science. (p. 175, italics mine).
Models of the kind Kuhn is referring to in the above quotation can influence, as paradigms or ideals, the development of some part of language use or theoretical practice. I shall call such models “p-models” (paradigm models).

I shall now explain in more detail the kind of model that applied mathematicians make use of in developing scientific models of events and processes in the physical world. For the most part, I shall be concerned, in what follows in the rest of this chapter, with am-models, and it should be understood that essentially all the models I shall be discussing will be am-models unless otherwise stated. For these reasons, I shall frequently drop, in what follows, the ‘am’ part of the expression ‘am-model’.

13. Models and Applied Mathematics

Maki and Thompson describe their conception of theory construction and analysis in the following way:

Once enough data have been collected and adequately analyzed, the researcher tries to imagine a process that accounts for these results. It is this activity, the mental or pencil-and-paper creation of a theoretical system that is the topic of this book. In the scientific literature this activity is commonly known as theory construction and analysis. We shall refer to it as the construction, development, and study of mathematical models. ([Maki, 1973 #461], p. 1, italics mine).

Their book, entitled “Mathematical Models and Applications”, could also be named “Applied Mathematics” according to the authors, to the extent to which applied mathematics can be understood to be the “construction, development, and study of mathematical models”.
Other applied mathematicians have written about mathematical modeling in the above vein: Thus, A. C. Fowler writes:

Mathematical modeling is a subject that is difficult to teach. It is what applied mathematics (or, to be precise, physical applied mathematics) is all about . . . (Fowler, 1977 #552, p. 3, italics mine).

C.C. Lin and L. A. Segel describe their applied mathematics book as follows:

This text, an introduction to applied mathematics, is concerned with the construction, analysis and interpretation of mathematical models that shed light on significant problems in the natural sciences. (Lin, 1974 #552, p. v, italics mine).

Certainly, the devising of a useful idealized and simplified representation of the type of empirical situation or process to be investigated is a key component of the modeling. Notice, however, that I say “a useful representation”—not “the right or correct representation”. Many applied mathematicians emphasize that generally there is no such thing as the right or correct representation. Frequently, there are many ways of constructing such representations. Furthermore, it is the nature of models that the empirical models devised are not required to be completely factually accurate depictions of the phenomena being modeled. This feature allows for a variety of empirical models utilized in analyzing and explaining physical phenomena. Thus, an early mathematical model devised to analyze the aggregations of stars and gases to

For example, Fowler writes:

Once a problem is identified and a mechanism proposed, then one must formulate it mathematically. Often the difficulty lies in the choice of complexity: one wants the ease of a simpler model, but on the other hand one should include every relevant process. Different modelers will differ on what is important, and there is no unique ‘right answer’. (Fowler, 1977 #551, p. 6).
form galaxies has several striking similarities to a model devised to analyze the aggregation of slime mold amoeba to form slugs. The phenomenon being analyzed by these two models are as different as can be: the aggregations forming galaxies involve huge astronomical bodies, whereas the aggregations forming slime mold slugs involve tiny unicellular organisms that one needs a microscope to see. Yet, in both models, the things that are aggregating are treated as if their mass were continuously distributed. This is because, as Lin and Segel express it:

[B]oth stars and amebae behave as if their mass were continuously distributed— notwithstanding the fact that one can see (using a microscope) gaps between amebae, while the nearest stars are light-years apart. But it is of little relevance that an idealized mathematical model such as the continuum is “wrong”. The real question is: Do the errors have a significant effect upon predictions concerning the phenomena under investigation? ([Lin, 1974 #552], p. 3).

As Fowler notes:

It is important to realize that all models are idealizations and are limited in their applicability. In fact, one usually aims to over-simplify; the idea is that if a model is basically right, then it can subsequently be made more complicated, but the analysis of it is facilitated by having treated a simpler version first. ([Fowler, 1977 #551], p. 3).

Let us now consider a specific model that has been devised to clarify some specific points about how a mathematical theory can be used both to aid us in drawing useful information about the behavior of light and also to show the falsity of some assumptions that philosophers make about particular models of mathematics.

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230 These two examples of modeling are described in Sections 1.2 and 1.3 of [Lin, 1974 #552]
14. A Theory of Light Waves

The essentials of this model will be given by the specification of a few definitions and an axiom:

Definition 1: The term ‘point’ is used to refer to points in Euclidean 3-space, i.e. the space of ordered triples of real numbers, the standard metric being used, and the points being ordered in the standard way.

Definitions and Axioms Assumed: In the following, all the usual axioms and definitions of the geometry of Euclidean 3-space will be assumed, as well as the usual properties of the real numbers—in particular, such terms as ‘angle’, ‘normal’, ‘plane’, ‘half space bounded by a plane’, ‘lies on’, and ‘continuous curve’ will be assumed to have been defined in standard ways.

Definition 2: Any continuous curve from point a to point b will be called a path from a to b. If f is any path from a to b, then a may be called a ‘source’ and b may called an ‘observer’.

Definition 3: A path of a light ray is a path from any source to any observer that is the shortest path from the source to the observer.

Definition 4: A light reflection instance consists of a path f, a plane p, and points a, b, and c such that:

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231 This theory was first put forward in [Chihara, 2008 #543].
c lies on p; a and b are not on p, but are both in the same half space bounded by p; and f is the union of a path of a light ray from a to c and a path of a light ray from c to b, all the parts of f being in the same half space bounded by p.

Such an f will be called a ‘path of reflection (relative to p) from a to b’; c will be called a ‘point of reflection’ of f; and the half-space bounded by p in which f is present will be called the ‘space of reflection’ of f, whereas the half-space bounded by p in which f is absent will be called the ‘complement space of reflection’ of f.

This theory has one principal axiom (in addition to all of the axioms assumed above to have been given):

Axiom: If p, f, a, b, and c constitute a light reflection instance--f being a path of reflection (relative to p) from a to b--then if b* is that point in the complement space of reflection of f that is symmetric to b with respect to p, point c must be such that the path consisting of the union of that part of f that goes from a to c and the path of a light ray that goes from c to b* is the shortest path from a to b*.

It can be shown that:

Theorem: Let p, f, a, b, and c constitute a light reflection instance, f being a path of reflection (relative to p) from a to b. If n is the normal to p at c directed into the space of reflection, then the angle between that part of f that goes from a to c and the normal n is a constant, independent of the position of the point c.

\[^{232}\text{The sense of ‘symmetric’ here is that p is a perpendicular bisector of the path of a light ray from b* to b.}\]
is a path from a to c and n is equal to the angle between n and that part of f that is a path from c to b.

This theory is a purely mathematical theory, despite the fact that such expressions as ‘observer’, ‘source’ and ‘path of a light ray’ may suggest otherwise. For example, by examining the definitions above, it can be seen that the first two of the above terms are only suggestive of how the theory is to be applied and in fact refer only to points. Also, seeing that not all mathematical structures satisfy the principal axiom, one can conclude that the term ‘path of reflection’ functions as a parameter that can refer to different “entities” in different structures. Thus, sentences of the theory involving this term do not express propositions about the actual world, and they are not true (or false), but only true (or false) of certain structures. In particular, the above theorem is not true (but only true of certain structures).233

However, this theory can in fact be applied to actual physical situations involving the behavior of light, by regarding paths of actual light rays to be approximately that of paths of light rays from a source to an observer (as characterized above). In so applying the theory, we need to make certain idealizations about the physical space in which we operate (e.g. that it is Euclidean and three dimensional), and we need to restrict the scope of the theory to light traveling in “homogeneous media”-- say, reasonably clear (fogless, smokeless) air—which is reflected by a flat object, such as a mirror, approximating a plane in certain geometrically relevant ways. Since, in such conditions, the paths of light rays in such homogeneous media are in fact very close to straight lines (for most practical purposes) and since the geometric properties of flat mirrors can be made to be very close (for practical purposes

233 Hence, there is no need to argue in this argument that such mathematical terms as ‘point’ and ‘continuous curve’ do not refer to particular entities but only places in structures.
relating to how paths of light are reflected by mirrors) to those of planes, we have a kind of “empirical model” of the theory. Then, since the empirical model satisfies the axioms (as an approximation), it would have to satisfy the above theorem. Hence, we can infer that, in such conditions, any path of reflection will be such that the path’s angle of incidence (approximately) equals its angle of reflection. This theorem will allow us to conclude that a light ray striking a mirror at a 35 degree angle will reflect off the mirror at (approximately) a 35 degree angle. Needless to say, knowing such a result can be used to make practical predictions in a variety of situations.234 Such theorems could easily be tested and verified.

15. The Structural Content of Theorems235

What we have above is a theory consisting of a set of axioms and definitions that, in effect, characterizes a kind of mathematical structure (an s-model of the first kind I discussed earlier). Each theorem $\phi$ of the theory will then be said to be true of such a structure. Then a theorem $\phi$ of the theory can then be understood to have what I call a “structural content” that tells us:

Any structure that satisfies the axioms of the theory
must satisfy the sentence $\phi$.

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234 For an indication of how the above theory of light rays can be developed into a more complex and versatile theory, see ([Maki, 1973 #461], Sec. 2.3.4).

235 The notion of “the structural content of a mathematical theorem” was first put forward in [Chihara, 2004 #468], pp. 65-6, 247-8.
In general, to explain how a theorem $\phi$ can be applied in science and everyday situations, it is not necessary to assume that $\phi$ is true. It is sufficient to assume only that the structural content of $\phi$ be true.\(^{236}\)

Thus, in applying the above theory of light rays to an actual physical situation, one can represent the phenomenon under investigation in a way that idealizes and simplifies the situation to be studied so that actual physical light rays are represented as behaving like the light rays from a source to an observer of the mathematical theory. This idealized and simplified representation gives us a model of the physical situation that is close enough in certain important respects to actual situations, so that, in terms of this model, many practical predictions can be made and verified to be approximately true. One can then explain this application of this mathematical theory without assuming that any theorem of the theory is true—one need only assume that the “structural content of the theorem” (mentioned above) is true. Furthermore, by focusing on various general features of the kind of situation being investigated, as well as on certain general features of the model itself, one can illustrate and explain how, in general, models can be applied in real situations to draw empirical conclusions from empirically determined facts—without assuming that the theorems of the model are true of the actual world and without claiming that the model should replace real mathematics or even some specific mathematical theory in use today.\(^{237}\)

16. The Kind of Models Constructed by Applied Mathematicians

\(^{236}\) See [Chihara, 2004 #468], pp. 247-8.
Let us now give some thought to the question ‘What are models?’, where the models we wish to characterize are the kind of am-model discussed in the previous section. Can one give a general characterization of the kind of models applied mathematicians construct? First, let us consider the kind of model that R. Harre calls “micromorphs”. X is a model (micromorph) of thing Y if X is a replica or analogue of Y (Harre, 1960 #555, p. 86). An example of a micromorph would be a scale model of an airplane used in wind tunnel studies of the air flow around the airplane being modeled when it is travelling at high speeds.

Now consider the way Fowler characterizes the models that the applied mathematician constructs. These models for Fowler are not “objects” or “physical things” like model airplanes. They are described as encapsulations of “some slice of the real world”. More specifically, Fowler tells us that:

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\text{[a model] is a mathematical representation of the modeler's reality, a way of capturing some aspects of a particular reality within the framework of a mathematical apparatus that provides us with a means for exploring the properties of the reality mirrored in the model} \] (Fowler, 1977 #551, p. 1).

He also asserts that the point of making a model is to ‘bring a measure of order to our experiences and observations, as well as to make specific predictions about certain aspects of the world we experience’ (Fowler, 1977 #551, p. 2). It turns out, however, that Fowler’s book is concerned with a particular kind of model that is directed at encapsulating what he calls “natural systems”, each of which has a set of “observables” such that a mathematical characterization can be given of the way in which these observables are linked. In general, the observables of a natural system are selected to form a subsystem of the natural system that can be encoded into a formal mathematical system, which
represents, or “models”, the phenomena of concern (Fowler, 1977 #551), p. 2).

Let us now introduce a sense of ‘model’ that is more general than the highly restricted notion of model that Fowler uses. I intend my term ‘model’ to apply not only to Fowler’s models but also to models of the simple sort presented earlier (of the theory of light waves), as well as to formalizations of such theories as, say, Intuitionistic reasoning and mathematical reasoning. So I shall expand Fowler’s characterization of “model” to include not only representations of “phenomena” (in Fowler’s sense), but also processes, procedures, and methods.

Thus, I shall use the term ‘mathematical model’ or ‘am mathematical model’ to refer to the idealized and simplified representations of phenomena, processes, procedures, or methods, that make use of mathematical concepts and theories.

It turns out that much of what has been written about models so far, including Fowler’s comment, can be understood to be about what I call “mathematical models”. In particular, the little theory of light rays I presented earlier can be seen to be a “mathematical model”. Thus, Micromorphs are almost always models of physical things like airplanes, cars, and pieces of furniture, whereas mathematical models are models of processes, procedures, or methods. Still, these two types of models are related in the following way: in the case of useful micromorphs, there will be rules or methods of transforming information obtained about the model into information about the

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238 It can be seen that my term ‘mathematical model’ applies to Fowler’s models, from the fact that he uses the term ‘phenomena’ to refer to “the natural systems” and “the reality” mentioned in his characterization of “models”.
thing modeled. Similarly, in the case of useful mathematical models, there will be rules or methods of transforming information obtained in or by means of the model into information about the phenomenon, process, procedure, or method being modeled.

17. PM Considered as a Model of Mathematics

Keeping in mind the ideas of the previous sections, let us reconsider the question, which I posed earlier in Section 10 with the words:

[Q#] How could a reconstruction of mathematics or of mathematical reasoning provide foundationalists and philosophers with an enlightening and theoretically fruitful way of regarding the nature of mathematics and mathematical reasoning, even though the reconstruction cannot reasonably serve as a complete replacement of mathematics or of mathematical reasoning?

As an aid to answering this question, consider how some mathematical logicians have answered [Q#] when the question is understood to concern mathematical logic. Joseph Shoenfield describes mathematical logic as “the study of the type of reasoning done by mathematicians” ((Shoenfield 1967), p. 1). Herbert Enderton is more specific about Shoenfield’s “study” by seeing it as involving the use of a model, specifically characterizing symbolic logic as a “mathematical model of deductive thought”. Clearly, Enderton is using the term ‘model’ here roughly in the sense in which I used the term ‘model’ in discussing above the models of applied mathematics (i.e. am-models). According to Enderton, the symbolic logician took “the deductions made by the working
A mathematician” as “real-life originals whose features are to be mirrored in our model”. Not surprisingly, he regards first-order logic (which he calls “our model”) as “admirably suited to mathematics”.

Now consider the question: How one might respond to [Q##] when the question is about a reconstruction, such as PA, of real arithmetic? The views expressed by Shoenfield and Enderston strongly suggest the following response: PA could contribute significantly to our understanding of the nature of real arithmetic by being an enlightening and theoretically fruitful model of arithmetic. In that light, consider the principal question concerning us in this section: “How could the reconstruction of mathematics (and of mathematical reasoning) by PM provide scholars with an enlightening and theoretically fruitful way of viewing these target intellectual endeavors, even though PM cannot reasonably serve as a complete replacement of mathematics?” Here’s my response: PM can be enlightening and theoretically fruitful in the way a great many mathematical models (some described in detail in such works as {Fowler, 1977 #552}, {Maki, 1973 #461}, and {Lin, 1974 #552}, mentioned in Section 15 on applied mathematics) are enlightening and fruitful—that is, as enlightening models of real mathematics. PM is not satisfactory as a hermeneutic account of mathematics, nor is it satisfactory as a revolutionary account of mathematics—but even so, one can argue that it has proven to be an enlightening model of real mathematics—especially in such areas as proof theory, foundations of mathematics, and mathematical logic.

It is noteworthy that in the historic paper in which Gödel presented his Incompleteness Theorems, the STT version of PM was taken as the system that was explicitly proven to be incomplete. The reason for taking PM as this target system to be proven incomplete was explained as follows:
The most comprehensive formal systems that have been set up hitherto are the systems of Principia mathematica... on the one hand and the Zermelo-Fraenkel axiom system of set theory (further developed by J. von Neumann) on the other. These two systems are so comprehensive that in them all methods of proof today used in mathematics are formalized, that is reduced to a few axioms and rules of inference.²³⁹

Thus, Gödel regarded PM as providing us with a system of logic “so comprehensive that in them all methods of proof today used in mathematics are formalized”—which is no small accomplishment.

Any proposal to replace our real mathematics with the artificially formal and complicated restrictions of PM would be greeted by incredulity or mirth, and not taken seriously. Still, a reconstruction of real mathematics can make a genuine contribution to our mathematical knowledge and understanding. It is just this fact that the PM example makes abundantly clear. Perhaps the PM example will enlighten those who, like Stewart Shapiro, simply cannot imagine how any reconstruction of mathematics that provides neither a successful hermeneutic analysis of mathematical language nor an attractive revolutionary alternative to current mathematical theories can still succeed in advancing our understanding of mathematical logic, the foundations of mathematics, or even the nature of mathematics itself.²⁴⁰ The example of PM makes it clearly evident that what determines that a reconstruction succeeds in advancing our knowledge of mathematics or logic may not be sufficient to justify taking it to be a worthy replacement for current real mathematics. For one thing, the advancement that the reconstruction achieves may not be sufficiently broad,

²³⁹ [Gödel, 1931 #538], pp. 596-7. I have omitted the footnotes in the quoted passage.

²⁴⁰ Shapiro once defended the Burgess-Rosen argument with the words:

So Burgess and Rosen argue that neither the revolutionary approach nor the hermeneutical approach has much of a chance of success, so long as success is understood in scientific terms. And what other terms are available to the naturalistically minded philosopher? . . . I do not have a third orientation to propose on behalf of nominalism . . . ([Shapiro, 1998 #364], p. 609).
important, or theoretically enlightening to justify the total replacement of current real mathematics with the reconstructed version—especially given the enormous practical costs that such a revolution would entail. Besides, the advances that PM made were of benefit and interest primarily to foundationalists and theoreticians—not to the vast majority of citizens or even day-to-day scientists, who do not concern themselves with the sort of highly intricate logical questions that PM addresses. To subject ordinary people and researchers to the intricacies of PM for such benefits would be quite unnecessary and unreasonable.

It is thus clear that the question of whether or not a reconstruction achieves any sort of advancement of our understanding of some field of mathematics does not determine, by itself, whether a global revolution of the sort that Burgess and Rosen envisage should take place: all sorts of other considerations (such as the kind of pragmatic matters that Burgess, himself, emphasized in his first paper on the topic) come into play if one is seriously considering actually replacing current real mathematics with the reconstructed version. But all of that makes evident that Burgess and Rosen have confused the question of whether a nominalistic reconstruction of mathematics is a useful model for philosophical analyses of real mathematics with the related (but very different) question of whether a nominalistic reconstruction of mathematics is an appropriate global replacement of real mathematics. Of course, no one (certainly not I) ever put forward the thesis that the Constructibility Theory I defended should replace real mathematics. That Burgess and Rosen (implicitly) attributed such a thesis to me is just one of the many unjustified attributions that seriously damage the case these Princeton philosophers make against my views.
18. The Models of Science Are Not Generally Competitors of Science

Besides the hugely unjustified assumption “Burgess’s fork” (that (A) and (B) are the only two reasonable answers to the question ‘What are the reconstructions good for?) which the Burgess-Rosen team made regarding my anti-nominalistic reconstructions of mathematics, there is another unjustified assumption in the team’s reasoning, which presents a completely distorted picture of the nature of my nominalistic reconstructions of mathematics. As they analyze the nominalist’s work, these reconstructions are assumed to be in direct competition with real mathematics—a competition in which both sides of the competition cannot win. (This assumption is probably at least part of the reason why Burgess and Rosen believe that any reconstruction that is not put forward as a hermeneutic reconstruction must be regarded as being revolutionary.) This assumption is completely mistaken.

In general, the many models constructed in science are not in competition with science—the construction and utilization of these models are essential elements of ordinary scientific reasoning and scientific practice. Thus, these models should be regarded as parts of science and not as revolutionary competitors of science that are put forward to replace our current science. Similarly, the nominalistic reconstructions of science can also be regarded, not as competitors to ordinary mathematics, but rather as novel and potentially useful ways of interpreting, and shedding light on, both real mathematics and its many applications in science.

Frege’s version of arithmetic is a case in point. I pointed out earlier that it would be absurd to advance the view that Frege’s arithmetic should actually replace arithmetic; but it would not be absurd to maintain that Frege’s version
of arithmetic is a useful model of arithmetic--useful for certain philosophical purposes. For example, as a model of real arithmetic, it can be used to explain, from a strictly logical standpoint, why typical everyday applications of arithmetic are sound. This way of regarding one such “reconstruction of mathematics” was developed in detail in one of my previous works.\textsuperscript{241}

\textsuperscript{241} See [Chihara, 2004 #468], Chapter 9 and [Chihara, 2005 #477], Section 5.
Chapter 7
The Remaining Campaigns

Introduction

In this chapter, I shall respond to the remaining of the Burgess-Rosen anti-nominalism campaigns mentioned in the previous chapter: these are the second, fourth, and fifth campaigns. Recall that I am attempting to discover whether there is some “common fallacy” underlying the anti-nominalism arguments that the Burgess-Rosen team has advanced. I shall show that the Fregean Assumption is playing a crucial role in all but one of these arguments.

1. The Second Campaign: Various Anti-Nominalism Dilemma Arguments

(a). Burgess’ Dilemma

On October 22, 1987, at a meeting of Berkeley Philosophy Department’s Colloquium, Burgess delivered a paper directed at refuting nominalism. During the discussion that followed the presentation, Burgess and I became involved in a heated dispute about the validity of the paper’s argumentation. One of the chief points of contention concerned some such theorem of arithmetic as:
[1] There are numbers greater than $10^{10}$ that are prime.

Since either [1] is true or [1] is not true, Burgess claimed that, for many years, he believed that the nomalist must choose between the following horns of a dilemma: he could maintain that [1] is true and then hold that [1] does not imply that numbers exist. Or he could maintain that [1] is not true and then hold that the mathematician’s justification (or proof) does not in fact justify the belief in the truth of [1].

The first option, which Burgess calls the “hermeneutic alternative”, requires maintaining that [1] does not actually mean what it seems to mean (since it seems to imply that there are numbers). This option, Burgess believes, runs into the difficulty of explaining the meaning of [1] in a way that is compatible with denying the implication in question. The second option—the “revolutionary” alternative—also faces some difficult questions, since it denies that the expert’s justification for accepting [1] in fact justifies its acceptance.

Thus, Burgess asks:

Is the aim of the mathematician, in deciding what results to accept, that of arriving at justified beliefs, or is it something else, perhaps that of devising useful fictions? If mathematicians are aiming to arrive at justified beliefs and are failing to do so, should philosophers attempt to get them to recognize their failure and take corrective measures? ((Burgess 2005), p. 88).

Since neither of the two described options is at all attractive to Burgess, he concluded that the nomalist was left with quite a troublesome dilemma. Burgess, on the other hand, is free to draw his favored Platonic conclusion that mathematical objects truly exist.

Of course, Burgess allows that the nomalist can simply refuse to choose either of the above two options, but such a refusal is seen as just a perverse refusal to face the consequences of the dilemma.
Let us analyze Burgess’s dilemma for the nominalist, keeping in mind the distinction I made earlier between the Fregean way of understanding theorems and the Hilbertian way. Why does Burgess believe that there are only the two alternatives: either [1] is true or [1] is not true? It is because he makes the Fregean Assumption: he believes that theorems of mathematics are propositions about the actual world. Thus, Burgess believes that only the following two options are available to the nominalist:

(i) [1] is a proposition about the actual world that is true

or:

(ii) [1] is a proposition about the actual world that is not true.

And this is why he thinks that the nominalist is faced with an insoluble dilemma.

But the reader can now see that there is a third option—namely, one can reject the Fregean Assumption that is being presupposed by the above argument. In other words, one can adopt the following Hilbertian option:

(iii) [1] is not a proposition about the actual world.

(iii) is the option that I believe is the correct one; it is also a way of going through the horns of Burgess’s dilemma. In next chapter, Chapter 8, I shall argue that the Fregean Assumption is false, and hence that the above argument of Burgess’s is unsound.
At this point, I cannot claim to have yet made a decisive case for the cogency of the third option, especially since I have not said much about the Hilbertian way of understanding mathematical theorems in general--something I will explain in detail in Chapter 8. However, even now it should be clear to the reader that the above Burgess dilemma rests upon the unstated Fregean Assumption—an assumption upon which I shall cast doubt later in this chapter, and which I shall refute in the Chapter 8.

(b). The “Ordinary Language” Dilemma

In addition to the above argument, Burgess has produced another anti-nominalist dilemma--this one with the somewhat different goal of showing that there exist “nonmathematical universals or attributes”. This argument also differs from the others insofar as this argument is a sort of ordinary language dilemma, not concerned with the nature of mathematics or of any specific mathematical theorem: it is the sort of argument concerned with the meaning of specific sentences of English that ordinary English speakers might utter in everyday situations. Interestingly, this “ordinary language” argument is the only one of the anti-nominalism arguments I consider in this chapter that does not presuppose the Fregean Assumption.

The argument begins with an analysis of the statement:

[A1] There are now four books authored by Charles Chihara.

Burgess argues that [A1] cannot be understood to be the assertion
[A2] There are now four book tokens authored by Charles Chihara since there are “not just four but hundreds or thousands of such tokens, scattered through various institutions and personal libraries” ([Burgess, 2005 #487], p. 89). So Burgess understands [A1] to be the assertion

[A1′] There are now four book types authored by Charles Chihara.

An important feature of [A1] is brought out by the fact that Burgess believes [A1] asserts that book types exist—specifically, that there exists in the actual world at least four book types. He thus argues that, if one is willing to accept the truth of [A1], then one is, in effect, accepting the Platonic thesis that book types exist in the actual world and hence that abstract entities exist in the actual world. Because of this conviction, Burgess concludes that the nominalist is faced with the following dilemma

Either she can deny that the usual grounds given for asserting [A1] are sufficient;

or she can deny that [A1] implies that there are such things as book types.

In the latter case, she would evidently have to argue that the assertion must really be about particulars (such as book tokens).
Let us consider the former alternative. What would it be to deny that the usual grounds for accepting an assertion such as [A1] are sufficient? Here’s Burgess’s answer:

If you asked me . . . for evidence to justify the belief that there are now four books (not in the sense of book tokens) by Charles Chihara, we could point to four book tokens, each with the name ‘Charles S. Chihara’ on the title page, and each, apart from that one common feature, quite unlike every other. ({Burgess, 2005 #487}, p. 89).

In ordinary everyday situations, the sort of evidence cited above seems to constitute perfectly adequate grounds for affirming [A1], although one might find evidence that would require that other sorts of evidence for [A1] be sought (as might well be the case if some of the book tokens mentioned in the above quotation were found to be mostly filled with blank pages). So to take the first horn of the dilemma is to reject what practically everyone, in ordinary everyday situations, takes to be acceptable.

What about the latter alternative? To deny the implication in question is to deny that the statement “There are now four books authored by Charles Chihara” implies that there are books (and hence book types). From the purely linguistic point of view, such an implication seems to be hard to resist (at least as Burgess views the situation). Thus, since neither alternative seems at all plausible to Burgess, he feels that the nominalist is caught on the horns of an impossible dilemma. He, on the other hand, can escape by simply accepting the Platonic position that book types exist.

In opposition to Burgess’s first-order logical, way of understanding or analyzing practically every existential sentence of English, one should take note of the extensive writings of such linguists as George Lakoff and Mark Johnson, who claim that an enormous amount of research by linguists and cognitive
scientists have shown that “metaphor is pervasive in everyday life, not just in language but in thought and action” and that “our ordinary conceptual system, in terms of which we both think and act, is fundamentally metaphoric in nature.” Now I am not in a position to support the scientific research of these linguists or vouch for the soundness of their conclusions, but it seems to me that there is an enormous amount of research discussed in the linguistic literature that contradicts in many ways and in various degrees the very literal, straightforward first-order logical, interpretation of the meaning and implications of ordinary English sentences that Burgess assumes and advances as essential parts of his anti-nominalist arguments. This is especially noteworthy since Burgess cites no linguistic research to support many of his own claims about the ontological implications of ordinary sentences of English. His many assertions about what various sentences of English mean (or must mean) are evidently backed (so far as I can gather from what he puts forward in his articles) only by his own linguistic intuitions.

Compare Burgess’s grounds for asserting his belief that book types exist with the grounds physicists provided for their belief that they had finally discovered the existence of a Higgs boson. On July 4, 2012, it was announced at CERN that the illusive particle had been finally discovered. These scientists were relying upon data, carefully gathered by two independently acting teams of physicist, after putting in a great many hours of experimentation and theorizing. The search for the Higgs boson involved 6,000 physicists and cost more than ten billion dollars. In the end, the research involved:

242 [Lakoff, 1980 #567]

1,000 trillion proton-proton collisions
240,000 Higgs bosons, whose existence was eventually justified
350 pairs of gamma rays
8 sets of lightweight particles from the lepton channel

Needless to say, before the scientific community accepted the existence of a new type of particle, such as the Higgs boson, an enormous amount of scientific research was required, some that required painstaking and time-consuming experimentation, involving a great deal of checking and analyses of data, frequently from several research teams working independently of each other. In general, scientists are cautious about making claims about the existence of new subatomic particles since the implications of accepting such existence claims are vast: they have many and enormously far-reaching consequences. I doubt that the fact that ordinary everyday talk includes talk about the authoring of books would ever be regarded by scientists as decisive grounds for believing in a new kind of entity such as book types (as Burgess does).

What ordinary English speakers say in everyday situations is scandalously filled with ontologically weighty implications. We say such things as “There is a good chance that I won’t be able to come to your presentation this Friday”. Now a philosopher might analyze the utterance as implying that there are such entities as “good chances”. Such a philosopher could be understood to be


245 Cf. “On the eve of the scheduled announcement, Dr. Incandela rehearsed his talk and found that his team was still nervous. Were they ready to go public?” op. cit., p. D7.
holding that the everyday utterances of English speakers imply that there are such entities as “good chances”. By similarly analyzing the utterances of ordinary speakers, a philosopher might conclude that everyday conversations imply that there are such “entities” as peoples, symphonies, epic poems, speeches, plays, worries, fears, possibilities, necessities, chances, and likelihoods.

I believe that Burgess was too quick to conclude, from the flimsy evidence he cites, that abstract entities such as book types exist. The grounds he cites are simply not sufficient to justify any such conclusion. His argument assumes that the statement “There are four books that Chihara has published” is the assertion that there exist in the actual world four book types, just as the mathematical theorem “There are four prime numbers less than ten” is taken by him to be the straightforward assertion of the existence (in the actual world) of prime numbers.

So let us reconsider Burgess’s ordinary language dilemma argument that was put forward to show that there exist “nonmathematical universals or attributes”. Recall that the argument begins with the following premise:

[A1] There are now, in the actual world, four books authored by Charles Chihara.

According to Burgess, this premise can be analyzed as asserting:

Either:

[A1’] There are now, in the actual world, four book tokens authored by Charles Chihara
Or:

[A1’’] There are now, in the actual world, four book types authored by Charles Chihara

As Burgess reasons, it is absurd to suppose that Charles Chihara authored book tokens, so he concludes that the only reasonable premise is [A1’’]. Thus, he again arrives at the Platonic conclusion that there exist abstract entities.

I should note that this anti-nominalism argument depends upon specific analyses of sentences and expressions of ordinary English. So we are dealing here with “the ontological commitments of ordinary language utterances”—something that is, in many cases, notoriously difficult to translate into sentences of first order logic.\textsuperscript{246} Hence we should view with caution the translations and interpretations that Burgess advances, many of which may have been strongly influenced by his own ontological proclivities. More importantly, there are good reasons for being suspicious of what may be ontologically implied by everyday utterances of ordinary sentences. There is no compelling reason why philosophers need to take seriously all or most of the utterances of ordinary speakers in casual conversations, many of which are figurative or metaphorical. “Mary is a real pain in the neck at fraternity parties”, for example, obviously should not be taken to be stating that Mary is literally “a real pain”. And the fact that we say such things in everyday situations should not lead us to conclude that there exist in the actual world an “entity” that is identical to a pain in the neck.

\textsuperscript{246} Cf. [Mates, 1972 #38], p. 86, problem 4.
Burgess is maintaining that \([A1]\) asserts that there now exist in the actual world four book types authored by Charles Chihara. Since Burgess’s argument begins with his own analysis of an ordinary sentence of English—an analysis according to which the sentence in question is taken to be expressing an ontologically weighty assertion of the existence of abstract entities that he calls “book types”—it is reasonable to scrutinize \([A1]\). The only reason he gives for accepting his way of understanding or analyzing \([A1]\) is the fact that \([A1]\) cannot be understood to be the assertion

\[\text{[A2]} \text{ There are now four book tokens authored by Charles Chihara.}\]

In other words, Burgess is assuming that, given that \([A1]\) is true, then there are only two possibilities:

Either:

[i] there are now, in the actual world, four book tokens authored by Charles Chihara,

Or:

[ii] there are now, in the actual world, four book types authored by Charles Chihara.

As in the case of Burgess’s fork, we have Burgess simply assuming a fork-like premise without providing any justification for his assumption. He just assumes from \([A1]\) that the above disjunction ([i] or [ii]) must be a truth, because,
evidently, he cannot see how it might be false. And since [i] is clearly false, he believes he can infer from [A1] that [ii] must be true.

I shall now cast doubt on his inference of the truth of the disjunction ([i] or [ii]) from the assumption of [A1]. I shall argue that he cannot affirm the truth of the disjunction because he is not in a position to assume that [i] and [ii] are the only two possibilities available. To see why, reconsider:

[ii] There are now, in the actual world, four book types authored by Charles Chihara.

Recall that [ii], as Burgess understands [ii], implies that there exists in the actual world abstract entities, namely book types. However, given that [ii] is to be so understood, then in addition to [i] and [ii], there is also a third possibility:

[iii] There are now four books authored by Charles Chihara, even though there exist in the actual world no abstract entities that are book types.

Given what Burgess has shown thus far, there is no reason why [iii] should not be taken to be a real possibility. I thus conclude that Burgess’s ordinary language dilemma argument fails to show that there exist “nonmathematical universals or attributes” or, more specifically, that there exist in the actual world four abstract entities (book types) authored by Charles Chihara.

2. Burgess’ Conception of Philosophy
Given the sort of conception of philosophy of mathematics that he has acquired: what, I wonder, does he think the goals of philosophy of mathematics are? (Compare Burgess’s minimalism with my own conception of philosophy of real mathematics that I sketched in the Introduction). This question came to mind when I learned recently that Burgess had bid “farewell to the issue of nominalism” and had “taken leave of the issue of nominalism” ({Burgess, 2008 #544}, p. 6). How could he, I thought, leave this area of philosophy in the messy, fragmented, and incomplete state in which he would be leaving it? He is convinced that myriad abstract mathematical objects exist; that the mathematical theorems of real mathematics are propositions about these abstract objects; and that mathematicians have come to know countless truths about these abstract objects. But then his minimalist conception takes hold and he seems to just stop there, leaving unanswered a number of fundamental epistemological questions that cry out for answers: How were we humans able to obtain our supposed knowledge of the existence and properties of these mathematical objects that he believes to exist? What sort of entities are these mathematical entities that he believes in? Do they exist eternally? Do they exist in space-time? What role do they play, if any, in the empirical world? How have mathematicians succeeded in discovering the many truths about these abstract objects that we call “theorems of mathematics”? Why do we have to refer to these entities? How do we manage to refer to them?

In contrast to Burgess’s minimalist response, Michael Resnik writes: “[N]o matter how strong the prima facie case for mathematical realism, it cannot stand as an ontological doctrine alone. It must be combined with a plausible epistemology” ({Resnik, 1997 #314}, p. 85).

Perhaps Burgess fails to respond to the above epistemological questions (about how we obtain knowledge of the existence and properties of
mathematical objects) because he believes that such questions can be answered in a straightforward and simple manner.

3. The Benacerraf Problem

Consider in this light Burgess’s answer to the following question, which he calls the “Benacerraf Problem”: How could we come justifiably to believe anything implying that there are numbers, given that it does not make sense to ascribe location or causal powers to numbers? Burgess’s response to the problem is summed up in the motto: “Don’t think, look!” In particular, look at how mathematicians come to accept:

[1] There are numbers greater than $10^{10}$ that are prime.

That, according to Burgess, is how one can come justifiably to believe something implying that there are numbers ([Burgess, 2005 #487], p. 87).

Burgess’s “solution” to the Benacerraf Problem is at the core of his antinominalism. Thus, recall the “minimal anti-nominalism” of [Burgess, 1997 #227], where they write: “Having studied Euclid’s Theorem we are prepared to say that there exist infinitely many prime numbers”. There, they understand Euclid’s theorem in the standard straightforward literal way that some logicians such as Quine tend to understand such theorems, namely as a proposition about the actual world, writing: “Moreover, when we say that there exist infinitely many prime numbers, we say so without conscious mental reservations or purpose of evasion. . .” ([Burgess, 1997 #227], pp. 10). This acceptance of the existence of infinitely many prime numbers amounts to their acquiescence

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247 In his review of my book A Structural Account of Mathematics ([Burgess, 2005 #487], p. 87).
in the existence of numbers, even though “numbers are not supposed to be like ordinary concrete things like rocks or trees or people” (Burgess, 1997 #227, pp. 11).

I emphasize that the above minimal anti-nominalistic argument depends crucially on the assumption, doctrine, or credo that Euclid’s theorem is a true proposition about the actual world—a consequence of the Fregean Assumption which the Princeton team accepts. The idea that such an existential theorem is a true proposition about the actual world is explicitly argued in great detail in another anti-nominalism argument that Burgess and Rosen present in their paper “Nominalism Reconsidered”. This argument (which contains the principal idea of the “fifth campaign”) has premises that they describe as “scarcely deniable” and will be investigated shortly.

Now the “Don’t think, look” answer that Burgess gave to the Benacerraf Problem may be thought to provide an answer to the above epistemological question about how we obtain knowledge of the existence and properties of mathematical objects. Recall that Burgess’s answer basically came down to this: One can learn of the existence and properties of mathematical objects by looking at the proofs of such theorems. The proofs tell us how it is that we can learn of the existence and properties of mathematical objects: it is by logical deductions.

Now, I find it hard to understand how Burgess could regard the above “Don’t think, look” justification as philosophically satisfactory. For there is a

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248 [Burgess, 2005 #483]. According to the jacket cover, each volume of The Oxford Handbook series “offers an authoritative and state-of-the-art survey of current thinking and research in a particular area.” Presumably, the anti-nominalism argument being discussed here presents the current thinking and research on nominalism of the two authors.
general problem with this kind of justification that Burgess has not addressed. Let us recall Aristotle’s discussion of deductive knowledge in his Posterior Analytics. If our knowledge of the existence and properties of mathematical objects is obtained by deductions (that is, the kind of knowledge that Aristotle called “scientific knowledge”)—deductions such as what the mathematician provides in proving Euclid’s theorem about the infinity of primes—then we need to take account of the fact that a deduction always involves deducing something from something—that is, there are premises to be considered. So suppose that we know that some proposition φ about mathematical objects is true by means of a deduction of proposition φ of the sort that is given by the “Don’t think, look!” answer. Now one cannot obtain knowledge of a proposition by means of a deduction of proposition φ from a list of premises if the premises are not themselves known to be true. So the question arises, how did we obtain the required knowledge of the premises of the deduction by which we came to know that φ is true? Well, it might be said that this knowledge of the premises was itself obtained by deduction. But then, how did we obtain the knowledge of the premises of that deduction? By deduction? Clearly, we cannot continue in this way ad infinitum. There must be an end to this sequence of answers. So we must have some way of obtaining knowledge of the existence and properties of mathematical objects that is not deductive knowledge. But what is that way?

Ponder a specific proposition about mathematical objects that was not obtained by deductions from other premises, say (for the sake of argument) that it is the proposition that an empty set exists. How have mathematicians obtained the knowledge that an empty set exists? Not through some sort of direct observation or perception. No mathematician claims to have observed or
perceived the empty set.\textsuperscript{249} Nor have any experiments been performed that allowed mathematicians to see that there must exist an empty set. So how have we come to have the knowledge that such an entity exists? More generally, how have we managed to obtain our supposed knowledge of the existence and properties of any sets? They are not supposed to exist in physical space. They are not supposed to be detectable with the most sophisticated of our scientific instruments. So how was this supposed knowledge obtained? Aren’t these questions that any philosopher of mathematics seeking to arrive at an acceptable and defensible account of the nature of mathematics should have asked and pondered? Should we not be surprised to find nothing in Burgess’s publications that indicates that Burgess has ever worried about such questions or that he has made any sort of serious attempt to answer them?

The epistemological problem I have just discussed is only one of the many problems facing a serious realist attempting to frame a coherent and adequate account of the nature of mathematics and mathematical knowledge. Another is the problem of trying to explain why the scientist needs to know so much mathematics in order to understand the workings of the physical world, given the realist’s view that mathematics is directed at informing us of the existence and properties of nonphysical entities that do not even exist in our physical world. These apparent facts about science and mathematics, interpreted in the metaphysical way the Realist advocates, implies that the empirical scientist needs to know a great deal about how various non-physical entities are related to one another in a world from which we are forever isolated--this in order to understand the workings of the physical entities in the world we do inhabit. Surely, this is a problem that realists (or anti-nominalists) should find intriguing,

\textsuperscript{249} Penelope Maddy held at one time that we humans are able to see sets (in \cite{Maddy,1980 #167} and \cite{Maddy,1990 #207}), but even she never claimed that the empty set was observable.
philosophically “meaty”, downright puzzling, and something that a realist should investigate.\textsuperscript{250} Evidently, these problems (and those I discussed in the Introduction to this work) are not sufficiently important to motivate Burgess to continue his researches into the Platonism-nominalism controversy and to prevent him from leaving this whole area of philosophy. So I am left with the picture of a philosopher spending many years attacking the nominalist’s conception of mathematics, but never taking the time and energy required to attempt to develop anything like an adequate alternative Platonic view of mathematics.

Contrast Burgess’s attitude toward philosophy of mathematics with Resnik who writes in his book Mathematics as a Science of Patterns: “[T]hroughout the book I have tried to combine my doctrines into a coherent system” (\textsc{Resnik, 1997 #314}, p. 270). Burgess tries instead to disclose as little as possible about how he views mathematics. We are left with little more than the minimalist view with which Burgess begins his exposition of his Realist view of mathematics.

4. The Fourth Campaign: Look and See

The fourth campaign consists of the “look and see” argument of \textsc{Burgess, 2005 #487}, according to which, to see that nominalism is false, one need only look and see how mathematicians have come to accept an existential mathematical theorem such as:

\textsuperscript{250} The problem discussed in this paragraph is clearly related to “the metaphysical problem of applicability” or “Dummett’s Puzzle” which I examined in \textsc{Chihara, 2004 #468}, pp. 233-5.
There are numbers greater than $10^{10}$ that are prime.\textsuperscript{251}

According to Burgess “That’s how one can come justifiably to believe something implying that [there are numbers]” and hence that nominalism is false. Thus, underlying Burgess’s reasoning is the belief that one can justifiably come to believe that the sentence

There are numbers greater than $10^{10}$ that are prime

expresses a true proposition about the actual world. And he draws this conclusion because he believes that, in general, mathematical theorems are propositions about the actual world (the Fregean Assumption). The role of the Fregean Assumption in Burgess’s anti-nominalism arguments will become even clearer in the following section.

5. The Fifth Campaign: The Scarcely Deniable Argument

This campaign consists of the main anti-nominalism argument that Burgess and Rosen present in their paper “Nominalism Reconsidered”.\textsuperscript{252} Recall that, because this argument has premises that are said to be “scarcely deniable”, I call this argument “the scarcely deniable” argument”. It constitutes the central part of their “fifth campaign”.

\textsuperscript{251} In [Burgess, 2005 #487], Section 6.

\textsuperscript{252} [Burgess, 2005 #483]. According to the jacket cover, each volume of The Oxford Handbook series “offers an authoritative and state-of-the-art survey of current thinking and research in a particular area.” Presumably, the anti-nominalism argument being discussed here presents the thinking and research on nominalism of the two authors that was “current” in 2005.
Here are “the scarcely deniable” premises of the “the scarcely deniable” argument:

(1) Mathematics abounds in existence theorems that seem to assert the existence of mathematical objects.

(2) Scientists and mathematicians accept these existence theorems in the sense both that they assent verbally to them without conscious silent reservations, and that they rely on them in both theoretical and practical contexts. ([Burgess, 2005 #483], p. 516).

(3) The existence theorems are not merely accepted by mathematicians, but are acceptable by mathematical standards”. ([Burgess, 2005 #483], p. 516).

(The authors add the explanatory comment that they are accepted and acceptable by such standards because they are proved in an acceptable way).

From these three premises, it is argued that:

[C-1] Competent mathematicians and scientists believe that prime numbers greater than 1000, groups of various orders, etc. exist

From this intermediate conclusion, they conclude:

[C-2] We are justified in believing that prime numbers greater than 1000, groups of various orders, etc. exist.
These authors support these conclusions additionally by pointing out that empirical scientists “generally defer to the mathematicians on mathematical questions, existence questions included” (Burgess, 2005 #483, p. 517). They add that there is no empirical scientific evidence against mathematical existence theorems, and there is no philosophical argument powerful enough to undermine mathematical standards of acceptability (Burgess, 2005 #483, p. 517). So they feel confident in drawing the “ultimate anti-nominalist conclusion”:

We are justified in believing (to some high degree) in prime numbers greater than a thousand, abstract groups of various orders, . . . , which is to say we are justified in disbelieving (to the same high degree) nominalism. (Burgess, 2005 #483, p. 517).

I shall begin my response by focusing on the three principal premises of the above argument.

The first premise (as I understand it) is unobjectionable. That there are existence theorems in mathematics, i.e. theorems of mathematics that are straightforwardly existential in logical form, would be disputed by few, if any, philosophers.

The second premise (that mathematicians and scientists accept these theorems) also seems unobjectionable—but in this case there is an ambiguity that should be clarified. Presumably, this premise is not the weak statement that there are some mathematicians and scientists who accept these theorems,
but rather the stronger claim that almost all, practically all, or the vast majority of mathematicians and scientists accept these theorems.

The third premise seems to be equally unobjectionable, at least as first stated. Yes, these existential statements are acceptable by mathematical standards. However, the question that needs to be asked is: In the case of these latter two premises, how should we understand ‘accept’? Let us concentrate on the following more specific question: What is it that is being accepted by mathematicians and scientists when they accept a mathematical theorem? More specifically, what proposition is being accepted as true when an existential theorem is accepted? We know what Burgess and Rosen believe is that the existential theorem is being accepted as true. More specifically, they believe that what is being accepted as true is a proposition of the form: “there exists in the actual world a mathematical entity x such that . . . “, and they have that belief because they make the Fregean Assumption. Thus, from the fact that

There are infinitely many prime numbers

Is a theorem of number theory, they conclude that

There are infinitely many prime numbers

Is a true proposition about the actual world. Clearly, the Euclidean theorem that there are infinitely many primes is, for them, the proposition

There exits, in the actual world, infinitely many prime numbers.
That is why they believe they can draw the “ultimate anti-nominalist conclusion” that we are justified in disbelieving nominalism. So the soundness of this argument is dependent upon their acceptance of the Fregean Assumption, as did the previous anti-nominalism argument.

Obviously, the Burgess-Rosen “scarcely deniable” argument cannot be considered a successful anti-nominalist argument as it stands, since it rests upon an unstated and unjustified assumption—namely, the Fregean Assumption. Later, in Chapter 7, I shall explore the plausibility of this unjustified assumption, but for now it can already be seen that, as it stands, the “scarcely deniable” argument is in no way the ultimate anti-nominalism argument that the authors had proclaimed it to be.

Before leaving the topic of the “campaigns”, it should be noted that each of the five campaigns, except one (the “Ordinary Language” dilemma of Section 1 B), depended in some way upon the acceptance of the Fregean Assumption.
Chapter 8

The Role and Truth Value of the Fregean Assumption

Introduction

This chapter will explore the role that the Fregean Assumption has played in arguments that philosophers have devised for justifying their belief that mathematical objects exist. It turns out that the Fregean Assumption has some important features possessed by what has been called, in the philosophy of science, the “Received View” of the nature of scientific theories. The Received View construed scientific theories as “axiomatic calculi which are given a partial observational interpretation by means of correspondence rules”. This view was so widely accepted at one time that one historian and philosopher of science has asserted that: “virtually every significant result obtained in philosophy of science between the 1920s and 1950 either employed or tacitly assumed the Received View” ({Suppe, 1974 #469}, p. 3). Of course, the “Received View” no longer has such an exalted position.

In the philosophy of mathematics, we might refer to the Fregean Assumption as “the Received View of the Philosophy of Mathematics”, since like the “Received View of the Philosophy of Science”, the Fregean Assumption is so generally accepted that it is now widely regarded as not in need of justification. In addition to the pioneering giants, Frege, Russell and Whitehead, Gödel, and Quine, many more recent well-known philosophers of mathematics, such as the Burgess-Rosen team, the neo-Fregeans Hale and Wright, the Mathematical
Structuralists Resnik and Shapiro, as well as the the “Fictionalists” Field and Balaguer, have all accepted the Fregean Assumption. Thus, it would not be too great a stretch to call the Fregean Assumption “the Received View of the Philosophy of Mathematics”. However, despite the impressive list of well-known philosophers of mathematics who have accepted this version of the “Received View”, it is, like the other one in the philosophy of science, an assumption that, I shall argue, is both unjustified and false. Thus, I shall begin this chapter by providing my answer the question: What is the truth value of the Fregean Assumption? The reader may be surprised to find me attempting to answer this question, since the Fregean Assumption seems to have been accepted as true by the majority of the well-known philosophers of mathematics of the Contemporary Period, generally without any fanfare, explanation, or justification--it is, for the most part, just tacitly assumed to be true.

1. Have the Existential Theorems of Mathematics Been Shown to Assert the Existence of Mathematical Objects in the Actual World?

Consider Euclid’s number-theoretic theorem that asserts that there are infinitely many prime numbers. Does that mathematical theorem assert that there exist in the actual world infinitely many prime numbers? More generally speaking, should the existential quantifier ‘there exists a number x such that’ be understood to assert ‘there exists in the actual world a number x such that’? I shall argue, in what follows, that it should not be so understood. My strategy will be to examine a standard proof of an existential theorem of number theory and then show that the proof does not prove that some mathematical object (or objects) exists in the actual world. In particular, I shall take the example that Burgess and Rosen use in their Platonic account of mathematics, namely
Euclid’s theorem that there are infinitely many prime numbers. Here’s how the proof of the theorem goes.

1. Assume, for a reductio ad absurdum proof, that there are only finitely many prime numbers: \( P_1, P_2, P_3, \ldots, P_n \).
2. Then the product of all these numbers, say, \( Pr \), exists.
3. \( Pr + 1 \) is greater than each prime number in the product, so it is not prime.
4. So some prime number, \( D \), divides \( Pr + 1 \).
5. Since \( D \) is prime, and \( Pr \) is the product of all the primes, \( D \) divides \( Pr \).
6. Hence, \( D \) divides both \( Pr + 1 \) and \( Pr \).
7. It follows that \( D \) divides 1 (which is impossible).

Now the question I shall consider is:

Does this proof show that there are infinitely many prime numbers in the actual world?

Well, the proof does show that the assumption that there are only finitely many prime numbers is not consistent with the standard postulates of our number system. But that is not tantamount to showing that infinitely many prime numbers exist in the actual world. After all, the standard postulates are neither self-evident truths about the actual world nor proved to be true from other self-evident truths about the actual world. Notice also that nowhere in the proof does the phrase “actual world” (or any phrase equivalent to it) occur. For example, line 2 does not assert that \( Pr \) exists in the actual world. Furthermore, most mathematicians do not conclude from Euclid’s proof that infinitely many
prime numbers exist in the actual world. The Burgess-Rosen team simply assumes that each existence assertion of mathematics is an assertion that some mathematical entity exists in the actual world. Such an assumption—a special case of the Fregean Assumption—is never justified or even explained. Hence, their inference, from the Euclidean theorem in question, to the conclusion that prime numbers exist in the actual world, and hence that nominalism is false, is simply a gratuitous inference made without justification or genuine mathematical grounds.

2. Burgess-Rosen Solution to the Benacerraf Problem

Recall (from the discussion of Section 20, Chapter 5) that what Burgess and Rosen call the “Benacerraf Problem” is expressed by the question:

How could we come justifiably to believe anything implying that there are numbers, given that it does not make sense to ascribe location or causal powers to numbers?

The Burgess-Rosen team’s solution to the Benacerraf problem, you will recall, can be summed up in the motto: “Don’t think, look!” Supposedly, we need only look at how mathematicians justify such theorems or assertions of number theory as:

[#] There are infinitely many prime numbers

to see how Benacerraf’s problem can be solved. Thus, if Euclid’s proof is treated as a proof from premises that are propositions about the actual world
(as Burgess and Rosen understand the premises), and if we extend the reasoning of the proof by one obvious step, then we seem to obtain what might considered a “proof” of the metaphysical conclusion that there exist numbers in the actual world, and hence, a “justification” for the belief in numbers that should convince a reasonable person. Such reasoning seems to underlie the Burgess and Rosen belief that they have solved Benacerraf’s problem.

However, upon reexamination, one can find solid reasons for questioning the above “justification” of the Platonic position. One reason I have doubts about the above reasoning is because, for it to be a sound and reasonable justification for the belief in the existence in the actual world of numbers, the existence in the actual world of numbers cannot be assumed or presupposed at any point in the above “proof” (assuming that the justification is not to beg the question at issue). Now consider the assumption that begins the proof:

Either there are infinitely many primes or there are finitely many primes.

How should we understand that premise? Either the phrase ‘there are’ is to be understood to mean ‘there exist in the actual world’ or one can give some other mathematical reading to the phrase ‘there are’ that is not about existence in the actual world.

Let us first give the phrase the “existence-in-the-actual-world” reading. The disjunctive premise then asserts:

Either there are in the actual world infinitely many primes or there are in the actual world finitely many primes.
Now this premise is highly questionable in the absence of any justification, since as it stands, it implies that there exist prime numbers in the actual world. Why on earth should any one accept such a premise without any justification being provided? It is hardly the sort of proposition that mathematicians as a group have accepted as “self-evident” or, indeed, should accept as “self-evident”. So why should they believe that any prime numbers at all exist in the actual world? More specifically, why should mathematicians start a proof with such a questionable premise?

So let us drop from the reconstructed Euclidean proof the idea that we are concerned with what exists in the actual world. Let us, in other words, think of ‘exist’ as being used in the proof in a particular mathematical sense in which we say such things as, “For every natural number n, there exists a natural number m that is the successor of n”. (This mathematical sense will be discussed in detail in the following chapter). Evidently, then, the Euclidean proof concludes that ‘There exists infinitely many prime numbers’, where it is not being asserted or implied that there exists in the actual world infinitely many prime numbers. If it is not being assumed in the proof that there are any prime numbers in the actual world, then the conclusion ‘There are infinitely many prime numbers’ is not the assertion that there are infinitely many prime numbers in the actual world. In other words, the Euclidean conclusion that there are infinitely many prime numbers does not imply, as Burgess and Rosen believe, that there exist in the actual world prime numbers; and hence the Euclidean theorem does not require a rational thinker to conclude that nominalism is false.

Euclid’s proof of his theorem does not involve proving that there exist prime numbers in the actual world. The proof does not first show that there are prime numbers in the actual world, and then show that there are infinitely many
of them in the actual world. It only proves that there are (in some appropriate mathematical sense) infinitely many prime numbers, given that there are (in that sense) prime numbers. And this fact suggests that Euclid did not intend his proof to be a proof that there exist infinitely prime numbers in the actual world: it provides us with yet another reason for doubting the truth of the Fregean Assumption.

3. The Falsity of the Fregean Assumption

Although I have already given several reasons for having serious doubts about the truth of the Fregean Assumption, I shall now provide a more direct undermining of that assumption, upon which so much of the Burgess-Rosen philosophy of mathematics hangs. Let us now suppose, for the sake of argument, that the Fregean Assumption were true. In other words, let us assume, as the Burgess-Rosen team does, that:

[##] The theorems of number theory are propositions about the actual world.

Since the theorems of number theory are generally thought to be true, it would then be reasonable to conclude, from [##], that:

[@] The theorems of number theory are true propositions about the actual world.

\[253\] I pointed out earlier that fictionalists believe that all mathematical theorems are false. This is because they believed the Fregean Assumption that all mathematical theorems are propositions about the actual world. Of course, as I also pointed out, there are good grounds for rejecting their position.
In that case, Euclid’s theorem about primes must also be a true proposition about the actual world. Furthermore, all the premises in the proof of Euclid’s theorem must also be true propositions about the actual world. So let us try to reconstruct the Euclidean proof, eliminating the (supposed) ellipses by making it explicit that each statement of the proof expresses a true proposition about the actual world. In other words, let us reconstruct, if we can, Euclid’s proof when it is considered as a proof that there exist infinitely many prime numbers in the actual world. Then, the target conclusion of the proof would be:

There exist in the actual world infinitely many prime numbers.

Since one is trying to prove by reductio ad absurdum that there exist in the actual world infinitely many prime numbers (as Burgess and Rosen believe), the supposition that begins the proof must be a proposition the negation of which expresses the proposition that there are infinitely many prime numbers in the actual world. So the supposition that begins the reductio ad absurdum proof must be the supposition that there exists in the actual world at most finitely many prime numbers—a supposition that is compatible with there being no prime numbers at all in the actual world. But in that case, the second sentence in the proof does not follow from the supposition: the supposition that there exist in the actual world at most finitely many prime numbers does not imply that, in the actual world, there is a product of all the primes (since there may be no primes at all in the actual world). Thus, if one were to interpret the Euclidean theorem, as well as the proof of the theorem, in the Fregean way that Burgess and Rosen do, then the Euclidean proof would turn out to be defective.
Since essentially all mathematicians believe that Euclid’s proof is perfectly valid and correctly reasoned, we can infer from the above reasoning that mathematicians do not interpret arithmetical theorems in the Fregean fashion that the Burgess-Rosen team does (that is, as being propositions about the actual world). And this implies that the Burgess-Rosen team’s Fregean manner of understanding arithmetical theorems does not fit the way mathematicians in general understand arithmetical theorems. More specifically, it implies that:

[C7-A] Arithmetical theorems are not standardly understood by mathematicians to be propositions about the actual world.

Now [C7-A] is not a surprising result, since it is supported by the fact that there are significantly many mathematicians who are not Platonists.

Continuing the reasoning that started with [C7-A], if mathematicians do not, in general, understand their theorems to be propositions about the actual world (as was just concluded in [C7-A]), then it is safe to say that arithmetical theorems are not in fact propositions about the actual world. This is because, if mathematicians do not, in general, understand their theorems to be propositions about the actual world, then arithmetical theorems are undoubtedly not propositions about the actual world. After all, who should know if arithmetical theorems are propositions about the actual world better than the people who formulate, prove, and promulgate the very propositions in question? Surely it would be the mathematicians.

So now we can conclude from [C7-A] that:
[C7-B] Arithmetical theorems are not propositions about the actual world.

And if mathematical theorems are not propositions about the actual world, then it directly follows that the Fregean Assumption is false, since the Fregean Assumption asserts that mathematical theorems are propositions about the actual world. Thus, we have strong grounds for concluding:

[C7-C] The Fregean Assumption is false

Now the case against the truth of the Fregean Assumption is stronger yet. Recall Cohen’s comment discussed in Chapter 1, Section 6 that “most of the famous mathematicians who have expressed themselves on the [Realism-nominalism] question have in one form or another rejected the Realist position” ([Cohen, 1971 #297], p. 13). If the Fregean Assumption were true—if, in other words, mathematical theorems were indeed propositions about the world—then one could expect most mathematicians to know that mathematical theorems were propositions about the world. Thus, if the Fregean Assumption were true, then one could also expect that most of the famous mathematicians would know that the Euclidean Theorem about the infinity of primes is a proposition about the world and hence that the theorem asserts the existence of infinitely many primes in the actual world. It follows that most of the famous mathematicians who have expressed themselves on the [Realism-nominalism] question could reasonably be expected to regard the Euclidean theorem about the infinity of primes to be a proposition about the actual world and hence as asserting the existence of infinitely many prime numbers in the actual world. Since mathematical theorems are believed to be true, one would expect most of
the famous mathematicians to believe that there are prime numbers in the actual world and hence one would expect most of these mathematicians to be Realists. Since such an expectation conflicts with Cohen’s observations about how most of the famous mathematicians have expressed themselves on the Realism-nominalism question, we have still another reason for doubting the Fregean Assumption that Burgess-Rosen make.

We now have substantial grounds for concluding that the Burgess solution to the Benacerraf problem is doubtful indeed. The solution is doubtful because it presupposes the truth of the Fregean Assumption—which has been shown to be seriously doubtful. In Chapter 9, I shall provide additional reasons for concluding that the Fregean Assumption is false.

4. Other Consequences of Rejecting the Fregean Assumption

The implications of the thesis that the Fregean Assumption is false are far from insignificant. In the following chapters, some of the far-reaching consequences of rejecting the Fregean Assumption will be presented in detail. It will be shown that a great many of the most eminent philosophers of mathematics of the Contemporary Period, including the “big four” pioneering philosophers of mathematics (Frege, Russell, Gödel, and Quine), have been fundamentally mistaken about the nature of mathematical truths and mathematical theorems, and once this mistake is properly understood, a number of difficulties that Platonic philosophers of mathematics have faced, will disappear.

Finally, I emphasize that, although I have already provided substantial grounds for concluding that the Fregean Assumption is false, since the
assumption has been accepted by such important philosophers of mathematics, additional grounds for this conclusion will be supplied in Chapter 9.

Before leaving the topic of the Fregean Assumption in this chapter, I need to say a few words about another thesis that I advanced earlier: this is the thesis that mathematical theorems are in some sense true. Since I just concluded that the Fregean Assumption is false, it would reasonable for the reader to assume that what I probably believe is that mathematical theorems are true propositions—just not true propositions about the actual world. Hence, the reader can anticipate that I shall propose that there is a very real sense of ‘true’ in which mathematical theorems are true—just not true propositions about the actual world. I shall develop this proposal in the next chapter.

5. A Reexamination of the Proof From “Scarcely Deniable” Premises

Let us now comment on the previously described Burgess-Rosen “scarcely deniable” argument described in Section 5 of Chapter 7. I shall concentrate on the three principal premises of that argument.

The first premise: That there are existence theorems in mathematics—that is, theorems of mathematics that are existential in logical form—would be disputed by few, if any, philosophers. This premise is unobjectionable.

The second premise: This premise asserts that mathematicians and scientists accept these existential theorems—something that, at first sight, certainly seems unobjectionable. However, one can see that the second premise is not “scarcely deniable” if the theorems are understood in the way Burgess and Rosen understand theorems—as true propositions about the actual
world. This is because the Burgess-Rosen way of understanding mathematical theorems is based on an assumption (the Fregean Assumption) that has been shown to be questionable. Furthermore, if one focuses on such areas of mathematics as set theory and analysis, then the second premise can be seen to be very doubtful indeed.

The third premise: This premise also seems to be unobjectionable. Yes, these existential statements of mathematics seem to be both accepted by mathematicians and also acceptable by mathematical standards—but not if the statements are understood in the Fregean way that the Burgess-Rosen team interprets them to be (i.e., to be true propositions about the actual world). Notice that, to be able to infer [C-1] from [1], [2], and [3], the existence theorems mentioned in premise [3] must be understood to be true propositions about the actual world. In other words, the Fregean Assumption is an unstated but tacit assumption of the argument. And that assumption is not “scarcely deniable”.

In summary, the Burgess-Rosen proof from “scarcely deniable” premises that we have overwhelming grounds for believing in the existence of mathematical objects makes use of an unstated assumption—the Fregean Assumption—that is crucial to the cogency of the argument. When this unstated assumption is uncovered to be part of the second and third premises, then those premises also can be seen to be dubious, and it can be inferred that the argument does not at all proceed from premises that are truly “scarcely deniable”. Given the earlier argument for the conclusion that the Fregean Assumption is false, we have more than enough reasons for rejecting the Burgess-Rosen proof from “scarcely deniable” premises.
6. The Fregean Assumption in Other Branches of Mathematics

So far, I have focused on theorems of number theory in assessing the reasonableness of the Fregean Assumption. This is because Burgess and Rosen generally take their examples of mathematical theorems and truths from the field of number theory, and also because number theory is one of the least philosophically controversial areas of mathematics. What about the other areas of mathematics such as analysis and set theory? Let us consider the theorems of set theory.

If the theorems of set theory have been shown to be truths about the actual world, then the axioms of set theory must also have been shown to be truths about the actual world. However, when we consider some particular set theory, say ZF, we see that set theorists do not, as a rule, attempt to prove that the axioms of ZF are true propositions about the actual world, as can be verified by examining text books that spell out the axioms of the theory. As an example, I shall examine, in what follows, how an influential advanced logic text (Shoenfield 1967) justifies the axioms of ZF.

(a) Shoenfield’s Views on Logic and Mathematics

In his text book, Joseph Shoenfield sets out to describe “the methods of the mathematician”, writing that a “conspicuous feature of mathematics, as opposed to other sciences, is the use of proofs” (p. 1). However, since it is impossible to prove all mathematical laws, he tells us that there are in mathematics certain first laws (called “axioms”) which mathematicians accept
without proof. He asks: “For what reason do we accept the axioms?” He answers that we “attempt to select as axioms certain laws which we feel are evident from the nature of the concepts involved” ((Shoenfield 1967), p. 1, italics mine). Corresponding to the “first laws” or axioms, there are “basic concepts” which are not defined, while the remaining concepts that the mathematicians use are defined in terms of the basic concepts. The basic concepts are supposed to be “so simple and clear that we can understand them without a precise definition” ((Shoenfield 1967), p. 1).

Here, then, is how Shoenfield describes what the mathematician does: He first presents us with certain basic concepts and certain axioms about these concepts. He then explains these concepts to us until we understand them sufficiently well to see that the axioms are true. He then proceeds to define derived concepts and to prove theorems about both the basic and derived concepts. The entire edifice which he constructs, consisting of the basic concepts, derived concepts, axioms, and theorems, is called an axiom system. ((Shoenfield 1967), p. 2).

To better understand the above overview of mathematics, consider now in more detail how Shoenfield explains and characterizes the axioms of Zermelo-Fraenkel set theory. Following the description given above, Shoenfield describes the basic concept of set by describing what is sometimes called the “iterative concept of set”: he does this by describing more specifically the way in which sets are formed in successive stages to form “the cumulative hierarchy” that characterizes Zermelo-Fraenkel set theory:

We start with certain objects to form a single object which is the set A. Thus before the set A is formed, we must have available all of the objects which are to be members of A. It follows that the set A is not one of the possible members of A . . .
We are thus led to the following description of the construction of sets. We start with certain objects which are not sets and do not involve sets in their construction. We call these objects urelements. We then form sets in successive stages. At each stage we have available the urelements and the sets formed at earlier stages; and we form into sets all collections of these objects. A collection is to be a set only if it is formed at some stage in this construction. ([Shoenfield, 1967 #375], p. 238).

This characterization is supposed to convince the reader that the basic concept of set is “so simple and clear that we can understand [it] without a precise definition”. The next step is to convince the reader that the axioms of ZF are evident from the nature of the basic concept of set we had been given. What follows are the usual “proofs” of the axioms—proofs that proceed from the descriptions given of the above hierarchical structure.

What is striking about Shoenfield’s procedure is how he describes what his proofs show. For example, he tells us that one proof he gives enables us to “see that the regularity axiom is true” (p. 239). Notice that he characterizes the axiom as being true, and not merely true of the iterative concept of set or true in the cumulative hierarchy structure. In other words, this mathematician is following the practice of the algebraist, who in proving the theorem ‘the inverse of a sum is the sum of the inverses in the reverse order’, asserts that the theorem is true, and not merely that the theorem is true of all group structures. What is clear from such examples, however, is that these mathematicians, in asserting the truth of the theorems they are proving, are not taking these theorems to be ontologically weighty assertions about the actual world, since they are in no position to make any such claim. But in proving that the theorem is true of the appropriate sort of structure, they may go on to say that the theorem is true (and not merely that they are true of the kind of structure in question).
Reexamining Shoenfield’s description of what the mathematicians does in general, we see that his description of how the mathematician justifies or proves the axioms of ZF is not supposed to be special—any axiom of a system that a mathematician may construct is supposed to be true in the way that the axioms of ZF are true—that is, true of the basic concept of the system or true in the structure that is conveyed by the basic concept. Thus, we can infer that Shoenfield does not regard any axiom or theorem of mathematics to be an ontologically weighty proposition about the actual world. And such a view is supported by other mathematicians. For example, Hirst and Rhodes write:

It is not the aim of mathematics to declare what is true and what is false in the world, but rather to produce conceptual models of certain aspects of the world and to study within the models which statements are logical consequences of other statements. ([Hirst, 1971 #553], p. 63).

Schoenfield’s manner of justifying the axioms of ZF is by no means unique. One can find a similar kinds of justification of the axioms of ZF in other writings on set theory. More generally, the axioms of ZF have been “justified” in the following way: One first gives a description of some such notion as “the iterative conception of set” or “the intuitive notion of set”; one then provides a “justification” of the axioms of ZF based on this conception or notion. The justification is supposed to show that the axioms are “true for (or with respect to) this concept” ([Wang, 1983 #484], p. 530) or the axioms are mere expressions or “formalizations of the iterative conception” ([Boolos, 1983 #485], pp. 490 and 499).254 One does not find anywhere the claim that these justifications allow us to conclude that there actually are such things as sets, which exist in the way described by the iterative conception. (I shall continue

254 See also [Benacerraf, 1983 #205], pp.27-30, and [Shoenfield, 1967 #375], pp.238-40.
my discussion of Shoenfield’s justification of the axioms of ZF later in this work).

Furthermore, set theorists do not even attempt to show that the axioms of ZF are propositions about the actual world—something that can also be verified by examining text-books on set theory. Thus, contrary to what Burgess and Rosen claim in their argument, it seems quite plausible to hold that mathematicians do not accept the existential axioms of set theory—when these axioms are understood to be propositions about the actual world. That is, mathematicians do not accept these axioms in the sense that they assent verbally to these axioms, nor do they rely on them in theoretical and practical contexts—when these axioms are understood to be propositions about the actual world. Hence, it is possible that set theorists do not accept the truth of the existential theorems of set theory either—when the theorems are understood in the ontologically weighty way they are understood by Burgess and Rosen.

Now if the axioms have not been proven or shown to be propositions about the actual world, then clearly the theorems have not been proven or shown to be true propositions about the actual world either. Since mathematicians do not even attempt to prove or justify the hypothesis that the theorems of set theory are true propositions about the actual world, even when they are attempting to justify (in some sense) the axioms of set theory, we have solid grounds for questioning the truth of the Fregean Assumption itself when set theory is included in its scope.

(b) The Theorems of Analysis
Have the theorems of analysis been shown to be true propositions about the actual world? Well, the theorems of analysis are based on set theory, in so far as these theorems require assumptions about set existence. So if the existential theorems of set theory have not been shown to be true propositions about the actual world, how have the existential theorems of analysis been shown to be true propositions about the actual world? Obviously, regarding the question that begins this paragraph, analysis is in no better position to provide a positive answer (epistemologically speaking) than set theory is.

Since we have solid grounds for doubting the truth of the Fregean Assumption, we also have solid grounds for doubting the philosophical views about the nature of mathematics (such as those of Frege, Russell, Quine, the neo-Fregeans, and the Burgess-Rosen team) that are based upon the Fregean Assumption.
Chapter 9

A Hilbertian View of Mathematical Analysis

Introduction

Let us begin by considering the principal conclusion of the previous chapter, namely, that the Fregean Assumption is false: mathematical theorems are not propositions about the actual world. But are not students of mathematics taught that the theorems they learn and study are true? So if the theorems of, say, mathematical analysis, are not true propositions about the actual world, are they then true propositions about something other than the actual world. Evidently, there is need for some fundamental investigation into the nature of the theorems of analysis. Are they true propositions—but somehow

just not true propositions about the actual world?

My rejection of the Fregean Assumption is related to the Frege-Hilbert dispute. Recall that, in Chapter 1, I analyzed the crux of their disagreement as coming down to a difference in how the disputants viewed the theorems of geometry. Frege took the Euclidean position that the theorems of geometry are propositions about the actual world; whereas Hilbert regarded the theorems of geometry to be sentences that are true of (or satisfied by) certain sorts of geometric structures. Turning now more generally to the theorems of
mathematics (number theory and analysis), by undermining the Fregean Assumption in the previous chapter, I was, in effect, providing grounds for rejecting one of the two ways of viewing the theorems of mathematics described in Chapter 1, namely, the Fregean way. Now if the alternative way of viewing mathematical theorems and mathematical truths can be developed in an attractive fashion and then plausibly defended, we would have additional grounds for rejecting the Fregean Assumption. It is the aim of this chapter to show that the Hilbertian view of mathematical truth not only is a defensible position, but in fact is an independently plausible view of the truths of number theory and mathematical analysis.

Consider the following two theses about the nature of mathematical theorems that I have adopted and defended:

(i) Mathematical theorems are, in an appropriate sense, true (Chapter 3).
(ii) Mathematical theorems do not express propositions about the actual world—i.e. the Fregean Assumption is false (Chapter 7).

These two theses raised the following questions: How can a mathematical theorem be “true“, if it does not express a proposition about the actual world? Can there be a kind of true sentence that does not express a fact about the actual world? A related question is: How can mathematical theorems be fruitfully applied to the physical world if they do not express propositions about the world? In this chapter, I shall advance answers to these questions by first putting forward a conception of how the theorems of mathematical analysis can be understood to be, in an appropriate sense, true, even though the theorems do not express propositions about the actual world. I shall then explain how
these mathematical theorems can be fruitfully applied to the physical world (in scientific and technological reasoning) in a way that is consistent with such a conception of mathematical truth. But first, I need to take up what I call:

1. The Maddy Phenomenon

In an earlier work, I discussed Maddy’s explanation of the following phenomenon (which I shall call “the Maddy Phenomenon”): empirical scientists, physicists in particular, “seem happy to use any mathematics that is convenient and effective, without concern for the mathematical existence assumptions involved” ({Chihara, 2004 #468}, p. 288). According to Maddy, scientists were simply not concerned with assessing the ontological implications of the mathematics they use, writing: “If it were in that business, it would treat mathematical entities on an epistemic par with the rest, but our observations clearly suggest it does not” ({Maddy, 1997 #317}, 157). Similarly, Burgess inferred from the Maddy Phenomenon that “[o]ntological concern is foreign to the scientific culture”. According to Burgess, the mathematical theories scientists use, when literally construed, are in fact ontologically committed to mathematical objects (even if these scientists do not concern themselves with such logical niceties); so when scientists apply these mathematical theories in their scientific work, they should acknowledge the existence of mathematical objects.

In A Structural Account of Mathematics, I responded to Burgess’s position as follows:

I . . . have not taken a stand on what the theorems of mathematics mean, and hence have not

255 See {Burgess, 1998 #367}, p. 213.
committed myself to the doctrine that the mathematical theories scientists use are ontologically committed to mathematical objects. I have also maintained that, to apply these theories in their scientific work, scientists do not have to accept the literal truth of the theorems of these theories— it is enough to accept the structural content\textsuperscript{256} of these theorems.\textsuperscript{257}

Since the claim that mathematical theorems provide this structural information is significantly weaker than the claim that these theorems assert what holds in the actual world: the former claim does not require a deep analysis of what the theorem means or asserts and hence it does not presuppose detailed empirical studies of the linguistic practices of mathematicians. The empirical scientist was thus provided with the means of justifying their lack of concern about the ontological commitments of the mathematics they use. In the present work, as I shall show later, I do not require the notion of structural content to explain why scientists are perfectly reasonable in ignoring the supposed “ontological commitments” of mathematics.

2. The Structural Approach to Algebra

Two historians of mathematics have noted that until the 19\textsuperscript{th} century, algebra was largely the science of (determinate and indeterminate)

\textsuperscript{256} The italicized phrase “structural content” was defined as follows:

If $\phi$ is a sentence of a mathematical theory T, then the sentence

\begin{center}
Any model of T will have to be a model of $\phi$
\end{center}

will be said to express (or to give) the \textit{structural content} of $\phi$.

\textsuperscript{257} (Chihara, 2004 #468), p.288, italics mine.
equations\textsuperscript{258}, whereas in the 19\textsuperscript{th} century there appeared in it completely new concepts and objects, such as groups, rings, fields, ideals, . . . [which] brought about a changed view of the subject matter of algebra. Specifically, the task of algebra was now seen to be the study of systems of arbitrary nature for which there are defined operations with properties more or less similar to those of addition and multiplication of numbers” (A. B. Kurosh and O. Yu. Schmidt, The Great Soviet Encyclopedia, 2\textsuperscript{nd} ed., Vol. 1(Russian)).\textsuperscript{259}

In 1930, Bartel van der Vaerden published an influential textbook entitled “Modern Algebra”, which articulated the developing view of algebra that was then coming into vogue. The historian of mathematics Leo Corry describes this new view of algebra as the structural approach to algebra:

The essence of the structural approach to algebra lies in the recognition that it is mathematically enlightening to conceive a handful of concepts (groups, rings, fields, etc.) as individual “varieties” of the same mathematical “species”. . . , namely, the species of algebraic structures. With the adoption of this approach, the study of algebraic structures gave algebraic research a new focus, subsuming under it the traditional tasks of the discipline, namely, the study of polynomial forms and polynomial equations, and the problem of the solvability of polynomial equations. Moreover, under the new approach to algebra, this discipline came to

\textsuperscript{258} It is noted that:

A system of polynomial equations is said to be indeterminate if it has fewer equations than variables, and to be determinate if the number of equations equals or exceeds the number of variables. . . . The term ‘indeterminate’ is most commonly applied to systems with fewer equations than variables for which integer or rational solutions are sought. Such systems, also called ‘Diophantine’, have been particularly influential in the development of algebra . . . ((Bashmakova & Smirnova, 2000), pp. xv-xvi).

\textsuperscript{259} (Bashmakova & Smirnova, 2000), p. xiii.
cover under its unified scope, the study of other related, but theretofore separated domains of research, particularly algebraic number theory. 260

The developments being described above had far-reaching and long-lasting consequences in the field. 261 The subsequent widespread adoption of the structural approach both in algebraic research and in the teaching of the subject yielded many benefits: many open problems were solved in an economic and elegant way, previously solved problems were presented in a new and interesting light, and new mathematically intriguing problems were formulated ((Corry, 1996), p. 9). In a word, the new approach to algebra, which emphasized the study of algebraic structures, turned out to be mathematically and pedagogically fruitful and popular.

Without attempting any sort of deep analysis of these developments in the subject, one can see one obvious reason why the structural approach has appeared attractive to algebraists: the efficiency with which the subject matter of algebra can be studied and developed in terms of algebraic structures is a real advantage. It is the sort of efficiency that aids mathematicians in learning

260 (Corry, 1996), p. 9. One should not infer from the above that van der Waerden was solely or even principally responsible for advancing the structural approach to algebra. But, according to Corry, his textbook “assembled the important results that had been obtained during the last decades of research in its domain of concern and exposed them in a systematic, and didactically clear, fashion” ((Corry, 1996), p. 8). It should also be noted that the quotation from (Bashmakova & Smirnova, 2000) emphasizes that, until the 19th century, algebra was largely the science of both determinate and indeterminate equations, whereas the quotation from (Corry, 1996) gives no indication of the importance of indeterminate equations for the early algebraists. In the Introduction to their work, Bashmakova and Smirnova write:

Modern history of mathematics seems to be dominated by the view that up to the 1830s the mainspring of the development of algebra was the investigation and solution of determinate algebraic equations, and especially their solution by radicals. We will show that this viewpoint is one-sided and gives a distorted representation of its evolution. In short, we claim that the role of indeterminate equations in the development of algebra was no less important than that of determinate equations. ((Bashmakova & Smirnova, 2000), p. xv).

261 van der Waerden’s algebra text was still being referred to in algebra courses, even when I was a graduate student in mathematics.
and teaching the material. This efficiency can be observed in the following way of presenting the fundamentals of algebra.

Imagine a course in abstract algebra that begins with the theory of groups (which, you will recall, I discussed in some detail earlier in Chapter 3, Section 6). Then, after proving from the axioms the basic features of all groups, the term ‘ring’ can be defined as follows:

A ring is a system consisting of a set $S$ and two binary operations on $S$, plus (symbolized ‘+’) and times (symbolized ‘x’), such that:

1. the system consisting of $S$ with the operation plus is an abelian group;
2. $S$ is closed under the operation times, and times is associative;
3. for every $a$, $b$, and $c$ in $S$,
   \[ a \times (b + c) = (a \times b) + (a + c) \]
   \[ (b + c) \times a = (b \times a) + (c \times a) \]

An example of a ring that is frequently given is the integers under addition and multiplication. One can then apply to that part of a ring consisting of $S$ with plus all that was learned earlier about abstract groups. For example, it can be immediately inferred that, in a ring, there is only one additive identity; that any element’s right inverse is unique and is identical to the element’s left inverse; and that the inverse-of-a-sum law holds—that is, the inverse of a sum of elements is identical to sum of the inverses of the elements, taken in reverse order. Clearly, much redundancy of exposition and learning can, in this way, be eliminated.
Let us now reconsider the question: How can a mathematical theorem be “true“, if it does not express a proposition about the actual world? In the case of modern algebra, one answer is: an algebraic theorem can be true if it expresses a true proposition about relations that obtain among algebraic structures and elements of algebraic structures. A statement of group theory, for example, can be true if it expresses a true proposition about relations that obtain among groups and elements of groups--and such a proposition evidently need not be directly about the actual world.

One reason for having doubts about the claim that ‘There are infinite abelian groups’ expresses a proposition about the actual world can be found in the fact that if the claim were true, then ‘There are infinite abelian groups’ would be tantamount to saying ‘There are in the actual world, infinite abelian groups’. Few mathematicians, I believe, would regard the proposition expressed by

There are infinite abelian groups

as implying that

There are in the actual world infinite abelian groups.

This is because a great many (if not most) mathematicians are certain that the former sentences expresses a true proposition, whereas many mathematicians would have serious doubts that the second expresses a true proposition—especially those, such as the great logician Alfred Tarski or the well-known
mathematicians who were said by Cohen to have expressed doubts about Mathematical Realism (see Chapter 1, Section 6).

Of course, as I showed in Chapter 2, there have been a number of influential philosophers of mathematics who believed that algebraic theorems are theorems about the actual world. But such a belief has never been justified in any truly satisfactory way, and, for the most part, has simply been assumed. Indeed, according to the view I shall be defending, such a view is in fact fundamentally mistaken. The question of whether the theorems of modern algebra are propositions about the actual world basically reduces to the question of whether such existential assertions as ‘There exist infinite abelian groups’ assert the existence in the actual world of infinite abelian groups. Quine, Russell, Godel, the Princeton team of Burgess and Rosen, the neo-Fregeans Hale and Wright, and Maddy, are a few of the well-known philosophers of mathematics I mentioned earlier, as understanding the existential assertions of mathematics in this “actual-worldly” way. I, on the other hand, shall propose and defend in this chapter, an account of such assertions that renders them acceptable even to those nominalists who allow that the theorems of mathematics are in some sense true.

3. Do the Theorems of Set Theory Express Facts About the Actual World?

There are some philosophers, such as Maddy and Lewis,²⁶² who have asserted in their publications that the theorems of set theory express facts about the actual world and indeed are true propositions. These philosophers thus faced the daunting task of squaring their position about mathematical

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²⁶² Maddy in her [Maddy, 1990 #207] and Lewis in his [Lewis, 1986 #3].
existence with our best scientific views about how we obtain knowledge of the world around us. As a result, they have attempted to explain how we humans have come to know that the standard axioms of set theory are true—explain, that is, in a way that is scientifically respectable? In Chapter 2, Section 7, I described some of the perplexing philosophical difficulties that arise for anyone accepting Gödel’s version of Set Theoretical Realism. Those difficulties that I described are just a few of the problems facing the Set Theoretical Realist. Similarly, in Chapter 2, Section 2, I discussed Quine’s attempt to justify his belief in the existence of sets. Recall that he attempted to give such a justification by appealing what has been called an “Indispensability Argument”—an argument, as I noted in that chapter, that presupposes the Fregean Assumption and hence is undermined by the refutation of that assumption given in Chapter 7. It should be noted also that Quine’s reasoning was not directed at justifying the belief that some particular axiom or axioms of some specific set theory (such as ZF or his New Foundations) were in fact true, so his argumentation does not accomplish what is required of the Realist trying to justify the belief in the factual truth of the axioms of some particular set theory.

On the other hand, if the believers in the factual truth of the axioms of set theory admitted that they could not provide any justification for their belief, Maddy makes a valiant attempt to give such explanations in [Maddy, 1990 #207]. I raise a number of objections in [Chihara, 1990 #34], Chapter 10, to her views about how we have come to know: (1) that sets exist in the actual world; and (2) how sets are related to one another by the membership relation in the way described by our set theories. I should note that she has since abandoned her set theoretical realism in, for example, [Maddy, 1997 #317].

I raise a number of philosophical objections to Gödel’s attempt to explain how we have obtained knowledge of the existence and properties of sets in [Chihara, 1982 #168] and [Chihara, 1990 #34], Chapter 1, Section 3.

See [Chihara, 1990 #34], Chapter 10, especially Section 4.
then they would have to admit that their belief in the factual truth of some particular set theory is based upon faith of some sort and not upon reason. So far as I know, no well-known textbook on set theory has explicitly put forward the thesis that the axioms of some set theory are factually true. In fact, most attempts to justify the axioms of set theory that I am aware of have turned out to be attempts to justify claims that the axioms are appropriate or acceptable for certain mathematical goals or purposes—and not attempts to show that the axioms are all true of the actual world. What follows is a discussion of one such attempt to justify, in some sense, the axioms of ZF.

4. Shoenfield’s “Methods of the Mathematician”

I suggest that we reconsider Shoenfield’s “methods of the mathematician” that were detailed in Section 7 of the previous chapter of how axiom systems of mathematics are produced. It was shown there how a particular axiom system of mathematics, namely ZF set theory, could be set up and developed. We were first given a characterization of the “basic concept” of ZF (essentially what is sometimes called the “iterative concept of set”). This characterization is supposed to convince the reader that the basic concept of set is “so simple and clear that we can understand [it] without a precise definition”. The next step was to show that the axioms of ZF were evident from the nature of the “basic concept” of set that he had characterized. What

266 Cf. Mary Tiles’s description of the aims of her book on the philosophy of set theory (Tiles, 1989 #608):

The aim here is not to attack or undercut the idea of the actual infinite, but to explore it by finding how and why it became mathematically important. This is an indirect way of shedding light on the sense in which the domain of transfinite sets and numbers might be thought to constitute a reality. (p, 40).
followed were the “proofs” of the axioms—proofs that proceeded from the
descriptions given of the above hierarchical structure.\footnote{267}

Let us ponder Shoenfield’s description of what his proofs show. Consider,
in particular, his statement that one proof he gives enables us to “see that the
regularity axiom is true” (\{Shoenfield, 1967 #375\}, p. 239). Notice that he
characterized the axiom as being true—not true of the iterative concept of set
or true in the cumulative hierarchy structure, but simply true. This
characterization shows that mathematicians sometimes use the term ‘true’ in a
way that allows them to call a mathematical theorem true, even though it is not
true of the actual world. Thus, it is clear from Shoenfield’s description of the
“methods of the mathematicians” that, in asserting the truth of the theorems
they are proving, mathematicians are not always (if ever) taking these
theorems to be true propositions about the actual world: evidently, such
theorems are supposed to be true in another way. So it would seem that at
least some mathematicians believe that mathematical theorems can be true
without being true propositions about the actual world, but instead by being
true of (or satisfied by) some type of structure—specifically, the kind of
structure Shoenfield characterized in his “methods of the mathematicians”.
Furthermore, reexamining Shoenfield’s description of what the mathematician
does in general, we see that his characterization of how the mathematician
justifies or “proves” the axioms of ZF is not supposed to be unique—any axiom
of a system that a mathematician may construct is supposed to be true in the

\footnote{267} It is important to note that the “proofs” mentioned in Shoenfield’s discussion presuppose
that the axioms of ZF are interpreted in such a way that the variables in the axioms are taken to
range over the points of the hierarchical structure that had been described and that the binary
relational term (membership) occurring in the axioms is taken to denote the membership
relation of the structure.
way that the axioms of ZF are true—that is, true of the “basic concept” of the system or true of the kind of structure that is conveyed by the basic concept.

5. The Maddy-Parsons Criticism of Shoenfield’s View

Commenting upon the various “justifications” of the axioms of ZF of the sort that Shoenfield has given, Charles Parsons complains that the author’s talk of forming sets from its elements is merely metaphorical, and that such talk does not make clear “to what extent they are to be taken literally”, writing:

When we come to sets of sufficiently high rank, moreover, it is very difficult to take seriously the idea that all the intermediate sets that arise in the construction of this set from individuals can be formed by us. This “forming” would have to take place in time and could require many more stages that there are points in time. . . . Thus some writers have offered serious reasons for seeing these arguments as heuristic principles that have a certain suggestive role but that are not adequate for the justification of the whole system of axioms.268

It is not clear to me how the criticisms described in the above quotation undermine Shoenfield’s goals sketched in his discussions of the foundations of set theory, since it is not obvious to me that Shoenfield was even attempting to give what Parsons calls a “justification of the whole system of axioms” of ZF (in his presentation of what he (Shoenfield) called the “methods of the mathematicians”), especially since it is not clear to me that anyone has given a truly adequate justification “of the whole system of axioms” of any system of set theory such as ZF. Indeed, it is not even clear to me what it would be to give a truly adequate “justification of the whole system of axioms” of some set

268 ([Parsons, 2008 #623], pp. 123-4, italics mine). In a footnote, Parsons adds that that he takes Maddy to have had the same skeptical intention in her paper [Maddy, 1980 #167] as that expressed by the last sentence of the above quotation. It should be noted that what I take away from Shoenfield’s “justification” of the axioms of ZF is in no way undermined by such skepticism.
theory such as ZF: it is certainly not something that Shoenfield explicitly claimed to be doing. In any case, in the following section, I shall be taking Shoenfield’s “methods of the mathematician” to be, not a “justification of the whole system of axioms” of ZF, but rather a sort of presentation of a kind of Big Picture model of mathematical truth.\textsuperscript{269}

6. A Hilbertian Account of the Truth of the Theorems of Mathematical Analysis

[a] How can the theorems of mathematical analysis be true?

I began this chapter with the puzzle: How can the theorems of mathematical analysis be true if they are not propositions about the world? One way of responding to this puzzle is to hypothesize, as did Field and Leng, that mathematical theorems are all fictional sentences that are neither true nor false. Such a position is consistent with my own long standing position that there are no mathematical objects; but it collides with the fact that many mathematical theorems are clearly true and yield valuable information that can be easily verified (recall Sections 7 and 8 of Chapter 3).

Shoenfield’s discussion of ZF suggests one possible way of answering the above puzzle: a mathematical sentence can be said to be ”true” by being true of an appropriate kind of structure, just as each axiom of ZF was said to be true by being a truth about the kind of structure described by Shoenfield that characterizes (even if only incompletely) Zermelo-Fraenkel set theory.

\textsuperscript{269} It might be useful to the reader to reread the
Now it might be said that the sentences of analysis that are classified as true, according to the above characterization of ‘true’, are not, strictly speaking, true at all. But is that because the theorems of analysis are not true in the way that simple statements of empirical fact are true, or because mathematical propositions are, in general, very different from everyday empirical propositions about the world? Let us agree that what I am calling mathematical truths are not true in the way that everyday statements about, say people or everyday objects, are true--that is, true propositions “about the actual world”. After all, over the years, mathematical propositions have been regarded by many philosophers (such as, for example, the 18th Century philosopher David Hume) as completely different in kind from such simple statements of fact as “Balsa wood is lighter and less dense than teak”.270

We can still allow that mathematical truths are “true” in an important sense of that term. But this sense of the term ‘true’ need not be entirely new. For example, the position Hilbert took in his dispute with Frege regarding the axioms of geometry is fundamentally the position I shall be espousing in this chapter except for the fact that Hilbert’s position was restricted to geometry. According to Hilbert’s position, geometrical theorems were to be those sentences that are true of certain types of geometrical structures that are intuitively or theoretically interesting, perhaps even mathematically promising, thus suggesting an answer to the puzzle: How can a mathematical theorem be regarded as, in some appropriate sense, “true”, if it is not a true proposition about the actual world? The suggested answer is: by being true of (or satisfied by) an appropriate kind of structure. The structure is what the sentence in

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270 See his An Enquiry Concerning Human Understanding, Section IV, Part I, where he distinguishes those theorems put forward in mathematics texts from the propositions put forward in works of the empirical sciences. The former express what he calls “relations of ideas”, whereas the latter express “matters of fact”. Carl Hempel promotes a very similar dichotomy in [Hempel, 1964 #98].
question is satisfied by: one might think of the structure as a sort of abstract model of that part of the mathematical universe in which the sentence is satisfied. One could then regard mathematical theorems as those sentences that are satisfied by some part of the mathematical universe.

One can test the correctness of the above answer about mathematical truths by considering how answers fit the cases of, specifically, algebraic truths. Thus, let us first translate what Shoenfield says about mathematical truths into descriptions of group theoretical truths. What we then get is the claim that the sentences of group theory—that is, the sentences that we classify as expressing “group theoretical truths”—are not straightforward expressions of propositions about the actual world. Indeed, one can think of the truths of group theory, not as propositions about the world at all, but instead as Hilbertian sentences that are true of certain kinds of abstract representations of the world—specifically, appropriate kinds of structures that are satisfied by all group structures.

I shall now expand the above ideas, as well as the conclusions of earlier chapters, in order to present a Big Picture view of the notion of mathematical truth when applied to the areas of number theory and analysis. The generalized Hilbertian notion of mathematical truth which has begun to emerge in this chapter will be contrasted with what I called the Received View of mathematical truth\(^\text{271}\)—that is, the Fregean view of mathematical truth that has dominated much of the philosophical work on the nature of mathematics during what I call ‘the Contemporary Period’.

Let us start with the following summary of the above conclusions:

\(^{271}\) The Received View was briefly discussed in Section 6 of Chapter 1.
Mathematical theorems are, in some appropriate sense, true.

Mathematical theorems do not express facts about the actual world.

From (i) and (ii), we can conclude that:

A mathematical theorem is not true in virtue of its expressing facts about the actual world.

From Shoenfield’s “Methods of the Mathematician”, we can discern a way of characterizing how mathematical theorems can be true. Mathematical theorems are not true of the actual world (certainly, in the way that empirical propositions are typically held to be true), but rather they are true in a way that is consistent with (iii). We can say:

Mathematical theorems can be said to be "true" when they are true of, or satisfied by, an appropriate kind of mathematical structure.

To make better sense of (iv), I need to characterize more generally what “an appropriate kind of mathematical structure” is to be. Here, let us focus on the theorems of analysis. Since the mathematical theorems of analysis are proved in the context of trying to discover and establish new mathematical truths about the kinds of mathematical structures studied in analysis, what the mathematician proves will be expressed in terms of those very kinds of structures mentioned in the previous sentence. We can infer that the sentences she constructs to formulate her discoveries will be framed in such a
way that the variables and constants in the sentence will be intended to result in a Hilbertian sentence that is true of the kind of structures being investigated, so that the mathematical truths so established will be satisfied by the kinds of structure in question.

Generally speaking, then, much mathematical research in analysis will enhance our understanding of the mathematical structures that have proven to be of interest to analysts (and frequently also, empirical scientists and engineers). Thus, one would expect that the enormous collection of mathematical truths of analysis that have been gathered over many centuries by mathematicians and scientists will typically consist of truths about certain kinds of mathematical structures. I suggest that it is, primarily, this great fund of structural information that has proven to be so important and fruitful in scientific and mathematical research.

In summary, the following theses are suggested: mathematical theorems—theorems that we classify as “expressing mathematical truths”—are not straightforward expressions of propositions about the actual world. The truths of mathematics are not propositions that are directly about the actual world at all, but instead are Hilbertian sentences that are true or false of (or satisfied by) appropriate kinds of structures. Obviously, I have taken the kind of view of geometry that Hilbert defended in his dispute with Frege (which I described in the first chapter) and applied the resulting Hilbertian view to number theory and analysis. This is why, in what follows, I shall call this account of mathematical truth my Hilbertian structural account of mathematical truth.

Consider a typical situation in which a mathematician is attempting to prove a theorem: imagine that the mathematician has in mind, and is theorizing
about, some kind of mathematical structure. This structure can be regarded as the “mathematical reality” which the mathematician is attempting to characterize or describe with her theorems-- the theorems she proves describe this “reality”.

[b] Are Structures Things That Actually Exist?

What kind of thing is a structure? Is it an entity of some sort that exists in the actual world? I do not consider structures to be things or entities in the actual world. How then can one make sense of my talk of structures when, according to my “ontologically parsimonious” views, structures do not actually exist? Let us start with the fact that a structure can be regarded as something consisting of a domain of objects and one or more relations on the domain. Hence, a structure can be defined in set theory to be a set of a certain sort. Given the Constructibility interpretation of simple type theory I that I laid out in Chapter 4, one can see how what I want to say about structures can be understood to be statements about the Constructibility of open-sentences. In other words, one can give a Russelian “no-class” type of explanation\(^{272}\) of how my talk of structures can be understood to be talk about the Constructibility of open-sentences, and not about existence in the actual world.

Let me examine and discuss this type of explanation in more detail. In PM, set theory is developed as a theory of what Russell called “propositional functions”. Now a propositional function seems to be little different from what

\(^{272}\) See (Chihara, 1973 #48), Chapter 1, Section 4, for a detailed exposition of the “no-class” theory of Russell and Whitehead.
Quine calls “attributes”\textsuperscript{273}. So, for many purposes, a propositional function can be regarded as what Frege called a concept. But a concept, as Frege described it, also has an “extension”, which is frequently regarded as the set of those objects that “fall under” the concept.

Strictly speaking, the ontology of PM does not include any sets or classes, but there are assertions or theorems of PM that, by means of translation rules, can be understood or regarded as statements about sets or classes, and these theorems constitute the theory of classes of PM—and it is within this theory of classes that classical mathematics (e.g. number theory and analysis) is developed. This is why Russell calls his mathematical theory a “no-class” theory.

The “no-structure” position I take in my version of classical mathematics requires an explanation of how there can be realizations of theories, since realizations are typically defined in terms of structures. In particular, I need to explain (if only briefly) how the “partially interpreted” theories of mathematics can be said to be true of structures even if there are, in the actual world, no such things as structures. My explanation is that, strictly speaking, the “partially interpreted” theories of mathematics are not true of structures (since, strictly speaking, there are no such things as structures in the actual world): so my talk of theories being “true of structures” is to be understood to be merely a manner of speaking.

Of course, mathematical theories can still be said to be realized and realizable—something that was explained in detail in \{Chihara, 2004 #468\}, Chapter 8, Sections 2 and 3. There, I explained how a typical description of a

\textsuperscript{273} See, for example, \{Quine, 1961 #344\}, pp. 8-11.
structure that is said to be “a model of a theory T” provides enough descriptive material for T to be realized in the sense set forth in my previous book, even though there are no such entities as structures. My plan was: first to show how ordered pairs of open-sentences could play the role that structures are supposed to play; and then to show how saying that something is realizable can be understood to mean that it is possible to construct an open-sentence of a certain sort.  Thus, the account of mathematical truth being presented here is compatible with the overall nominalistic viewpoint that I championed in my earlier works.

We now have a Big Picture of the material for a general sketch of how mathematical theorems can be treated as true, even though they are not propositions about the actual world. Summarizing this general sketch, we can say that mathematical theorems are, in a way, true: they are true of an appropriate kind of structure—in the case of analysis, the kind of structures that were utilized by scientists and engineers modeling and representing various aspects of physical reality. And this way of regarding mathematical theorems can be modeled in the way Hilbert regarded geometrical theorems, thus transferring an essential feature of Hilbert’s treatment of geometry to the case of number theory and analysis.

[c] Numerical terms

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274 For details, see [Chihara, 2004 #468], pp. 223-4.

275 I give a much more detailed account of this aspect of my view of mathematics in [Chihara, 2004 #468], Chapter 8.
There is a feature of mathematical truths that is importantly linked to the notion of structure I have been discussing and that I discussed in the Introduction: typically, a mathematical term, such as ‘4’, ‘pi’, ‘e’, and ‘the square root of 2’, is not defined or specified in such a way that a particular abstract entity is picked out to be its referent. It is for this reason I called the mathematical terms “partially interpreted” and “incomplete”. As I noted in the Introduction, these features have led mathematical Realists, such as Resnik, to infer that the mathematical objects denoted by these terms are “incomplete” entities. However, from the perspective of the position described in the previous section, this “partially interpreted” feature of mathematical terms is exactly what one would expect if one believed, as I do, that mathematical truths yield information about kinds of structures: this is because one would expect the mathematical terms occurring in the theorem to be functioning, not as names denoting particular entities, but rather as what might be called “structural parameters” that stand for positions in structures—hence, the “partially interpreted” nature of mathematical terms.

7. The Attractions of the Hilbertian Structural Account of Mathematical Truth

(a). Comparison with the Platonic account

The Hilbertian Account of mathematical truth described above is philosophically attractive for many reasons. Acceptance of the account allows one to abandon the Platonist’s view according to which the set theorist is seen to be a discoverer of a vast realm of abstract entities that are interrelated by

276 Benacerraf, on the other hand, drew the conclusion from these features that numbers are not objects at all ([Benacerraf, 1965 #113], p. 70).
such mathematical relations as membership and less than. No longer will there be a need to explain how a set theorist, sitting alone in an office, can discover the existence and properties of things that are supposedly in another realm of existence that is beyond the reach of our most sophisticated scientific instruments. No longer will there be a need to postulate some sort of mysterious faculty of perception, of the sort that Gödel postulated, by means of which we are able to obtain knowledge of sets that are thought to exist in the actual world.\textsuperscript{277} On this view, the set theorist proves theorems that are true of certain kinds of structures. But as I pointed out above, these structures need not be regarded as abstract entities that exist in the actual world independently of the set theorist: these structures can be understood to be the products of the mathematician’s creative imagination—they can even be regarded as open-sentences made up or invented as described earlier (in Chapter 4).

Also, there is no problem, such as the Platonist faces, of determining which of the many different set theories is (or are) the true one(s). On the view under consideration, none of the theorems of the set theories is actually true (i.e. true of the actual world), but only true of certain kinds of structures. In other words, the theorems of the various set theories that have been developed are true in the way theorems of other branches of mathematics are true: they are true of structures.

(b) Other grounds supporting the Hilbertian account

Let us now turn to assessing the soundness of the Hilbertian structural account, independently of how well it fares in its comparison with the Platonic

\textsuperscript{277} Recall the discussion in Chapter 2, Section 2.
account. For this purpose, we need to focus on how well it fits, or is consistent with, actual mathematical practice and theory. In the following, I provide positive reasons for thinking that the Hilbertian Account of mathematical truth competes satisfactorily in such a test of its soundness.

[1] It Fits How Mathematicians Treat the Natural Number System

The natural numbers are generally presented axiomatically in some such system as the first-order theory PA discussed earlier. Such systems do not single out a unique set of abstract entities as “the natural numbers”; they only determine a class of first-order structures as models of the natural numbers—a conclusion that comfortably fits the above Hilbertian Account of mathematical truth.


A quotation from Richard Martin’s Intension and Decision heads Paul Benacerraf’s classic paper “What Numbers Could Not Be”. Part of the quotation consists of these words:

The attention of the mathematician focuses primarily upon mathematical structure, and his intellectual delight arises (in part) from seeing that a given theory exhibits such and such a structure, from seeing how one structure is “modeled” in another, or in exhibiting some new structure and showing how it relates to previously studied ones . . . . ([Benacerraf, 1965 #113], p. 47).

Commenting on this quotation, Benacerraf writes:
Martin correctly points out that the mathematician’s interest stops at the level of structure. If one [mathematical] theory can be modeled in another (that is, reduced to another) then further questions about whether the individuals of [the] one theory are really those of the second just do not arise. ([Benacerraf, 1965 #113], p. 69).

These ideas about the roles that structures play in the mathematician’s interest and theories support the above Hilbertian Account of mathematical truth and also are illustrated in the way that the system of real numbers has been defined and characterized by mathematicians. Typically, one begins with the system of natural numbers, starting with Peano Arithmetic (PA). In other words, one begins with an axiomatic characterization of a kind of structure: the structure of the system of natural numbers. One then proceeds to develop the system of integers by taking the integers to be equivalence classes of ordered pairs of natural numbers. The arithmetic operations on the integers are defined in the usual way, again resulting in a definition of the system of integers--again a characterization of a kind of structure. The system of rational numbers is then “constructed” from the system of integers, by taking rational numbers to be equivalence classes of ordered pairs of integers, and then by defining the appropriate arithmetic operations on the totality of rational numbers. This then is followed by “constructing” (figuratively speaking) the system of real numbers from the system of rational numbers, usually by taking the real numbers to be either Dedekind cuts or convergent sequences of rational numbers. Needless to say, what results from all these “constructions” is a characterization of a kind of structure.²⁷⁸

²⁷⁸ This is the procedure followed, for example, by A. H. Lightstone in [Lightstone, 1965 #47], Chapters 4 & 5, where the reals are taken to be sequences of rationals. The reader can easily fill in the details of my brief outline above by reading the above chapters. See also [Landau, 1960 #410], where the reals are taken to be Dedekind cuts.
sometimes called “the arithmetization of analysis” and sometimes cited under the title: “the foundations of analysis”\textsuperscript{279}.

A simpler way of characterizing the system of real numbers is to adopt the axiomatic approach, as used, for example, by Birkhoff and Mac Lane in \cite{Birkhoff, 1953 #380}, where they simply postulate that the totality of real numbers forms a complete ordered field.\textsuperscript{280} From this postulate, they note, “one can actually deduce all the known properties of the real numbers” (p. 91).

Both approaches described above characterize kinds of structures. Each characterization ends up specifying a kind of structure that is unique up to isomorphism. One can thus state, as Birkhoff and Mac Lane do, that “there is one and only one complete ordered field” (p. 99). So all structures that are complete ordered fields are isomorphic—which is compatible with there being infinitely many such structures.

The main point I have been emphasizing can be expressed as follows:

The mathematician’s method of “constructing” the real numbers ends up being a characterization of a kind of structure so that the numerical terms of real mathematics (such as ‘pi’, ‘3\textsuperscript{4}’, and ‘2 + 3’) turn out not to be proper names of particular abstract objects, but rather points in kinds of structures.

Thus, imagine that the constructions have been carried out in some set theory—say, for purposes of specificity that Quine’s version of simple type theory has been chosen. Then by the characterizations described above, specific numerical terms

\textsuperscript{279} See \cite{Landau, 1960 #410}.

\textsuperscript{280} See \cite{Birkhoff, 1953 #380}, Chapter 4, Section 3.
terms such as ‘2 + 3’ could not name any specific set. This is because the “constructions” do not specify some specific set theoretical structure as the real numbers; infinitely many structures satisfy the above characterizations, so that the numerical terms end up being “Hilbertian parameters”, each term standing for different points in different structures and not for a specific abstract entity in Plato’s world. The main point is that no single set of entities is singled out as “the real numbers”: the procedure only specifies a certain kind of structure as the real number structure, and this fits the above Hilbertian Account of mathematical truth.\textsuperscript{281}

[3] It Fits Mathematical Practice:
Set Theory

Another way in which the above Hilbertian account fits mathematical theory and practice can be found in set theory. Examine the common practice of presenting, and in a way, “justifying” the axioms of ZF set theory—a practice that is found in such books as Shoenfield’s Mathematical Logic. Reconsider briefly Shoenfield’s manner of developing ZF that was described in some detail earlier. Recall that he first presented what he called the “intuitive notion of a set”, by giving a detailed description of how sets are formed. The point to notice is that Shoenfield’s characterization of the “intuitive notion of a set” can be seen to be a description of a kind of structure. So when he proceeds to justify the axioms of ZF, suggesting that the axioms have thereby been shown

\textsuperscript{281} The above structural account of mathematical truth is similar in some respects to the \textit{ante rem} version of “structuralism” advanced by Stewart Shapiro in his [Shapiro, 1997 #299], and also to the “science of pattern” version of “structuralism” put forward by Michael Resnik in his [Resnik, 1997 #314]. I give reasons for rejecting certain aspects or features of both these accounts of mathematics in Chapter 9 of my earlier book [Chihara, 2004 #468]. For this reason, I do not take up these rival structuralist views of mathematics here.
to be, in some sense, “true”, what his justifications show is not that the axioms are actually true (or true in the actual world), but only that they are true of structures of the sort described by his characterization: that is, the axioms are structurally true of that kind of structure. But that is exactly what one would expect if one regarded mathematics in the Hilbertian structural way I characterized above. In other words, the common practice described above fits, and provides support for, my Hilbertian account.

Now some set theorists believe, as did Gödel, that the axioms of standard set theories are truths about the actual world. But such set theorists are expressing their own personal philosophical beliefs about set theory and cannot reasonably be regarded as merely describing what set theorists in general have discovered or proved about set theory. Qua mathematicians, they are not in a position to make any such ontological claims about set theory. Qua mathematicians, they have not proved any theorem that has such significance. They may, of course, theorize about the philosophical significance of what set theorists have proven—but in so theorizing, they would not be acting as pure mathematicians but rather as philosophers leaving the solid ground of mathematics to embark upon the kind of philosophical reasoning that may involve a great deal of speculation of the sort that was exemplified by the writings of Gödel discussed in Chapter 2, Section 7.

8. Resolution of the Puzzles of the Forward

Additional grounds for accepting the above Hilbertian account of mathematical truth can be generated from the fact that it provides the means for resolving the puzzles I laid out in the Forward. Let us reexamine these puzzles with that possibility in mind.
(a) Azzouni’s Epistemic Role Problem

Recall Azzouni’s suggestion that we imagine mathematical objects ceasing to exist at some time, while the common mathematical work of proving theorems went on as usual. It is hard to see how the work of proving theorems would be disrupted by the elimination of mathematical objects—which provides strong grounds for thinking that the mathematical objects Realists postulate have no role to play at all in the common everyday practice of proving mathematical theorems. We thus get what Azzouni calls “the epistemic role puzzle”: What role in traditional mathematical practice do mathematical objects play?

This problem does not arise from the perspective of my Hilbertian account, since according to that account, there exist in the actual world no such entities as mathematical objects (all talk of mathematical entities is to be understood to be talk about positions in structures). As a result, no puzzle arises, within the framework of this account, about the lack of a role for mathematical objects to play.

Let us now examine another puzzle set forth in the Forward:

(b) The Problem of Reference to Particular Abstract Objects

This is the problem that arises from the fact that we cannot specify or point to a specific abstract entity, nor can we even describe a specific abstract entity, except in terms of other such entities that we cannot describe except in terms of other such entities. This problem becomes acute for the Realist (who
This puzzle is heightened by the fact that, as Azzouni puts it, the “current philosophical concern with how mathematical terms pick out what they refer to is an oddity from the point of view of mathematical practice, which, in broad respects, is simply not concerned with reference” ({Azzouni, 1994 #309}, p. 31).

This is not a puzzle for the Hilbertian Account since it does not require the existence in the actual world of any mathematical objects to be referred to. That mathematicians are not concerned with reference is, according to the Hilbertian Account given here, a perfectly reasonable attitude to have.

I turn now to another puzzle from the Forward:

(c) Puzzles About the Identity of Numbers of Different Types

Is the natural number 2 the same entity as the integer 2? Is the integer 2 the very same thing as the rational number 2? Is the rational number 2 = the real number 2? Is the real number 2 the very same object as the complex number 2 + 0i? Do the mathematical terms ‘the rational number 2’ and ‘the real number 2’ refer to one and the same entity?

These problems arises from the belief that there are such entities as “the natural number 2”, “the integer 2”, etc. Once one drops these beliefs, the problem becomes easy to resolve. From the perspective of the Hilbertian
account of mathematical truth, all talk of the natural number 2 is not genuine talk about an entity but rather talk about positions in structures. So asking if the natural number 2 is the same entity as the integer 2 is like asking if the hotel clerk in some Ellery Queen novel is the very same person as the hotel clerk in some Agatha Christy story: according to the Hilbertian Account I have been developing, such questions have no sensible answers, and there is no puzzle about which identity holds.

(d) The Incomplete Nature of Mathematical Objects

Recall that, as mathematicians define the natural numbers, the rational numbers, the real numbers, and so on, the specific number terms fail to pick out specific abstract entities, and hence were thought to be only “partially interpreted”. This led Resnik to characterize the referents of specific number terms as “incomplete” entities. Now is my Hilbertian account encumbered by such weird “incomplete” entities? Not at all. According to the Hilbertian account, there are no mathematical entities, so the account is certainly not committed to “incomplete” mathematical entities. Hence, the Hilbertian account need not populate its ontology with such weird entities as “incomplete” mathematical objects, as Resnik’s did, and another problem from the Forward simply does not arise for the Hilbertian account.

9. Another Benefit of Accepting the Hilbertian Account: A Comparison With Russell’s View

It is enlightening to compare and contrast the above Hilbertian account of mathematical truth with Russell’s view put forward in PM. Russell was forced to appeal to an Axiom of Infinity in PM in order to develop a satisfactory account
of the natural numbers. The Axiom of Infinity of PM asserts the existence in the world of infinitely many “individuals”. However, the inclusion of the Axiom of Infinity among the axioms of PM raised a problem for Russell because he accepted the Fregean Assumption and held that the axioms of PM were truths about the actual world. Thus, it was by no means obvious that the Axiom of Infinity could legitimately be considered a logical truth, on a par with the other axioms of PM, since the proposition that there are infinitely many things in the actual world does not seem to be a necessary truth, much less a logical truth.

By the time Russell was working on his book Introduction to Mathematical Philosophy, he had developed serious doubts about the plausibility of the Axiom of Infinity, writing “nothing can be known a priori as to whether the number of things in the world is finite or infinite” (Russell, 1920 #103, p. 141). Indeed, he came to believe that the axiom “will be true in some possible worlds and false in others; whether it is true or false in this world, we cannot tell” (p. 141). So when he ended his chapter on the Axiom of Infinity in his book with the words, “whether the axiom is true or false, there seems to be no known method of discovering” (Russell, 1920 #103, p. 143), he was, in effect, conceding that he should not have listed the Axiom of Infinity among the

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282 Here, I continue using the policy I adopted in Chihara, 1973 #48 of referring to the author of philosophical remarks appearing in PM as “Russell”, even though he co-authored that work with Whitehead. This is primarily for stylistic reasons, but also because Russell wrote: “Broadly speaking, Whitehead left the philosophical problems to me” (Russell, 1959 #611, p. 74).

283 See Russell, 1920 #103, Chapter 13.

284 This is why I classify Russell as a Fregean.

285 [Russell, 1920 #103].
logical axioms (or logical truths) of PM. Evidently, by that time,\textsuperscript{286} he had lost confidence in the way he and Whitehead had developed number theory in PM.

This problem with the Axiom of Infinity arose for Russell, because of his acceptance of the Fregean Assumption: if mathematical theorems are thought to be propositions about the actual world, then to include the Axiom of Infinity among the axioms of PM is to regard it as a true proposition about the actual world. Who knows if there are, in fact, infinitely many individuals in the actual world? And how could one know? On the other hand, the Hilbertian account of mathematical truth I have been defending in this chapter does not face such a problem, because it is not developed from the acceptance of the Fregean Assumption. Mathematical theorems are not, from my Hilbertian position, propositions about the actual world. Instead, mathematical theorems are satisfied by structures of a certain sort—structures that have to be infinite if one is to have the structure of the natural numbers. Hilbertians working on the foundations of arithmetic need not concern themselves with the question of how many individuals there are in the actual world.

10. The Problem of Applications in Science

It might be replied that the above Hilbertian thesis (that the theorems of real mathematics are not straightforwardly true of the actual world, but only structurally true) does not provide us with an explanation of how it is that mathematical theorems are successfully applied in science. So the question arises: how can the theorems be applied to the actual world if these theorems

\textsuperscript{286} Russell wrote [Russell, 1920 #103] while he was in prison for his pacifism. About his time in prison, he wrote that it “kept my self-respect alive, and gave me something to think about less painful than the universal destruction. . . . I was able to read and write as much as I liked, provided I did no pacifist propaganda” ([Russell, 1968 #609], p. 34).
are not true of the actual world? One cannot answer this question in the way Quine did in Word and Object, where he maintained that mathematics is essentially the same kind of theory of the world that the empirical sciences are. Thus, if one accepts the Hilbertian thesis described above, how should I respond to Maddy’s demand: “if mathematics isn’t true [of the actual world], we need an explanation of why it is all right to treat it as true when we use it in physical science” (Maddy, 1990 #207, p. 24). Since the Hilbertian position I have sketched above is incompatible with the view that mathematical theorems are straightforwardly true propositions about the actual world (as “the big three” philosophers Frege, Russell, and Quine, as well as a large number of other philosophers of mathematics of the Contemporary Period, believed), how can applications of mathematical theorems in the empirical sciences be explained from my Hilbertian position? I cannot explain applications of mathematics in the way the big three did, because I cannot assume the Fregean Assumption in my explanation as they did. From my perspective, mathematics is not essentially the same kind of theory of the world as are the empirical sciences. Here, it is important to keep in mind that, from my perspective, mathematical theorems are not true in the way that typical statements of biology are true: they are true in a particular mathematical sense.

To emphasize this Hilbertian perspective, one might express my position in the following way: statements of biology are typically true of organisms or biological phenomena, whereas mathematical statements are typically true of the kind of thing that mathematics is primarily about, namely, mathematical structures.
The central role in mathematics I have assigned to structures is in line with the emphasis on structures to be found in the works of Bourbaki, who has written:

[M]athematics appears thus as a storehouse of abstract forms—the mathematical structures; and it so happens—without our knowing why—that certain aspects of empirical reality fit themselves into these forms, as if through a kind of preadaptation . . . The unity which it [i.e. structure] gives to mathematics is not the armor of formal logic, the unity of a lifeless skeleton; it is the nutritive fluid of an organism at the height of its development, the supple and fertile research instrument to which all mathematical thinkers since Gauss have contributed.

But now we must face the problem of explaining how the results of the mathematician theorizing about structures can be applied so fruitfully and successfully in the empirical sciences. Why should theorizing about structures

287 ‘Bourbaki’ is “the collective pseudonym of a changing and secret group of mathematicians, most of them French, who have collaborated since the 1930s with the intention of achieving a complete and definitive compilation of mathematical knowledge” ((Borowski & Borwein, 1991), p. 60). This group has emphasized the importance of structure in mathematics, publishing more than 36 volumes of mathematics, in which they classify the various areas of mathematics in terms of structure.

288 ((Bourbaki, 1950), p. 231). Some may argue that what mathematicians mean by ‘structure’ is not what philosophers or physicists mean by it, in which case the above quotes do not provide any support for the view I have put forward. In response, I would argue that, although there are some minor differences in how the three groups mentioned above use the term, there is a core meaning to the terms ‘structure’ and ‘model’ which are common to all three groups, and that the above quotes all use this core meaning. Thus, in the article from which the above quotation was taken, Bourbaki undertakes to make clear what is to be understood by ‘mathematical structure’: The term, we are told, “can be applied to sets of elements whose nature has not been specified: to define a structure, one takes as given one or several relations, into which these elements enter . . . ; then one postulates that the given relations, or relations, satisfy certain conditions (which are explicitly stated and which are the axioms of the structure under consideration).” This is basically a characterization of what mathematicians take structures to be. Thus, consider what the mathematician Mac Lane writes, after giving the Peano axioms: “This is a typical description of a structure by axioms” ((Mac Lane, 1986), p. 44)—which with relatively minor differences fits my view beautifully. He also gives the “general notion” of an algebraic structure [note: there are also non-algebraic structures!] as follows: “A set \( X \) with nullary, unary, binary, ternary . . . operations satisfying as axioms a variety of identities between composite operations” ((Mac Lane, 1986), p. 26). See also (Mac Lane, 1986), p. 33. It can be seen that Mac Lane’s characterization of structure is essentially what philosophers of mathematics have in mind when they speak of “structures”. See (Chihara, 2004), Chapter 3, Section 1. Cf. (Barbut, 1970). See also (Suppes, 1967a), where Suppes argues that “the meaning of the concept model is the same in mathematics and the empirical sciences” (p. 289). Cf. also (Suppes, 1967b), pp. 57-9.
(which, according to my account, is what mathematicians primarily do) yield so much information that is so very valuable to physicists, chemists, biologists, and economists? This question about applications of mathematics is featured in the following sentence from the above Bourbaki quotation:

Mathematics appears thus as a storehouse of abstract forms—the mathematical structures; and it so happens—without our knowing why—that certain aspects of empirical reality fit themselves into these forms, as if through a kind of preadaptation . . . ([Bourbaki, 1950 #478], p. 231, italics mine).

However, this quotation makes the fruitfulness and usefulness of mathematics in the empirical sciences appear to be somewhat mysterious and inexplicable. I think, however, that one can provide, in a very general Big Picture sort of way, a kind of explanation of this kind of fruitfulness and usefulness that will diminish the air of mystery.

The theorems of mathematical analysis are typically true of the kinds of structures that are to be found in the very basic framework of the theories utilized by physical scientists in describing, analyzing, and explaining physical phenomena. This is why such theorems hold in just the kinds of structures that provide the scientist with an excellent framework for theorizing about the physical world.

To illustrate the above explanation, let us consider again how the theorems of mathematical analysis proved to be so valuable to physicists and astronomers in the seventeenth and eighteenth centuries, when so many of the great achievements in analysis were being made. At that time, the mathematicians developing the infinitesimal calculus were also the very physicists who were in a unique position to apply the mathematics being
developed. Thus, one of the principal mathematical researchers in the development of the calculus, Isaac Newton, was also one of the leading physicists at the forefront of researches on gravity and the laws of motions.\textsuperscript{289} And the kind of structure about which the theorems of the field of mathematical analysis being created were true were also the very kind of structure that Newton and his colleagues were using to represent and to analyze the motions of physical bodies in space. Since the theorems being proved by the mathematicians at that time were devised to be structurally true of the very kind of mathematical structures that the physicists were using in their theories about physical motions in space, the mathematical theorems provided scientists with a crucial tool for representing and analyzing motions in space, generating the mathematical theories about space and motion that made Newton justly famous.

The close connection between the mathematics being developed at this time and the applications of the mathematics in science has been emphasized by the historian of mathematics Judith Grabiner, who wrote:

In the eighteenth century the problems considered to be most important were those which could be treated without paying attention to the foundations of the calculus. No strict line was drawn between the calculus and its applications. Many of the results obtained in the calculus had immediate physical applications; this circumstance made attention to rigor less vital, since a test for the truth of the conclusions already existed—an empirical test. (\cite{Grabiner, 1981 #458}, pp. 16-7).

Francois De Gandt was also struck by the fact that mathematicians developing the calculus were frequently identical to the scientists applying the calculus in science and astronomy, writing:

\textsuperscript{289} Newton was by no means the only researcher who was prominent in both fields of study during this period. As Francois de Gandt has written: “jusqu’a Cauchy environ, tous les mathématiciens étaient aussi des physiciens . . .” (\cite{De Gandt, 1982 #594}, p. 190).
Le XVII\textsuperscript{e} siècle a vu naître en même temps le calcul infinitésimal et la science du mouvement, à peu près entre 1610 et 1690. Les deux directions de recherche sont inséparables; elles font partie d’un unique effort global pour élucider les phénomènes du mouvement. Ce sont souvent les même hommes qui ont enrichi à la fois la réflexion philosophique, les procédés mathematiques et l’appréhension physique de la nature. ([De Gandt, 1982 #594]{}, p. 167).

Thus, there is no need to suppose that the theorems of analysis had to be true of the actual world (as Fregeans believe they are) in order to explain how the mathematics being developed at that time was successfully applied in science. Such applications can be explained by the fact that the theorems were what I shall call “structurally true” of the kinds of structure being used in science to describe, analyze, and explain physical phenomena. What I mean is that the theorems were true of (or satisfied by) the scientist’s models that were used to represent, analyze and explain various aspects of physical reality. In this way, one could answer Maddy’s query about why the scientists of that time could treat mathematical theorems as true in applying mathematics in science. The explanation is: the mathematical theorems could be treated as true, because they were, in an appropriate sense, true. They were “structurally true” of the scientist’s models of physical reality.

Of course, contemporary mathematicians are not, for the most part, also empirical scientists, as were the mathematicians of the seventeenth and eighteenth centuries. But many theoretical physicists today are, in fact, applied mathematicians—specialists in applying mathematics to the physical world. Indeed, the discussion of mathematical models in Chapter 5 was written to prepare the reader for just the sort of explanation given above of how a mathematics that is structurally true--although not true in the more obvious
way “mercury is more dense than water” is true—can be applied in science. Thus, the structural account of mathematics explains how the theorems of contemporary mathematics can be applied in science today, in essentially the above structural manner, since the structures of today’s mathematics are now routinely involved in the construction, by applied mathematicians and empirical scientists, of a great many of the scientific models of the events, processes, and interactions that take place in the physical world.

11. A Comparison with Field’s Account of Mathematics

In order to provide some perspective on the account of mathematical truth presented above, I shall now compare and contrast my Hilbertian account with Field’s account discussed in Chapter 3. It might seem to the reader that my account of mathematical truth is very similar to Field’s account, since:

[F] Field’s position that no mathematical theorem is true

seems to be little different from:

[C] My position that no mathematical theorem is true of the actual world

Since I was very critical of [F] in Chapter 3, the above mentioned similarity between [F] and [C] may suggest that my critical discussion of [F] was not really fair.

290 See, Chapter 5, Section 13, for a detailed example of a theory—a theory of light rays—that is formulated in such a way that it can be easily seen to have theorems that are not true, but instead are true of structures that are used in describing, analyzing, and explaining physical phenomena.
In response to such a suggestion, one should note the very important differences between [F] and [C]. [F] is part of Field’s overall view of mathematics according to which mathematical theorems are false propositions about the world. (Remember, Field was a Fregean). By contrast, [C] was part of my very different Hilbertian view of the nature of theorems of mathematical analysis, according to which mathematical theorems in general are not propositions at all (and hence not false propositions about the world) and instead are Hilbertian sentences that are true of (or satisfied by) mathematical structures studied in analysis. This difference has far-reaching consequences, as the earlier discussions in this work have shown.
Chapter 10

Does Contemporary Philosophy of Mathematics Rest on a Mistake?

Introduction

What I propose to do in this final chapter is similar to what David Hume suggested he would do in the last paragraph of his An Enquiry Concerning Human Understanding, where he wrote: “When we run over libraries, persuaded of these principles, what havoc must we make?” More generally, I seek in this chapter to lay out some of the chief consequences for the philosophy of mathematics of results obtained earlier. I shall do this in connection with an investigation of a philosophical question that is related to one that H. A. Prichard asked early in the Twentieth Century in his widely read and highly influential defense of Ethical Intuitionism entitled “Does Moral Philosophy Rest on a Mistake?”.

There, Prichard claimed that moral philosophy rested on the mistake of giving arguments for normative principles from non-normative premises: from attempting to derive “ought” from “is”. In this chapter, I raise a
question analogous to Prichard’s “Does Moral Philosophy Rest on a Mistake?”. The question is, of course, “Does philosophy of mathematics rest on a mistake?” If there is such a mistake, it would have to be a very fundamental one that permeates a great deal of the philosophy of mathematics -- one that is made by most of the well-known philosophers of mathematics of the Contemporary Period.

1. What Is the Fundamental Mistake?

   Recall that, in the Introduction, I listed the following philosophers of mathematics:

   Gotlob Frege, Bertrand Russell, Alfred North Whitehead, Kurt Gödel, Willard Quine, Hilary Putnam, David Lewis, Hartry Field, Mark Balaguer, John Burgess, Gideon Rosen, Penelope Maddy, Allvin Plantinga, Michael Resnik, Stewart Shapiro, Bob Hale, Crispin Wright, Mark Steiner, Philip Kitcher, Mark Colyvan, and James Brown.

   and then raised the question:

   Is there some fundamental feature that each of the philosophers on the above list has at one time or other either attributed to, or else tacitly assumed to be possessed by, those propositions generally regarded as mathematical truths?

   This question was then rephrased in the following way:
What, if anything, do all the various views about the nature of mathematical theorems and mathematical truths, expressed at some time by each (but not all) of the philosophers on the above list, have in common?

Some of you readers, who have followed the philosophical train of thoughts I have been delineating in this work thus far, may have already guessed what my answer to the italicized question above is to be. Here now is my answer: I claim that each of the philosophers in the above list has, at one time or other, expressed or assumed the belief that the truths of mathematics are all propositions about the actual world—a belief, in other words, in the Fregean Assumption. Thus, I claim that there are significant grounds for concluding:

Much of the philosophy of mathematics that was put forward, developed, and widely accepted, during the Contemporary Period does indeed rest on a mistake—the mistake of accepting or simply assuming what I have called the Fregean Assumption.

The above claim about the Fregean Assumption raises the question I touched on earlier: Why have so many philosophers made the mistake of adopting the Fregean Assumption? And more specifically, why have so many of the well-known philosophers of mathematics of the Contemporary Period adopted the Fregean Assumption, rather than, say, the Hilbertian Alternative described in Chapter 1? It was not because they were persuaded by superior argumentation supporting the Fregean position. There was no such argumentation. No, as I suggested earlier in Chapter 2, Section 1, to a large
extent, most of the philosophers of mathematics of the Contemporary Period seemed to have simply followed the example of Frege and the three “giants” of philosophy mentioned in that chapter by adopting the Fregean view. But if this suggestion is true, the following questions arise: (a) Why did Frege adopt the Fregean Assumption in the first place? And (b) Why did so many philosophers of mathematics also adopt the Fregean Assumption? In this chapter, I shall attempt to come up with some answers to these question.

2. Why Did Frege Adopt the Fregean Assumption?

Recall from my discussion of the Frege-Hilbert dispute in Chapter 1 that Frege was a huge defender of Euclid’s geometry. He went so far as to express a complete dismissal of non-Euclidean geometry, writing:

The question at the present time is whether Euclidean or non-Euclidean geometry should be struck off the role of the sciences and made to line up as a museum piece alongside alchemy and astrology . . . Well, is it Euclidean or non-Euclidean geometry that should get the sack? That is the question. Do we dare to treat Euclid’s elements, which have exercised unquestioned sway for 2000 years, as we have treated astrology? It is only if we do not dare to do this that we can put Euclid’s axioms forward as propositions that are neither false nor doubtful. In that case non-Euclidean geometry will have to be counted amongst the pseudo-sciences, to the study of which we still attach some slight importance, but only as historical curiosities. (Frege, 1979 #607, p. 169, italics mine).

In view of his surprisingly extreme defense of Euclid’s geometry, it is not surprising that Frege’s version of logic and arithmetic ended up being, in many important ways, similar to Euclid’s version of geometry. Specifically, Frege’s system of logic and arithmetic had the following features that are important features of Euclid’s geometry:
(a) The system is axiomatized;

(b) Its axioms are put forward as the basic and simple first principles of the system (they are, in Frege’s case, the “fundamental laws” of the system);

And most importantly,

(c) Its theorems are understood to be propositions about the actual world (the Fregean Assumption).

In brief, Frege developed arithmetic on the model of Euclid’s way of developing geometry. Thus, one can thus regard what Frege accomplished, with his Euclidean way of analyzing arithmetic exemplified in his Foundations of Arithmetic and The Basic Laws of Arithmetic, as a sort of carrying out, for the field of arithmetic, what Euclid accomplished in his Elements for geometry. So Euclid’s geometry can be regarded as the great ancestor of Frege’s arithmetic.

Since it seems reasonable to hypothesize that most philosophers of the early Twentieth Century were more familiar and comfortable with Frege’s version of arithmetic than they were with the more radical (for that time) model theoretic versions of mathematical theories that Hilbert and his colleagues in
mathematics were developing and promoting, it seems plausible to suppose that the adoption of the Fregean Assumption by so many philosophers of mathematics of the early Twentieth Century was to some extent due to the fact that this assumption was simpler and easier for them to grasp than was the Hilbertian alternative, which required a genuine understanding and appreciation of the advantages of its model theoretic underpinnings—something that most philosophers and non-mathematicians at that time lacked.

I suspect that many philosophers of that period (as well as the above mentioned “giants”) might not have followed Frege in adopting the Fregean Assumption had not the assumption had an immediate attraction: it provided an apparently straight-forward and plausible explanation for why mathematics is applicable to the actual world. Given the Fregean Assumption, one could explain why mathematics is applicable to the actual world in just the way one could explain why propositions of physics are applicable to the physical world—they are both thought to be applicable to the real world because, assuming the truth of the Fregean Assumption, both the propositions of mathematics and the proposition of physics are about the real world—these propositions would thus provide scientists with information that is directly about the actual world.

See {Mancosu, 2009 #632}, for description of what was taking place in the philosophy of mathematics and logic during this period. There is, however, one feature of Frege’s logical system that, it should be noted, is not a feature of Euclid’s, and this is because Frege’s system has certain ontological primitives—concepts and extensions of concepts—which are not to be found in Euclid’s geometry. There are no axioms in the Elements that assert the existence of abstract entities of the sort assumed by Frege’s fundamental laws. It was, of course, the axioms about the ontological entities of Frege’s system—concepts and extensions—that gave rise to the inconsistency of the system and, ultimately, to his complete abandonment of his whole Logicist enterprise.

Of course, the above reasons why so many philosophers adopted the Fregean Assumption are also reasons why Frege himself, as well as Russell, Whitehead, and Quine, may have adopted the assumption.
(despite the fact that mathematical objects were very different from physical objects).

But this takes us back to the question: Why did so many philosophers of mathematics become Fregeans? That is, why did most philosophers of mathematics of this period follow Frege (and not, say, Hilbert)? The previous paragraph provided the beginnings of an explanation for this fact. One reason why so many of these philosophers adopted the Fregean Assumption is because Frege’s axiomatic view of mathematics was, in its overall basic features so much more familiar to philosophers than was the Hilbertian model theoretic view. It was more familiar to philosophers because it had many of the basic features of Euclid’s geometry, about which most philosophers of mathematics were already familiar, whereas few philosophers had a good grasp of the details and fundamentals of Hilbert’s view of geometry, especially since it was being presented by a mathematicians whose presentation of technical material was aimed at mathematicians and not philosophers. I shall now provide an elaborations of the explanation started above.

During the first seventy five years of the Twentieth Century, among philosophers, the most influential mathematical logicians were Frege, Russell and Whitehead, and Quine, none of whom seems to have played a truly important role in the development of model theory; so it is not surprising that relatively few philosophers made probing studies of the model-theoretic versions of geometry being developed by mathematicians at that time. However, I believe that most philosophers of that period were familiar with Euclid’s Elements. The system of geometry presented in that work was, for over two thousand years, taken as a sort of model of what a good mathematical theory should be. One can thus regard what Frege accomplished, with his logical analysis of arithmetic
put forward in his Foundations of Arithmetic and The Basic Laws of Arithmetic, as a sort of carrying out, for the field of arithmetic, what Euclid accomplished for geometry in his Elements. So it seems reasonable to hypothesize that most philosophers of the early Twentieth Century were more comfortable with Frege’s version of arithmetic than they were with the more radical (for them) model theoretic versions of mathematical theories that were being developed by Hilbert and such colleagues in mathematics as Alfred Tarski and his students. Summarizing the above, it seems quite possible that the adoption of the Fregean Assumption by so many philosophers of mathematics of the early Twentieth Century may have been largely due to the fact that the assumption was simpler and easier for them to grasp and appreciate than were the Hilbertian alternatives, which required a real understanding of the advantages of their model theoretic underpinnings.

As the film critic Mick Lasalle has commented: “The hardest things to see, in any era, are the assumptions that are so accepted as true that they’re not even questioned; they’re merely regarded as reality.” I suspect that the Fregean Assumption became part of the weltanschauung of the majority of philosophers of mathematics simply by a kind of intellectual osmosis in which

There is, however, one feature of Frege’s logical system that, it should be noted, is not a feature of Euclid’s, and this is because Frege’s system has certain ontological primitives—concepts and extensions of concepts—which are not to be found in Euclid’s geometry. There are no axioms in the Elements that assert the existence of abstract entities of the sort assumed by Frege’s fundamental laws. It was, of course, the axioms about the ontological entities of Frege’s system—concepts and extensions—that gave rise to the inconsistency of the system and, ultimately, to his complete abandonment of his whole Logicist enterprise.

294 On page 28 of the October 20, 2013 issue of the Sunday Datebook Section of the San Francisco Chronicle.
the Fregean Assumption had become something that was “regarded as reality”. Since the Fregean Assumption was so widely accepted as true without explicit argumentation or attempts at justification, there was little in the philosophical literature evaluating the reasons for accepting or rejecting it. One might say that “It just fell under the radar”.

We can thus appreciate how Burgess and Rosen could have put forward an argument for Realism with premises they characterized as “scarcely deniable”\(^{296}\), while leaving out of their list of premises of the argument the one premise they use that, I have argued, is truly questionable—the Fregean Assumption: that assumption was not even perceived by the Princeton team to be among the premises of their argument (it was, to use Lasalle’s terminology, “merely regarded as reality”).

3. Consequences of the Falsity of the Fregean Assumption

Let us reconsider the result, obtained in Chapter 7, that the Fregean Assumption is false. What can be inferred from this result which should be of interest to all philosophers of mathematics? Obviously, straight off and directly, we can infer that mathematical theorems are not truths about the actual world as so many philosophers of mathematics (such as the “giants” mentioned earlier) assumed. Thus, the important philosophers of mathematics of the Contemporary Period I listed above were all in error about a truly fundamental feature of mathematics: they all mistakenly assumed that the theorems and truths of mathematics were propositions about the actual world, even though this belief was never justified by explicit argumentation or proof by

\(^{296}\)Described in Part III of Chapter 5.
these pioneering philosophers: it was simply an assumption. Again, the Fregean Assumption was “merely regarded as reality”.

To illustrate, in particular, how the Fregean Assumption has been tacitly accepted by philosophers of mathematics of the Contemporary Period without any explicit acknowledgment of its use in their reasoning, this chapter will highlight the crucial role that the Fregean Assumption has played in the various Indispensability Arguments that I discussed earlier in Chapter 6. Since the Indispensability Arguments mentioned earlier are widely considered by many (if not the majority of) philosophers of mathematics of the Contemporary Period as expressing the foremost reasons for abandoning nominalism in favor of Platonism (recall Field’s comments on the Indispensability Argument in Chapter 3), I shall devote much of this final chapter to a scrutiny of the various forms of the Indispensability Arguments that were presented in Chapter 6, showing in each case how the Fregean Assumption was an unstated presupposition of each of these arguments, implicitly assumed without justification, argumentation, plausible grounds, or even discussion. My discussion will show that the Fregean Assumption has impacted the course of philosophy of mathematics in the Contemporary period in a most unfortunate way.

4. Quine’s Indispensability Argument

Let us reconsider the crucial quotations from Quine’s writings (given in Section 2 of Chapter 2) which gave rise to so much discussion in the philosophical community about Quine’s Indispensability Argument. Here are the two I cited:
[The nominalist] is going to have to accommodate his natural sciences unaided by mathematics; for mathematics, except for some trivial portions such as very elementary arithmetic, is irredeemably committed to quantification over abstract objects. (Quine, 1960 #78, p. 269, italics mine).

Also:

Classical mathematics . . . is up to its neck in commitments to an ontology of abstract entities. . . . The issue is clearer now than of old, because we now have a more explicit standard whereby to decide what ontology a given theory or form of discourse is committed to . . . (Quine, 1961 #344, p. 13).

The heart of the Indispensability Argument is to be found in the above quotations—especially the claims that mathematics is “irredeemably committed to quantification over abstract objects” and “Classical mathematics . . . is up to its neck in commitments to an ontology of abstract entities.” We can see the essence of these claims in Resnik’s version of Quine’s Indispensability Argument, which contains the premise:

In stating its laws and conducting its derivations, science assumes the existence of many mathematical objects and the truth of much mathematics.

Underlying the above claims is the (unstated) belief that the mathematical theorems of real mathematics are propositions about the actual world—which, after all, is the crux of the Fregean Assumption. Because of this belief, Quine thinks that the existential quantifier ‘there exists a real number x such that . . . ‘ occurring in mathematical theorems should be understood to be asserting that “there exist in the actual world a real number x such that . . . .” As a result, Quine, and the well-known philosophers of mathematics Putnam, Maddy, the

297 See Chapter 3 of [Chihara, 1973 #48] for a detailed discussion of Quine’s views on ontological commitment.
Princeton team of Burgess and Rosen, the neo-Fregeans Wright and Hale, and the Mathematical Structuralists Shapiro and Resnik, in addition to the fictionalists Field and Balaguer, as well as the others listed in Section 1, all came to believe, at some time in their careers, that the existential theorems of real mathematics asserted the existence in the actual world of abstract mathematical objects. It is no wonder then that Quine and the above mentioned philosophers all came to believe that mathematics is “irredeemably committed” to the existence of mathematical objects. In other words, it is because of his belief that mathematical theorems are propositions about the actual world—that is, it is because of his acceptance of the Fregean Assumption—that Quine believed that the acceptance of the truth of mathematics committed one to the Realistic position that mathematical objects truly exist. Thus, from Quine’s reasoning, we can assert:

The Fregean Assumption plus the thesis of the truth of mathematics yields Platonism.

Fundamentally then, underlying Quine’s Indispensability Argument is the Fregean Assumption: it is argued that the truth of the existential theorems of mathematics provides us with the premise needed to yield the existence (in the actual world) of mathematical objects. Since Quine could not arrive at this ontological conclusion without the Fregean Assumption, my case against the Fregean Assumption, which was detailed in the previous chapter, supplies the key element of my argument against the various forms of the Indispensability Argument. Thus, it can be seen that the chief conclusion of Chapter 6 that the Fregean Assumption is false undermines these Indispensability Arguments for Platonism. Once one rejects Quine’s assumption that mathematical theorems
are propositions about the actual world (that is, once one rejects Quine’s adoption of the Fregean Assumption), then his case for Platonism collapses. This shows how my refutation of the Fregean Assumption (and hence of Quine’s assumption that mathematical theorems are propositions about the actual world), was supported by showing, in Chapter 8, how analysis can be understood to be like the uninterpreted version of such formal theories as PA, ZF, and Q-STT, whose theorems are not truths about the actual world at all, but rather are true of structures of certain sorts.

All of this shows how the various reasons I provided for accepting the account of mathematical truth in Chapter 8 also provides grounds for disputing the very foundations of Quine’s case for the Indispensability Argument and more generally for his Mathematical Platonism. For this reason, the analyses of real mathematics, set forth in the previous three chapters, cast doubt on the one crucial element of the Indispensability Argument that had been accepted by both its critics and supporters.

To see more clearly how the Fregean Assumption plays an essential role in the Indispensability Argument, reconsider the following versions of the argument, the reasoning of which is especially clear:

5. Putnam’s Version of the Indispensability Argument

Here, again, is Putnam’s argument (which was presented in Section 2 of Chapter 4):
(4) Quantification over mathematical entities is indispensable for science, both formal and physical.

(5) Therefore we should accept such quantification.

(6) This commits us to accepting the existence of the mathematical entities in question.

Premise (1) will be the focus of my analysis of this argument. Why does Putnam assert that quantification over mathematical entities is indispensable for science? Well, there can be no doubt both that the use of mathematics is indispensable for science and also that a great many of the theorems of mathematics are frequently expressed using quantifiers that are said to range over mathematical entities. So one can see why Putnam would have accept (1). But what, precisely, does Premise (1) assert? It clearly takes the existential theorems of mathematics to be asserting the existence of mathematical objects in the actual world since Putnam believed that (1) committed us to accepting the existence (in the actual world) of mathematical entities. So let us reconsider Putnam’s affirmation of (1) with the understanding that he is taking (1) to be asserting the existence of mathematical objects in the actual world.

But why does he believe that (1) asserts the existence of mathematical objects in the actual world? Evidently, it is because he had accepted, as did Quine, the Fregean Assumption—an assumption that was just “regarded as reality”: he believed that the existential theorems of mathematics (that are indispensable for science) asserted the existence in the actual world of mathematical entities; and the reason he believed that was because he had accepted the Fregean Assumption. It would seem then that Putnam’s reason
for believing (1) was based ultimately upon his acceptance of the Fregean Assumption.

Of course, I rejected premise (1) because of the many reasons I gave earlier for rejecting the Fregean Assumption. Furthermore, there are two other considerations, you may recall, that support my rejection of the Fregean Assumption:

(A) The Constructibility Model

The Constructibility model of Simple Type Theory (presented and discussed in Chapter 4) is a version of mathematics that, arguably, is theoretically adequate for the theoretical needs of science, even though none of its assertions presupposes the existence of any mathematical entities or makes use of quantification over mathematical entities. In other words, it provides the tools for maintaining that science, as it is now being practiced, does not presuppose the existence of mathematical objects; and that contrary to premise (4), the existence of mathematical entities is not “indispensable for science”. This is because the scientific theories used today could all be formulated and expressed using versions of mathematics (such as STT) that can be interpreted in such a way that no mathematical objects are required to be “quantified over”.

(B) The Theorems of Mathematics as Hilbertian Sentences

It was also shown (in Chapter 8) that it is not necessary to interpret the theorems of real mathematics to be propositions about the

298 This is the view of the Constructibility Theory defended in Chapter 4.
actual world in order to understand how mathematics can be applied in
science and engineering: one can, instead, understand the theorems to be
Hilbertian sentences that are true of structures of certain sorts.

6. Lewis’s Rhetorical Argument

What can now be said about Lewis’s extravagant rhetorical argument
against the nominalist, which I presented earlier in Chapter 4, Section 3? Well
it, too, is based on the unrecognized, unstated, and unjustified acceptance of
the Fregean Assumption. Because of this assumption, Lewis believed that
ordinary practicing mathematicians--set theorists in particular--are affirming the
existence in the actual world of sets, when they prove their theorems about
sets. This allowed him to conclude that nominalists, who affirm that there are
no sets in the actual world, are implicitly contradicting set theorists and their
set theories. Thus, Lewis’s comment that he is “moved to laughter at the
thought of how presumptuous it would be to reject mathematics for
philosophical reasons” discloses his assumption that there is a conflict between
what mathematicians have proven about sets and what nominalists maintain—
an assumption that he believes is fundamentally mistaken. As I showed in the
previous section in discussing Putnam’s version of the Indispensability
Argument, there is no compelling reason why I, a nominalist, must tell
mathematicians that they “must change their ways, and abjure countless
errors”. This is because, according to my analyses of real mathematics, Lewis
was simply mistaken in regarding the theorems of ordinary set theory as
implying that sets truly exist in the actual world--his mistake was based upon
his unjustified acceptance of the Fregean Assumption.
From my perspective, ordinary mathematicians are not, in general, metaphysicians who are putting forward ontological views about what exists in the actual world. Rather, it was the philosopher Lewis who, because of his implicit acceptance of the Fregean Assumption, misunderstood the nature of the set theorist’s proofs and attributed heavy ontological implications to the theorems of set theory. Thus, his amusing and rousing ad hominem argument described above attests to his rhetorical skills—but not to the accuracy of his judgments.

7. Resnik’s Pragmatic Version of the Indispensability Argument

Let us now examine Resnik’s Pragmatic Indispensability Argument (discussed earlier in Section 8 of Chapter 4) the premises of which, you will recall, were:

(R-1) In stating its laws and conducting its derivations, science assumes the existence of many mathematical objects and the truth of much mathematics.

(R-2) These assumptions are indispensable to the pursuit of science, moreover, many of the important conclusions drawn from and within science could not be drawn without taking mathematical claims to be true.

From these premises, he concluded:
(R-3) So we are justified in drawing conclusions from and within science only if we are justified in taking the mathematics used in science to be true.\textsuperscript{299}

My analysis of this argument will focus on (R-1). Notice that what (R-1) asserts, making explicit one of its crucial presuppositions, is that science assumes the existence in the actual world of mathematical objects. It is being assumed by Resnik that, since science presupposes the truth of mathematics, science must assume the existence (in the actual world) of mathematical objects. This last assumption is due to the fact that it is being assumed that mathematical theorems assert the existence in the actual world of mathematical objects.\textsuperscript{300} And of course this last mentioned assumption is due to Resnik’s acceptance of the Fregean Assumption. In short, Resnik’s Pragmatic Indispensability Argument is based upon his acceptance of the Fregean Assumption.

My view, by contrast, is that premise (R-1) should be rejected, because of my earlier refutation of the Fregean Assumption. From my perspective then, Resnik’s Pragmatic Indispensability Argument ultimately fails because it presupposes the Fregean Assumption.

8. Maddy’s Versions of the Indispensability Argument

(a) Maddy’s Indispensability Argument

\textsuperscript{299} [Resnik, 2005 #613], p. 431).

\textsuperscript{300} He was, I suspect, influenced on this matter by the writings of his teacher Quine, many of whose views about the nature of mathematics were accepted by Resnik.
In this section, I reexamine Maddy’s version of the Indispensability Argument which she put forward in Realism in Mathematics.

Recall from Chapter 6 that Maddy made use of the following claims:

(M-1) Mathematics is indispensable to our best theory of the world (namely, contemporary science)

Because the existence of mathematical objects is indispensable to mathematics,

(M-2) The existence of mathematical objects is indispensable to our best theory of the world.

So,

(M-3) “We are committed to the existence of mathematical objects because they are indispensable to our best theory of the world [advanced by contemporary science] and we accept that theory” (Maddy, 1990 #207, p. 30).

It can be seen that this argument too presupposes the Fregean Assumption: it is assumed in this argument that the theorems of mathematics are truths about the actual world—which is why, when we accept the truths of mathematics, we commit ourselves to the existence in the actual world of mathematical objects.

(b) Maddy’s 92 and 95 Rejections of the Indispensability Argument
In a 1992 publication, Maddy did an about-face and produced reasons for rejecting the Indispensability Argument based on examples from the history of science. These examples were purported to show that “the indispensability of mathematics in a well-confirmed scientific theory no longer serves to establish its truth” (Maddy, 1992 #211, p. 281). I discuss her 1992 publication here in order to contrast the reasons given there for rejecting the Indispensability Argument with what I have developed in this work. Maddy’s grounds for abandoning of the Indispensability Argument did not rest upon her giving up either (M-1) or (M-2), but rather on rejecting (M-3). That is, Maddy allows that mathematics is indispensable to our best theory of the world, and also that the existence of mathematical objects is indispensable to mathematics (and hence to our best theory of the world), but she rejects the idea that these conclusions imply that mathematics must be true. Indispensability does not imply truth (she claimed)—so we are not committed to the truth of mathematics. You will recall that, earlier, I considered her inference from the examples to be unsatisfactory.

By contrast, my response to this argument is to reject (M-2). It is true that mathematics asserts the existence of mathematical objects in so far as there are theorems of mathematics that assert the existence of such things as numbers, sets, functions and the like. But these existential theorems should not be assumed to assert the existence in the actual world of mathematical objects—especially in view of my arguments that the Fregean Assumption is false. Thus, my position is: there is no good reason for affirming (M-2), and hence, the Indispensability Argument does not provide any solid grounds for affirming that the existence of mathematical objects is indispensable for
mathematics. Maddy, on the other hand, had assumed $(M-2)$ in Realism in Mathematics, because she had accepted the Fregean Assumption.

The question I shall now take up is whether the grounds Maddy presented in her 1992 paper “Indispensability and Practice”\(^{301}\) for rejecting the Indispensability Argument are truly compelling. Recall (from my discussion in Chapter 4, Section 6) that Maddy drew inspiration from the controversy over the reality of molecules and atoms that took place in the 19th Century to fashion an argument for rejecting the Indispensability Argument. Recall also that I illustrated Maddy’s reasoning with an example that Mary Jo Nye used to show how some of the strongest arguments for the “absolute reality of atoms” arose in organic chemistry. Now Nye noted that, even though the best explanation, available at that time, of an established phenomenon (the transformation of ammonium cyanate into isomeric urea) implied the existence of “real, structural molecules”, there still was a great deal of hesitation among experts to conclude that molecules in fact exist. The lesson Maddy extracted from such historical examples prompted to her to draw a distinction between those parts of a scientific theory that are deemed to be true and those parts that are regarded as useful—even indispensable—but not true. And Maddy reasoned that, in the light of such examples, we can conclude that the Indispensability Argument of Quine and Putnam was not valid.\(^ {302}\)

Maddy developed other objections to the Indispensability Argument, which were based upon examples from the history of science. In her 1997 book

\(^{301}\) [Maddy, 1995 #236].

\(^{302}\) I give a fuller discussion of Maddy’s grounds for rejecting the Indispensability Argument in [Chihara, 2004 #468], Chapter 5, Section 9.
Naturalism in Mathematics, she cited the role of continuous spacetime in contemporary scientific theory as an example of an entity that is now indispensable for our best scientific theory, even though it is not clear that a responsible use of indispensability can warrant an ontological commitment to the reality of continuous spacetime. Here are the lessons she learned from such examples:

First, we’ve seen that scientists do not take the indispensability appearance of an entity in our best scientific theory to warrant the ontological conclusion that it is real; for this conclusion, the appearance must be in a hypothesis that is not legitimately judged a “useful fiction” . . . Second, we’ve seen that the status of some applications . . . are as yet unsettled . . . ([Maddy, 1997 #317], p. 152).

She concluded that a responsible indispensability argument “seems unlikely to support the existence of more than a few (if any) mathematical entities” ([Maddy, 1997 #317], p. 153).

Now I do not find Maddy’s reasons for rejecting the Quinean Indispensability Argument in these cases to be altogether convincing. For example, the indispensability of “real structural molecules” for obtaining the best explanation of some specific physical phenomenon (such as the transformation of ammonium cyanate into isomeric urea) seems to me to be of a different order entirely from the indispensability of mathematics for science. The case of the indispensability of “real structural molecules” discussed above is much more specific and narrow in scope than the indispensability of mathematics for science. After all, mathematics plays an indispensable role in essentially all scientific reasoning. That cannot be said about the thesis of “real structural molecules”. So let us grant, as did the

303 Similarly, the theory that spacetime is entity has not the generality and universality of mathematics.
scientists described earlier, that the indispensability of “real structural molecules” for obtaining the best explanation of some specific physical phenomenon did not justify concluding that “real structural molecules” exist. However, I cannot see how that fact would justify our inferring that the Quinean Indispensability Argument for the existence of mathematical objects is invalid.

Besides, even granting Maddy that the indispensability of mathematics in some particular well-confirmed scientific theory does not, by itself, establish the truth of mathematics, the fundamental question about the Quinean Indispensability Argument is whether such an indispensability substantially increases the probability or likelihood that mathematics is true or, at least, provides us with additional reasons or grounds for thinking that mathematics is true. And here, Maddy’s historical examples provide us with no solid reasons for rejecting the beliefs of supporters of Quine and Putnam that this kind of indispensability does indeed provide us with this sort of additional support. And if such a specific example of indispensability significantly increases the probability of the truth of mathematics, then the almost completely universal use of mathematics throughout science surely contributes substantially and importantly to our confidence that the mathematics we use is, in some way, true.

To put the basic point in another way, although the indispensability of mathematics in a particular well-confirmed theory does not, by itself, establish the truth of mathematics, there is no serious doubt that the enormous number of cases in which mathematics plays a significant role in the acceptance of some scientific theory and thus can contribute (as far as Maddy’s reasoning shows) to the wealth of grounds we already have (recall the examples I provided in Chapter 4, Sections 5 and 6) for concluding that mathematical theorems are, in some important sense, true. So I consider it somewhat
misleading to proclaim, as Maddy did, that “the indispensability of mathematics in a well-confirmed scientific theory no longer serves to establish its truth”. For the question is not whether “the indispensability of mathematics in a well-confirmed scientific theory” establishes the truth of mathematics, but rather whether such an indispensability does contribute importantly to the probability or likelihood that mathematical theorems are true. And I do not see how Maddy’s examples from the history of science undermine that kind of indispensability reasoning.

I shall now take up Maddy’s objections to the Indispensability Argument put forward in Naturalism in Mathematics. These are objections of an entirely different sort from the earlier ones.

(c) Maddy’s 1997 Objections to the Indispensability Argument

Some time after her 1992 paper, Maddy provided another way of rejecting the Indispensability Argument, which was based somehow on her “mathematical naturalism”. Supposedly, it is from the perspective of her proposed version of mathematical naturalism that Maddy rejected her earlier reliance upon the Indispensability Argument: the Quinean argument, which she had accepted and defended in her Realism in Mathematics, was later regarded in Naturalism in Mathematics as being “at odds with scientific and mathematical practice” and hence unsound.\(^\text{304}\)

Although I agree, in general, with her thesis that the Indispensability Argument is unsound, I feel reluctant to endorse her specific reasons for rejecting the argument provided in her 1992 work. First of all, Maddy never

\(^{304}\)\cite{Maddy,1997 #317}, pp. 191-2, fn. 5.
proves or establishes that the invalidity of the Indispensability Argument does follow from the fact (if it is a fact) that the argument is “at odds with scientific and mathematical practice”. After all, the Indispensability Argument is a philosophical argument put forward by a philosopher. If the argument is indeed “at odds with scientific and mathematical practice” (whatever that means), what does that prove? The burden of proof is surely upon Maddy to show that the unsoundness of the Indispensability Argument follows from the fact (if it is a fact) that the argument is “at odds with scientific and mathematical practice”. And that, she has not done.

Secondly, I do not see that she has provided her readers with a sufficiently clear characterization of her “mathematical naturalism” to warrant much confidence in either the truth of her version of naturalism or in her supposed refutation of the Indispensability Argument that is supposed to follow from that version.305

To see why I have some qualms about her reasoning, consider the following description she gives of “mathematical naturalism”:

To judge mathematical methods from any vantage-point outside mathematics, say from the vantage-point of physics, seems to me to run counter to the fundamental spirit that underlies all naturalism: the conviction that a successful enterprise, be it science or mathematics, should be understood and evaluated on its own terms . . . . Where Quine holds that science is ‘not answerable to any supra-scientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method’ . . . the mathematical naturalist adds that mathematics is not answerable to any extra-mathematical tribunal and not in need of any

305 I must confess that, in general, I find her reasoning in Naturalism in Mathematics to be much fuzzier than her reasoning in Realism in Mathematics. I can’t help but wonder if this reduction in clarity is due to her being strongly influenced by Wittgenstein’s philosophical writings, which she highlighted in Naturalism (cf. [Maddy, 1997 #317], III.1). Perhaps the Wittgensteinian influence explains her reluctance even to attempt to define or sharply characterize what her version of mathematical naturalism is (cf., for example, Wittgenstein’s discussion of games and language-games in [Wittgenstein, 1953 #214], I, section 7 ff).
justification beyond proof and the axiomatic method. . . . in short, from any external standard. ([Maddy, 1997 #317], p. 184).

There is much in Maddy’s view that I find obscure, but I do not wish to test the reader’s patience by going into such matters here in any real detail. So I shall content myself with expressing my utter frustration with what I consider her very fuzzy characterization of her brand of naturalism. In particular, I find it difficult to attach anything like a precise meaning to her assertion that “mathematics is not answerable to any extra-mathematical tribunal”, which seems to me to rest largely on a figurative description of her version of naturalism. I can imagine some philosopher of science saying in response to her argument described above that there does seem to be a kind of “extra-mathematical tribunal” of the sort indicated by the quotation from Grabiner given in Chapter 8:

Many of the results obtained in the calculus had immediate physical applications; this circumstance made attention to rigor less vital, since a test for the truth of the conclusions already existed—an empirical test. ([Grabiner, 1981 #458], pp. 16-7)

Also, what does it mean precisely to say that mathematics is “not in need of any justification beyond proof and the axiomatic method. . . . in short, from any external standard” or that “a successful enterprise, be it science or mathematics, should be understood and evaluated on its own terms”? My ideas of what Maddy is claiming are at best vague and cloudy. Thus, I would like to see, in addition to the figurative arguments she gives in her book, a truly rigorous, clearly and precisely stated argument that should convince a logician that the conclusion she is asserting to be true does indeed follow from the premises she puts forth.
She seems to be suggesting that the “enterprise of mathematics” (whatever that is\textsuperscript{306}) is, in fact, justified by “proof and the axiomatic method” and that it does not need any additional justification. If this is indeed what she is claiming, I certainly have some questions about her “justification”. I allow that specific mathematical theorems can be justified by “proof and the axiomatic method”, but when it is the enterprise of mathematics which one is attempting to justify, then it is not at all clear to me that “proof and the axiomatic method” do justify such an “enterprise”. Indeed, if the existence of such a “justification” is being claimed by Maddy, then what specifically is the justification? And why does she not supply, in detail, the justification in question? So far as I can see, her readers are left only to speculate upon the nature of such a justification.

I shall not express here any more of my qualms about her reasons for rejecting the Indispensability Argument. It is enough, for my purposes in this work, to note that: (a) Maddy and I seem to agree on doubting the validity of the Indispensability Argument, and (b) I am not at all convinced that Maddy’s rather vague and sketchy justifications for her rejection of the Indispensability Argument are indeed sound and compelling.

9. Colyvan’s Version of the Indispensability Argument

Let us now reconsider Colyvan’s argument which was discussed earlier in Section 7 of Chapter 4. The premises are:

\[\textsuperscript{306} \text{Is the “enterprise of mathematics” supposed to be mathematics regarded as an enterprise—some sort of business or venture of some sort?}\]
(CV-1) We ought to have ontological commitment to all and only those entities that are indispensable to our best scientific theories;

(CV-2) Mathematical entities are indispensable to our best scientific theories;

From these premises, we are supposed to conclude:

(CV-3) We ought to have ontological commitment to mathematical entities.307

Leaving aside the strange normative premise of the argument that, interestingly, was not supported with any justification, I reject the second of the above premises for reasons that I essentially gave earlier in Section 2 in discussing Putnam’s version of the argument: first of all, (a) it was shown in Chapter 4 how one can develop a Constructibility version of Simple Type Theory that does not involve quantification over mathematical entities; and second of all, (b) a chief consequence of the structural account of real mathematics presented in Chapter 8 is that our best scientific theories do not require the existence in the actual world of any mathematical entities. I see no reason at all for thinking that the existence in the actual world of mathematical entities is indispensable for our scientific theories.

10. Does Contemporary Philosophy of Mathematics Rest on a Mistake?

307 [Colyvan, 2001 #452], p. 11.
Let us briefly review some of the main points of my discussion of the Frege-Hilbert dispute about geometry (the “Clash of the Titans”) with which I began this work. The dispute basically came down to a disagreement about the nature of geometrical theorems. Following the example of how Euclid viewed geometry, Frege held that the theorems of geometry were propositions about the actual world, whereas Hilbert maintained that they were not propositions at all, but were rather sentences true of certain kinds of geometric structures. Clearly Frege’s position on geometry was the more direct and straightforward. Yet, from the perspective of the history of mathematics, Hilbert, and not Frege, is considered to have been right about geometry. Indeed, from that perspective, Frege’s attitude toward non-Euclidean geometry (comparing it to alchemy), especially given the works of Lobatchevsky, Bolyai, Riemann, Helmholtz, and Hilbert that were available to him, now seems strangely out of touch with how the field had developed and was developing.

However, despite his serious error of judgment regarding geometry, if we focus on the nature of non-geometric mathematics, Frege could be regarded as having won at least a sort of popularity contest, in the sense that the majority of philosophers of mathematics of the Contemporary Period seem to have followed his example in accepting the Fregean Assumption: certainly the large number of philosophers of mathematics who had maintained or, in some examples, just assumed that the theorems of mathematics were propositions about the actual world is very impressive. Here again it is easy to see why philosophers tended to side with Frege and not Hilbert when the focus is on non-geometric mathematics: Frege’s position, according to which the theorems of mathematics are propositions about the actual world (which amounted to his acceptance of the Fregean Assumption) was the simpler and more intuitive
alternative, and close in spirit to the Euclidean position on geometry, with which most philosophers were already accustomed.

Concluding Thoughts:

Despite the impressive list of the influential philosophers of mathematics of the Contemporary Period who had accepted the Fregean Assumption, I have been arguing in this work that this acceptance was, itself, a fundamental and far-reaching mistake—one that misled a great many philosophers into adopting a whole host of unsound and erroneous ontological theses and arguments about the nature of both mathematics and the actual world. In particular, the acceptance of the Fregean Assumption is a presupposition of many versions of the Indispensability Argument—an argument that has led many a philosopher to adopt such incredible “entities” as possible worlds.\(^{308}\)

The refutation of the Fregean Assumption (in Chapter 7) thus cleared the ground for refuting the various Indispensability Arguments (which presuppose that assumption), as well as for maintaining the ontologically parsimonious account of mathematical truth (characterized and defended in Chapter 8), according to which mathematical truths are not sentences that are true of the

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\(^{308}\)I especially have in mind David Lewis, who advanced in [Lewis, 1986 #3] several arguments for the existence of possible worlds—one of these arguments being a version of Quine’s Indispensability argument (see pp. 3 – 5). Lewis thought that the fruitfulness of postulating the existence of possible worlds gives us good reason to believe that possible worlds exist: “It’s an offer you can’t refuse. The price is right; the benefits in theoretical unity and economy are well worth the entities” ([Lewis, 1979 #6], p. 4). For example, in terms of possible worlds, Lewis defined such concepts as being a property and being actual. Another work postulating possible worlds, which was greatly influenced by [Lewis, 1986 #3], is [Bradley, 1979 #249]. See also [Plantinga, 1974 #15] and [Plantinga, 1979 #8]—works that postulate the existence of countless possible worlds in terms of which the author, Alvin Plantinga, analyzes many modal concepts and propositions (although Plantinga’s notion of possible worlds is quite different from that of Lewis’s). See my [Chihara, 1998 #285] for a detailed discussion of both Lewis’s and Plantinga’s philosophical accounts of modality. [Chihara, 1998 #285] also contains a general discussion of both the role that possible worlds plays in modal logic and also many of the principal ontological issues involving possible worlds that arise in the metaphysics of modal logic.
actual world, as is assumed by those who accept the Fregean Assumption, but instead should be regarded in the way sentences of geometry were regarded by Hilbertians: that is as sentences that are true of structures of certain sorts. Of course, it needs to be kept in mind that, for me, strictly speaking, there is no such abstract entity as a structure—when I assert that some sentence is satisfied by a structure, I am not presupposing the existence of some sort of abstract object, a structure, that somehow exists in the actual world. No, I am making use of Bertrand Russell’s idea of “incomplete symbol”, which was the central logical device he used in developing the “no-class” theory in PM. According to Russell’s no-class theory, the sentences containing terms that seem to refer to classes (or sets) are to be understood as abbreviations (or short-hand) for sentences that refer only to what he called “propositional functions”: This is why Russell called his class theory the “no-classes theory”. So my account of structures presented in Chapter 9 might be called a “no-structure theory”. Using that terminology, the “no structure theory” is a crucial element in my rebuttal to the above argument that I need to assume the existence in the actual world of the huge infinities of mathematical abstract entities.

We now have an answer to the question with which I began this chapter: ‘Does philosophy of mathematics rest upon a mistake?’ One can now say “yes” a great deal of the philosophy of mathematics of the Contemporary Period is, indeed, based upon a mistake: the mistake of accepting the Fregean Assumption and of thus thinking of mathematical theorems as propositions about the actual world. For this reason, the acceptance of the Fregean Assumption by Frege, and later by Russell and Whitehead, Quine and Gödel, can

309 This is explained in detail in [Chihara, 1973 #626], Chapter 1, Section 4.

310 This was done in Sections 5 and 6. For more details, see [Chihara, 2004 #468], Chapters 7 – 9.
be regarded as a sort of “original sin” that has led astray countless philosophers of mathematics of the Twentieth Century (and beyond). That mistaken assumption is something to be discarded, but not forgotten, for it provides philosophers with an important lesson of how a large number of well-known philosophers of mathematics were led wildly astray by an assumption about the nature of mathematical theorems—the Fregean Assumption—that was never justified but simply adopted by a small number of key influential philosophers.