

SEMANTIC AND MATHEMATICAL FOUNDATIONS FOR INTUITIONISM

Michael Randall Koss

Submitted to the faculty of the University Graduate School

in partial fulfillment of the requirements

for the degree

Doctor of Philosophy

in the Department of Philosophy

Indiana University

June 2013

Accepted by the Graduate Faculty, Indiana University, in partial fulfillment of the requirements for the degree of Doctor of Philosophy.

Doctoral Committee

David Charles McCarty, Ph.D.

Gary Ebbs, Ph.D.

Amit Hagar, Ph.D.

Neil Tennant, Ph.D.

February 27th, 2013

Copyright © 2013
Michael Randall Koss

To my parents

Acknowledgements

I would like to express my gratitude to Professors David McCarty, Gary Ebbs, Amit Hagar, and Neil Tennant, who together comprised my research committee, for their guidance and advice during this project. I owe special thanks to Professor McCarty for his invaluable encouragement and help improving my work and developing as a writer, teacher, and philosopher. I am also especially grateful to Professor Ebbs for coordinating a weekly discussion group that allowed me to receive helpful feedback on this work and to the other members of that group: Susan Blake, Matt Carlson, Marija Jankovic, Andrew McAninch, and Blakely Phillips.

The community of philosophers and logicians at Indiana University was an indispensable source of support and encouragement. In addition to those mentioned above, I want to thank especially David Fisher, Derek Jones, Professor Kari Theurer, and Professor Paul Spade. The Indiana University Logic Program, the Indiana Philosophical Association, and the Department of Philosophy at Indiana University-Purdue University, Fort Wayne provided opportunities for me to present work that appears in a revised form here. I thank those groups, as well as Professor Larry Moss, Professor Bernd Buldt, and Dr. Ioan Muntean for organizing these opportunities.

This research was supported in part by a Louise P. McNutt Dissertation Year Fellowship from the Indiana University College of Arts and Sciences. I thank the College for providing this financial support.

Finally, I wish to thank my parents, Gerald and Barbara Koss, to whom this project is dedicated in gratitude for all of the love, support, and encouragement without which it could not have been completed.

Michael Randall Koss

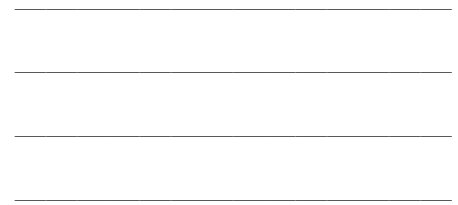
Semantic and Mathematical Foundations for Intuitionism

My dissertation concerns the proper foundation for the intuitionistic mathematics whose development began with L.E.J. Brouwer's work in the first half of the 20th Century. It is taken for granted by most philosophers, logicians, and mathematicians interested in foundational questions that intuitionistic mathematics presupposes a special, proof-conditional theory of meaning for mathematical statements. I challenge this commonplace. Classical mathematics is very successful as a coherent body of theories and a tool for practical application. Given this success, a view like Dummett's that attributes a systematic unintelligibility to the statements of classical mathematicians fails to save the relevant phenomena. Furthermore, Dummett's program assumes that his proposed semantics for mathematical language validates all and only the logical truths of intuitionistic logic. In fact, it validates some intuitionistically invalid principles, and given the lack of intuitionistic completeness proofs, there is little reason to think that every intuitionistic logical truth is valid according to his semantics.

In light of the failure of Dummett's foundation for intuitionism, I propose and carry out a reexamination of Brouwer's own writings. Brouwer is frequently interpreted as a proto-Dummettian about his own mathematics. This is due to excessive emphasis on some of his more polemical writings and idiosyncratic philosophical views at the expense of his distinctively mathematical work. These polemical writings do not concern mathematical language, and their principal targets are Russell and Hilbert's foundational programs, not the semantic principle of bivalence. The failures of these foundational programs has diminished the importance of Brouwer's philosophical writings, but his work on reconstructing mathematics itself from intuitionistic principles continues to be worth studying.

When one studies this work relieved of its philosophical burden, it becomes clear

that an intuitionistic mathematician can make sense of her mathematical work and activity without relying on special philosophical or linguistic doctrines. Core intuitionistic results, especially the invalidity of the logical principle *tertium non datur*, can be demonstrated from basic mathematical principles; these principles, in turn, can be defended in ways akin to the basic axioms of other mathematical theories. I discuss three such principles: Brouwer's Continuity Principle, the Principle of Uniformity, and Constructive Church's Thesis.



Contents

| | | |
|----------|--|-----------|
| 1 | Reflections on the Revolution in Amsterdam | 1 |
| 1.1 | Varieties of Constructivism | 1 |
| 1.2 | A Linguistic Rescue? | 7 |
| 1.2.1 | The BHK Interpretation | 9 |
| 1.2.2 | Dummett's Contribution | 11 |
| 1.3 | The Present Work | 13 |
| 1.4 | Intuitionism and its Rivals | 15 |
| 1.5 | Conventions Adopted in the Present Work | 18 |
| 1.5.1 | Notation | 18 |
| 1.5.2 | Vocabulary | 19 |
| 1.5.3 | Citation and Translation | 22 |
| 2 | A Semantic Foundation for Intuitionism | 23 |
| 2.1 | Realism versus Antirealism | 24 |
| 2.2 | From Antirealism to Intuitionism | 31 |
| 2.3 | Arguing for Antirealism | 39 |
| 2.4 | Summary | 46 |
| 3 | Against Semantic Antirealism as a Foundation for Intuitionism | 47 |
| 3.1 | Criteria for an Adequate Foundation | 47 |
| 3.2 | Semantic Antirealism and Intuitionistic Logic | 49 |

| | | |
|----------|---|------------|
| 3.3 | Semantic Antirealism and Classical Mathematics | 58 |
| 3.4 | What is a Proof? | 63 |
| 3.5 | Away with Semantic Foundations | 70 |
| 4 | Brouwer’s Philosophical Argument for Intuitionism | 72 |
| 4.1 | The Debut of the Philosophical Argument | 73 |
| 4.1.1 | The Semantic Reading | 75 |
| 4.2 | Hilbert and <i>Ignorabimus</i> | 76 |
| 4.3 | <i>Ignorabimus</i> and the <i>Tertium non datur</i> | 79 |
| 4.4 | Against Solvability | 83 |
| 4.4.1 | Weak Counterexamples | 85 |
| 4.5 | Other Appearances of the Argument | 87 |
| 4.5.1 | Brouwer’s Later Work | 88 |
| 4.5.2 | Heyting’s Quasi-Empirical Mathematics | 90 |
| 4.6 | Evaluating Brouwer’s Argument | 91 |
| 5 | Brouwer’s Mathematical Arguments for Intuitionism | 93 |
| 5.1 | The Uniform Continuity Theorem | 94 |
| 5.1.1 | Strong Counterexamples Based on the UCT | 95 |
| 5.1.2 | Weaker Premises for Strong Counterexamples | 98 |
| 5.1.3 | The Weak Continuity Principle | 99 |
| 5.2 | The Creative Subject | 108 |
| 5.2.1 | Formalized Creative Subject Theories | 111 |
| 6 | A Mathematical Foundation for Intuitionism | 118 |
| 6.1 | Foundations and Axioms | 118 |
| 6.1.1 | Epistemology, Metaphysics, and Mathematics | 119 |
| 6.1.2 | Pluralism | 121 |
| 6.1.3 | Naturalism | 125 |

| | | |
|-------|--|-----|
| 6.1.4 | Philosophy as First Mathematics | 127 |
| 6.2 | A Survey of Foundational Principles for Intuitionistic Mathematics . | 133 |
| 6.2.1 | General Form of the Discussion | 133 |
| 6.2.2 | Brouwer's Continuity Principle | 134 |
| 6.2.3 | The Uniformity Principle | 137 |
| 6.2.4 | Constructive Church's Thesis | 140 |
| 6.3 | Summary and Conclusion | 144 |

Chapter 1

Reflections on the Revolution in Amsterdam

A famous article by Hermann Weyl ends with a bold proclamation: “*und Brouwer—das ist die Revolution!*” (Weyl 1921: p. 56) Since Weyl published this article in 1921, the reader might expect the revolution to be a political one, but this would be a mistake. The revolutionary, Brouwer, was a professor of mathematics in Amsterdam and the revolution incarnated in him was a mathematical one. The idea of a *mathematical* revolution may seem alien. What could such a thing be and what would justify it? Part of the task of the present work is to answer these questions.

Brouwer and his followers use the term ‘intuitionism’ to refer to their revisionary approach to mathematics. They are also frequently called constructivists. In order to dispel any confusion about mathematical intuitionism and mathematical constructivism, a few words on their relationship are in order.

1.1 Varieties of Constructivism

When applied to mathematics and its attendant philosophical questions, the term ‘constructivism’ can mean several things. One of them is ontological. Constructivists

in this ontological sense maintain that the objects studied by mathematicians, such as numbers and sets, depend for their existence and properties on the creative activity of cognitive agents. The details of this account can vary among its proponents. One might maintain that mathematical objects come into existence only when first thought about; another might hold that properties of mathematical objects reflect fundamental structures of human thought. We need not survey all the options available to ontological constructivists. It suffices to note the chief point of agreement: all of them maintain that human cognitive activity plays a major role in determining which mathematical objects exist and what these objects are like.

Understood in this ontological sense, constructivism is an ancient doctrine. In Book VI of his *Physics*, Aristotle appeals to constructivism about geometric points in order to challenge Zeno's paradoxical arguments about motion. Somewhat more recently, Kant endorses a kind of constructivism in the *Critique of Pure Reason* when, in the Preface to the second edition, he celebrates Thales for advancing geometry by discovering

that what he had to do was not to trace what he saw in this figure, or even trace its mere concept, and read off, as it were, from the properties of the figure; but rather that he had to produce the latter from what he himself thought into the object and presented (through construction) according to *a priori* concepts and that in order to know something securely *a priori* he had to ascribe to the thing nothing except what followed necessarily from what he himself had put into it in accordance with its concept. (Kant 1998: Bxii)

Later in the same work, Kant describes mathematical cognition as cognition “from the *construction* of concepts.” (*ibid.*, A713/B741, original emphasis) Mathematicians have also endorsed such a view. In the preface to his famous monograph *Was sind und was sollen die Zahlen* (Dedekind 1893), Richard Dedekind answers the question posed by the title of his work: “numbers are free creations of the human mind.” (Quoted from (Ewald 1996: p. 791)) The dependence of numbers on the mind is no idle speculation

on Dedekind's part; in Section 66 of the same work, he proves the existence of an infinite set by appealing to the contents of his thought-realm (*Denkebereich*).

Another sense of 'constructivism' is more purely mathematical. This is the thesis that classical logic does not provide the correct canon for mathematical reasoning. Rather, this is given by intuitionistic logic. Constructivism in this sense comes in degrees. A mathematician might temporarily restrict her reasoning to intuitionistically valid inferences as an exercise in restraint, akin to giving up fast food for a week or writing a novel without using certain letters of the alphabet. In such a case, she is doing constructive mathematics, but it would be a mistake to call her a constructivist in any strong sense. It is better to reserve this meaning of the term 'constructivism' for those who think that mathematicians *ought* to reason intuitionistically. Brouwer and some of his followers represent such a position, but they are not alone. The school of constructive mathematicians founded by the Russian mathematician A.A. Markov Jr. belongs to this category; so do Errett Bishop and some of his disciples.¹

A difficulty facing any attempt to give a unified characterization of this properly mathematical kind of constructivism has to do with what counts as intuitionistic logic. This might refer to the formal intuitionistic logic first codified partially by Kolmogorov and fully by Heyting (Heyting 1930, Kolmogorov 1925). This logic is fixed in that its theorems are exactly the formulae that follow from the axioms using the accepted rules of inference. In general, Bishop and his followers, as well as the Russian constructivists, understand intuitionistic logic in this fixed, formalized sense.

¹There seems to be an ambivalence in the Bishop school about whether classical mathematics actually gets things wrong. The following remarks from some of Bishop's more prominent followers illustrate this ambivalence.

We [constructivists] have a message that implies, no matter how tactfully it is phrased, that you [classical mathematicians] really ought to be doing mathematics in a different way. (Richman 1996: p. 256)

At no stage in our presentation so far have we suggested that constructive methods are the best, let alone the only proper, ones for mathematics. (Bridges and Mines 1984: p. 37)

Brouwerian intuitionists, on the other hand, tend to have a more dynamic conception of logic. While most of them accept the theorems of Heyting's formal logic, they also recognize that this system is incomplete. This leaves open the possibility that new theorems or inference rules can be discovered that are not built into Heyting's system but should be accepted as universally valid. A third possibility is to think in model-theoretic terms. That is, we could select some standard semantics, such as that given by Kripke models, and maintain that intuitionistic logic is the collection of formulae that are valid according to the semantics.

In the present work, we take intuitionistic logic to be that developed axiomatically by Heyting and as a system of natural deduction by Gentzen (1935). In Heyting's case, the goal was to represent the inferences that Brouwer would admit into his mathematical proofs; it was not, as is often thought, to characterize a special intuitionistic semantics for mathematical statements. Except for the unqualified deference to Brouwer, we adopt the same attitude. This is because the principal task of a given system of formal logic (intuitionistic, classical, or otherwise) is to capture precisely which inferences are valid and which are invalid; ϕ is a logical truth just in case it can be inferred validly from the empty set of premises. Semantic systems can be useful technical devices for the development this inquiry, but the inferential aspect takes priority.²

It will facilitate our discussion if we establish some terminological conventions. The first sense of constructivism just discussed, according to which mathematical objects depend for their existence and properties on human cognitive activity, will be called 'ontological constructivism'. When the word 'constructivism' is used without

²In the classical first-order case, the distinction between inferential and model-theoretic accounts of logic is not important because soundness and completeness guarantee that both pick out the same valid inferences and logical truths. Since intuitionistic logic is not complete with respect to any of its well-developed semantic theories, the distinction between inferential and model-theoretic accounts is real and will be important for us, particularly in Chapter 3. Classically inclined readers should remember that classical second-order logic is also incomplete, so this is not a distinction peculiar to intuitionism.

qualification, or with the qualification ‘mathematical’, this will refer to the view that mathematics should be done according to intuitionistic logic. Sometimes, one finds Bishop’s mathematical method called ‘constructivism’ without qualification, but we will avoid this: references to Bishop’s school will always mention his name explicitly. Similarly, Markov’s school will always be referred to as ‘Russian constructivism’, even though Slavic ancestry is neither necessary nor sufficient for adherence to it. Brouwer’s school will, of course, be referred to as ‘intuitionism’.³

Mathematical constructivism is a radical thesis. According to it, classical mathematics, i.e., the collection of results, methods, and principles used by most working mathematicians and taught to legions of students, is severely flawed. This is because classical mathematics is developed using classical logic, which permits inferences that are invalid by the lights of intuitionistic logic. The constructivist therefore maintains that results obtained using these inferences are unjustified, even though most mathematicians would accept the purported proofs as correct. In some cases, the constructivist even claims to be able to demonstrate that a certain theorem of classical mathematics is false, e.g., because it entails the validity of an intuitionistically invalid principle. Brouwer, Markov, Bishop, and their allies therefore think that mathematics is in dire need of reform. The three schools disagree about the nature and extent of the reform that is required. Every theorem of one of Bishop’s theories is a theorem of the corresponding classical theory. By contrast, intuitionists and Russian constructivists claim to be able to prove results that are false according to the classical mathematician. If we set this internal conflict aside, however, it remains the case that a committed constructivist takes on a revolutionary attitude toward mathematics.

³This catalog of constructivists leaves out several figures who influenced the development of constructive mathematics, such as Kronecker and Poincaré. It is only with Brouwer, however, that one finds attempts to develop a constructive mathematics from the ground up.

We also leave out the predicative mathematics of, e.g., latter-day Weyl and Feferman. This is because our focus is on Michael Dummett’s semantic foundation for intuitionism and constructivism, and the immediate consequences of Dummett’s arguments concern logic. Predicativism has a constructive flavor, but in practice its advocates tend to use classical logic. Thus, Dummett’s arguments threaten predicativism along with classical mathematics.

In this respect, Brouwer et al. should be contrasted with the ontological constructivists mentioned earlier. Neither Aristotle nor Kant nor Dedekind ever thought that mathematics itself is in such desperate straits. Dedekind is an especially useful figure for comparison. Like all good German intellectuals of his day, he was well aware of his contemporary philosophical milieu, but he was first and foremost an outstanding and prolific mathematician. He had plenty of opportunities to apply his philosophical views about numbers to his mathematical work, and occasionally he did so (e.g., in his proof of the existence of an infinite set). He also took positions on the mathematical controversies of his day, such as those surrounding the emergence of set theory, but he was not looking to dismantle established results. This suggests that ontological constructivism alone should not be taken to entail mathematical constructivism. Dedekind was not infallible, of course; neither was Aristotle or Kant. Nevertheless, the burden of proof is on the ontological constructivist if he thinks that his philosophical thesis entails the need for mathematical reform.

What about the converse entailment? It might seem that any call for mathematical reform must depend on a particular mathematical ontology. If the task of mathematics is accurately to describe its objects along with their properties and mutual relations, then to say that mathematics has gone wrong is to say that its descriptions are mistaken. What evidence, however, could be mustered in favor of such a claim? A good argument for ontological constructivism might do the job. If the ontological thesis can be established, then mathematics had better conform and, if necessary, reform itself so that it describes the real nature of mathematical objects.

Here, Bishop stands as a helpful contrast. He has nothing but disdain for the idea that metaphysics might intrude upon mathematics. In fact, he criticizes Brouwer on precisely these grounds.

Most important, Brouwer's system itself had traces of idealism and, worse, metaphysical speculation. There was a preoccupation with the philosophical aspects of constructivism at the expense of concrete mathematical

activity. (Bishop 1967: p. 6)

Bishop himself does think that classical mathematics has its flaws and should be corrected. He just thinks that Brouwer was motivated by extraneous philosophical considerations. In other words, he reads Brouwer as an ontological constructivist whose call for mathematical reform was motivated by a particular metaphysics for mathematical objects; Bishop's criticism is directed at the motivation, not the result.

Just as Dedekind's ontological constructivism and mathematical conservatism did not conclusively demonstrate that the two views are independent, so too Bishop's aversion to philosophical intrusions into mathematics does not prove that we should shun such influence. Still, Bishop's attitude highlights that the mathematical constructivist is in a bind. He insists that classical mathematics is getting things wrong. The only way to establish this, however, seems to require a peculiar understanding of mathematical objects. From where does this understanding come? One option is that we get it from a purported non-mathematical grasp into the structure of the mathematical universe, but this quickly raises the specter of philosophical speculation that seems to have no place in mathematics. Another option is that we get it by examining mathematical results, but this is exactly where the constructivist and the classical mathematician disagree, so any argument from these results risks begging the question. Popular opinion is on the side of the classical mathematician, so any appeal to mathematical practice will support the *status quo* (assuming there is some kind of unified mathematical practice, which is doubtful). The situation looks bad for the constructivist.

1.2 A Linguistic Rescue?

In view of this apparently insurmountable challenge, it is common for constructivists to have recourse to language as a way of justifying their position. If one reflects

on the historical setting in which Brouwer attempted his intuitionistic reconstruction of mathematics, this should not be surprising. His most fruitful period of work lasted from 1917 until roughly 1928, although his first challenge to classical logic and mathematics appeared in 1908. This same period saw the publication of Russell and Whitehead's *Principia Mathematica* and Wittgenstein's *Tractatus Logico-Philosophicus*. Meanwhile, the members of the Vienna Circle were attempting to carry out their own philosophical revolution. The *au courant* hope was that a precise analysis of language could be used to resolve or dissolve notoriously intractable philosophical problems.

In this atmosphere, it is not surprising that intuitionism came to be regarded as a linguistic doctrine. There are at least two reasons why this is the case. First, Brouwer claimed to show that generally accepted principles of logic and mathematics were false. Reflections on these fundamental subjects were what initially led to the “linguistic turn” in philosophy, so of course a challenge to mathematical orthodoxy was going to be interpreted linguistically at the time. Second, shifting the debate to the linguistic arena gives the constructivist the resources for a new argument, one that does not require settling an intractable metaphysical question. If one can show that the meanings borne by mathematical statements entail that we should reason according to intuitionistic logic, then the constructivist will have established his thesis. Hence, in Heyting's lecture “Die intuitionistische Grundlegung der Mathematik” (1931), we find him arguing against classical logic on explicitly linguistic grounds.

We here distinguish between propositions and assertions. An assertion is the affirmation of a proposition. A mathematical proposition *expresses* a certain expectation. . . . The affirmation of a proposition *means* the fulfillment of an intention. . . . Thus the formula ‘ $p \vee \neg p$ ’ *signifies* the expectation of a mathematical construction (method of proof) which satisfies the aforementioned requirement. (Benacerraf and Putnam 1983: p. 59, emphasis added)

Heyting goes on to argue that we are not entitled to assert that certain principles of

classical logic are valid. He justifies this by an appeal to the meanings of statements of these principles.

The constructivists' emphasis on meaning is not isolated to the early part of the 20th Century. One sees it appear in Mark van Atten's account of the evolution of Brouwer's thought concerning the principle *tertium non datur* (called 'PEM' here).

In his dissertation of 1907, Brouwer still accepted PEM as a tautology, understanding $A \vee \neg A$ as $\neg A \rightarrow \neg A$. Curiously, he did realize at the time that there is no evidence for the principle that every mathematical problem is either provable or refutable; *this is the constructively correct reading of PEM*. (van Atten 2009: §2.4, emphasis added)

Later, we will argue that van Atten misinterprets Brouwer. For now, though, it suffices to note that he takes Brouwer to be insisting on a special constructive meaning of a statement of the *tertium non datur*. Furthermore, in the passage just quoted, van Atten (who is an intuitionist) seems also to endorse this himself.

1.2.1 The BHK Interpretation

This linguistic understanding of constructive mathematics finds its general expression in the so-called Brouwer-Heyting-Kolmogorov (BHK) interpretation of the logical constants. An early version of this was introduced by Heyting in his (1934) and independently by Kolmogorov in his (1932). Brouwer himself never articulated anything like this. In Chapter 4, we will argue that the inclusion of his initial in the acronym 'BHK' is due to a mistaken interpretation of his work.

Since we will have cause to discuss to the BHK interpretation frequently in what follows, it will be helpful to set it forth now. The task is to characterize the meaning of each logical constant in terms of what would count as a proof of a statement in which that constant appears as the main connective or quantifier. Here is a statement of the interpretation given by Troelstra and van Dalen (1988: §1.3.1). We have changed the variables to conform to the conventions adopted in this work and discussed below.

Here, ϕ and ψ range over all mathematical statements, A is an arbitrary mathematical predicate, and D is an arbitrary mathematical domain.

\wedge : A proof of $\phi \wedge \psi$ is a proof of ϕ together with a proof of ψ .

\vee : A proof of $\phi \vee \psi$ is a proof of ϕ or a proof of ψ .

\rightarrow : A proof of $\phi \rightarrow \psi$ is a construction that transforms a proof of ϕ into a proof of ψ .

\perp : There is no proof of \perp (i.e., contradiction).

\forall : A proof of $\forall xAx$ is a construction that transforms a proof that $d \in D$ into a proof of Ad .

\exists : A proof of $\exists xAx$ is a proof of $d \in D$ together with a proof of Ad .

Each of the constants mentioned above is taken as primitive; their meanings are given by the associated proof descriptions. Using these, we can characterize a proof of a negation in terms of \rightarrow and \perp :

\neg : A proof of $\neg\phi$ is a proof of $\phi \rightarrow \perp$.

The goal of the BHK interpretation is twofold. First, it is supposed to give a semantic theory for intuitionistic logic analogous to the classical semantics in terms of interpretations and satisfaction that was first given by Tarski (1935). To succeed in this task, the interpretation must entail that all and only the theorems of intuitionistic logic are valid when interpreted according to the BHK clauses. Second, it is supposed to make explicit the way that intuitionists purportedly understand mathematical statements. Recall Heyting's claim, quoted above, that the affirmation of a mathematical proposition expresses the fulfillment of an intention. If mathematical

intentions are fulfilled by proofs, then the BHK interpretation makes clear what we are asserting when we assert a logically complex proposition.⁴

It is an open question whether the BHK interpretation accomplishes these goals. For now, however, let's assume that it does. Does this help the constructivist's case? Clearly it does not, at least on its own. By itself, the BHK interpretation might be a useful heuristic device for a mathematician who decides to reason according to intuitionistic logic, but it does not show that one *ought* to reason intuitionistically. To establish this stronger conclusion, one must provide an argument showing that the BHK interpretation gives the *correct* theory of meaning for mathematical statements.

A gesture at such an argument is given by Heyting in his "Grundlegung" lecture. There, he claims that mathematical propositions express intentions and assertions express fulfillments. A reference to the phenomenologists makes clear that he has Husserlian categories in mind. If he is right about propositions and assertions, then it may follow that the BHK interpretation gives the correct theory of meaning for logically complex mathematical statements. Still, he gives no argument that his Husserlian view about propositions is correct, and this (or something like it) is needed in order to establish the BHK interpretation as the correct theory of meaning for the logical constants.

1.2.2 Dummett's Contribution

The most influential attempt to provide the kind of argument just mentioned is that made by Michael Dummett. In his (1975b), (1991), and (2000), among other works, Dummett has argued for a proof-conditional semantic theory for mathematical statements, a theory that yields the BHK interpretation as giving the correct account

⁴For now, we set aside certain difficulties surrounding the BHK interpretation, although we will address them later. For one thing, different versions of the BHK interpretation can be found in the literature, e.g., those given by Dummett (2000), Heyting (1934; 1966), Kolmogorov (1932), Troelstra and van Dalen (1988), van Atten (2004). Their equivalence is nontrivial. In addition, there is no clause to handle logically simple statements; in order for the BHK semantics to succeed in its task, something must be said about atomic formula.

of the meanings of the logical constants. A corollary of this result, he claims, is that mathematicians should use intuitionistic logic in their proofs; those that depend on intuitionistically invalid principles do not establish their conclusions.

Dummett's argument and his followers' adaptations of it are part of a more general metaphysical-*cum*-linguistic project. The dispute between intuitionists and classical mathematicians is, on this view, representative of a class of traditional philosophical questions about the ontological status of entities of various sorts. In the mathematical case, the objects in question are sets, numbers, and the like. Another case on which Dummett has written concerns the status of events that occurred in the past. We might also include questions about material substrata of physical objects, about unobservable entities posited by scientific theories, etc. (Dummett 1978a) contains a fairly extensive list of the kinds of debates that may fall into this class

What these debates have in common is that, in their traditional form, they are about whether the entities under discussion “really exist,” at least in some way that is independent of human cognitive activity. Hence, Dummett proposes the general term ‘realist’ for those who defend the affirmative and ‘antirealist’ for the realists’ opponents. Clearly, one mathematical version of the antirealist’s thesis is ontological constructivism. Dummett suggests that Brouwer, because of his ontological constructivism, challenged the way that we reason when we do mathematics. The result is mathematical intuitionism, a central feature of which is the rejection of classical logic in favor of intuitionistic logic. As we saw, however, it is difficult to argue for ontological constructivism *per se*, and attempts to argue immediately for the use of intuitionistic logic tend to preach only to the converted. Dummett’s proposal is to collapse these two theses. His view is that the genuine content of ontological constructivism is captured by mathematical constructivism.

The general hope of the Dummettians is that this approach can settle a number of the seemingly intractable metaphysical questions mentioned above. The argument

for the use of intuitionistic logic in mathematics is very schematic. First, one argues for a verificationist theory of meaning for a certain class Σ of statements. If this is established, then one simply adjusts the relevant notion of verification to fit the subject matter of the statements in Σ : mathematical statements are verified by proofs, historical claims by testimony and archeological investigation, etc. What makes the mathematical case special is that we already have a robust and rigorous alternative to the realist’s classical mathematics, namely, constructive mathematics and its attendant intuitionistic logic.

1.3 The Present Work

Dummett’s argument is often taken to be the strongest that has been given for the mathematical constructivist’s position. For instance, Geoffrey Hellman (1989: p. 48) takes Dummett as a standard-bearer for what he calls an “extremist, revisionist stance *vis-á-vis* classical mathematics;” in this article, only Brouwer receives the same honor, and Hellman interprets even him as a proto-Dummettian. Similarly, according to Susan Haack (1974: p. 103, original emphasis), “Dummett’s arguments make admirable sense of much that is fragmentary in earlier Intuitionist work; so that if *they* can be shown to be inadequate, this thesis will be quite seriously discredited.”

A primary claim of the present work is that Dummett’s argument on behalf of constructive mathematics does not establish its conclusion; more generally, the intuitionist should not try to argue first for the BHK interpretation or any other special theory of meaning for the logical constants. The first portion of what follows will be devoted to establishing this. Dummett’s argument is complicated and subtle, so we defer a full presentation of it to the next chapter. This exposition is followed by a series of challenges to the Dummettian position. Our focus there and throughout will be on this position’s application to mathematics, although some of the discussion will

apply to the more general project of resolving metaphysical questions via the theory of meaning.

Once this portion of the work is complete, intuitionism will seem to be in trouble. A position that already has little popular support will have lost its best theoretical foundation as well. The remaining task is to rebuild the intuitionist's tower on a stronger foundation. To do this, we return to Brouwer's own work, which is in need of a close reexamination. First, we look at his earliest published challenge to the principles of classical logic in (Brouwer 1908b). Secondary literature on Brouwer and intuitionism often places undue weight on this argument. Insofar as it is supposed to be a challenge to classical logic, it is not successful. We argue, however, that Brouwer's paper is best understood as a challenge to his rival David Hilbert, not primarily to classical logic or mathematics. In this respect, the paper marks Brouwer's entrance into an important scientific dispute, one in which Hilbert was central but that has often been ignored more recently because of later mathematical results. Besides clarifying Brouwer's initial motivation for intuitionism, our discussion of Brouwer's early argument (and its later manifestations) will allow us to dispel the mistaken idea that Brouwer was ever driven by linguistic considerations. For a variety of reasons, this mistake is common, to the point that it has affected translations of Brouwer's writings and interpretations of his more promising arguments.

With the ground cleared, we can consider the more powerful arguments found in Brouwer's work beginning in 1918, emphasizing two of their features. First, the theorems Brouwer proves entail results contrary to classical logic and mathematics (notably, but not at all exclusively, the invalidity of the *tertium non datur*). Second, the theorems and their proofs are purely mathematical in content (they are about things like numbers and functions) and form (they use accepted standards of mathematical proof and appear in the leading mathematical journals of the day). In these papers, we find Brouwer proving results the way any mathematician does: he starts

from axioms and established results and deduces their consequences.

Another central claim of the present work is that Brouwer's approach to mathematics should continue to stand as a model for intuitionists, but not for the reasons it usually so stands. Rather, the intuitionist should attempt to establish his results in a familiar mathematical way, namely, by identifying more fundamental principles and giving mathematical proofs from these principles. As for these principles themselves, they should be treated like other proposed theoretical axioms. In the concluding chapter, we discuss how such a treatment would proceed, what kinds of principles the intuitionist might rely on in order to demonstrate his results, and what reasons might be given for taking these to be true.

1.4 Intuitionism and its Rivals

One more word is in order about the relationship between intuitionism and constructive mathematics. According to the terminological convention established in §1 above, intuitionism is a version of mathematical constructivism insofar as its adherents reason using intuitionistic logic. Indeed, the discussion thus far has used 'intuitionism' and 'constructivism' almost interchangeably; strictly speaking, this is a mistake, since the two approaches to mathematics are not identical. Henceforth, our focus will be on intuitionism, which brings forth two questions. First, what distinguishes intuitionism from its constructive brethren? Second, why focus on it rather than the equally constructive alternatives?

Bishop's constructive analysis is a proper part of both intuitionism and Russian constructivism in the sense that all of its theorems are also theorems of the latter two schools. In a sense, Bishop's approach is the most purely constructive, insofar as the only thing distinguishing it from classical mathematics is its use of intuitionistic logic. Still, it is a striking feature of Bishop's theories that all of their results are perfectly

acceptable to the classical mathematician, since intuitionistic logic is strictly weaker than its classical counterpart.

As a result of these relationships, Bishop's analysis provides common ground among all of the camps being discussed. In addition, many of the results he and his followers proved are necessary for any constructivist who wants her mathematics to be usable for natural science. Since this is a *sine qua non* of any approach to doing mathematics, the intuitionist and Russian constructivist are in the debt of Bishop and his disciples.⁵ On the other hand, Bishop's own reasons for constructive self-restraint are disappointing. In the preface to his primary textbook on the subject, he complains about the lack of "numerical meaning" in classical mathematics, and a rare slip into metaphysical speculation consists of assertion rather than argument.

A set is not an entity which has an ideal existence. A set exists only when it has been defined. To define a set we prescribe, at least implicitly, what we (the constructing intelligence) must do in order to construct an element of the set, and what we must do to show that two elements of the set are equal. (Bishop 1967: p. 2)

In a later lecture (Bishop 1985), one finds similar appeals to things like meaning, meaningful distinctions, and common sense, but it is hard to see how to craft a convincing argument for constructivism on this imprecise basis. The Dummettian argument for intuitionistic logic in mathematics, based as it is on more nuanced views about meaning and language acquisition, is the kind of argument that Bishop seems to need, but Dummett himself argues (2000: §7.5) that his semantic proposal might be used to justify principles that go beyond anything Bishop would accept. (Of course, we contend that Dummett's argument also does not establish its conclusion, in which case the point is moot.)

In addition, there is something unsatisfying about focusing solely on Bishop's

⁵According to the summary of Brouwer's dissertation given by Mancosu (1998: p. 5), "it is the application of mathematics in experimental science and logic that is exposed as the root of all evil." Needless to say, Brouwer's philosophy has plenty of idiosyncratic features. Fortunately, one can be an intuitionist without accepting the Brouwerian gospel as holy writ.

analysis, precisely because all of it is acceptable to the classical mathematician. One advantage to studying intuitionism or Russian constructivism is that they automatically bring with them arguments against classical logic. This gives them a certain force that Bishop's approach lacks. His requires an extra-mathematical argument; without one, there seems to be no reason not to return to the *status quo* of classical methods. Intuitionism and Russian constructivism establish their divergence from orthodoxy via recognizably mathematical methods.

What about those two schools and their relationship to each other? A useful survey of technical points on which they diverge is given by Bridges and Richman (1987: §6.1). For our present purpose, it suffices to note some sociological phenomena. First, much discussion of constructive mathematics focuses on intuitionism. This includes Dummett's work, such as his book *Elements of Intuitionism* (2000), which is conspicuously not titled '*Elements of Constructivism*'. A standard textbook on constructive mathematics is that by Troelstra and van Dalen (1988), who are both intuitionists and students of Heyting, although their book includes discussions of alternative approaches.

Second, intuitionism seems to be the only approach that has been the subject of robust philosophical discussion. In part, this is due to an overemphasis by philosophers on the place of logic in the controversy. By the time Russian constructivism emerged in the 1950's, philosophers tended to think that intuitionistic logic represents an interesting formal system, but that no good reason had been given why a full-scale reform of mathematics was required. Since the Russian approach called for a similar reform, it was relegated to the same pile as full-blown mathematical intuitionism. Also, Brouwer himself articulated a philosophical picture that serves to motivate his mathematical revisionism, giving later philosophers some raw material to study in their investigations of intuitionism. By contrast, neither Markov nor his followers

seem to have articulated such a system.⁶ For these reasons, then, we have chosen to focus on intuitionism.

1.5 Conventions Adopted in the Present Work

We conclude our introduction with a few words about conventions that we have been adopted concerning notation, vocabulary, and textual matters.

1.5.1 Notation

As far as possible, we have attempted to use standard logical and mathematical symbols. The following symbols are used in their familiar ways to denote the corresponding logical constants:

$$\wedge, \vee, \rightarrow, \leftrightarrow, \neg, \forall, \exists.$$

In addition, since contradiction is frequently taken as a primitive notion in intuitionism (particularly in the context of the BHK interpretation), we will use the symbol ‘ \perp ’ to denote it. The reader who is uncomfortable with the idea that contradiction is primitive is free to treat “ \perp ” as an abbreviation for some standard contradiction like ‘ $\forall\phi(\phi)$ ’.

The use of letters of the Greek and Roman alphabets will generally be explained when necessary. With one exception, therefore, we will not set down any uniform convention for their use. The exception concerns the use of lowercase Greek letters ‘ ϕ ,’ ‘ ψ ,’ and ‘ χ ,’ which will be reserved as variables ranging over truth values. In particular, we allow for quantification over these truth values, e.g.,

$$\forall\phi(\phi \leftrightarrow \phi).$$

⁶Kushner (2006: p. 560) alludes to Markov’s “mathematical worldview” but says little about what this worldview is or what Markov’s motivations were for adopting it.

The reader concerned with quantification over truth values should bear in mind that in both the classical and the intuitionistic case, truth values are identified with elements of $\mathcal{P}(\{0\})$, the power set of the singleton set containing 0. (Incidentally, this illustrates our notation for the power-set operation.) In classical ZF set theory, this is just the set $\{0, 1\}$. In IZF, the intuitionistic version of ZF, the set in question cannot be finite, but Gödel (1932) showed that intuitionistic logic must be infinitely valued, so we do not want the set-theoretic object containing all of the truth values to be finite. In any case, quantification over truth values poses no threat if we allow ourselves a modicum of set theory. On that note, set theoretic notation is also standard.

Other notation will be explained as it appears. In quotations from other authors, we have occasionally modified the notation to conform to the conventions described here. This facilitates discussion of the quotations without abrupt changes in notation. When such modifications occur, a note will indicate this.

1.5.2 Vocabulary

One important terminological ambiguity has already been discussed above. This concerns our uses of the term ‘constructivism’ in the context of mathematics and its philosophy. To reiterate, our primary, unqualified use of this word (or its variant ‘mathematical constructivism’) will be to refer to mathematical approaches that insist on reasoning according to intuitionistic logic. Particular varieties of constructivism will be referred to by their more precise names, as in the previous section. ‘Ontological constructivism’ will be used for the doctrine that mathematical objects are created by, constructed by, or otherwise dependent on the human mind.

Another range of terms for which some stipulation is necessary is that concerning Dummett’s position and argument for it. As mentioned above, Dummett’s proposal is to understand metaphysical questions in semantic terms. In particular, metaphysical antirealism about x ’s, according to his suggestion, collapses to the view that the

correct semantic theory for statements about x 's is in terms of what would verify those statements. (A corollary is supposed to be that we should reason about x 's using intuitionistic logic.) For this reason, one who adopts a verificationist theory of meaning (about x 's) is sometimes called a semantic antirealist (about x 's), while the contrary position is sometimes called semantic realism.

The problem with this vocabulary is that Dummett's proposal is still a conjecture. It is neither obvious nor established that every such metaphysical debate can be recast in semantic terms. Indeed, Dummett himself concedes that the debate over universals may not fit this mold.

It does not appear that the anti-realists in this case—the nominalists—who denied the existence of universals and the referential character of general terms, were anti-realists in the sense of the characterisation I have now adopted: that they were necessarily committed to a different view of the kind of truth possessed by statements containing general terms (that is by all statements) from that of the realists. (Dummett 1978a: p. 147)

Nevertheless, our primary concern in this work is with the mathematical case, which Dummett takes to be a paradigm for how his program can be applied to settle traditional questions in metaphysics concerning realism. Thus, we will adopt the expression 'semantic antirealism' (about x 's) to refer to the view that the correct theory of meaning (about x 's) is a verificationistic one and the correct logic for reasoning (about x 's) is intuitionistic logic.

This raises the question of the terms 'semantics' and 'meaning'. Philosophers (Davidson, for example, and Dummett himself) have observed that there are two forms that a theory of meaning might take. One would be a theory relative to a given language. In this way, we might give (in English, say) a theory of German meaning. The other would be a philosophical characterization of what counts as the meaning of linguistic items in any language. Thus, one might say that the meaning of a statement (in any language) is given by the way in which it would be verified.

Following Dummett's proposal (1991: p. 22), we will refer to theories of the first

kind as meaning-theories and theories of the second kind as theories of meaning. For the sake of stylistic variety, we will also use the term “semantic theory” as a synonym for “theory of meaning” in the sense just described. Since our focus in this work is on a proposed semantic theory (i.e., a theory of meaning) for *any* language in which mathematics can be expressed, we will have little reason to refer to meaning-theories, but it will be helpful to bear this distinction in mind nevertheless.

Finally, there is the matter of logical principles, many of which have multiple names. In particular, intuitionists are most notorious for their rejection of a principle variously called the law (or principle) of the excluded middle (or third). Whatever we call it, this logical principle says that

$$\forall\phi(\phi \vee \neg\phi).$$

This is sometimes mistakenly identified with the semantic principle of bivalence, according to which every well-formed statement is either true or false. In fact, one might accept the logical principle but reject the semantic one by denying that a true disjunction must have at least one true disjunct. To treat the two as equivalent therefore requires some additional theory in the background.

Still, in light of this confusion and the variety of English names for the logical principle under discussion, we have elected to refer to the principle by its traditional Latin name, the *tertium non datur*. The same will apply to other logical principles whose Latin names are standard and familiar, such as *ex falso quodlibet*:

$$\forall\phi(\perp \rightarrow \phi).$$

1.5.3 Citation and Translation

The publication history of some of Brouwer's and Dummett's writings raises special issues that we will discuss here. The most readily available source for Dummett's early papers on language, logic, and mathematics is his (1978b). In such cases, it will always be stated explicitly which article is being quoted, although the citation will refer to Dummett (1978b). Full information for the original publication is available in the bibliography.

Similarly, Brouwer's papers are more easily available in collections rather than in their original places of publication. Also, Brouwer wrote primarily in Dutch and German (occasionally in French and English), so the matter of translation must be addressed. For our purposes, the most valuable collections are Brouwer (1975), van Heijenoort (1967), Benacerraf and Putnam (1983), Ewald (1996), and Mancosu (1998). Except for the first, all of these contain English translations of the writings contained therein. In the case of the *Collected Works*, only the Dutch papers are translated; those in German are left in the original language.

We have adopted a citation policy like the one adopted for Dummett. When an English translation of a paper is available, we quote it and refer to the collection in which it can be found, making explicit which paper is being quoted. Again, full bibliographic details for the originals are given. On occasion, we found reason to quote one of the German-language papers that appears only in the *Collected Works*. In such cases, an English translation from the German is given with the original German in a footnote. Also, at one point in Chapter 4, we emphasize a small but important error in Heyting's translation of one of Brouwer's early Dutch papers. There, we give Heyting's translation from the *Collected Works*, together with the original Dutch and our own more accurate English rendering.

Quotations from and references to other writings do not involve nearly as much complexity. They are left as self-explanatory.

Chapter 2

A Semantic Foundation for Intuitionism

In his (1975b), Michael Dummett asks how somebody might defend intuitionism as the uniquely correct foundation for mathematics. The answer he proposes, in the remainder of that article and elsewhere, is that such a defense must be based on considerations concerning the meanings of mathematical statements. According to Dummett, reflection on these statements, especially on how we come to understand them, reveals that they cannot have the meanings that classical mathematicians treat them as having. Rather, Dummett argues, the statements mean precisely what is needed in order to justify the major results of intuitionistic mathematics and logic.

We will begin by examining exactly what kinds of meanings Dummett thinks are presupposed by classical and intuitionistic mathematicians. After that, we will consider how Dummett argues for intuitionistic mathematical (including logical) principles on the basis of his preferred theory of meaning.¹ Then, we will examine Dummett's arguments for his underlying semantics. The purpose of this chapter is simply

¹In keeping with the spirit of L.E.J. Brouwer, who maintained throughout his life that logic is a branch of mathematics and not vice versa, we will treat logic as a part of mathematics alongside algebra, analysis, etc.

to expound the Dummettian position; we will delay our evaluation of it until the following chapter.

2.1 Realism versus Antirealism

Dummett's attempt to give a semantic defense of mathematical intuitionism is an effort to carry out one component of a larger project concerning a common kind of metaphysical debate. There are and always have been disputes in philosophy about whether the members of a certain kind X of entity "really exist" (whatever that might mean when spelled out). Let us stipulate that the term '(ontological) realist' about X describes those who defend the affirmative in these disputes about the existence of entities in kind X . Then we can find, throughout the history of philosophy, realists about universals, numbers, the external world, matter, etc. Let us stipulate further that the term 'antirealist' will describe the opponents of the realists; then there will be antirealists about the members of various kinds. Immediately, this vocabulary demands justification. For instance, it is hardly obvious that realism about numbers has anything to do with realism about matter. This is reflected in the tendency to use more precise "-isms" when conducting these various disputes. Mathematical realists, for example, are usually called 'Platonists', and their opponents '(ontological) constructivists'. Realists about matter are materialists, and they argue with idealists. And so on.

Is there anything to justify assigning every player in each of these disputes to one or the other of two large groups, realists and antirealists? Dummett's overall project can be understood as an attempt to provide such a justification by identifying a single semantic issue at the core of all realism/antirealism controversies. As he puts it (1978b: 146, original emphasis),

The dispute thus concerns the notion of truth appropriate for statements of the disputed class; and this means that it is a dispute concerning the

kind of *meaning* which these statements have.

The two competing notions of truth conflict about whether a statement's truth, and therefore its meaning, is tied in an essential way to a human being's capacity to verify it. When the statement is mathematical, the realist insists that it is true or false even if nobody is able to prove or refute it. The antirealist, by contrast, maintains that its truth-value must depend in some central way on a person's capacity to produce a proof or refutation of it. More generally, realists insist that a statement about a certain kind of entity is either true or false even if nobody could in principle figure out which alternative holds. Antirealists, by contrast, insist that a statement is true only if it is possible for a person to verify it and false only if somebody can falsify it. In general, the appropriate techniques for verifying a statement will depend on its subject matter. Empirical observation, for example, will play a role in determining whether a statement about a physical object is true, but it is widely thought to be irrelevant to a statement about mathematical entities; in the latter case, notions of mathematical proof are at issue.

To be fair, Dummett concedes that a meaning-theoretic analysis might not characterize every debate that we would intuitively regard as concerning realism about something or other. Shortly after the passage just quoted, he allows that it might not accurately describe the problem of universals, at least as that problem was understood in the Middle Ages. For this reason, it is best to understand Dummett's larger project as a program of research into how far his proposal can go as we examine various kinds of entities. However this turns out, Dummett does think he has provided an accurate characterization of the conflict between classical and intuitionistic mathematicians. In fact, he thinks it provides him a paradigm case for his larger program, and that is what interests us here.

Let us now examine Dummett's philosophy of mathematics more closely. Again, as Dummett himself admits, it is not clear that his characterizations of realism and

antirealism accurately describe the core of every debate about the real or objective existence of members of a kind of entity. He thinks, however, that he has definitely succeeded in at least one case, namely, the debate between classical mathematics, as practiced by most working mathematicians today, and the intuitionistic mathematics developed by L.E.J. Brouwer and others. He contrasts his interpretation of this conflict with a more obviously metaphysical one. According to the metaphysical version, classical mathematicians think of mathematical objects as “independently existing abstract objects” (*ibid.*, p. 229), while intuitionists insist that they are produced by the human mind.²

The problem with this metaphysical question, Dummett thinks, is that there seems to be no way to settle the issue without resorting to an appeal to a theory of meaning.

Preliminary reflection suggests that the metaphysical question ought not to be answered first: we cannot, as the second [i.e., metaphysical] type of approach would have us do, *first* decide the ontological status of mathematical objects, and then, with that as premiss, deduce the character of mathematical truth or the correct model of meaning for mathematical statements. (*ibid.*, p. 229, original emphasis)

Furthermore, Dummett is interested in determining how to argue for intuitionism as a rival to classical mathematics. He is “not concerned with justifications of intuitionistic mathematics from an eclectic point of view” (*ibid.*, p. 215). In other words, Dummett is not interested in treating intuitionism as one interesting approach to mathematics, standing alongside classical mathematics (and other approaches) but not uniquely privileged. Like Brouwer, he is interested in how one might argue for intuitionism as *the* correct way to do mathematics, and this will mean showing that the activity of classical mathematicians is flawed in some way. According to him, the only possible argument for this conclusion is one that begins by establishing semantic antirealism

²An intuitionist need not accept this psychologistic metaphysics, though Brouwer did. So too, a classical mathematician need not be a metaphysical Platonist. The point here is simply that the dispute is often framed in these ontological terms, and Dummett hopes to use it as something like a case study for his larger research program.

for mathematical statements and then showing how that leads to the core principles of intuitionism.

In the next two sections, we will examine both steps of this two-part argument. First, we will see how semantic antirealism is supposed to yield intuitionistic logic and mathematics. Then, we will examine how Dummett argues for the semantic thesis. First, however, we should explore exactly how each of a realistic and an antirealistic semantics makes a difference to our understanding of a mathematical statement. So, let ϕ be such a statement, and assume it is well-formed and coherent. Dummett's realist will insist that the truth-value of ϕ is a settled matter: it is either true or false. This is the case regardless of what ϕ says, whether anybody has ever proved or refuted it, and even whether anybody ever will prove or refute it. Its truth conditions obtain or fail to obtain independently of anything concerning human cognitive abilities and attainments. Furthermore, according to the realist, these independent truth conditions yield the meaning of ϕ , and this meaning is what a person knows when she understands the statement.

The Dummettian antirealist will reject the claim that the truth conditions just mentioned really are independent of human capacities. He will insist that they must be given in terms of what human beings are able to do with respect to ϕ , especially whether anybody knows how to determine its truth value. For this reason, we should really speak of the verification conditions or, since mathematical statements are verified by proofs, the proof conditions of ϕ . The antirealist will therefore insist that the meaning of the statement is given by what counts as a proof of it and that a person who understands ϕ is able to recognize a correct proof of it were she presented with one.³

Although Dummett never articulates the matter this way, it will be helpful to

³Following Dummett, we are subsuming calculation under proof. For example, when somebody correctly calculates that $242+136 = 378$ using familiar and reliable arithmetic methods, this amounts to a proof of the statement ' $242 + 136 = 378$ '.

recall Tarski’s material adequacy condition on a truth definition as formulated in his (1935: §1). According to it, any definition of truth for a language \mathfrak{L} must entail every sentence of the form

$$S \text{ is true in } \mathfrak{L} \leftrightarrow p.$$

In this schema, now famously known as Schema T, the precise relationship between S and ‘ p ’ depends on a number of factors. Tarski himself treated S as the name of a sentence in \mathfrak{L} and ‘ p ’ as a translation of it into the metalanguage—English, in this case. One criterion for a good translation is then that S and ‘ p ’ have the same truth conditions. Tarski and followers of his like Davidson (1967) take it for granted that, since we understand English, we will understand the truth conditions of ‘ p ’. We can then say that ‘ p ’ provides a description, in familiar English, of the truth conditions of S .

For the realist, there isn’t much more to say. He looks at Schema T and nods approvingly, resting content in his knowledge of English and his view that the truth conditions described by ‘ p ’ either obtain or fail to obtain (and, therefore, that S is either true or false). The antirealist thinks this is too hasty and that more can be said. In particular, she can give a general characterization of what ‘ p ’ must be like: it must assert the existence of evidence for ‘ p ’. In other words, the antirealist understands Schema T to be shorthand for the following, which we can call Schema P:

$$S \text{ is true in } \mathfrak{L} \leftrightarrow \exists \pi (\pi \vdash 'p').$$

Here, π ranges over pieces of evidence for statements. In the case of mathematical statements, these will be proofs. ‘ $\pi \vdash 'p'$ ’ means that π can be recognized as a correct proof of the statement ‘ p ’. Thus, by accepting the finer-grained Schema P as an explication of Schema T, the antirealist incorporates verifiability into his theory of truth for a language.

With these schemata in place, we can see that there are really two related disagreements between the realist and the antirealist. The first concerns the meaning of a statement; the second has to do with whether a given statement is true once its meaning is fixed. (A third is over what a person who understands a statement knows, but since Dummett takes this to reveal a statement's meaning, it really is just a version of the first.) In some cases, both sides will agree on the second of these. This is because the realist is happy to grant the right-to-left conditional of Schema P. For him, the existence of the proof of ' p ' is evidence that the truth conditions described by ' p ' obtain. So, when ' p ' is a statement with a recognizable proof that is acceptable to both the realist and the antirealist, they will agree that ' p ' is a true statement.

Despite this, it is important to remember that there will also be some statements about whose truth the realist and antirealist will disagree. Obvious examples are given by the many familiar mathematical problems for which nobody has been able to provide either a proof or a refutation: Goldbach's Conjecture, the Twin Primes Conjecture, Riemann's Hypothesis, etc. In these cases, the antirealist will insist that we refrain from either asserting or denying statements of these problems. This is because nobody has been able to solve these problems.⁴ The realist, on the other hand, insists that the truth conditions for ' p ' obtain or fail to obtain independently of whether human beings can verify which alternative holds. Thus, he will insist that ' p ' is either true (if the conditions obtain) or false (if they fail to obtain).

So, although the realist and antirealist will frequently agree about the truth values of individual statements, they will disagree about whether the principle of bivalence holds for mathematical statements. This is the principle according to which every statement is either true or false. (Here, of course, we are restricting the principle to

⁴This assumes that the existential quantifier on the right-hand side of Schema P is understood antirealistically. If we assume that there exists, for every statement, a proof of the statement or its negation (even if nobody can produce this proof), then every statement is true or false even according to Schema P. This shows that the semantic antirealist must insist on a verificationistic theory of meaning both for the object language and the metalanguage (and any other language up the hierarchy).

mathematical statements.) Indeed, Dummett sometimes suggests that bivalence is really what's at issue in disputes between realists and antirealists.

It is difficult to avoid noticing that a common characteristic of realist doctrines is an insistence on the principle of bivalence—that every proposition, of the kind under dispute, is determinately either true or false. . . . What anti-realists were slow to grasp was that, conversely, they had in the most typical cases equally compelling grounds to *reject* bivalence and, with it, the law of excluded middle (Dummett 1991: p. 9, original emphasis).⁵

Bivalence is central to understanding Dummett's view of the conflict between classical and intuitionistic mathematics. Still, we shouldn't forget that ultimately, semantic realists and antirealists disagree about the correct theory of meaning and the meanings of individual statements. The antirealistic theory of meaning provides the compelling grounds Dummett mentions in the passage just quoted.

Note too that, when Dummett says that antirealists reject bivalence and the *tertium non datur*, he is not claiming that they can produce a counterexample to these principles. Antirealists deny that *every* statement is true or false and that the universal closure of $\phi \vee \neg\phi$ is a logical truth, but it would be inconsistent with intuitionistic logic to claim to have a counterexample to these.

So, for Dummett, mathematical intuitionism is the result of applying semantic antirealism to the statements of mathematics. On this view, when the intuitionist insists that the classical mathematician is in error, his accusation is founded on a particular theory of meaning he endorses. For Dummett, or anybody who takes this to be a correct interpretation of intuitionism and a conclusive argument against classical mathematics, there are two pressing tasks. One is to show how the results of intuitionistic *mathematics*, such as the invalidity of the *tertium non datur*, are consequences of an antirealistic theory of meaning. The other is to show that the realist's preferred theory of meaning cannot be sustained.

⁵We will discuss the connection between the semantic principle of bivalence and the law of excluded middle (or, in our preferred vocabulary, the *tertium non datur*) in the next section.

2.2 From Antirealism to Intuitionism

Before seeing how Dummett argues that his antirealistic theory of meaning entails the core principles of mathematical intuitionism, a few preliminary remarks are in order concerning the relationships among these principles. The most notorious feature of intuitionism surely is its rejection of the logical principle *tertium non datur*. This is the principle according to which the formula

$$\forall\phi(\phi \vee \neg\phi)$$

is true, with ‘ ϕ ’ ranging over all mathematical statements. Any purported foundation for intuitionism must provide some reason for this rejection. As we will see, Dummett’s antirealism does yield such a reason. Whether it is a good one will be discussed in the next chapter.

It is a mistake, however, to think that intuitionists reject the *tertium non datur* simply in order to be difficult or because they confuse Brouwer’s writings with sacred scripture and can cite chapter and verse where he rejects it. Rather, the intuitionist claims to be able to demonstrate its invalidity by presenting *mathematical* proofs, starting from *mathematical* first principles, whose conclusions are that the principle is invalid. This is at least part of what the intuitionist means when he insists that mathematics is prior to logic: using mathematics, one can obtain results about logical principles. Brouwer’s own papers feature such proofs, and later developments in intuitionistic mathematics have yielded more.

So, although intuitionists do reject the *tertium non datur*, this should not be treated as the single starting point from which the rest of their mathematics springs.⁶

⁶That people make this mistake so frequently may be a consequence of the sometimes formidable difficulty of those writings in which Brouwer develops intuitionistic mathematical theories. In his earlier intuitionistic papers, which are more philosophical and polemical (and easier to read), the rejection of the *tertium non datur* does appear as something more like an axiom. See Chapter 4 for a discussion of the philosophical arguments and Chapter 5 for a discussion of the mathematical proofs against classical logic.

This is important to keep in mind for two reasons. First, intuitionistic logic is not simply classical logic without the *tertium non datur*. For example, the law of testability,

$$\forall\phi(\neg\phi \vee \neg\neg\phi),$$

is a theorem of classical but not of intuitionistic logic. Still, one can consistently reject the *tertium non datur* while accepting the law of testability, thereby obtaining a logic that is neither classical nor intuitionistic. So, even if we are interested in providing a justification for intuitionistic *logic* only, not every argument for the invalidity of *tertium non datur* will accomplish the task.⁷

Second, we aren't interested in providing a justification for intuitionistic logic only. The most this will give us is a foundation for what we have elected to call constructive mathematics, i.e., mathematics developed by reasoning according to intuitionistic logic. This is simply a genus, however, and intuitionism is one of its species.⁸ Intuitionism's specific difference is that it accepts non-logical principles entailing results like the (distinctly intuitionistic) Continuity Theorem first proved by Brouwer (1924). (Indeed, the invalidity of *tertium non datur* is a simple consequence of this theorem.) So, a foundational program for intuitionism must provide some way to demonstrate theorems like this one.

These remarks are necessary because Dummett himself tends to place more emphasis on logic than on the non-logical branches of mathematics. His article "The Philosophical Basis of Intuitionistic Logic" contains the following description of his task there.

I am concerned only with the most fundamental feature of intuitionistic mathematics, its underlying logic, and not with the other respects (such

⁷Umezawa (1959) shows that there are uncountably many logics intermediate in strength between intuitionistic and classical logic.

⁸As we observed in our discussion of terminology in Chapter 1, there are at least two other well-developed approaches to constructive mathematics, those of Markov and Bishop. All of these accept intuitionistic logic as formalized by Heyting or a non-classical extension of it, but they disagree about various non-logical results.

as the theory of free choice sequences) in which it differs from classical mathematics. (Dummett 1978b: p. 215)

This is heterodox from the point of view of the typical intuitionist. She will insist that logic is *not* the most fundamental feature of her mathematics. Rather, intuitionism's most fundamental distinguishing feature is how it disagrees with classical mathematics. A focus on logic alone, or even primarily, will not help us to understand this disagreement.⁹

Still, in fairness to Dummett, we should grant that he ultimately wants to show that the most fundamental feature of intuitionism is semantical. Suppose that, on the basis of his antirealistic theory of meaning, he can justify intuitionistic logic on the one hand and intuitionistic algebra, analysis, topology, etc. on the other, but that these justifications are independent of each other. Then, assuming he can also provide a conclusive argument for antirealism, the intuitionist may opt to endorse Dummett's view and reject the priority of mathematics over logic. After all, a direct justification for intuitionistic logic would be no mean accomplishment.

With that said, let us turn to Dummett's attempt to provide this justification. In the remainder of this section, we will assume an antirealistic theory of meaning, of the kind described in the previous section, for mathematical statements. First, let's see how one might argue on this basis for the correctness of intuitionistic logic rather than its classical counterpart.

To begin with, recall that Schema T was originally formulated by Tarski in the process of showing how one can construct a truth definition for a suitable language.¹⁰

⁹It is interesting that in *Elements of Intuitionism*, Dummett's book-length treatment of intuitionistic mathematics, he discusses arithmetic and analysis, including the theory of free choice sequences, before his two chapters on logic. This reflects the presentation by Heyting (1966), where logic is treated only in the penultimate chapter, following chapters on arithmetic, algebra, topology, and analysis — and this despite Heyting's status as the creator of intuitionistic logic!

¹⁰We should emphasize that, although both the realist and the antirealist accept Schema T as an adequacy condition on a truth definition, neither need thereby be committed to a deflationary theory of truth. Tarski's definition is a useful technical achievement and leaves open the question of what truth "really" is. Indeed, the realist and antirealist disagree precisely about this latter question.

This definition proceeds recursively, where the recursion clauses specify the effect each logical constant has on the truth of sentences in which it appears principally. So, for example, the clause for conjunction is

‘ $\phi \wedge \psi$ ’ is true $\leftrightarrow \phi$ is true and ψ is true.

One way to understand why this works is that we want to make sure that the \wedge symbol on the left side of this biconditional names something with the same meaning as our English word ‘and’.¹¹ As competent English speakers, reflection on our own linguistic knowledge suffices to tell us what ‘and’ means. Furthermore, the recursive character of the construction allows us to assume that we already know the truth conditions for ϕ and ψ . So, the clause above allows us to understand the meaning of the object-language sentence ‘ $\phi \wedge \psi$ ’ and its component connective \wedge .

Dummett thinks that the antirealist can give a similarly recursive specification of the meanings of the logical constants.

The meaning of each constant is to be given by specifying, for any sentence in which that constant is the main operator, what is to count as a proof of that sentence, it being assumed that we already know what is to count as a proof of any of the constituents (Dummett 2000: p. 8).

The antirealist and realist both think that whatever is on the right-hand side of their preferred schemata gives the meaning of the statement asserted to be true on the left-hand side. The antirealist, however, prefers Schema P, since he takes the meaning of a statement to be given by what counts as a proof of it. Dummett therefore proposes that the antirealist use a recursive approach to give the antirealistic meanings of the logical constants in terms of the effects they have on the proof-conditions of statements in which they appear principally. And, just as in the Tarskian case our competency with English allowed us to specify what the clause for each connective should be, so

¹¹We are deliberately setting aside Tarski’s use of the notion of satisfaction, which he introduced in order to solve certain technical problems that otherwise threatened his construction. For our purposes, we can speak directly of truth (or proof) and ignore satisfaction.

too the antirealist can appeal to our competency as reasoners to justify the given specifications.

Dummett (*ibid.*, §1.2) proposes the following recursion clauses. He restricts himself to arithmetical statements (hence the reference to numbers in the quantifier clauses). His proposal can easily be generalized to any countable domain, although if we wish to consider uncountable ones like the real numbers, we have to face the fact that not all of the elements will have names. Also, we have changed his $\&$ to a \wedge and his Roman letters to Greek ones in keeping with our preferred notation.

\wedge : A proof of $\phi \wedge \psi$ is anything that is a proof of ϕ and of ψ .

\vee : A proof of $\phi \vee \psi$ is anything that is a proof either of ϕ or of ψ .

\exists : A proof of $\exists x A(x)$ is anything that is a proof, for some n , of the statement $A(\bar{n})$.

\forall : A proof of $\forall x A(x)$ is a construction of which we can recognize that, when applied to any number n , it yields a proof of $A(\bar{n})$.

In the quantifier clauses, ' \bar{n} ' is the numeral that names the number n .

Dummett does not give such succinct clauses for \neg and \rightarrow . Here is his initial proposal for the former.

A proof of $\neg\phi$ is usually characterized as a construction of which we can recognize that, applied to any proof of ϕ , it will yield a proof of a contradiction (*ibid.*, p. 8).

This, however, threatens to be circular. If we understand a contradiction to be a statement of the form $\phi \wedge \neg\phi$, then the \neg symbol appears in the explanation of its own meaning. There are a few ways to avoid this. One is to adopt \perp as a new constant and add a clause insisting that there is no proof of \perp . This is the path proposed by Troelstra and van Dalen (1988: p. 9).

Dummett suggests that we instead pick “some one absurd statement, say $0 = 1$.” This will not work in general, however, because ‘ $0 = 1$ ’ is not a logical contradiction: by tinkering with the Peano axioms, one can build consistent (albeit nonstandard) theories of arithmetic in which ‘ $0 = 1$ ’ is true. For this reason, we should treat \neg as governed by this clause:

\neg : A proof of $\neg\phi$ is a construction of which we can recognize that, applied to any proof of ϕ , it will yield a proof of \perp .

We will also add a clause for \perp to the mix.

\perp : There is no proof of \perp .

Dummett also has a good deal to say about \rightarrow , although he never gives a succinct description of what counts as a proof of a conditional. All he offers is a hint when he says that “an intuitionistic proof of $\phi \rightarrow \psi$ is an operation upon proofs yielding proofs” (*ibid.*, p. 10). Dummett’s idea is that a proof of the conditional is a function taking proofs of ϕ as inputs and yielding proofs of ψ as outputs. This is similar to Heyting’s (1934: p. 14) description of the proof conditions for a conditional: $\phi \rightarrow \psi$ means that “the solution of B results from the solution of A .”¹² Dummett’s contribution is to insist that the proof in question be a function or operation taking proofs into proofs. Thus, we can attribute the following proposal to Dummett.

\rightarrow : A proof of $\phi \rightarrow \psi$ is an operation of which we can recognize that, applied to any proof of ϕ , it will yield a proof of ψ .

A little reflection reveals that these clauses do seem to capture some of our standard mathematical proof techniques as represented in a Gentzen-style natural deduction system. For example, the \neg clause provides an antirealistic description of what we often think of as proofs by *reductio ad absurdum*: we begin by assuming ϕ (which,

¹²The translation from the German is my own, and the variables have been changed. The original reads “die Lösung von b auf die Lösung von a zurückzuführen.”

for the antirealist, is equivalent to assuming that we have a proof of ϕ) and show that, from this assumption, we can produce a proof of a contradiction, i.e., of \perp . Since no contradiction has a proof, we have thereby shown that ϕ cannot have a proof either, so we infer $\neg\phi$.

If Dummett is right that antirealism justifies assigning each logical constant the meaning specified by its recursion clause, then he has made an important step toward showing that his theory of meaning justifies intuitionistic, as opposed to classical logic. To see why, notice that his clauses correspond closely to the BHK interpretation of the logical constants.¹³ According to Heyting (1966: p. 97), the BHK interpretation is intended “to fix, as firmly as possible, the meaning of the logical connectives.” Troelstra and van Dalen (1988: p. 9) say that it serves to “explain the use of the logical operations in a constructive context.” So, the authors just mentioned clearly think of the BHK in the same spirit as Dummett does when he presents his similar interpretation. Of these authors, however, only Dummett attempts to argue that these clauses provide the *correct* semantics for the logical constants. Other authors tend to rest content by saying that the BHK clauses give the meanings of the constants as they are used by intuitionists without explaining why anybody should accept those meanings as correct.¹⁴

Still, once we accept the meanings given by the BHK semantics, we should be able to use them to show that the theorems of intuitionistic logic are valid and to motivate the rejection of certain laws of classical logic. Indeed, as we will see in Chapter 3, if Dummett’s project is to succeed, he needs something like soundness and completeness proof for intuitionistic logic relative to the BHK interpretation. Without this, we cannot be sure that the meanings he proposes for the logical constants will license all and only the inferences that are valid according to intuitionistic logic. We will

¹³See Chapter 1, §2.1.

¹⁴Heyting will occasionally make references to “mental mathematical constructions” or “intuitive clarity” (1966: pp. 1, 97), but these are hardly arguments; they amount to saying “See!” in the hope that this can constitute a proof.

examine the prospects for such a proof in the next chapter. For now, we will show only that, if the antirealist's argument succeeds, the *tertium non datur* cannot now be asserted with justification.

The *tertium non datur* is the principle according to which

$$\forall\phi(\phi \vee \neg\phi),$$

where ϕ ranges over mathematical statements or, more precisely, truth values. By Schema P, to say that the *tertium non datur* is true is to say that there exists a humanly recognizable proof of it, i.e., that

$$\exists\pi(\pi \vdash \forall\phi(\phi \vee \neg\phi)).$$

By the \exists clause, this means that there is some particular proof π_0 such that

$$\pi_0 \vdash \forall\phi(\phi \vee \neg\phi).$$

In other words, π_0 is a recognizable proof of the *tertium non datur*.

What would a proof like π_0 demonstrate? It is a proof of a universally quantified statement. So, applying the antirealistic meaning of \forall , we can say that π_0 is a construction of which we can recognize that, given any mathematical statement ϕ , it yields a proof of $\phi \vee \neg\phi$. Applying the \vee clause, the latter proof would have to be a proof of ϕ or a proof of $\neg\phi$. By the \neg clause, a proof of $\neg\phi$ is a method that converts any proof of ϕ into a proof of \perp .

Putting this all together, we see that π_0 would be a statement operator of which we can recognize that, for any mathematical statement, it will either prove the statement or reduce it to absurdity. In other words, π_0 would be a function that can be recognized as providing a proof or refutation of any mathematical statement. So, if we

accept both the BHK interpretation and *tertium non datur*, we end up committed to something like David Hilbert's Axiom of Solvability, according to which every mathematical problem has a solution that we can obtain.¹⁵ Despite Hilbert's optimism, there is currently no universal technique for solving mathematical problems and none is immediately forthcoming. Thus, we cannot presently accept both the *tertium non datur* and Dummett's version of the BHK interpretation as the correct theory of meaning for the logical constants. Since Dummett's antirealist is committed to the latter, he cannot endorse the former. Therefore, Dummett can show, on antirealist grounds, that the validity of the *tertium non datur* is not yet established, thereby satisfying a necessary condition for an intuitionistic foundation for mathematics.¹⁶

As we said, the next chapter will look more closely at the prospects for something like a soundness and completeness proof with respect to the semantics that Dummett proposes. All that we have established so far is that antirealism requires the rejection of classical logic, not that it forces us to accept intuitionistic logic. For now, however, we will turn to Dummett's argument for an antirealist semantics for mathematical statements.

2.3 Arguing for Antirealism

Dummett's argument for antirealism begins with reflection on two aspects of our linguistic activity. First, he considers how we come to learn the meanings of statements and their component words. Second, he asks how somebody might exhibit or manifest her understanding of some part of a language. The relationship between these two

¹⁵See Chapter 4 for more on Hilbert's Axiom and his contribution to the early development of intuitionistic mathematics.

¹⁶This argument does not show that the *tertium non datur* is invalid. That would require a proof that the existence of a recognizably universal problem-solving method is absurd. Whether this can be done depends on what counts as a legitimate method. As Tennant (1997: §7.5) shows, if we understand only recursive methods to be legitimate, then the *tertium non datur* is provably invalid. It would take further work to show that this result holds for classes of methods that are wider in extent.

items can be obscure. Burgess (1984) understands Dummett as offering two distinct arguments, one turning on acquisition and the other on manifestation.

Dummett's philosophy is not always that tidy, however, and we should be wary of thinking that the two arguments are entirely independent of each other. Unless we wish to accept an innate-knowledge theory à la Plato or Chomsky, a person's ability to manifest understanding of a statement must depend in some way on her ability to acquire that understanding. One cannot exhibit knowledge one has never learned. Furthermore, Dummett does not think that it makes sense to talk about acquired linguistic knowledge that cannot be exhibited.

Implicit knowledge cannot, however, meaningfully be ascribed to someone unless it is possible to say in what the manifestation of that knowledge consists: there must be an observable difference between the behaviour or capacities of someone who is said to have that knowledge and someone who is said to lack it. (Dummett 1978b: p. 217)

Thus, *pace* Burgess, we should treat the manifestation considerations and the acquisition considerations as two aspects of one larger argument to establish semantic antirealism.

In any case, Dummett begins by taking inspiration from a familiar Wittgensteinian slogan, "meaning is use." Here is how Dummett understands it.¹⁷

The meaning of a mathematical statement is determined by its *use*. The meaning of such a statement cannot be, or contain as an ingredient, anything which is not manifest in the use made of it, lying solely in the mind of the individual who apprehends that meaning: if two individuals agree completely about the use to be made of the statement, then they agree about its meaning. (*ibid.*, p. 216, original emphasis)

Here, the manifestation requirement is made explicit. Somebody who understands the meaning of a statement ϕ must be able to exhibit that knowledge publicly. The manifestation can take multiple forms. One is via an explanation of the meaning using

¹⁷We will not address the complicated question of whether this interprets Wittgenstein correctly.

another bit of language. According to Dummett, however, this cannot be the only way to manifest understanding.

But to suppose that, in general, a knowledge of meaning consisted in verbalisable knowledge would involve an infinite regress: if a grasp of the meaning of an expression consisted, in general, in the ability to *state* its meaning, then it would be impossible for anyone to learn a language who was not already equipped with a fairly extensive language. . . . Hence it follows, once more, that a grasp of the meaning of a mathematical statement must, in general, consist of a capacity to use that statement in a certain way, or to respond in a certain way to its use by others. (*ibid.*, p. 217, original emphasis)

Setting aside for the moment how one learns language, Dummett here seems to be ignoring the possibility of a purportedly vicious circle rather than an infinite regress. If I explain the meaning of the numeral ‘2’ to somebody by telling her that it stands for the number that comes after 1 in the natural number sequence, then it does the other person no good if I explain ‘1’ as representing the number preceding 2 in the same sequence. I have explained ‘2’ in terms of itself, and it is hardly clear that I thereby know what the numeral means. Here, the problem is circularity, not infinite regression.

Dummett’s argument, whether cast in terms of a circle or a regress, relies on important assumptions about the structure of a language. If Dummett is right, a person must be able to manifest her understanding of at least some linguistic items via nonverbal means.¹⁸ That is, she must be able to show via nonlinguistic behavior that she understands these basic sentences and their constituents. If, when told to fetch two books, she brings back three, this casts doubt on her claim to know what ‘two’ means, but if she brings back two, this provides evidence that she does understand the numeric expression.

¹⁸Indeed, the argument is really a familiar epistemological argument with a linguistic twist. A canonical argument for foundationalism in epistemology highlights the need for some foundational beliefs in order to avoid either a vicious circle or an infinite regress in the structure of our knowledge. The special linguistic items understanding of which can be exhibited nonverbally are the analogue of the foundationalist’s basic beliefs.

So much for manifestation. What about acquisition, i.e., learning a language? Here is what Dummett has to say.

Another approach is via the idea of learning mathematics. When we learn a mathematical notation, or mathematical expressions, or, more generally, the language of a mathematical theory, what we learn to do is to make use of the statements of that language: we learn when they may be established by computation, and how to carry out the relevant computations, we learn from what they may be inferred and what may be inferred from them, that is, what rôle they play in mathematical proofs and how they can be applied in extra-mathematical contexts, and perhaps we learn also what plausible arguments can render them probable. (*ibid.*)

He continues by pointing out that, if meaning consisted in more than use, then somebody could correctly use mathematical language (or some portion of it), and yet we would have no guarantee that the person knew the language. This would, according to Dummett, “make meaning ineffable, that is, in principle incommunicable.” We would have no guarantee that anybody understood us when we spoke.¹⁹ If we take as premises that we successfully communicate and that we know we do this, meaning must therefore consist in use alone.

In the previous quotation but one, Dummett does seem to suggest that he is offering two separate arguments, one built around considerations of manifestation and the other around considerations of acquisition. (Witness his phrase ‘another approach’ in the quoted passage.) We can, however, identify a common core to both, and when we do, it becomes clear that these are really two perspectives on one basic argument. The common core is the idea he draws from Wittgenstein, namely, that there is a close connection between the meaning of a linguistic item and the way in which it is used. We see this in the manifestation version of the argument when Dummett insists that, since verbal explanations of meaning can’t go all the way down, linguistic understanding must sometimes be exhibited by correct non-linguistic behavior. It also

¹⁹To keep track of the factions, we should note that Brouwer accepted—even embraced—what Dummett calls the ineffability of meaning. It plays an important role in his criticisms of both logicism and Hilbert’s formalism.

appears in the acquisition version as follows.

These things [how to calculate, infer, etc.] are all that we are shown when we are learning the meanings of the expressions of the language of the mathematical theory in question, because they are all that we can be shown: and, likewise, our proficiency in making the correct use of the statements and expressions of the language is all that others have from which to judge whether we have acquired a grasp of their meanings. *Hence it can only be in the capacity to make a correct use of the statements of the language that a grasp of their meanings, and those of the symbols and expressions which they contain, can consist.* (*ibid.*, p. 217, emphasis added)

This is one major part of Dummett's argument for antirealism with respect to mathematical language. If he is correct so far, he has established three theses.

1. When one learns the language of mathematics, all one learns to do is to use its sentences and their components in correct and appropriate ways.
2. To understand a mathematical sentence or word, one needs only to be able to use the sentence or word correctly and to respond to correct uses by others.
3. The meaning of a mathematical statement consists in the use we make of it.

On Dummett's view, there is a very close connection between meaning (a feature of statements and words), understanding (a feature of human beings), and learning (an activity performed by human beings). Given this, it is hard to see how a Dummettian could reject any one of these three theses without rejecting all three.

One might worry that theses 1 and 3 are mutually inconsistent. The first refers to "correct and appropriate" uses of linguistic expressions. The third, however, calls into question whether there can ever be such a thing as incorrect use. After all, correct use must be constrained by meaning, but if meaning consists in nothing but use, this is no constraint at all. Presumably, then, Dummett is committed to accepting that there are general patterns of use in a linguistic community; while these determine meaning,

individual utterances or inscriptions that fail to conform to the general patterns can still be incorrect.

Ultimately, antirealism is a theory of meaning for Dummett. So, thesis 3 is the most relevant to understanding his position on mathematical intuitionism. Notice, furthermore, that we have yet to arrive at the point with which we started the previous section. The arguments so far, if successful, have shown that the meaning of a statement consists in its use, but we have said little about how we use mathematical language. Indeed, in this section we have frequently dropped the qualifier ‘mathematical’, since nothing so far has had anything to do with *mathematics*. This helps to motivate Dummett’s idea that most or all disputes about realism might be framed and settled within the philosophy of language. If the argument discussed in this section is a good one, it should apply to any part of any language.

So, the next step in Dummett’s argument is to consider how we use statements, particularly those of mathematics. In general, we have no reason to expect that ethical statements, for example, will be used in the same way as mathematical statements. This is why Dummett’s argument, if successful, will not necessarily lead to antirealism across the board: the uses of some class of statements may be such that a realistic theory of meaning for that class is appropriate.

For mathematical statements, however, this is not the case. Or so Dummett says.

What we actually learn to do, when we learn some part of the language of mathematics, is to recognise, for each statement, what counts as establishing that statement as true or false. . . . We must, therefore, replace the notion of truth, as the central notion of the theory of meaning for mathematical statements, by the notion of *proof*: a grasp of the meaning of a statement consists in a capacity to recognise a proof of it when one is presented to us, and a grasp of the meaning of any expression smaller than a sentence must consist in a knowledge of the way in which its presence in a sentence contributes to determining what is to count as a proof of that sentence. (*ibid.*, pp. 225-226, original emphasis)

Two features of this quote are important. First, Dummett is committing himself to

a particular view about the use we make of mathematical statements. By doing so, and given the argument we examined earlier, he is staking a claim to a particular semantic theory for mathematics, namely, one in which the meaning of a statement is given by specifying what counts as a correct proof of the statement.

The second important feature is Dummett's commitment to the statement as the fundamental unit of meaning. It is what allows him to call for the revision in logic required by intuitionism. As he acknowledges (*ibid.*, p. 220), his argument requires that holism about mathematical language is false. Otherwise, such revision would require moving to an entirely new language. Some authors have thought that intuitionists speak an entirely different language that may or may not be as acceptable as the language of classical mathematics (Carnap 1934, Hellman 1989, Quine 1986), but Dummett rejects this. He thinks that intuitionists and classical mathematicians can talk to each other; their languages overlap enough to provide some common ground on which the disputes can be settled.

In addition to treating sentences as semantic fundamentals, Dummett tells us how we can determine the meanings of individual words. These emerge from the effects those words have on the proof conditions of the sentences in which they appear. This holds true, in particular, for the logical constants.²⁰ Thus, to give the meaning of the connective 'and', one specifies how its presence in a sentence like ' ϕ and ψ ' affects what would count as a proof of the sentence. We might say something like

When 'and' appears between two sentences ϕ and ψ , a proof of the whole is anything that proves each of the two component sentences.

This should look familiar. Except for stylistic variants, it is precisely the clause we saw governing the connective \wedge in the previous section. Presumably, were we to give similar explanations of the other connectives, each would resemble its corresponding

²⁰These need not be individual words—consider 'if ... then ...' in English. Still their meanings are determined in the same way regardless of the surface structure of the expressions.

clause from earlier. At least, Dummett suggests that this is the case (2000: §1.2). As we observed, these clauses were introduced by Heyting in an attempt to provide a semantics that would capture formally the reasoning used in intuitionistic mathematics. Hence, if Dummett's argument succeeds, he has shown that his premises concerning use, manifestation, and acquisition entail that our mathematical reasoning should take place according to the canons of intuitionistic logic.

2.4 Summary

This, then, is the Dummettian argument for semantic antirealism in mathematics. If it succeeds, then the intuitionistic mathematician can muster it in order to show that his preferred mathematics is not just interesting or useful, but *correct*. That is, it has as a consequence that classical mathematics is getting things wrong; its practitioners use mathematical statements in ways that go beyond any understanding that they could have of these statements. Intuitionistic mathematicians, by contrast, use their statements and make their inferences in ways that cohere with the actual meanings borne by the language. If this is correct, then the intuitionist's call for mathematical revolution is a principled one. We turn now to an evaluation of Dummett's argument.

Chapter 3

Against Semantic Antirealism as a Foundation for Intuitionism

3.1 Criteria for an Adequate Foundation

We are now in a position to examine whether Dummett's semantic antirealism can serve as a foundation for intuitionistic mathematics. As the reader might expect, our conclusion will be that it cannot perform this task to satisfaction. To establish this claim, we first need to say what tasks a foundation for intuitionism must serve.

At its root, intuitionism is mathematics. This is worth emphasizing precisely because it is frequently overlooked. Semantic antirealism, Brouwerian idealism, and Husserlian phenomenology have all been associated with intuitionism, but they are no more essential to it than Hilbertian formalism is to classical arithmetic or Cartesian dualism is to analytic geometry.

This is not to say that intuitionists should dispense altogether with attempts to develop a philosophical foundation for their mathematics. Intuitionistic mathematics contains plenty of theorems and some of these are contrary to theorems of classical mathematics. Faced with this, the intuitionist is obliged to be able to explain why she

accepts the intuitionistic results rather than their classical contraries. (The classical mathematician has a similar obligation with respect to his theorems, of course.) Even those who endorse mathematical pluralism thereby assume a philosophical position about the nature of mathematical disagreement. According to the pluralist, there is no genuine conflict between classical and constructive mathematics. Either the two approaches deal with different objects (classical numbers, sets, functions, etc. and their constructive counterparts, respectively) or mathematical statements are not really about anything and so there is no room for real disagreement. (On the latter view, there may be legitimate disputes about other so-called theoretical virtues like simplicity and pragmatic utility.) A full treatment of pluralism would require a separate essay. For now, we note that, on the present author's view, a mathematical statement ϕ means the same thing regardless of the theoretical proclivities of those who assert it. Furthermore, mathematical statements are true when they correctly describe mathematical objects, which exist. Thus, when ϕ is a theorem of one mathematics and $\neg\phi$ is a theorem of another, the requirement of consistency prohibits us from accepting both as true.¹

So, a good foundation for intuitionism should provide arguments that can be given in favor of the core theses of intuitionism. This is a tall order because these core theses do not arrive prepackaged on one's philosophical doorstep; there is disagreement within intuitionistic and constructivist circles concerning such principles as Church's Thesis, Bar Induction, Markov's Principle, and the Creative Subject. Still, we can isolate some *results* that make intuitionism what it is. Intuitionists reject the *tertium non datur*, for example, and even claim to be able to demonstrate mathematically that it is invalid. (In the latter respect, they distinguish themselves from, e.g., Bishop's school of constructivists.) They also maintain that every total function from the real numbers into the reals is continuous and uniformly so. (Other well-established con-

¹See (Hellman and Bell 2006) and (Davies 2005) for sympathetic discussions of pluralism.

structive approaches to mathematics deny *uniform* continuity.) There are yet other core theorems of intuitionistic mathematics. A foundation for intuitionism should provide the material to argue for as many of these results as possible.²

Still, the intuitionist cannot hope to convince her most dogmatic opponents, and a foundation for intuitionism should not be expected to do so. So too, we should not demand that our philosophical foundation justify anything that any intuitionist has ever asserted. Nor should we demand that it convince everybody that they should give up classical mathematics. Arguments for intuitionism inevitably must show that, e.g., the *tertium non datur* is invalid. The proliferation of non-classical logics over the past century has weakened the idea that logic is beyond controversy. Still, bad habits are hard to break, and advocates of classical mathematics can always treat an argument against classical logic as a *reductio ad absurdum* of one of the argument's premises.

3.2 Semantic Antirealism and Intuitionistic Logic

With these remarks in mind, we can evaluate Dummett's semantic antirealism as a foundation for intuitionistic mathematics. First, we will examine more closely the connection between semantic antirealism and intuitionistic logic. In the previous chapter, we saw that one corollary of the antirealist's position is that the BHK interpretation provides the correct theory of meaning for the logical constants in virtue of characterizing, for each constant, what counts as a proof of a statement in which that constant appears as the main connective or quantifier. So, in order for antirealism to play its foundational role, the BHK interpretation must be an adequate semantic theory for intuitionistic logic.

For reasons that we described in Chapter 1, intuitionistic logic is best understood

²It may turn out that the best available foundation for intuitionism does not secure *all* of these results. Imagine, for example, that ϕ is a very plausible intuitionistic axiom that entails the invalidity of classical logic but only the non-uniform continuity of all total real-valued functions. In light of ϕ 's intrinsic plausibility and its other consequences, the intuitionist might opt to give up uniform continuity in favor of something slightly weaker but still contrary to classical analysis.

in terms of an axiomatic or natural-deductive system that characterizes valid inferences. Since the BHK interpretation is a semantic theory, the antirealist should seek to produce soundness and completeness results to show that her theory of meaning justifies all and only the inferences that are valid according to intuitionistic logic itself. This is the approach that Dummett suggests in some places, e.g., in his article “The Justification of Deduction” (Dummett 1975a). According to him, one task of a formal semantics for a language is to codify our deductive linguistic practices.

Prominent among the practices which make up our use of language are those of deductive inference and deductive argument. Any satisfactory theory of meaning must, therefore, be able to relate these practices to the model of meaning which it employs: just this is what is done by a semantics for a logical theory. (Dummett 1978b: p. 310)

Dummett goes on to argue that, if a semantics fails with respect to either soundness or completeness, this reveals an underlying flaw in the theory of meaning on which the formal semantics is based.

In so far as the logical theory embodies our actual practice, that is, has primitive rules of inference which we in practice treat as valid, a theory of meaning, if it is to provide a model of our practice, must bring out those rules of inference as semantically valid, and should not bring out as semantically valid any rules which we cannot be brought to accept. . . . A semantics which can be shown not to justify a form of inference which is in standard use in ordinary discourse, or to justify one which we should unhesitatingly reject, is, by that fact, subject to criticism. (*ibid.*)

By itself, this quote does not show that Dummett is committed to soundness or completeness results for intuitionistic logic relative to the BHK semantics. It does, however, indicate that his position is precarious without such results. As we will see, there are a great many logical systems similar to but distinct from intuitionistic logic. To be sure that the set of valid formulae and inferences according to the BHK semantics is not the set corresponding to any one of these other logics, soundness and completeness proofs are necessary.

That we might be able to obtain these proofs looks like wishful thinking.. For example, the completeness of full first-order intuitionistic logic (without identity) with respect to Beth trees entails Markov's Principle, which intuitionists generally reject.³ This is an obstacle for anybody who insists on a complete intuitionistic logic adequate for intuitionistic mathematics. Fortunately, completeness is not required for intuitionistic mathematics *per se* any more than it is for classical mathematics done using second-order logic. The semantic antirealist does require it, however, if her position is meant to serve as a successful foundation for intuitionism, and McCarty (2006) shows that the antirealist's recognition condition together with the soundness and completeness of intuitionistic logic entail that there are no counterexamples to the *tertium non datur*.

One can show that first-order intuitionistic logic is complete with respect to Beth trees and their close cousin, Kripke semantics, but the existing proofs of this require one either to reason classically in the metatheory or to allow \perp to be forced at a node of the model.⁴ The latter option is not acceptable to the antirealist because it requires treating \perp as something that can have a proof, contrary to the BHK clause governing it. The former is interesting as a technical result, but the committed antirealist cannot accept a completeness proof that requires classical reasoning. If the Dummettian argument for antirealism is cogent, then it applies to all mathematical language. Since any metatheoretical proof of completeness is perfectly mathematical, it is subject to the proof constraints given by semantic antirealism. If these are captured by intuitionistic logic, as the antirealist purports, then no proof that essentially features intuitionistically invalid inferences can be valid.

Indeed, the proof just mentioned cannot even be used to challenge the classical mathematician on antirealistic grounds. Such an argument would proceed as follows: the classical completeness proof would show that, on his own grounds, the classical

³Kreisel (1962) reports that this was first shown by Gödel. McCarty (1994) gives a simpler proof.

⁴For a proof of the latter, cf. Veldman (1976) and de Swart (1977).

mathematician cannot complain that intuitionistic logic is unstable because it lacks a complete semantics. The general argument for semantic antirealism would show that the classical mathematician must revise his own canons of reasoning, and the completeness proof (together with a soundness proof) would show that intuitionistic logic is an acceptable alternative.

There are problems with this argument, however. For one thing, the argument for antirealism undermines the completeness proof, since the former (if successful) shows that the classical reasoning required for the latter is not valid. More to the point, though, the antirealist's argument emphasizes things like the learnable content of a language and the way that the language is used in practice; these are what give linguistic items their meanings. Beth's and Kripke's semantics, by contrast, are technical frameworks for the metatheoretic study of intuitionistic logic; they neither represent the actual use of the logical constants nor purport to do so.⁵ Thus, the antirealist's argument does not show that the intuitionistic completeness theorems are of interest except as purely technical results; in particular, they cannot play the role of justifying intuitionistic logic as a canon for deduction in the way described in "The Justification of Deduction."

Finally, McCarty (2008) gives a survey of completeness and incompleteness results and demonstrates the incompleteness of intuitionistic *sentential* logic with respect to intuitionistic metatheories. Putting all of this together, it is clear that we should not expect antirealistically acceptable completeness proofs for full first-order intuitionistic logic. This might sound scandalous to philosophers and mathematicians raised on a diet of classical first-order logic and its Tarskian models. In fact, completeness is a fairly rare phenomenon in logical and mathematical space; mundane theories like arithmetic and classical second-order logic are incomplete. It also entails the existence

⁵Dummett (2000: p. 287) concedes this in his discussion of Beth trees: "... the Beth trees prove to be a legitimate tool for the study of intuitionistic logic, although not for the construction of an actual intuitionistic semantics."

of unpalatable objects like nonstandard models of arithmetic.

In addition, incompleteness captures the spirit the early development of intuitionism. Hao Wang reports that Brouwer's attitude in 1961 towards Gödel's incompleteness results was hardly one of surprise.

Of G's incompleteness results, [Brouwer] expressed astonishment that so much had been made of them, saying that the conclusions had been evident to him for a long time before 1931. (Wang 1987: p. 57)

Meanwhile, Heyting acknowledges the limitation of his own formalization of intuitionistic reasoning.

It must be remembered that no formal system can be proved to represent adequately an intuitionistic theory. There always remains a residue of ambiguity in the interpretation of the signs, and it can never be proved with mathematical rigour that the system of axioms really embraces every valid method of proof. (Heyting 1966: p. 102)

This echoes our earlier observation that it is hard to say what counts as intuitionistic logic. We can stipulate that it is the logic given by, e.g., the set of formulae valid according to Kripke semantics or the set of formal consequences of Heyting's axiomatic system, but intuitionists are happy to acknowledge that, like any other field of inquiry, logic makes progress over time. It is open to us to discover new logical truths that are not syntactic consequences of previously accepted axioms and rules of inference. Similarly, we can come to discover that what we took to be a logical law is not universally valid; in a course of lectures delivered at Cambridge in 1951, Brouwer rightly regarded this as the situation regarding the *tertium non datur*.

The belief in the universal validity of the principle of the excluded third in mathematics is considered by the intuitionists as a phenomenon of the history of civilization of the same kind as the former belief in the rationality of pi, or in the rotation of the firmament about the earth. (van Dalen 1981: p. 7)

Indeed, even antirealists have challenged the validity of certain laws of intuitionistic logic. In his *Anti-Realism and Logic*, Tennant challenges the rule *ex falso quodlibet*,

$$\forall\phi(\perp \rightarrow \phi).$$

This appears as Axiom X in Heyting’s list of axioms for intuitionistic logic and as one of the rules in Troelstra and van Dalen’s natural deduction system. It is also valid according to Kripke semantics. Nevertheless, Tennant (1987: p. 188) argues that *ex falso quodlibet* cannot be justified by an appeal to what we learn when we learn how to reason. Since the antirealist takes this “learnable content” (to use Tennant’s term) to give the meanings of the logical constants, she should reject *ex falso quodlibet*.⁶

If we shift our focus from learnable content to the BHK interpretation, the principle also faces problems. How would a BHK-based proof of *ex falso quodlibet* go? Given the BHK-meaning of the universal quantifier, such a proof would need to provide a method that transforms a proof of \perp into a proof of ϕ , where ϕ is arbitrary. It appears that any method μ will do the job; since there are no proofs of \perp , if β is a proof of \perp , then $\mu(\beta)$ is a proof of ϕ .

Of course, this argument assumes the validity of *ex falso quodlibet*. Without it, we couldn’t maintain that the conditional at the end of the previous paragraph is necessarily true in virtue of having a necessarily false antecedent. Hence, the argument is circular. This doesn’t mean that we should reject *ex falso quodlibet* any more than the stubborn tortoise of (Carroll 1895) shows that we should reject *modus ponens*.

What the circularity does show, in this case, is that semantic considerations alone may not give us a good reason to accept *ex falso quodlibet*. Those of us who accept it aren’t always motivated by reflection on the meanings of \perp and \rightarrow . Surely part of the reason is pragmatic: the principle facilitates the development of mathematics

⁶Tennant actually focuses on inferences from premises to conclusions rather than on conditionals. Within standard intuitionistic logic, these are interchangeable, although they come apart in a relevance logic such as that endorsed by Tennant.

and allows us to construct simple proofs of results that we find intuitively plausible (e.g., that the empty set is a subset of every set). Perhaps *ex falso quodlibet* also has some intuitive plausibility of its own, although certainly not to the degree that *modus ponens* does.⁷

Neither Tennant’s argument concerning learnable content nor the BHK-based argument are mathematical demonstrations that *ex falso quodlibet* is invalid. Thus, they do not show that intuitionists ought to reject the principle. (By contrast, intuitionists should and do reject the *tertium non datur* on the basis of mathematical demonstrations of its invalidity.) What these considerations do show is that semantic antirealism and intuitionistic mathematics may not be a perfect match. If antirealism and the BHK interpretation cannot motivate *ex falso quodlibet*, then the logic that they yield is weaker than intuitionistic logic.

It should not come as a surprise that antirealistic semantics may not pick out intuitionistic logic even if it avoids commitment to classical logic. As (Cook: pp. 16ff.) points out, there are 2^{\aleph_0} logical systems whose strength lies strictly in between classical and intuitionistic logic. Of course, the antirealist cannot examine each of these logics one by one; even Sisyphus would quickly abandon this absurd project. Instead, the antirealist has to find a way to cut out large swaths in one pass. The problem the antirealist faces is that it is not clear what criteria are available for this purpose. Intuitionistic logic has plenty of features that distinguish it from one logic or another. For example, it has the disjunction property: whenever $\phi \vee \psi$ is a theorem

⁷Heyting concedes this point in one of his presentations of axioms for intuitionistic logic. Concerning his tenth axiom, which is a statement of *ex falso quodlibet*, he concedes that “Axiom X may not be intuitively clear.” (Heyting 1966: p. 102) He then proceeds to give an argument for it. The argument is obscure, but it seems to be a version of the circular one presented above.

Griss was motivated to initiate his program of negationless mathematics because he worried that the BHK interpretation of negation required us to intuit impossible objects like square circles or proofs of false statements. The chief problem facing negationless mathematics is likewise pragmatic. As Brouwer showed in his (1948), the most natural negationless translations of certain ordering relations on the reals turn out not to be equivalent to the versions containing negations. Thus, it is not at all obvious that the negationless program can be carried out throughout all of mathematics; the burden of proof is squarely on Griss and his allies.

of intuitionistic logic, either ϕ or ψ is a theorem as well. This fits nicely with the \vee clause of the BHK interpretation, but it does not isolate intuitionistic logic. Indeed, there are still 2^{\aleph_0} many intermediate logics with the disjunction property.

Nor can the antirealist appeal to the relative weakness of intuitionistic logic. Yes, it is the weakest logic between intuitionistic and classical logic (inclusive, of course), but there are yet weaker logics available, e.g., minimal logic or Tennant's intuitionistic relevance logic. Even if weakness is an antirealistic virtue (and there is no obvious reason why it should be), it won't isolate intuitionistic logic.

The existence of these alternative logics that are nonclassical but, so to speak, in the vicinity of intuitionistic logic highlights the importance of soundness and completeness proofs for the antirealist. Were he armed with such proofs, he could insist that his semantic theory does yield intuitionistic logic as *the* correct canon of reasoning within mathematics. Without them, there is no good reason to think that semantic antirealism can justify the logic that intuitionists actually use in their mathematical reasoning.

There is another reason why the BHK interpretation is not sufficient as a foundation for intuitionistic logic. The BHK clauses are supposed to give a recursive characterization of the proof conditions for the statements of a language, but they do not tell us what counts in general as a proof of an atomic statement. It is not enough to know that a proof of a conjunction is given by presenting a proof of each conjunct; we must also know what counts as a proof of the conjuncts. When these subformulae are logically complex, we can refer to the relevant BHK clauses, but this process cannot continue indefinitely. Eventually, we will arrive at atomic formulae, and the BHK interpretation tells us nothing about what counts as a proof of these.

In general, the proof conditions for an atomic formula will depend on the subject matter of the language. For atomic formulae of arithmetic, proofs are usually given by calculations, while for those of set theory we use other mathematical methods. Outside

of mathematics, proofs might be given in terms of empirical observations or even the testimony of experts. It is hard to give a universal and informative characterization of what counts as a proof of an atomic formula; the most we can say is, e.g., that a proof of ‘ Fa ’ is a proof that the object a has the property F .

Still, if one insists on the BHK interpretation as the correct semantic theory for mathematical statements, one is obliged to say something about atomic formulae. Attempts have been made at developing a formal theory of constructions by e.g., Kleene (1950: §82) and Kreisel (1967). These point in the right direction. Notably, however, attempts at theories of constructions are built on more fundamental mathematical theories, e.g., arithmetic in the case of Kleene’s realizability interpretation of intuitionistic logic. This is a problem for the antirealist, who uses his semantic theory to argue for the use of intuitionistic logic throughout mathematics. A formal theory of constructions will presuppose some mathematical theory or other, together with the logic used to reason in that theory, and it is open for the classical mathematician to insist that the metatheory be classical, so that the *tertium non datur* and other intuitionistically invalid logical principles turn out to be valid on the BHK semantics with certain background assumptions.

Besides, this assumes that we have an adequate formal theory of constructions. Those proposed by Kleene and Kreisel would not conform well to the rest of the antirealist’s picture. Kreisel’s theory turned out to be inconsistent; Goodman (1970) Goodman that the inconsistency could be removed through something akin to a type heirarchy for constructions, but then we are once again moving away from the actual learnable content of mathematical statements. Furthermore, such a stratification introduces complications into the informal characterization of the Heyting semantics. For example, we can no longer have a single clause for conditionals stating that a proof of $A \rightarrow B$ is a construction that transforms *any* proof of A into a proof of B ; this kind of unrestricted universal quantification cannot be accomodated in the

formal theory.

The basic notion in Kleene's theory is not that of a proof but that of a computation. When we learn to prove mathematical theorems, however, we do not learn computation methods involving Gödel codes; we learn about proofs in the ordinary sense. Thus, Dummett's view that we learn the meanings of statements by learning how they're used would preclude taking Kleene's realizability as giving a semantics for mathematical statements. (Besides, as Rose (1953) shows, realizability is not itself adequate even for intuitionistic propositional logic.)

3.3 Semantic Antirealism and Classical Mathematics

According to semantic antirealists, the principal problem facing classical mathematics is not that some of its theorems are false. Rather, the problem is that the inferences used to prove those theorems are unjustified because they outstrip the meanings of the logical constants. Of course, it does not follow that anything is wrong with the results themselves. Plenty of mathematical results have both constructive and non-constructive proofs, and the intuitionist is happy to accept these results as true in virtue of the former, intuitionistically valid proofs.

On the other hand, there are theorems of classical mathematics that are demonstrably false according to intuitionistic principles. The Bolzano-Weierstrass Theorem is one example. Since there cannot be constructive proofs of these theorems, the antirealist must hold that they are intrinsically flawed in some way. That is, there must be something about the meanings of statements of these theorems that prevent them from being proved using constructive methods. Otherwise, we would not be able to show that such proofs are impossible; at most, we could say that they have yet to be discovered.

It is worth considering what the status of these statements is according to the antirealist. Let ϕ be a statement of a theorem that is demonstrably false according to intuitionism but has a classically acceptable proof. *Prima facie*, there seem to be three options that the antirealist might take concerning the semantic status of ϕ .⁸

1. ϕ is utterly ambiguous. In this case, we need to distinguish between ϕ_c , the classical version of the statement, and ϕ_i , the intuitionistic version.
2. ϕ has the exact same meaning in a classical context and in an intuitionistic one.
3. The meaning of ϕ differs in the two contexts, but there is some degree of overlap. We still need to distinguish between ϕ_c and ϕ_i , but there is a common kernel, ϕ_k , that both versions of the statement share.

Each of these options is problematic for the antirealist.

The first option is problematic for at least two reasons. First, it does not allow for genuine disagreement between classical and intuitionistic mathematicians. Rather, we have the classical mathematician stating her theorems, all implicitly subscripted with ‘*c*’, and proving them using classical methods. Meanwhile, the intuitionist utters his ‘*i*’-subscripted statements and proves them without going beyond the bounds of intuitionistically acceptable inferences and principles. Occasionally, there is the appearance of disagreement. For example, the classical mathematician asserts a statement of the Bolzano-Weierstrass Theorem and proudly displays a proof of it. When the intuitionist tries to claim that the result is false, he can’t do it: every time he says “actually, the Bolzano-Weierstrass Theorem is false, and here’s why,” he finds himself talking about something other than what the classical mathematician is discussing.

⁸The reader might object that we are introducing a disjunction out of the blue here and that this presupposes the validity of the *tertium non datur*. This is not the case, however, because Dummett insists that there is something that distinguishes realistic construals of statements from antirealistic ones. Each of the listed options represent a different way of understanding how this distinguishing feature does its job. We assume that the antirealist understands his own position and is able to articulate it publicly; this gives something like a decision procedure for determining which of the disjuncts below holds.

If the intuitionist really wanted to deny the classical result, he would have to start talking about the Bolzano-Weierstrass Theorem_c, but then he's wrong to deny the theorem, which has a perfectly acceptable classical proof.

If this were the whole story, we might decide to embrace a kind of pluralism and the dawn of a new era of peace in the world of mathematical foundations. This isn't open to the antirealist, however, because his position entails that the statements and methods of classical mathematics are not faithful to the meanings of the logical constants and other linguistic items that appear in those statements. So, if we have two really distinct statements ϕ_c and ϕ_i with no semantic overlap, the former must be totally meaningless. If it were anything but gibberish, its meaningful aspect would be shared by ϕ_i , but on the present hypothesis this is not the case. Thus, option 1 and antirealism together have as a consequence that classical mathematics is an incoherent mess despite appearances to the contrary.

What about option 2, according to which a mathematical statement ϕ has the same meaning in a classical context as it does in an intuitionistic one? This allows for genuine disagreement between members of the two schools, since the intuitionist who denies ϕ is denying exactly what the classical mathematician asserts.

Combined with antirealism, however, option 2 has a problem similar to that faced by option 1. According to the antirealist, the meaning of a mathematical statement ϕ is given by what counts as a proof of the statement. Furthermore, the antirealist's argument is supposed to show that a genuine proof of ϕ cannot rest on intuitionistically invalid moves like double-negation elimination or arbitrary applications of the *tertium non datur*. So, when the classical mathematician accepts a proof that uses these moves, she displays her ignorance of the meaning of ϕ . If she really knew what ϕ meant, she would realize that the "proof" presented to her is flawed.

Instances of this phenomenon are familiar. Famously, Andrew Wiles's original "proof" of Fermat's Last Theorem contained a flaw that was subsequently corrected;

only after this correction could we say that the result had been demonstrated. Indeed, anybody who has experience with mathematical proof has, at some point, probably accepted a flawed proof or failed to accept a genuine one. In isolated cases, such errors can be corrected by further reflection on the proof.

According to the antirealist, however, these errors are not isolated. Rather, they pervade classical mathematics. Furthermore, it is unlikely that thinking harder about her proofs will convince the classical mathematician that they are flawed. For the intuitionist, the problem with classical mathematics is not that its practitioners apply their proof methods incorrectly; rather, the problem is that these methods themselves are invalid or otherwise incorrect. So, although mathematical statements have the same meaning in a classical context as they do in an intuitionistic one (according to the option under discussion), classical mathematicians routinely mistake what count as correct proofs of those statements. Since the meanings of the statements are in terms of what counts as proofs of them, this means that the classical mathematician systematically misunderstands the various theorems that together constitute a large portion of mathematics.

The third option presents similar problems yet again. According to it, there is some overlap between the meanings of statements when used by the intuitionist and the orthographically identical statements used by the classical mathematician. This overlap can take two forms. On the one hand, there might be certain linguistic components that have the same meanings in both contexts and other components whose meanings are different. The latter category would presumably include at least some of the logical constants, as well as terms like ‘function’ over which there is robust disagreement.⁹

⁹This makes things sound simpler than they are. Intuitionists and classical mathematicians agree that a (unary) function is a set of ordered pairs such that any two pairs with identical first elements have identical second elements. So, if there is disagreement about the meaning of ‘function’, it cannot be about this definition. Ultimately, the present work contends that disagreements about functions are about the things themselves, not about the meaning of the term.

What would we include in the category of items about which there is agreement? There is not an obvious candidate because it is hard to find mathematical terms the use of which is the same in classical and intuitionistic mathematics. There is even divergence when it comes to such mundane terms as numerals. Granted, it is only when we start quantifying over the natural numbers that we find disagreement, so perhaps number-theoretic disagreements have more to do with logical terms than numerical ones. Still, for one who insists that classical and intuitionistic mathematicians disagree fundamentally about meaning, it is difficult to identify isolated points of disagreement; it is more plausible to say that the semantic disagreement is pervasive (thereby adopting option 2 above).

The antirealist might instead hold that, for a given term (e.g., \forall), there is some common semantic core about which classical and intuitionistic mathematicians agree. According to this view, we do not have to identify any terms about which there is complete agreement, so it avoids the problem just discussed. Still, it is hard to identify this common core for a given term. In the case of a logical constant, we might take it to be given by introduction and elimination rules, but the intuitionistic and the classical mathematician disagree about which rules are valid in the first place.

In any case, on this third option, there is still systematic disagreement about meaning. Coupled with the antirealist's view that only the intuitionistic meanings are compatible with our linguistic and mathematical activity, this option also entails that the classical mathematician fails to understand much of what she says and writes.

Indeed, this conclusion follows from all three options. According to the antirealist, the community of classical mathematicians is in a position akin to that described at the end of the story of the Tower of Babel. Indeed, the classical mathematician's situation is even more troubling. After the Biblical confusion of tongues, no person could understand any other, but if the antirealist is right, the classical mathematician misunderstands a great deal of her own speech.

Given this, it is nothing short of a miracle that classical mathematics has been so successful. The evidence of this success can be found in a number of places: the large number of journals publishing papers containing classical proofs, the existence of many university mathematics departments full of classical mathematicians, etc. If classical mathematics involved as much semantic confusion as antirealism would assign to it, it should have long since gone the way of alchemy: the silly features (philosophers' stones, discontinuous total functions on the reals) abandoned, the legitimate ones (chemistry, constructive mathematics) flourishing. That this has not happened indicates that the antirealist's criticism of classical mathematics is too strong to fit the data.

3.4 What is a Proof?

In Part I of his article "What is a Theory of Meaning," Dummett answers the question posed by his title by maintaining that "a theory of meaning is a theory of understanding" (Dummett 1993: p. 3). This is to say that a theory of meaning for a language L should tell us what it is that a speaker of L knows in virtue of which she is a speaker of the language. Also, for the antirealist, as we have seen, the meaning of a statement is given by what counts as verifying that statement; in particular, the meaning of a mathematical statement is given by what counts as a *proof* of the statement.

Putting these two Dummettian views together, it follows that, for the antirealist, a person understands a mathematical statement ϕ just in case she knows what counts as a proof of ϕ . Given the antirealist's emphasis on the ability to exhibit such knowledge publicly, we can conclude that, according to semantic antirealism, a person understands ϕ just in case she is able to recognize a proof of ϕ when presented with one.

It is worth considering exactly what this thesis about understanding language

requires and what it entails. First, note that, in order for it to be plausible, it must be given a strong reading that might not be apparent at first glance. In order for a person to understand ϕ , it is not enough merely that she be able to recognize proofs of ϕ when presented with them. If this were the case, then a person who automatically takes any purported proof of ϕ to be a genuine proof would thereby understand the statement. Thus, the antirealist's view is better expressed by saying that understanding ϕ requires correctly recognizing both proofs and non-proofs of ϕ when presented with them.¹⁰

Even with this clarification, there are some obvious questions about how we should understand this antirealistic principle of understanding. For example, consider a statement of Fermat's Last Theorem (with all variables ranging over natural numbers):

$$\neg \exists a \exists b \exists c \exists n (n > 2 \wedge a^n + b^n = c^n).$$

On its surface, this looks like a fairly simple statement of number theory. Nevertheless, its only known proof is famously lengthy and complicated.¹¹ Indeed, it is difficult to understand even for professional mathematicians, let alone people who have never learned to recognize or construct proper mathematical proofs. One strong reading of the antirealist's principle regarding understanding is that a person who understands a statement must be able to recognize *any* correct proof of the statement as such. If this is correct, then there are very few people in the world who understand a statement of Fermat's Last Theorem. Nevertheless, the theorem itself is easily stated using only notions from basic number theory and can surely be grasped by any working mathematician (to say nothing of reasonably intelligent high school students).

It is easy to contrive similar examples involving, e.g., sums of numbers too large

¹⁰Non-proofs include both purported proofs that do not establish their conclusions (e.g., because of an invalid inference) and objects that are not serious candidates for mathematical proofs at all (novels, culinary recipes, etc.).

¹¹The paper (Wiles 1995), where the proof first appears, runs to 109 pages.

for the human mind to write down or even think about in full. In such cases, no person is able to recognize the relevant calculations. Nevertheless, to say that somebody does not understand the result calls into question his grasp of addition, and it is a mistake to make one's understanding of addition contingent on the computational resources of the human mind. The case of Fermat's Last Theorem is especially striking, however, because it is an actual example of an easily stated result with a sublimely difficult proof.

The lesson to draw from this example is that the antirealist cannot adopt such a strong principle of understanding. The grasp of a mathematical statement cannot require that somebody be able to recognize *every* correct proof of the statement as such.¹² Indeed, the best case for the antirealist would be if there were a way to identify, for each mathematical statement, a single proof that could be used to determine whether a person understands the statement.

Dummett (2000: §7.2) acknowledges the problems just described regarding proofs like that of Fermat's Last Theorem.

Are we, then, to say that any (constructively) valid written proof, such as might appear in an article in a mathematical journal or in a textbook, is, considered relative to the intended meanings of the words and symbols employed, a proof in the sense in which this word is used in the explanations of the logical constants? It seems to follow from the character of those explanations that we are not. (*ibid.*, p. 270)

Dummett motivates this worry not by calling attention to particular examples but by pointing out that, in general, proofs in mathematical journals do not conform perfectly to the BHK clauses. For example, a mathematician might assert a statement of the form $\exists xA(x)$ without naming any individual object a such that $A(a)$. According to a literal interpretation of the BHK clause for \exists , the mathematician has failed to justify

¹²Note that it will not do to say that understanding requires that a person *could* come to recognize every proof. This is just to say that somebody *could* come to understand statements of Fermat's Last Theorem, but the point is that plenty of people now understand these statements without now being able to recognize proofs of them as correct.

his existential assertion; such a justification would *require* a reference to an individual.

Nevertheless, according to Dummett, we are happy to accept existential claims that don't meet the strict BHK requirements as long as "we have some effective method of finding an individual satisfying $A(x)$ " (*ibid.*). This leads him to distinguish between what he calls demonstrations, which are the kinds of arguments one finds in journals, and *canonical* proofs. Demonstrations do not conform explicitly to the BHK interpretation; canonical proofs do. Nevertheless, a demonstration does provide evidence for its conclusion because it "supplies an effective means of constructing an actual [i.e., canonical] proof," as Dummett puts it.

Ultimately, canonical proofs are what matter for the semantic antirealist's theory of meaning and understanding. In order for the antirealist to allow that somebody can understand Fermat's Last Theorem without understanding Wiles's complicated demonstration of it, he must have recourse to the existence of a canonical proof of the theorem, which proof can be extracted from the demonstration by an effective method. Understanding a statement of the theorem can then be taken to consist in the ability to recognize this canonical proof as a correct proof of the statement. What distinguishes the canonical proof from the demonstration is that every inference in the former is articulated explicitly. By contrast, the demonstration is difficult to understand because its cogency relies on a great deal of unstated background knowledge concerning, e.g., the theory of elliptic curves.

As a result of this explicit articulation, it is quite easy to check whether each step in the canonical proof is valid. One need only look to see whether each inference is licensed by the BHK-meaning of the main logical constant in question. A drawback of making everything explicit, however, is that many canonical proofs will be enormously long. For instance, each time a demonstration appeals to a result proved earlier, the corresponding canonical proof must contain the entire proof of that result. Similarly, in order to conform to the BHK interpretation, certain familiar inference rules cannot

appear in canonical proofs. These are the rules that implicitly appeal to a more detailed means of obtaining the conclusion: *modus ponens*, disjunction elimination, and universal instantiation. For example, the BHK interpretation says that to assert a conditional $\phi \rightarrow \psi$ requires a method for transforming an arbitrary proof of ϕ into a proof of ψ . Thus, an inference from $\phi \rightarrow \psi$ and ϕ to ψ implicitly appeals to this method, and a fully explicit canonical proof will make this clear by replacing the instance of *modus ponens* with a step-by-step application of this method to the premises.

Even when a demonstration is not conceptually complicated, the corresponding canonical proof might be gargantuan. For example, any algorithm for adding numbers will not be able actually to be applied to numbers beyond a certain magnitude. Nevertheless, in order for the antirealist to be able to assert universal arithmetic principles like the laws of commutativity, such methods must apply to all numbers. This is why Dummett and other antirealists place a great deal of emphasis on the notion of an effective method. This allows them to maintain that we can assert more statements than we have in fact proved (e.g., the infinitely many instances of the commutative law for addition); these assertions are justified because we have an effective method for proving them in principle, even if it cannot be carried out in practice.

Similarly, although canonical proofs will often be too large to comprehend *in toto*, they can be examined in an effective way that checks each step for validity. Thus, the antirealist's claim that understanding a statement consists in being able to recognize a proof of it when presented with one means that somebody who understands ϕ is able in principle to determine whether a candidate proof is a canonical proof of ϕ in virtue of knowing a process that evaluates the candidate proof in a step-by-step fashion.

By introducing the distinction between canonical proofs and demonstrations, the antirealist encounters a difficult dilemma. We saw above that the antirealistic inter-

pretation of intuitionism is, at best, an incomplete project absent proofs of soundness and completeness for intuitionistic logic relative to the BHK semantics. It turns out that we can say more. On certain assumptions about the ‘proves that’ relation \vdash , there cannot be such proofs. The key assumption is that the relation is decidable, i.e., that for any candidate proof P (which may or may not be canonical) and statement ϕ ,

$$P \vdash \phi \vee P \not\vdash \phi.$$

Given the antirealistic recognition condition on assertions, this assumption captures the requirement that understanding a statement requires being able to recognize correct proofs of the statement as such and incorrect “proofs” as such. McCarty (2013) shows that this assumption entails the incompleteness of intuitionistic logic with respect to the BHK semantics. In particular, he shows that the law of testability,

$$\forall \phi (\neg \phi \vee \neg \neg \phi),$$

is valid on the BHK semantics even though it is not a theorem of intuitionistic logic. This result is central enough to our own argument that we give a sketch of the proof here.

Theorem. *If the \vdash relation is decidable, then the law of testability is valid on the BHK interpretation.*

Proof. By the decidability of \vdash , for any candidate proof P and mathematical statement ϕ ,

$$P \vdash \phi \vee P \not\vdash \phi.$$

We will say that a formula is *almost negative* when it is equivalent to a formula constructed from atomic formula by means of \neg , \wedge , \rightarrow , or \forall only.

By Lemma 1 of McCarty (*ibid.*), $P \vdash \phi$ iff ϕ_{BHK} when ϕ is almost negative, where

ϕ_{BHK} is the formula ϕ interpreted according to the BHK semantics. So, from the decidability of \vdash , if ϕ is almost negative, then

$$\phi_{BHK} \vee \neg\phi_{BHK}.$$

By Lemma 2 of McCarty (*ibid.*), it follows from the decidability of \vdash that, for any formula ϕ , $\neg\phi$ is almost negative. Thus, for any ϕ , we can infer

$$\neg\phi_{BHK} \vee \neg\neg\phi_{BHK}.$$

Since this holds for *any* ϕ , the law of testability,

$$\forall\phi(\neg\phi \vee \neg\neg\phi)$$

is valid under the BHK interpretation. □

Since the law of testability is not a theorem of intuitionistic logic, a corollary of this result is that intuitionistic logic is incomplete with respect to the BHK semantics.

In order to sustain the antirealist's project in light of this result, one has to reject that the \vdash relation is decidable. This might not seem like a fatal concession on the antirealist's part; our earlier observations about demonstrations of Fermat's Last Theorem show that the \vdash relation cannot be decidable in general.

Nevertheless, the relation ought to be decidable when restricted to canonical proofs. That is, for any purported canonical proof C and statement ϕ , it ought to be the case that

$$C \vdash \phi \vee C \not\vdash \phi.$$

Were this not the case, the notion of a canonical proof would not be able to do its job. The point of a canonical proof is that it can be checked for correctness on its

own merits and without presupposing advanced background knowledge, lucky guesses, or anything else not built into the meaning of the statement. In other words, there must be an effective method for checking whether a purported canonical proof C justifies the assertion of its conclusion ϕ or not. On the antirealistic understanding of disjunction, this just means that the \vdash relation must be decidable at least for canonical proofs.

Once we have this, however, it is easy to show that the relation must be decidable for demonstrations as well. Let D be a purported demonstration of ϕ . Then D can be effectively transformed into a canonical proof C_D . Since the \vdash relation is decidable for canonical proofs, it follows that either $C_D \vdash \phi$ or $C_D \not\vdash \phi$. If the former case holds, then $D \vdash \phi$. If the latter case holds, $D \not\vdash \phi$. Thus, the \vdash relation is decidable for both canonical proofs and demonstrations.¹³ Anything that warrants the assertion of a mathematical statement is either a proof or a demonstration, according to the antirealist. Thus, the \vdash relation must be decidable in general. Since, as McCarty shows (*ibid.*), this entails the universal validity of the law of testability, the antirealist's project of developing a semantics that is provably sound and complete with respect to intuitionistic logic cannot be brought to fruition.

3.5 Away with Semantic Foundations

The arguments of this chapter show that Dummettian semantic antirealism cannot serve as a foundation for intuitionism. One might draw the lesson that we should attempt to find a new semantic foundation. The present work rejects this proposal. Instead of doing the same thing and expecting a different result, we propose that the theory of meaning is not the place to look for mathematical foundations. In the sequel, we will consider a foundational program for intuitionism that better respects

¹³Note that, if $C_D \not\vdash \phi$, then the attempt to transform D into a canonical proof of ϕ fails. The essence of a demonstration is that it can be effectively transformed into a canonical proof. Thus, if the attempt fails, D is not a demonstration of anything, from which it follows that $D \not\vdash \phi$.

its mathematical character. Our starting point will be the work of the school's founder, L.E.J. Brouwer, to which we now turn.

Chapter 4

Brouwer's Philosophical Argument for Intuitionism

Having examined Dummett's foundational approach and found it lacking, we turn to the work of L.E.J. Brouwer, the founder of the intuitionistic program in mathematics. Brouwer stands out as the most conspicuously forceful advocate of intuitionism. Many other intuitionists, insofar as they offer any foundational arguments for their views, do so only via half-hearted waves at Brouwer's own arguments or the latest philosophical trends. (This is especially conspicuous in Heyting's work.) Even Dummett sometimes expresses hesitation about his own argument. Gideon Rosen describes this nicely in his critical notice of Dummett's *The Seas of Language* (1993).

The Dummett of popular imagination is a confident anti-realist; the dogmatic purveyor of a sophisticated and comprehensive brand of idealism. As these essays make plain, however, the real Dummett is very different. He's not an anti-realist at all; he's just a guy with an argument — the celebrated Dummettian case for Anti-Realism — according to which the various “realisms” we tend to treat as Moorean data are in fact philosophically untenable. Unlike his imaginary counterpart, however, the real Dummett does not assert this conclusion. (Rosen 1995: pp. 599-600)

By contrast, Brouwer's work is the vector sum of his combative personality, bold writing style, and unshakable confidence that he has things right and his opponents are

mistaken. The result is a passionate (albeit sometimes flawed) defense of intuitionism that is well worth investigating.

We will examine Brouwer's work in two stages. This corresponds to two species of argument identifiable in his writings. We will call one of these his mathematical argument, and it will be the subject of the next chapter. The other, which we discuss here, we can call, for lack of anything better, his philosophical argument. (For a defense of the claim that there is a principled distinction here, see the first section of the following chapter.) Versions of both arguments appear throughout his career, although the philosophical argument is more common in his early and late writings, while the mathematical one is more prominent in his middle period.

4.1 The Debut of the Philosophical Argument

In 1907, Brouwer completed his dissertation, *Over de Grondslagen der Wiskunde* (1907), at the University of Amsterdam. Its title, which translates to “On the Foundations of Mathematics,” reveals that Brouwer was interested in philosophical and foundational matters very early in his career, and the dissertation's assault on logicism fits well in the tradition of proto-intuitionism represented by mathematicians like Borel and especially Poincaré. Nevertheless, it does not contain any challenge to the unrestricted validity of the *tertium non datur*. This principle is mentioned briefly in the dissertation, but it is dismissed as a logical shortcut around properly mathematical reasoning.

While in the syllogism a mathematical element could be discerned, the proposition:

A function is either differentiable or not differentiable

says *nothing*; it expresses the same as the following:

If a function is not differentiable, then it is not differentiable.

But the logician, looking at the *words* of the former sentence, and discovering a regularity in the combination of words in this and similar sentences, here again projects a mathematical system, and he calls such a sentence an *application of the tertium non datur*. (Brouwer 1975: p. 75)

Here, we see Brouwer treating

$$\forall\phi(\phi \vee \neg\phi)$$

as equivalent to

$$\forall\phi(\neg\phi \rightarrow \neg\phi).$$

It is worth noting that this equivalence holds in classical logic but not in intuitionistic logic. This should suffice to dispel any notion that intuitionistic logic sprang fully armed from Brouwer's head.

The first appearance of Brouwer's philosophical argument for intuitionism appears in his paper "De Onbetrouwbaarheid der Logische Principes" (1908b) (in English, "The Unreliability of the Logical Principles"). After claiming that, in mathematics, the principle of non-contradiction and principle of hypothetical syllogism are universally valid, Brouwer discusses the *tertium non datur* (here called the '*principium tertii exclusi*').

Now consider the *principium tertii exclusi*: It claims that every supposition is either true or false; in mathematics this means that for every supposed imbedding of a system into another, satisfying certain given conditions, we can either accomplish such an imbedding by a construction, or we can arrive at the arrestment of the process which would lead to the imbedding. It follows that the question of the validity of the *principium tertii exclusi* is equivalent to the question *whether unsolvable mathematical problems can exist*. There is not a shred of a proof for the conviction, which has sometimes been put forward, that there exist no unsolvable mathematical problems. (Brouwer 1975: p. 109, original emphasis)

The equivalence that Brouwer mentions in this passage is striking. Why would anyone think that the *tertium non datur* is equivalent to the solvability of all mathematical problems? Since the argument stands or falls with this claim, we will do well to look

closely at it.

4.1.1 The Semantic Reading

A natural reading of Brouwer’s argument has it turn on semantic matters. This is to take Brouwer as arguing in favor of a special, constructive meaning of the logical constants—a meaning like that given by the BHK interpretation. In other words, it would turn Brouwer into a proto-Dummettian. This reading is suggested by Troelstra and van Dalen’s (1988: p. 31) claim that the BHK interpretation “may be regarded as implicit in Brouwer’s writings.” It also seems to be in the background of a reference by Carnap (1959: p. 48) to “the form of language suggested by Brouwer.”

Furthermore, Brouwer’s argument, as quoted above, begins with a statement about what the *tertium non datur* means. Also, the argument appears to be setting down truth conditions for mathematical “suppositions.” As such, there seems to be internal evidence that Brouwer had something about language in mind.

This would be too hasty, though. The Dutch original contains nothing corresponding to the word ‘means’. Also, the word translated as ‘true’ is *juist*, which is better translated as ‘right’ or ‘correct’ (cf. English ‘just’). Here is a more literal translation of the relevant part of the passage.

Now the principium *tertii exclusi*: this requires that every supposition is either correct or incorrect, mathematically: that for every supposed embedding of systems ...¹

Heyting, who translated Brouwer’s paper into English for the latter’s *Collected Works*, imports his own interpretation of the argument by translating the adverb *wiskundig*

¹The translation is my own. In Dutch, the passage reads,

Nu het principium *tertii exclusi*: dit eischt, dat iedere onderstelling òf juist òf onjuist is, wiskundig: dat van iedere onderstelde inpassing van systemen ... (Brouwer 1908b: p. 156)

with the expression ‘in mathematics, this means’. Any argument based on Heyting’s translation for a semantic reading of the passage therefore begs the question.

Another problem with the semantic interpretation of Brouwer’s argument is chronological. As we noted earlier (Chapter 1, §2.1), ‘BHK interpretation’ is not a particularly appropriate name for the semantics introduced by Heyting et al., since Brouwer never articulated it. Indeed, even after Kolmogorov and Heyting did, Brouwer seems not to have remarked on it in his own work or his correspondence. Granted, we should not expect him to mention it as early as 1908. As late as the 1950s, however, Brouwer was giving arguments similar to the one from 1908.² By then, he was surely aware of the BHK interpretation; had he been assuming it in his argument, we could expect him to mention this. The lack of any explicit discussion of it at any point in his career casts doubt on the claim that the philosophical argument is semantic at its core.

4.2 Hilbert and *Ignorabimus*

A clue to the correct understanding of Brouwer’s argument is given by a footnote attached to the passage in question. In it, Brouwer refers to David Hilbert’s (1901) lecture *Mathematische Probleme*. This lecture is known mainly for its list of the twenty-three Hilbert problems. Of course, these were all unsolved at the time, and the list is preceded by a powerful exhortation addressed to Hilbert’s audience of mathematicians.

The conviction of the solvability of every mathematical problem is a powerful incentive to the worker. We hear within us the perpetual call: There is the problem. Seek its solution. You can find it, for in mathematics there is no *ignorabimus*. (Ewald 1996: p. 1102)

This might seem like an exhortation only: Hilbert is challenging his audience to invest all their energy in solving his problems, and at the same time encouraging them to

²See §5.1 below.

remain confident if some appear intractable. Still, the use of the Latin ‘*ignorabimus*’ is conspicuous in an otherwise German-language address, especially since it is not a familiar Latin phrase (à la ‘*quod erat demonstrandum*’). Indeed, it implicitly reveals that Hilbert’s discussion is not only hortatory, but also polemical.

The term ‘*ignorabimus*’ is clearly an allusion to an 1872 lecture by the physiologist Emil du Bois-Reymond. In this lecture, du Bois-Reymond argues that, with respect to some traditional philosophical and scientific questions like the nature of matter and the relationship between the mind and the body,

one must render once and for all the much more difficult verdict: “*Ignorabimus!*”³

The lecture sparked a European controversy, the *Ignorabimusstreit*, about whether to embrace ignorance when it comes to these questions. It entered the mathematical arena when Emil du Bois-Reymond’s brother Paul, a mathematician, published his book *Die allgemeine Funktionentheorie* (du Bois-Reymond 1882). In this book, Paul du Bois-Reymond argues that there are questions in the foundations of mathematics (particularly concerning the nature of the continuum) for which the appropriate verdict is *Ignorabimus*.

Hilbert was having none of this. His rejection of mathematical *Ignorabimus* in his 1900 lecture is undoubtedly a stand against the du Bois-Reymonds’ position.⁴ He would continue this crusade throughout his life, to the point that his retirement address in 1930 concludes with the proclamation “Wir müssen wissen. Wir werden wissen” (which became the epitaph on his tombstone). This could be a more explicit rebuke of the *Ignorabimus* camp only if he had broken into Latin. It is no exaggeration

³The translation is my own. The German reads,

...muss er ein für allemal zu dem viel schwerer abzugebenden Wahrspruch sich entschliessen: “*Ignorabimus!*” (du Bois-Reymond 1886: p. 141)

⁴For details about the ways in which Hilbert’s individual problems are also anti-*Ignorabimus* maneuvers, see McCarty (2005), on which much of the present discussion draws.

to say that Hilbert's life's work was devoted to defending the principle according to which every mathematical statement can be either proved or refuted. This, which we might call Hilbert's Axiom of Solvability, appears in his 1900 lecture in its equivalent form: "in mathematics, there is no *ignorabimus*."

This historical background and Hilbert's eminent status in the European mathematical universe of his day sheds light on Brouwer's argument in his 1908 paper. It is no accident that in this paper, he refers to Hilbert's famous lecture precisely when he insists that there is no reason to expect every problem to have a solution. Furthermore, the rest of Brouwer's paper is devoted entirely to giving examples of unsolved problems (e.g., whether a certain sequence of digits appears at any point in the decimal expansion of π) and proposing classificatory schemes to distinguish solvable from unsolvable problems. For example, Brouwer distinguishes a certain class of finitistic problems as ones that are sure to have a solution.

Insofar as only finite discrete systems are introduced, the investigation whether an imbedding is possible or not, can always be carried out and admits a definite result, so in this case the principium tertii exclusi is reliable as a principle of reasoning. (Brouwer 1975: p. 109)

This is followed by a remarkable footnote: "This investigation can even in every case be made by a machine or a trained animal." Here, Brouwer seems to be anticipating the notion of a Turing machine (or trained Turing horse). Perhaps he was familiar with the ideas of Charles Babbage and other 19th Century forerunners of computability theory, but it is nevertheless surprising to find this footnote three decades before Turing's work appeared.

In any case, the evidence suggests that, although Brouwer purports to be discussing the *tertium non datur*, his primary target in his 1908 paper is Hilbert's Axiom of Solvability. He is throwing his hat into the ring of the *Ignorabimusstreit* and siding against Hilbert and with the du Bois-Reymonds.⁵

⁵We should note here that Paul du Bois-Reymond's *Die allgemeine Funktionentheorie* contains a discussion of choice sequences, which would later play an important role in intuitionistic analysis

4.3 *Ignorabimus* and the *Tertium non datur*

Even if the chief target of Brouwer’s philosophical argument is Hilbert’s Axiom of Solvability, he still claims that this is equivalent to the *tertium non datur*. On the surface, this seems to be a mistake. Of course, Hilbert’s axiom entails the logical principle. To say of an arbitrary statement ϕ representing a mathematical problem that it has a solution is to say that there is a proof of ϕ or a proof of $\neg\phi$. In the former case, ϕ is true; in the latter case, $\neg\phi$ is true. In either case, we can infer $\phi \vee \neg\phi$, and since ϕ was arbitrary, we can generalize and infer the *tertium non datur*.

The other entailment, from *tertium non datur* to the axiom of solvability, is harder to justify. We can make some progress by observing some of the peculiar features of Brouwer’s own philosophy of mathematics. It is rooted in the tradition of German idealism, as evidenced by Brouwer’s emphasis on the temporal “basic intuition” (Dutch: *oer-intuïtie*; German: *Ur-Intuition*) in his dissertation and elsewhere. This basic intuition produces the fundamental objects of mathematical study, as is clear from “Die mögliche Mächtigkeiten” (Brouwer 1908a).

If one investigates how mathematical systems come about, one finds that they are built out of the basic intuition of duo-unity. The intuitions of the continuous and the discrete are found here together, because a second thing alone is never thought by itself, but rather under the retention of the memory of the first. The first and the second therefore come to be *held together*, and the intuition of the continuous consists in this holding together (continere = to hold together). This mathematical basic intuition is nothing other than the contentless abstraction of temporal experience, i.e., the experienced of ‘fixed’ and ‘vanishing’ together, or of ‘constant’ and ‘varying’ together.⁶

as developed by Brouwer and others. This is more evidence that Brouwer was familiar with du Bois-Reymond’s work and the *Ignorabimusstreit*, and therefore that his agreement with the du Bois-Reymond brothers in 1908 is not *per accidens*.

⁶The translation is my own. The German text reads,

Wenn man untersucht, wie die mathematischen Systeme zustande kommen, findet man, dass sie aufgebaut sind aus der Ur-Intuition der Zweieinigkeit. Die Intuitionen des kontinuierlichen und des diskreten finden sich hier zusammen, weil eben ein Zweites gedacht wird nicht für sich, sondern unter Festhaltung der Erinnerung des Ersten. Das Erste und das Zweite werden also *zusammengehalten*, und in dieser Zusammenhaltung besteht die

The first chapter of Brouwer’s dissertation is devoted to showing how more advanced mathematical objects (e.g., those required for analysis and geometry) can be developed on this basis.

With this in mind, we can attempt to construct a Brouwerian argument that the *tertium non datur* entails the Axiom of Solvability. There are two features of a subject’s temporal experience that are central to Brouwer’s conception of mathematics. The first is that time has a determinate initial moment that occurs when the subject grasps two things together in intuition and memory. This gives rise to the numbers 1 and 2, as well as the continuum. Since time has a starting point, only finitely many temporal moments have passed at any given point. This does not mean that infinite mathematical objects cannot be constructed, but only that infinite objects (like the collection of natural numbers) must be given by a rule for generating them, which rule must be capable of being specified in a finite amount of time. The continuum is an exception to this requirement, but not because the subject is allowed an infinite time to construct it. Rather, it cannot be constructed at all, according to Brouwer; it is the background against which all mathematical constructions take place.⁷

The second important feature of the subject’s temporal intuition is its potentially infinite character. The subject can generate a new mathematical construction at every moment, and new mathematical possibilities are always on the horizon. Now, consider a mathematical problem that as yet has not been solved. Brouwer’s dynamic conception of the mathematical universe allows for the possibility that there is no solution because the relevant objects have not yet been constructed in intuition. This

Intuition des kontinuierlichen (continere = zusammenhalten). Diese mathematische Ur-Intuition ist nichts anderes als die inhaltslose Abstraction der Zeitempfindung, d.h. der Empfindung von ‘fest’ und ‘schwindend’ zusammen, oder von ‘bleibend’ und ‘wechselnd’ zusammen. (Brouwer 1975: 102, original emphasis)

⁷Brouwer abandons the requirement more drastically in (Brouwer 1918) because he is worried that it prohibits a mathematically sophisticated theory of the continuum. Although his worry is unfounded, it eventually led him to introduce the notion of free-choice sequences into intuitionistic mathematics. In the next chapter, we will discuss this feature of Brouwer’s real analysis.

does not mean that there will never be a solution, but only that mathematics has not yet developed to the point where such an investigation can take place. Nevertheless, there is no guarantee that we will ever reach a point where the required development occurs. It is possible that some problems will remain unsolved *in perpetuum*.

This allows us to make sense of Brouwer's talk of embeddings of systems into each other. The term 'system' appears in the writings of other mathematicians roughly contemporary with Brouwer. For instance, in an 1899 letter to Dedekind, Cantor treats the terms 'multiplicity' (*Vielheit*), 'system', and 'totality' as synonyms corresponding to the present-day term 'class'. (van Heijenoort 1967: p. 114) Dedekind similarly uses 'system' in his *Was sind und was sollen die Zahlen* (1893: *passim*); indeed, it seems to be his preferred term for the notion (whereas Cantor prefers 'multiplicity').

In light of this, Brouwer surely also had the notion of a class in mind when he talks about systems. The other key term in his argument is 'embedding' (*inpassing*). By this, he seems to mean what we would call an injective function. If these interpretations are correct, then Brouwer's argument might be reconstructed as follows. Let ϕ be some mathematical proposition (or supposition, to use Brouwer's term). Note that ϕ is equivalent (classically and intuitionistically) to the existence of an injection (an embedding) of the natural numbers ω into the set $\omega_\phi = \{n \in \omega : n = n \wedge \phi\}$. This is because, if ϕ is true, then $\omega_\phi = \omega$, so the identity function will serve as the required function. On the other hand, if $\neg\phi$ holds, then $\omega_\phi = \emptyset$, so there is no injection from ω into ω_ϕ .

Thus, if we assume the *tertium non datur*, ω_ϕ is either ω or \emptyset . Given Brouwer's dynamic conception of mathematical objects, the construction of the required embedding must take place in time and cannot take an infinite amount of time. Similarly, the impossibility of constructing such an embedding is itself a mathematical fact. So, on Brouwer's view, it requires a finitary construction. It is not enough that the attempted construction never finishes; the process must reach a point where we can see

that it cannot be continued, or, as Brouwer puts it, that the construction is arrested (*stuiting*).

Since we have assumed the *tertium non datur*, either we can construct such an embedding or we cannot. In the former case, we have a solution to ϕ since we know that ω_ϕ must be inhabited and so cannot be empty. In the latter case, the impossibility of constructing the embedding can only be because ω_ϕ is empty, in which case we have a refutation of ϕ . In either case, we have solved the problem posed by ϕ . Therefore, if we assume the *tertium non datur* and Brouwer's philosophy of mathematics, we can show that every problem is solvable. We already saw that the Axiom of Solvability trivially entails the *tertium non datur*, so the two principles are equivalent given Brouwer's other philosophical views.

The argument as Brouwer presents it is very elliptical. Our reconstruction of it is plausible in light of the idealistic philosophy he advocates in his early work and his use of words like 'system' at a time when this was common mathematical parlance, but other interpretations are possible. It is almost certain, however, that any reasonable interpretation will emphasize the place that time plays in Brouwer's philosophy. After all, this is a recurring theme in Brouwer's work, particularly early in his career. This begets a problem: why should we accept Brouwer's Kantian idealism about mathematics? Positive arguments for it are conspicuously absent in his papers and dissertation. Instead, we find a number of criticisms of competing philosophies. For example, "The Unreliability of the Logical Principles" contains an attack on logicism that highlights the familiar semantic and set-theoretic paradoxes like the Liar and the Burali-Forti Paradox. "Intuitionism and Formalism" (1912), Brouwer's inaugural lecture at the University of Amsterdam, reproaches Cantorian set theory for being contradictory (again because of the paradoxes) and Zermelo's axiomatic set theory for being *ad hoc*. In his dissertation, Brouwer does show how various mathematical objects can be built up from the natural numbers, but there is nothing particularly

intuitionistic about his method (e.g., he defines integers as ordered pairs of natural numbers just as a classical set theorist today would), and he does not say why mathematics must be founded on our intuition of time.

Without a cogent argument for his view of mathematical objects, Brouwer’s philosophical argument against the *tertium non datur* comes up short. This might look like a problem, since many mathematicians, philosophers, and hagiographers influenced by Brouwer point to “The Unreliability of the Logical Principles” as the opening salvo in the *Grundlagenstreit* between intuitionists and Hilbertian formalists that would come to a head in the 1920s. We see now that this is a mistake. Brouwer’s 1908 paper is best understood as a contribution to the *Ignorabimusstreit* and a challenge to Hilbert’s Axiom of Solvability; the *Grundlagenstreit* would have to wait until Brouwer began his full-blooded reconstruction of mathematics in 1918.

4.4 Against Solvability

The rest of Brouwer’s paper is devoted to justifying his assertion that there is no reason to expect that every mathematical problem is solvable. He argues by example.

An instructive example is provided by the following unproved proposition [*onbewezen stelling*] which, on the basis of the principium tertii exclusi, is generally trusted and applied in the theory of transfinite numbers, namely that every number is either finite or infinite. This means that for any number γ we can construct:

either a mapping of γ into the sequence of natural numbers in such a way that some number α from this sequence is the *last one* (while the numbers $\alpha + 1, \alpha + 2, \alpha + 3, \dots$ remain free),

or a mapping of γ or one of its parts onto the full sequence of the natural numbers. (Brouwer 1975: p. 110, original emphasis)⁸

⁸Once again, the translation in Brouwer’s *Collected Works* inserts “means” where no corresponding term appears in the Dutch. Rather, Brouwer uses the abbreviation ‘m.a.w.’. This is short for ‘*met andere woorden*’, i.e., ‘in other words’.

Also, the reader should note that Brouwer’s definition of ‘finite number’ differs in form from the one commonly used today. We would say that γ is finite if there is an injective function from it into some finite subset of \mathbb{N} . Brouwer puts things in terms of a finite sequence of natural numbers. This

So interpreted, the statement ‘every number is either finite or infinite’ is not an instance of the *tertium non datur*. If a finite number is as Brouwer describes it, then a number γ is not finite just in case there is no bijection from γ to a natural number. It would require a further proof to show that every non-finite number can be mapped bijectively to the entire set of natural numbers. Brouwer does not mention whom he has in mind when he talks of the statement being trusted on the basis of the *tertium non datur*. Nevertheless, either he or his opponents, whoever they may be, seem to be getting misled by the surface structure of the terms ‘finite’ and ‘infinite’, which are patent antonyms grammatically but not mathematically.

Brouwer continues by offering three problems whose solvability is in question because of his example. His choices are confusing because they seem to have little to do with the theory of transfinite numbers. Two of them have to do with the decimal expansion of π .

- “Is there in the decimal expansion of π a digit which occurs more often than any other one?”
- “Do there occur in the decimal expansion of π infinitely many pairs of consecutive equal digits?”

The third is far more general:

- “Does the principium tertii exclusi hold in mathematics without exception?”
[*ibid.*]

Brouwer does not tell us how to go from questions about finite and infinite numbers to those about the decimal expansion of π . He seems to think that the former cast doubt on the universal validity of the *tertium non datur*, so that we cannot instantiate this general principle to show that, e.g., either some number appears most often in the expansion of π or no number does. Rather, we have to establish one or the other

is merely a cosmetic difference, however.

disjunct using mathematical methods, and there is no antecedent guarantee that we will ever be able to do so. In other words, once we have shown that the *tertium non datur* is invalid, there are statements throughout mathematics that must be handled carefully since our trust in them might be based on the presumed validity of an invalid logical principle.

4.4.1 Weak Counterexamples

Brouwer's example helps shed light on what have come to be known in literature on intuitionism as weak counterexamples to the *tertium non datur*. Later presentations of these tend to be more sophisticated. Examples include those presented by Troelstra and van Dalen (1988: pp. 13-14), Dummett (2000: pp. 31-32), and van Atten (2004: pp. 26-28). All of these begin with some decidable one-place predicate A of natural numbers for which it is not known whether A holds of all natural numbers. One then defines a real number ρ in terms of this latter unsolved problem. The definition is such that $\rho = 0$ iff $\forall n \in \mathbb{N}.A(n)$. Since it is not known whether $\forall n \in \mathbb{N}.A(n)$, it is not known whether $\rho = 0$ or $\rho \neq 0$, which is an instance of the *tertium non datur*.

What makes this kind of weak counterexample more sophisticated than Brouwer's 1908 version is that, in the former, ρ is defined in terms of some unsolved problem. In Brouwer's example, there is no direct connection between transfinite numbers and the decimal expansion of π . Still, both Brouwer's example and the more recent ones have a common structure. We begin with some unsolved problem and show that, so long as this one problem remains unsolved, we can find other problems that are also unsolved.

So understood, however, weak counterexamples are not merely weak but utterly impotent, at least if they are supposed to be counterexamples to the *tertium non datur*. Weak counterexamples show only that if we can cast doubt on one instance of the *tertium non datur*, we can cast doubt on infinitely many. Classical mathematicians

can happily accept this conditional while rejecting its antecedent.

Indeed, those who appeal to weak counterexamples in order to challenge classical logic are faced with a dilemma. On the one hand, if a weak counterexample is supposed to be sufficient for the challenge, then it is fallacious. For instance, our real-number example above is meant to demonstrate the invalidity of the *tertium non datur* by showing that

$$\neg\forall\rho \in \mathbb{R}(\rho = 0 \vee \rho \neq 0).$$

Its key premise is that it is not the case that

$$\forall n \in \mathbb{N}.A(n) \vee \neg\forall n \in \mathbb{N}.A(n),$$

but this assumes the invalidity of the *tertium non datur*. Thus, the argument is circular.⁹

On the other hand, if weak counterexamples need to be supplemented by other considerations, then they become superfluous. Intuitionists also discuss strong counterexamples to logical laws, and we will take these up in the next chapter. A strong counterexample is a mathematical proof showing that an instance of the *tertium non datur* is false. Furthermore, strong counterexamples make no illicit appeal to unsolved problems. With them in hand, the intuitionist does not need weak counterexamples, at least for his polemical project. (McCarty 2011: p. 218-219) shows how a weak counterexample can be transformed into a strong one, but this requires extra principles that are already anticlassical.

In certain respects, Brouwer's 1908 examples are exempt from the charges just leveled against more recent presentations of weak counterexamples. For one thing, his

⁹Another fallacy that appears in this argument is an appeal to ignorance. If a weak counterexample is supposed to show that instances of the *tertium non datur* are false, then appeals to unsolved problems involve an illegitimate leap from what we know at a time to what is the case regardless of what we know. On the other hand, if weak counterexamples are not supposed to demonstrate that instances of the *tertium non datur* are false, then it is a mistake to call them counterexamples. Cf. McCarty (2011: p. 217).

argument relies on his peculiar philosophy of mathematics. While he gives us scant reason to accept that philosophy, it does mitigate the threat of circular argumentation. More to the point, his chief target is Hilbert's Axiom of Solvability. What he has really shown is that Hilbert's faith in the solvability of every mathematical problem is unjustified and grossly so: one finds mathematical *Ignorabimus* not only in the dark corners where the continuum hypothesis lurks, but even in such brightly lit regions as number theory.

Still, it is a mistake for intuitionists today to place too much polemical emphasis on weak counterexamples. They are of mathematical interest insofar as they allow the intuitionist to show easily that, e.g., trichotomy on \mathbb{R} fails, but these proofs already assume the invalidity of classical logic. What our historical discussion highlights is that such "counterexamples" never had much to do with classical logic at all. Despite Brouwer's talk about the *tertium non datur*, his arguments are directed at Hilbert's Axiom; it is only his idiosyncratic philosophy that leads him to conflate the two.

In light of the various incompleteness, undecidability, and independence results obtained by the 20th Century's pantheon of logicians, there is hardly any reason to take the Axiom of Solvability seriously anymore except perhaps as an exhortation to continue producing mathematical results; even an intuitionist will not object to this motivational aspect of Hilbert's program. For the same reason, there is little reason to take weak counterexamples seriously as challenges to classical mathematics; intuitionists ought therefore to emphasize the stronger, mathematical arguments we will discuss in the next chapter.

4.5 Other Appearances of the Argument

Our focus so far has been on the argument Brouwer gives in 1908. Although the strong counterexamples he first developed in 1918 form the core of his mathematical

argument, he never abandoned the philosophical argument. Heyting, too, gestured at Brouwer's philosophy in some of his philosophical excursions. In this section, we will take a look at these later presentations of the argument.

4.5.1 Brouwer's Later Work

Throughout his career, Brouwer periodically returned to versions of the argument that we have been discussing. In the lecture "Mathematik, Wissenschaft und Sprache" (Brouwer 1929), he begins by discussing a familiar topic, "the intellectual ur-phenomenon [*Urphänomen*] of the falling apart of a life-moment into two qualitatively distinct things." On this basis, "the *temporal appearance-sequence of arbitrary multiplicity* arises by means of the self-unfolding of the intellectual ur-phenomenon." (Ewald 1996: p. 1176, original emphasis) Once again, Brouwer is appealing to our intuition of distinct temporal moments to explain the origin of the natural numbers.

Later in the same lecture, Brouwer defines a real number in terms of what he calls a *fleeing property*,

a property such that, for any given natural number, either the existence or the absurdity of the property can be proved, while one can neither calculate a natural number that possesses the property, nor prove the absurdity of the property for all natural numbers. (*ibid.*, p. 1183)

Fleeing properties are just the kind of properties discussed earlier in connection with weak counterexamples. Indeed, Brouwer uses this notion to define a real number λ such that neither ' $\lambda = 0$ ' nor ' $\lambda \neq 0$ ' can be demonstrated. This is exactly the same argument we saw in the previous section. It is also subject to the same problems, although one should keep in mind that by 1928, Brouwer had already given stronger arguments against classical logic. The end of "Mathematik, Wissenschaft und Sprache" makes clear that Brouwer's reason for discussing fleeing properties is not to give a decisive argument against classical mathematics, but only to show how intuitionism diverges from it. After defining λ , he mentions a number of its uniquely

intuitionistic properties (and those of other mathematical objects) before summarizing his discussion.

These examples will make clear that intuitionism has far-reaching consequences for mathematics. In fact, if the intuitionistic insights prevail, then considerable portions of the previous mathematical edifice must collapse, and new portions must be erected in an utterly new style. And the parts that remain stand in need of thorough reconstruction. (*ibid.*, p. 1185)

Fleeting properties make another appearance in an even later lecture, “Historical Background, Principles, and Methods of Intuitionism” (Brouwer 1952). Once again, Brouwer talks about mathematics as “an essentially languageless activity of the mind having its origin in the perception of a *move of time*.” (*ibid.*, p. 1200, original emphasis) Here, fleeting properties *per se* are given as weak counterexamples, rather than being used to define real numbers. Much of the lecture, however, is devoted to a proof of the Uniform Continuity Theorem, which can be used to give a strong counterexample to the *tertium non datur*. If this lecture contains a powerful challenge to classical mathematics, it is by way of this strong counterexample, not the philosophical discussion that precedes it.

We mention these two later appearances of the philosophical argument to point out that, although Brouwer never abandoned it (or his general philosophical views, for that matter), it became less important as his career developed. By the later 1920s, and certainly by the 1950s, Brouwer was giving mathematical arguments against classical logic as part of his general project of reconstructing mathematics to conform to intuitionistic principles. The philosophical arguments still appear, but they do not play a starring role.¹⁰

¹⁰We should note that, although “Mathematik, Wissenschaft und Sprache” consists almost exclusively of philosophical discussion, Brouwer delivered a second lecture, “Die Struktur des Kontinuums” (Brouwer 1928) to the same conference. This lecture is concerned with real analysis and, except for a short historical introduction, is entirely mathematical in content.

4.5.2 Heyting's Quasi-Empirical Mathematics

Whereas Brouwer developed his philosophical views early in his life and stuck with them, Heyting took a more pragmatic approach to the foundations of mathematics. As we have seen (Chapter 1, §2), the argument against classical logic in his 1930 Königsburg lecture draws on phenomenological methods. His attempt to articulate the meanings of the logical constants through the BHK interpretation resembles the work of Tarski. Sometimes, as in his (1966), he sounds like a card-carrying philosophical disciple of Brouwer. The book begins with a dialogue between advocates of competing philosophies of mathematics, and Int, the intuitionist, defends his position with explicit references to Brouwer.

But it was Brouwer who first discovered an object which actually requires a different form of logic, namely the mental mathematical construction. . . . [Brouwer's program] consisted in the investigation of mental mathematical construction as such, without reference to questions regarding the nature of the constructed objects, such as whether these objects exist independently of our knowledge of them. (*ibid.*, p. 1)

In the dialogue, Int's complaint about classical mathematics is that it is "metaphysical." That is, it presupposes that mathematical objects exist independently of human mental activity; according to Int (and Heyting, we may suppose), this underlies the classical mathematician's use of the *tertium non datur* when applied to infinite collections. Intuitionists, by contrast, make no such assumption, so their mathematics is free of metaphysical commitments.

Brouwer did seem to think that mathematical entities have their existence in virtue of being constructed. Heyting's view, by contrast, is weaker, since it seems to allow that Platonism about mathematics could be true, but that this is irrelevant to mathematical reasoning.¹¹ In other parts of the dialogue, however, Heyting seems to

¹¹So interpreted, Heyting's view is in tension with the strong counterexamples that prove the invalidity of the *tertium non datur* when applied to infinite sets. Heyting must have been aware of these counterexamples; it is curious, however, that his book contains no explicit mention of them even though he proves, e.g., the uniform continuity theorem in §3.4.3.

extend Brouwer's views even further than the master would want to take them.

Intuitionistic mathematics consists, as I have explained already to Mr. Class, in mental constructions; a mathematical theorem represents a *purely empirical fact*, namely the success of a certain construction. [*ibid.*, p. 8, emphasis added]

Brouwer never suggests that mathematics is an empirical matter. Insofar as he advocates psychologism, he has in mind the transcendental psychology of the idealists, not the empirical psychology that Heyting seems to embrace. In fact, Heyting's empiricism about mathematics is too strong to be compatible with intuitionism. Let n be some incomprehensibly large natural number on the order of, say, Graham's Number. The intuitionist is happy to assert that n is either prime or composite. After all, being prime is a decidable property of natural numbers. Nevertheless, the mental constructions that would be required to show that n is either prime or composite are such that nobody will ever carry them out. This is because, on the one hand, there is not enough time to run a decision procedure to determine whether n is prime. On the other hand, any inference from 'all natural numbers are prime or composite' to ' n is prime or composite' requires an additional premise, namely, ' n is a natural number'. This latter statement, on Heyting's view, would have to reflect another empirical fact, e.g., that somebody successfully counted to n . Again, the magnitude of n makes this impossible. Clearly, then, Heyting's empiricism needs to have room for idealization, but if we allow this, then mathematics cannot be wholly empirical.

4.6 Evaluating Brouwer's Argument

We are forced to conclude that Brouwer's philosophical argument for intuitionism is not successful. In his 1908 paper, Brouwer does argue cogently against Hilbert's Axiom of Solvability. Looking back, we can identify results like Gödel's Incompleteness Theorems and Church's proof of the undecidability of first-order logic as victories for

Brouwer and the other champions of the *Ignorabimus* thesis and find anticipations of these in Brouwer's paper (not to mention the work of Paul du Bois-Reymond).¹²

To reiterate the point made at the end of §4.1 above, however, it would be a mistake to think that Brouwer thereby succeeded in demonstrating the invalidity of any laws of classical logic. His claim that Hilbert's axiom entails the *tertium non datur* is unconvincing. Perhaps one who is sympathetic with Brouwer's philosophy would be willing to accept this equivalence, but intuitionism is in trouble if accepting it requires such sympathy.

Thus, followers of Brouwer who emphasize the philosophical argument do intuitionism a disservice. Instead of struggling to justify Brouwer's idealism, Heyting's empiricism, or the use of weak counterexamples, intuitionists should seek out other arguments for their positions. Some of the best of these can be found in the mathematical arguments Brouwer began giving in 1918, and we turn now to these.

¹²Wang (1987: p. 88) reports an interesting anecdote about Brouwer's reaction to Gödel's theorems.

In the spring of 1961 I visited Brouwer at his home. He discoursed widely on many subjects. Among other things, he said that he did not think G's incompleteness results are as important as Heyting's formalization of intuitionistic reasoning, because to him G's results are obvious (obviously true).

Chapter 5

Brouwer's Mathematical Arguments for Intuitionism

We have seen that the weak counterexamples discussed in the previous chapter are poorly named: by themselves, they are not convincing counterexamples to laws of classical logic. In order to establish the invalidity of, e.g., the *tertium non datur*, weak counterexamples need to be supplemented with other anticlassical assumptions. The Dummettian proof-conditional semantics does not justify any such assumption. The most it shows is that, if ϕ is a sentence expressing an unsolved problem, then we cannot now assert either ϕ or $\neg\phi$. Absent a proof that a universal method for solving mathematical problems would be absurd, semantic antirealism does not show that $\neg\forall\phi(\phi \vee \neg\phi)$.

Intuitionists claim to have proofs of this last claim. Obviously, these proofs are not trivial or obvious; if they were, classical logic would have lost its privileged position long ago. They are worth studying in detail, however, because they provide so-called strong (which is to say, genuine) counterexamples to classical logic and because they involve results outside of logic alone that help to distinguish intuitionism from both classical mathematics and other varieties of constructivism. In this chapter, we will

study two sources of weak counterexamples, both of which appear in Brouwer’s own work. The first is Brouwer’s Uniform Continuity Theorem, according to which every total function on the real numbers is uniformly continuous. The second is the theory of the Creative Subject.

5.1 The Uniform Continuity Theorem

Brouwer’s Uniform Continuity Theorem (UCT) is perhaps the zenith of Brouwer’s work in the 1920s, the most creative and productive decade of his career. The first proof of it appears in (Brouwer 1924), but the best known presentation is (Brouwer 1927). This is partly because the latter was written in German, a *lingua franca* of mathematics in the 1920s, while the former originally appeared in Dutch (although a German translation appeared in the same year). Also, an English translation of the relevant part of the 1927 paper was included in the influential collection (van Heijenoort 1967), cementing its status as the definitive presentation of the proof.

Once Brouwer achieved his result, he continued to give proofs of it in his later writing, e.g., (Brouwer 1954). Other textbooks and monographs on intuitionistic mathematics invariably discuss it as well.¹

In this section, we will look both at the UCT and the details of Brouwer’s 1927 paper in which he proves it. The details are important because, as we will see, one does not need the full UCT to generate strong counterexamples to classical logic. This is not to say that the UCT is superfluous or should be excised from intuitionistic mathematics. This would be the case only if the chief end of intuitionism was to antagonize classical mathematics. It is true that Brouwer’s early work, as well as some of the views of proto-intuitionists like Poincaré, was motivated by perceived flaws in the dominant mathematics of the day. By the 1920s, however, Brouwer was

¹See Heyting (1966: §3.4.3), Bridges and Richman (1987: pp. 110ff.), and Dummett (2000: §3e6). Bishop mentions the theorem in (1967: p. 74), but dismisses it as depending on “extra-mathematical considerations.”

engaged in his full-scale intuitionistic reconstruction and it was the classicists, particularly Hilbert, who were on the defensive. While it is true that the full UCT is not necessary to construct strong counterexamples, it is nevertheless a profound result of intuitionistic analysis

5.1.1 Strong Counterexamples Based on the UCT

The UCT itself says that every totally defined function from the set of real numbers into itself is uniformly continuous. It is important to remember that the definition of uniform continuity in play here is the usual one familiar from classical analysis. A function $f : A \rightarrow B$ on metric spaces A and B is uniformly continuous just in case

$$\forall \epsilon > 0 \exists \delta > 0 \forall x \forall y (|y - x| < \delta \rightarrow |f(y) - f(x)| < \epsilon),$$

where x and y range over members of A and ϵ and δ are real numbers.

Once the UCT is established, we can construct strong counterexamples to the *tertium non datur*. Consider the following function $f : \mathbb{R} \rightarrow \mathbb{R}$ defined by

$$f(x) = \begin{cases} 1 & x = 0 \\ 0 & x \neq 0 \end{cases}$$

Assume also that the *tertium non datur* holds. From this assumption, it follows that

$$\forall x \in \mathbb{R} (x = 0 \vee x \neq 0).$$

So, given our assumption, f is a total function on \mathbb{R} . Therefore, by the UCT, f is uniformly continuous. Nevertheless, f is discontinuous at 0. Hence, we have a

contradiction. So, by *reductio ad absurdum*,

$$\neg \forall x \in \mathbb{R}(x = 0 \vee x \neq 0),$$

and it thereby follows that

$$\neg \forall \phi(\phi \vee \neg \phi).$$

It is worth pointing out that the definition of f is perfectly acceptable to an intuitionist and satisfies the definition of a function. In general, intuitionists find nothing objectionable about the existence of discontinuous functions on the reals. The lesson to draw from the argument just given is that f is not a *total* function, i.e., it is not defined on every element of its domain.

It would be a mistake to conclude that our result about f shows that intuitionistic analysis has no room for the notion of a point of the continuum. This misleading idea is sometimes encouraged by off-the cuff remarks in writings on intuitionism.

Every part of a continuum is itself a continuum. This means that if one keeps dividing a continuum, one never arrives at something that is not continuous, not even ‘ideally’ or ‘in the limit’. (van Atten 2004: pp. 31-32)

In the paragraph preceding the one in which this passage appears, classical definitions of the continuum (e.g., those of Dedekind and Cantor) are criticized for being “atomistic.” This is a bizarre objection, since Cantor’s definition of the continuum in terms of Cauchy sequences is the standard one used in intuitionistic analysis.² If Cantor’s definition atomizes the continuum and this is objectionable, then the objection applies to intuitionistic analysis as well. As far as the mathematics goes, however, intuitionists are happy to talk about points of the continuum. Indeed, (Brouwer 1927) opens with a precise mathematical definition of “point of the linear continuum” in

²For that matter, there is nothing intrinsically classical or objectionable about Dedekind’s definition in terms of sets of rational numbers, although the intuitionist denies that this is equivalent to the Cantorian definition.

terms of Cauchy sequences of rational intervals.³

The strong counterexample just given can be used to obtain other important intuitionistic results. If we add a double negation to the first condition in the definition of f , we can obtain a counterexample to the law of testability. We can also establish the invalidity of double negation elimination. Since

$$\forall\phi\neg\neg(\phi \vee \neg\phi)$$

is valid, the validity of double negation elimination would allow us to infer the *tertium non datur*, which we have just seen to be invalid.

If we apply the same argument to $\chi_{\mathbb{Q}}$, the characteristic function of the rational numbers over the reals, we can show that

$$\neg\forall x \in \mathbb{R}(x \in \mathbb{Q} \vee x \notin \mathbb{Q}).$$

The UCT also allows us to obtain a result that Brouwer mentions in footnote 10 of his 1927 paper: the indivisibility (*Unzerlegbarkeit*) of the continuum. Let A be a collection of sets A_i such that the following hold.

1. $A_i \subset \mathbb{R}$ for each i ,
2. $\bigcup_i A_i = \mathbb{R}$, and
3. for any $A_i, A_j \in A$, $A_i \cap A_j = \emptyset$.

Then there is some i such that $A_i = \mathbb{R}$. This follows from the UCT because, were A to contain some inhabited proper subset A_i of \mathbb{R} , then the characteristic function of A_i on \mathbb{R} would be a total, discontinuous function on \mathbb{R} . Indeed, the proof that not every

³Bell (2008) catalogs demands that continua not be atomized, none of which explain why the Cantorian definition is objectionable. Worries about atomizing the continuum go back at least to Aristotle. Later, we will see that such worries can be used to motivate certain foundational principles for intuitionistic mathematics. (See Chapter 6, §2.3.) This is not the same as motivating the rejection of the very definitions that intuitionists use to construct the real numbers.

real number is either rational or irrational is a special case of the *Unzerlegbarkeit* theorem.

5.1.2 Weaker Premises for Strong Counterexamples

The reader may have noticed that the strong counterexample given in the previous section did not need the full-strength Uniform Continuity Theorem. The proof exploits f 's discontinuity at 0, not its failure to be uniformly continuous. Thus, weaker notions of continuity would suffice for the proof to succeed. Indeed, Brouwer distinguishes three different kinds of continuity in his 1927 paper. Uniform continuity is one of these. A second is what he calls positive continuity, which is defined in the familiar way:

$$\forall \epsilon > 0 \forall x \exists \delta > 0 \forall y (|y - x| < \delta \rightarrow |f(y) - f(x)| < \epsilon).$$

(Continuity at a point p is defined by fixing the value of x to be p .) Finally, there is negative continuity, the definition of which is obtained by inserting a double negation into the consequent of the conditional in the definition just displayed. (Of course, this makes negative and positive continuity equivalent classically, but not intuitionistically.)

A discontinuous function fails to satisfy any of these definitions. Thus, since the function f above is discontinuous at 0, it fails even to be negatively continuous, let alone uniformly continuous. So, it is noteworthy that, in the portion of (Brouwer 1927) translated in (van Heijenoort 1967), the first main result Brouwer proves is that every total function on the real numbers is *negatively* continuous. This result appears before the Fan Theorem, which is the second main result of Brouwer's paper, and the Uniform Continuity Theorem, which is the third main result.

We mention this because, so long as our concern is to develop strong counterexamples to laws of classical logic, there is no need to engage with the Byzantine proof

of the Uniform Continuity Theorem. The divergence with classical mathematics must come earlier in the game, so to speak. We reiterate that this does not mean that the UCT and its proof should be dismissed, since intuitionism is much more than a thorn in the side of classical mathematics. The proof of the UCT is of intrinsic interest. Furthermore, other kinds of constructive mathematics reject the UCT, so it stands as a nice distinguishing feature of intuitionism.

Still, our chief interest is in how an intuitionist might defend her position to a classical mathematician. If she is going to use a strong counterexample based on intuitionistic analysis, a result like the Negative Continuity Theorem (NCT) will do the job. So, it is worth taking a closer look at the premises used in the proof of the NCT.

5.1.3 The Weak Continuity Principle

In their monograph on intuitionistic analysis, Kleene and Vesley (1965: pp. 69-70) state what they call Brouwer's Principle for Numbers.

Suppose that, to each choice sequence $\alpha(0), \alpha(1), \alpha(2), \dots$ a natural number b is correlated. . . . Since intuitionistically choice sequences are considered as continually growing by new choices rather than as completed, this correlation can subsist intuitionistically only in such a manner that at some (finite) stage in the growth of the sequence $\alpha(0), \alpha(1), \alpha(2), \dots$ the correlated number b will be determined (effectively). That is, intuitionistically the b must be determined effectively by the first y choices $\alpha(0), \dots, \alpha(y-1)$ of α for some y (depending in general on these choices).

This seems to be the first explicit formulation of what has come to be called the Weak Continuity Principle (WCP). Brouwer himself never seemed to give it a special name or status, although it does make appearances in his work.

For, since *the natural number in question has to be known for each arrow of K at one of its nodes*, the nodes yielding this knowledge constitute a species of nodes which each arrow of K is bound to meet, and which therefore is a crude bar $C(K)$ of K . (Brouwer 1954: p. 15, emphasis added)

Another passing use of the WCP prior to its baptism by Kleene and Vesley appears in Heyting's proof of the Fan Theorem in his monograph on intuitionistic mathematics.

As $f(d)$ must be calculable, its value must be determined by a finite number of the components of d . (Heyting 1966: p. 43)

The WCP usually makes its appearance in intuitionistic proofs of the Fan Theorem, but it (or some other nonclassical principle) must also be used in the proof of the UCT from the Fan Theorem; otherwise, we would have a classically valid demonstration of the UCT, since the Fan Theorem is classically acceptable. Besides this, Veldman (1982) shows that the WCP alone can be used to demonstrate the (not necessarily uniform) continuity of total functions on the unit continuum, which is enough to obtain strong counterexamples to classical logic.

Indeed, the WCP alone can be used to give a strong counterexample. The principle concerns assignments of natural numbers to infinite sequences of mathematical objects. For simplicity's sake, we can assume that the latter objects are all natural numbers. Let Σ be the collection of these sequences and the relation $A : \Sigma \rightarrow \{0, 1\}$ be defined by

$$A(\sigma) = \begin{cases} 0 & \text{iff } \forall n \in \omega. \sigma(n) = 0 \\ 1 & \text{otherwise} \end{cases}$$

The WCP says that the assignment of 0 or 1 to a sequence $\sigma \in \Sigma$ must be determined on the basis of a finite initial segment of σ so that any sequence with the same initial segment will be assigned the same value. Let y denote the length of this finite segment and let σ_1 and σ_2 be sequences such that $\sigma_1(n) = \sigma_2(n) = 0$ for $n < y$ while $\sigma_1(y) = 0$ and $\sigma_2(y) = 1$. By the WCP and the definition of A , $A(\sigma_1) = A(\sigma_2) = 0$, but σ_2 is not the constantly 0 sequence. This contradicts the definition of A .

This shows that, if we denote the constantly 0 sequence by $\langle 0 \rangle$, then

$$\neg \forall \sigma \in \Sigma (A(\sigma) = \langle 0 \rangle \vee \neg A(\sigma) = \langle 0 \rangle),$$

contrary to the *tertium non datur*.

The classical mathematician who wishes to challenge the strong counterexamples that emerge from intuitionistic analysis would do well to focus his attack on the WCP. After all, it is the engine that drives the other results we have discussed. In particular, the classical mathematician is likely to object to the demand that relations between natural numbers (or other mathematical objects) and infinite sequences be determined on the basis of finite initial segments of those sequences. We will consider two possible arguments for this requirement, one based on Brouwer's philosophy of mathematics, a second based on Dummettian semantic antirealism.

A Brouwerian Argument for WCP

Veldman (2000: p. 2) gives a helpful summary of how one might argue for WCP on Brouwerian grounds:

if α [an infinite sequence] is coming into existence step-by-step and we calculate a number m suitable for α , the construction of this number m will be completed when only finitely many values of α have been decided upon. The number m will be suitable not only for α itself but for every infinite sequence β that has these first finitely many values the same as α .

According to Veldman, the Principle is “a natural axiom, born out by experience” (*ibid.*, p. 3), but this attitude is probably not widespread. Nevertheless, the argument he gives does cohere nicely with Brouwer's ontological constructivism about mathematics, which we discussed in the previous chapter.

Recall that, for Brouwer, mathematics is a creation of the human mind and is founded on our *a priori* intuition of time. Furthermore, the subject who constructs mathematics has always experienced only finitely (albeit increasingly) many temporal instants. The WCP applies when we consider an assignment of mathematical objects to infinite sequences. If such an assignment is going to be constructed, this has to be done in time, but at any point, only finitely many values of any given sequence have

been constructed.

So, if the subject sets about assigning a particular object to a sequence, it might seem (but see below) that all he has to go on is a finite initial segment of the sequence. For instance, he might assign 0 to a finite sequence σ . The sequence, however, is supposed to be infinite, so eventually it will be extended to a longer one. Indeed, σ might be extended multiple times, giving rise to distinct sequences σ_1 and σ_2 that share a finite segment. Still, σ , understood as an infinite but incomplete sequence, has already been assigned 0, so both σ_1 and σ_2 , as well as any other sequences that begin with σ , will be assigned 0.

The key to this argument is the claim that an infinite sequence is never completed. Without this, the mathematical subject could, as it were, wait around to construct his assignment once all of the sequences are finished. It is precisely here, however, that the argument fails. The WCP is supposed to apply to assignments of objects to *all* infinite sequences. Among these are sequences that can be fully described by finitely specified laws for their construction: the constantly 0 sequence, for example, or the sequence of natural numbers starting with 4. In contrast to these, we have the so-called lawless sequences, which are generated by more or less random processes like throwing dice or exercising one's own free will.

If we accept the Brouwerian philosophy of mathematics, then the validity of the WCP for lawless sequences is plausible, but this will not suffice to obtain the major results of intuitionistic analysis. These require that the WCP apply to *all* sequences, including those that can be given fully by a law for their construction. There is no reason to think that the WCP is valid for the latter. Even if we insist that such sequences are never completed (as Brouwer sometimes suggests), their finitely specified laws contain all of the information required to define functions on them that need not be determined by finite initial segments. Thus, if Σ is a collection of entirely determinate sequences, the WCP will not hold for relations on Σ . *A fortiori*, it will

not hold in general.

It is not open to the Brouwerian to reply that choice sequences are necessary to develop an adequate, constructively acceptable theory of the continuum. This seems to have been Brouwer's initial motivation for introducing choice sequences into his ontology.

... one might fear that intuitionist mathematics must necessarily be poor and anaemic, and in particular would have no place for analysis. But this fear would have presupposed that infinite sequences generated by the intuitionist self-unfolding of the basic intuition would have to be fundamental sequences, i.e., predeterminate infinite sequences which, like classical ones, proceed in such a way that, from the beginning, the m th term is fixed for each m . (Brouwer 1952: p. 142)

He goes on to discuss what he calls the "Second Act of Intuitionism," part of which is the introduction of

infinitely proceeding sequences p_1, p_2, \dots , whose terms are chosen more or less freely from mathematical entities previously acquired. (*ibid.*, original emphasis)

In fact, Brouwer's worries proved to be unfounded. Bishop's claim to constructivistic fame is his success in developing a constructively acceptable theory of analysis, one that happens not to use the notion of a Brouwerian choice sequence or the WCP. Despite his own objections to Brouwer's "metaphysical" speculations, Bishop's success does not mean that the theory of choice sequences should be excised from constructive mathematics entirely. It does show, however, that it cannot be justified by the need for a constructive theory of analysis.

A Dummettian Argument for WCP

As we have seen, the BHK interpretation alone does not seem to entail the invalidity of the *tertium non datur*. The status of classical logic with respect to the BHK semantics depends on what background assumptions one adopts. In particular, a good deal

depends on how we understand the term ‘effective’ in the clause for the universal quantifier. This is because a statement of the *tertium non datur* is a universal sentence; a refutation of the principle requires that the assumption of a single *effective* method to solve any mathematical problem is contradictory. Tennant (1997: §7.5) shows that, if we explicate ‘effective’ by ‘recursive’, then we can refute the BHK reading of the *tertium non datur*. On the other hand, if we introduce an omniscient being into our metatheoretic ontology and include asking questions of this being among the effective methods available to us, then the validity of the *tertium non datur* will follow.

Thus, the consequences for classical logic that follow from the BHK semantics depend in large part on what other assumptions are in the background. If we can show that the WCP is a consequences of the BHK interpretation with a plausible set of background assumptions, this will be a point in favor of the idea that Dummett has uncovered the hard core of intuitionistic mathematics.

To facilitate the discussion, it will help to have a statement of the WCP in the language of formal first-order logic.

$$\forall\alpha\exists nA(\alpha, n) \rightarrow \forall\alpha\exists m\exists n\forall\beta(\bar{\beta}m = \bar{\alpha}m \rightarrow A(\beta, n)). \quad (\text{WCP})$$

Here, α and β range over infinite sequences of natural numbers; m and x range over natural numbers; $\bar{\alpha}m$ refers to the m -length initial segment of α (*mutatis mutandis* for $\bar{\beta}m$).

Dummett’s argument for the WCP proceeds on the basis of what he claims is a basic tenet of intuitionism.

In intuitionistic mathematics, all infinity is potential infinity ... any infinite sequence, whether wholly determined in advance or not, must be taken as ‘in process of growth’; that is, we must not regard it as something all of whose terms can be surveyed. (Dummett 2000: p. 45)

As we might expect, Dummett understands this in terms of meaning.

The intuitionist holds that the expressions of our mathematical language must be given meaning by reference to operations which we can in principle carry out... The platonist, on the other hand, believes that they can be given meaning by reference to operations which we cannot even in principle carry out, so long as we can conceive of them as being carried out by beings with powers which transcend our own. These are deep questions in the theory of meaning which we cannot pursue further here. (*ibid.*, p. 43)

Among the deep questions include those concerning semantic antirealism. In particular, we should ask whether the BHK semantics gives us a reason to adopt the WCP.

According to the BHK interpretation, we can assert the antecedent of the WCP just in case we have a construction that will transform any infinite sequence α of natural numbers into a proof of ' $\exists n(A, n)$ '. The latter proof consists of a definite number n and a proof that ' $A(\alpha, n)$ ' holds. So, if we can assert the antecedent of the WCP, then we have an effective method for producing, for each sequence α , the number n to which it is related by A .

This alone is not enough to justify the WCP. We need the further assumption that the method given by the antecedent of the statement of the WCP can operate only on finite entities. After all, the classical mathematician can maintain that there is a method for determining the number than n assigns to each infinite sequence: ask a hypothetical omniscient entity that can comprehend at once the entire infinite collection $\mathbb{N}^{\mathbb{N}}$. We grant that this is not in the spirit of the BHK interpretation or the usual understanding of constructive mathematics, but a BHK-based argument for the WCP must somehow rule such a "method" out.

Dummett seems to be aware of this when he attempts to characterize the treatment of infinity proper to intuitionism.

In intuitionistic mathematics, all infinity is potential infinity: there is no completed infinite... The intuitionist holds that the expressions of our mathematical language must be given meaning by reference to operations which we can in principle carry out. (*ibid.*, pp. 41, 43)

Thus, we are back to the debate about realism and antirealism understood in Dummettian terms. The WCP is justified, on Dummett's view, because it represents the only meaning of quantification over a totality like $\mathbb{N}^{\mathbb{N}}$ that does not attribute superhuman cognitive abilities to us.

Thus, the Dummettian case for the WCP ultimately stands or falls with the more general case for semantic antirealism. Since we have already discussed this in Chapter 3, we will not linger on it a great deal more. We should, however, note a general objection to semantic antirealism that is especially pertinent here. Briefly, the objection is that, if Dummett's argument for antirealism is a good one, then the degree of mathematical revision required is greater than that called for by the intuitionists; we should endorse a finitistic approach to mathematics that excises both actual and potential infinities and places an upper bound on the size or complexity of the objects and proofs dealt with in mathematics.

A version of this objection is developed by Wright (1982). In effect, the objection proceeds by noting that the Dummettian position places a good deal of weight on the notion of an ability to execute a process *in principle*. That is, it dismisses what Russell famously called "mere medical impossibilities," such as limitations on the human lifespan, as irrelevant for our understanding of mathematical statements. Thus, I understand and can assert a statement of the commutative law for integers,

$$\forall n \forall m (n + m = m + n),$$

even though the quantifier ranges over all integers, including those so large that I could never carry out calculations with them on paper or grasp them intuitively in my mind.

The finitist objects that on Dummett's own principles, there is an upper bound to the numbers with which one has learned to calculate. To put it another way, if

the meaning of a term is determined by the use of the same term, then what do we do about the fact that there is an upper bound to the size of the number terms that we are able to use? According to the objection, Dummett can draw no principled distinction between the infinite and the incomprehensibly large finite. Thus, we should restrain our mathematics to deal only with entities small and simple enough to be understood in practice.

The response to this on the part of Dummett (1975c) is unsatisfactory for our purposes. He argues that any finitistic mathematics would ultimately be incoherent, but he does not explain why his own position is vindicated; if the Dummettian argument does entail finitism and finitism is incoherent, so much the worse for the argument. Tennant responds to the finitist's challenge by positing a modified form of the antirealist's recognition condition on understanding a statement.

The speaker's understanding, on this account, is conceived somewhat along the line of a (potentially unsurveyable) host of true dispositional conditionals, one for each aspect of the piece of discourse Π . . . We can have good grounds for believing that each of these conditionals is true of S , even while conceding that S is perforce unable, given the sheer size of Π , to pronounce on the correctness of all aspects of Π . (Tennant 1997: pp. 154-155)

Even here, however, there is recourse to a "potentially unsurveyable" host of conditionals. On the antirealist's own understanding of truth, these conditionals are true only if there is some recognizable proof of their conjunction or universal closure. If the collection of all of them is unsurveyable, then it is not clear how there could be such a proof; therefore, the finitist could respond by calling into question our ability to attribute understanding when the collection of conditionals is unsurveyable.

We bring this up here because the finitist's challenge is especially acute for any attempt to establish the WCP on antirealistic grounds. In many cases, the antirealist can block the challenge by appealing to our grasp of a finitary process. Thus, even though the commutative law for addition holds of numbers whose size is far beyond

anybody's ability to grasp directly, one learns to add by learning a simple algorithm for calculating the sums of any two numbers; the antirealist can appeal to a grasp of this algorithm in order to explain how one can know (in principle) how to add incomprehensibly large numbers.

The WCP, however, applies precisely when the objects in question outstrip the limits of easily-grasped algorithmic processes. *Prima facie*, the principle might seem to be exactly the kind of thing to which the antirealist should appeal in light of the finitist's objection, but since the quantifiers that appear in a statement of the principle range over the collections of *all* infinite sequences and *all* natural numbers, the finitist can still respond by calling into question our grasp of these infinite collections. Thus, even if the finitistic objection to Dummett's version of antirealism does not hold in general, there is still some reason to think that it will block an antirealistic argument for the WCP; in other words, the burden of proof is on the antirealist to show that his position is able to ground the full sweep of intuitionistic mathematics, including the theory of free-choice sequences governed by the WCP.

5.2 The Creative Subject

The other chief source of strong counterexamples in Brouwer's own work is found in his notion of the creative subject (CS).⁴ This first appears in (Brouwer 1948) in the course of an argument showing that negation cannot be eliminated from intuitionistic mathematics.⁵

As a first sketch of what Brouwer has in mind, notice that when we presented the Brouwerian argument for the WCP above, we did so in terms of a subject that brings

⁴This translates the Dutch *scheppende subject*. Brouwer (1952) uses 'creating subject' when writing and lecturing in English. As translations, either "creating" or "creative" is acceptable and the latter has become the standard one in the secondary literature.

⁵See (Griss 1946) for one contribution to the project of a negationless mathematics. Besides Griss, Hans Freudenthal and David van Dantzig are associated with this idea; other discussions include those by Heyting (1966: pp. 120ff.) and Troelstra and van Dalen (1988: p. 31).

the objects of mathematics into existence. Indeed, this seems to be what Brouwer has in mind in his applications of the CS, with one important qualification. In the case of a choice sequence, the generation is direct: at each stage, the subject chooses what the next item in the sequence will be. Brouwer allows that these choices might be restricted in various ways, e.g., to even numbers, but the point is that there is ultimately nothing other than the subject's choice that we can point to as an explanation for the values that the sequence takes.

In his later applications, however, Brouwer stipulates that the subject's generative activity proceeds in terms of solutions to unsolved mathematical problems. (In his 1948 reply to Griss, he gives Fermat's Last Theorem and a problem about the decimal expansion of π as examples.) Infinite sequences are still part of the picture, but the subject determines the values that the sequence takes in terms of whether a problem given by a statement ϕ has been solved or not. Such a sequence acts as a kind of record of research into ϕ . Among other things, this allows us to reduce *every* mathematical problem to a question of the values of a sequence of natural numbers—even a binary sequence if we want to keep things especially simple.⁶ For example, the sequence could be such that, so long as ϕ remains unsolved, the subject extends it by appending 0, and once ϕ is solved (if it ever is), the subject extends the sequence by appending 1. (If we wanted to distinguish between positive and negative solutions, we could require that the subject begin appending 2 if $\neg\phi$ turns out to be the case.)

So, although explicit mentions of the CS do not appear in Brouwer's work before 1948, he seems to have been using it implicitly for at least three decades. In short, Brouwer allows that infinite sequences can be generated in a variety of ways: fully determinate rules, partially restricted choices, random processes like rolling dice, sheer feats of will subject to no restrictions, and investigations into unsolved problems. It

⁶Readers familiar with the literature associated with Bishop's constructivism will recognize this as the source of the power of the limited principle of omniscience, which Bishop takes to be the dominant nonconstructive principle of classical mathematics. Cf. Bishop (1967: p. 9).

is worth noting that the last of these is neither fully random nor fully determinate. In general, we cannot say whether a proposition or its negation holds, nor can we predict when we will figure this out. In this respect, sequences based on the status of such propositions are not fully determined in advance. On the other hand, they are no more random than the activity of working mathematicians.

Our claim that the earlier use of choice sequences depends implicitly on the CS requires some evidence. In a lecture manuscript from 1927, Brouwer describes the second act of intuitionism as

the admission as a modality of the self-unfolding of the *primordial intuition of mathematics* not only of the assemblage of finite sequences of mathematical systems and of lawlike indefinitely proceeding sequences of mathematical systems pre-formed by induction, but also of the assemblage of sequences of mathematical systems proceeding indefinitely in complete freedom or in freedom subject to (possibly changing) restrictions. (van Stigt 1990: p. 483, emphasis added)

As early as (Brouwer 1905), it is clear that this primordial intuition belongs to a subject. Thus, as soon as Brouwer introduced sequences based on free choices into his mathematical ontology, he was implicitly introducing the CS. He just waited until much later to put it into print explicitly.

So much for exegesis. What should we say about Brouwer's actual use of the CS? Does he succeed in using it to refute laws of classical logic? There seems to be little reason for a classical mathematician to accept Brouwer's arguments in his (1948). There, the CS is used to define a real number ρ such that $\rho \neq 0$, but the most natural negation-free translation of this is not equivalent to it.

Of course, this does not spell doom for the negationless program, since there may be less natural negation-free translations of ' $\rho \neq 0$ '. From the classical mathematician's point of view, however, the problem is that, in defining ρ , Brouwer must appeal to an assertion ϕ such that both $\neg\phi$ and $\neg\neg\phi$ fail. In other words, he presupposes the invalidity of the law of testability. This is not fatal to his goal in (Brouwer 1948);

there, he is trying to show that negations cannot be eliminated from intuitionistic mathematics and so is allowed to presuppose the logical commitments of intuitionism. A classical mathematician, however, accepts the law of testability and denies that unsolved problems (i.e., weak counterexamples) refute classical logic. That is, she will refuse to accept that ρ is well-defined, since for her there is no untested proposition ϕ . This shows that, in general, counterexamples to classical logic based on the CS cannot be fundamental when it comes to convincing the unconverted. They must be supplemented with a reason to accept that numbers defined in terms of the experience of the CS are well-defined.

5.2.1 Formalized Creative Subject Theories

Although Brouwer himself never developed a robust theory of the creative subject, later authors have attempted to do so.⁷ In formulating the axioms of such a theory, there are two constraints. Obviously, one is that the theory must be consistent, both internally and with respect to the rest of intuitionistic mathematics. The second is that the theory should be strong enough to derive counterexamples to the *tertium non datur* and other laws of classical logic, perhaps using arguments like those in (Brouwer 1948).

What sort of principles might we introduce? Following Troelstra (1969), we will use the notation ‘ $\vdash_n \phi$ ’ to mean ‘the creative subject has evidence for the truth of ϕ at time n ’. Here, ϕ is a mathematical statement, and n ranges over natural numbers. (This means that we are treating time as proceeding in discrete instants.) The following three axiom schemata are frequently proposed in the literature.

$$\text{A1: } \vdash_n \phi \vee \neg \vdash_n \phi$$

$$\text{A2: } \vdash_n \phi \rightarrow \vdash_{n+m} \phi$$

⁷See Kreisel (1967), Myhill (1968), Troelstra (1969).

A3: $(\exists n \vdash_n \phi) \leftrightarrow \phi$

A1 says that, at any given time, the CS knows whether or not it has evidence for a proposition's truth at that time. Epistemological externalism notwithstanding, this seems plausible. A2 says, in effect, that the CS never forgets; if it obtains evidence for ϕ at some instant, it retains that evidence at all future instants. Here, it is important to keep in mind that the CS is an *idealized* mathematician. So long as we do, A2 also seems plausible.

A3 is more problematic. Clearly, one of the conditionals is true, namely

$$(\exists n \vdash_n \phi) \rightarrow \phi.$$

This just says that, if the CS ever obtains evidence for ϕ , then ϕ is true. Mathematical evidence is given by proofs, so this amounts to the trivial observation that only true mathematical statements have (correct) proofs. Surely nobody will doubt this.

What about the other conditional in A3, namely

$$\phi \rightarrow \exists n \vdash_n \phi?$$

This amounts to saying that, given any true statement, the CS will eventually prove it. One might worry that accepting this requires too much idealization. It is one thing to attribute eternal life and a perfect memory to the CS, but A3 looks like it requires attributing (potential) omniscience as well.⁸

Nevertheless, we will provisionally accept A3 and examine some of the consequences of the theory. Since the ' n ' in ' \vdash_n ' ranges over natural numbers, we can use the operator and a mathematical statement ϕ to define an infinite sequence σ of

⁸As Bishop (1967: p. 2) remarks, "If God has mathematics of his own that needs to be done, let him do it himself."

natural numbers.

$$\sigma(n) = \begin{cases} 0 & \neg \vdash_n \phi \\ 1 & \vdash_n \phi \end{cases}$$

Intuitively, this sequence records the CS's evidence for the truth of ϕ . If we think of n as representing numerically-indexed instants of time, then the sequence's entries are 0 as long as the CS has no evidence that ϕ is true. At the instant m when the CS obtains such evidence, $\sigma(m) = 1$. Since, by A2 above, the CS retains this evidence at every future instant, the sequence will also output 1 for every number greater than m . Brouwer used sequences like σ in many of his papers in the 1940s and 1950s.⁹

Note that, if the CS obtains evidence for $\neg\phi$ at some point, then σ becomes the constantly 0 sequence. One thing the CS cannot do is predict the future. As long as it never finds any evidence for either ϕ or $\neg\phi$, we find that

$$\neg(\exists n.\sigma(n) = 1 \vee \forall n.\sigma(n) = 0)$$

which contradicts classical logic. So, if we allow that a sequence like σ , defined in terms of the theory of the CS, actually defines a sequence, we obtain a counterexample to classical logic. We might worry, however, that allowing a definition like σ comes with less savory consequences. From the definition of σ , we have that

$$\exists n.\sigma(n) \neq 0 \rightarrow \phi.$$

Also, from axiom A3 above, we have that

$$\phi \rightarrow \exists n.\vdash_n \phi$$

⁹Normally, however, he used them to define real numbers, and so their outputs were more complicated because he had to guarantee that the sequences would converge. We do not need this guarantee here, so we can use 0 and 1 as possible outputs.

and, given the definition of σ the consequent of this is equivalent to

$$\exists n.\sigma(n) \neq 0.$$

So, by substitution, we have that

$$\phi \rightarrow \exists n.\sigma(n) \neq 0$$

and this, together with our first formula, entails

$$\phi \leftrightarrow \exists n.\sigma(n) \neq 0.$$

By existential generalization on this biconditional, we obtain what is known as Kripke's Schema.

$$\exists \alpha(\phi \leftrightarrow \exists n.\alpha(n) \neq 0) \tag{K}$$

where α ranges over sequences of natural numbers. Since ϕ was an arbitrary mathematical statement, K says that, for every such statement, there is a sequence “tracking” the statement's evidential status with respect to the CS. K is weaker than A3, and not just in the sense that the former follows from the latter but not vice versa. While A3 requires us to accept that the CS will eventually *prove* any true mathematical statement, K intuitively says only that the CS can investigate any statement, not that this investigation will ultimately result in a proof or refutation.

Still, there are reasons to think that the CS theory we have outlined here does not meet the constraints we mentioned at the beginning of this section. The first of these was a twofold consistency requirement. We want the theory to be internally consistent (i.e., to derive no contradictions) and to be consistent with central results of intuitionistic mathematics (e.g., the Continuity Theorems). It appears that our theory may fail on both of these counts.

With regard to the latter, van Dalen (1999: 310) points out that Kripke’s Schema entails the negation of a Continuity Principle on sequences

$$\forall\alpha\exists n.A(\alpha, n) \rightarrow \exists F\forall\alpha.A(\alpha, F(\alpha)). \quad (\text{CP})$$

This is stronger than the Weak Continuity Principle discussed earlier. (For this reason, Troelstra and van Dalen (1988: 212) suggest that we might call it the Strong Continuity Principle.) It’s stronger because it asserts the existence of a (continuous) functional F on sequences. For each sequence α , F tells us the length of the initial segment needed to assign n . All WC tells is that there is some such length or other, not that we can find a functional telling us what that length is.

Still, despite the added strength, some authors have suggested why an intuitionist might want to accept CP. Iemhoff (2008: §3.5) is one example. (In the following quote, we have changed some of the notation to match our conventions.)

Weak continuity does not exhaust the intuitionists’ intuition about the continuum, for given the weak continuity axiom, it seems reasonable to assume that the choice of the number m such that $\forall\beta \in \alpha(m)A(\beta, n)$ [i.e., the consequent of WC] could be made explicit. Thus $\forall\alpha\exists nA(\alpha, n)$ implies the existence of a continuous functional F that for every α produces the m that fixes the length of α on the basis of which n is chosen.

As it stands, this argument is simply an appeal to what seems reasonable, but we can try to strengthen it. For example, we might argue from WCP to CP by appealing to general constructivist principles governing the existential quantifiers in WCP. (This seems to be what Iemhoff has in mind.)

This isn’t the place to settle whether an intuitionist should accept CP, K, or neither. The unsurprising but important point is simply that, at a foundational level, an intuitionist is going to have to weigh her options and make choices about which principles to adopt.¹⁰

¹⁰The classical mathematician is in the same position, of course. Consider the debates about classical set-theoretic axioms entailing the Continuum Hypothesis versus those entailing its negation.

Let us turn now to the other side of our first constraint, namely, the internal consistency of a CS theory with Kripke's Schema. Troelstra (1969) points out that the theory we have formulated leads to an inconsistency, at least if we make the natural assumption that the CS proves *some* new statement at every temporal instant. Given this assumption, we can begin enumerating the statements proved by the CS at each stage: $\phi_0, \phi_1, \dots, \phi_n, \dots$

Now, let $L(\alpha)$ mean that α is a lawlike (i.e., fully determined) sequence of natural numbers and define a sequence β of natural numbers as follows.

$$\beta(n) = \begin{cases} \alpha(n) + 1 & \exists \alpha (\phi_n \leftrightarrow L(\alpha)) \\ 0 & \text{otherwise} \end{cases}$$

The first condition obtains at n just in case the CS proves at n that some sequence is lawlike. Troelstra asserts (*ibid.*, p. 106) without argument that β is a lawlike sequence. Niekus (1987: p. 441) does somewhat better. He suggests that we can motivate Troelstra's assertion via our axiom A1.

The reason for calling a CS-sequence (also called *empirical* sequence) lawlike, is presumably that, in interpreting the CS as an idealized mathematician with a definite description of the stages (acceptance of A 2.1 [our A1]), there does not seem to be an element of choice in the determination of its values.

We will return to this argument below, but first we should finish deriving the contradiction.

Since we are taking β to be lawlike, $L(\beta)$ is true. So, by A3, we can infer that $\exists n. \vdash_n L(\beta)$. Call this stage n_β . Then $\phi_{n_\beta} \leftrightarrow L(\beta)$. By the definition of β , this means that $\beta(n) = \beta(n) + 1$, a contradiction.

Troelstra suggests multiple possible responses to this result. One is to reject the idea that the CS proves only one conclusion at a time. Another is to modify our understanding of the \vdash_n operator. A third is to introduce a kind of hierarchy of types

(or, as he puts it “levels of self-reflection”) and an assumption similar to Russell’s Axiom of Reducibility. He doesn’t explore these in detail, however.

Another way to respond to the paradox is to deny that the sequence β is lawlike. Niekus’s argument for this is hardly convincing. There seems to be no determinate law telling us what the CS will prove at a given stage. Thus, although β might not be a completely lawless sequence, the CS is never in a position to know the value of $\beta(n)$ for an arbitrary n .

Absent a clear way out, Troelstra’s paradox should give us pause, especially combined with our other observations about the CS. Together, these show that developing a formal theory to model Brouwer’s idea is challenging. At the very least, it is hard to see why one would want to take a formal CS theory as foundational for intuitionism. When developing a foundation for mathematics, one usually aims for the simplest and most intuitively plausible basic principles. The theory of the CS meets neither of these conditions.

Chapter 6

A Mathematical Foundation for Intuitionism

6.1 Foundations and Axioms

To make clear the approach to mathematical foundations that we pursue in this chapter, it will be helpful to contrast our proposal with other items that fall under the “Philosophy of Mathematics” heading. At times, the philosophy of mathematics seems concerned primarily with ontological and epistemological questions that arise in light of our possession of mathematical knowledge. This knowledge presents a challenge to philosophers because it seems to be simultaneously

1. certain,
2. not obvious, and
3. not trivial.

The certainty of mathematics has to do both with its method (deductive argument and, in many cases, algorithmic procedure) and with its relative immunity to discon-

firming evidence (compared, e.g., with the natural sciences).¹ It is not obvious in the sense that even the best mathematician generally cannot look at an arbitrary mathematical statement and know right away whether it is true or false. It is not trivial both because it is not obvious and because it has myriad theoretical and practical applications.

The challenge is to explain how we can have knowledge with these three features. Indeed, philosophers sometimes use mathematical knowledge in order to motivate very general epistemological theories. For example, in the *Meno*, Plato argues for the theory of recollection using an example from geometry. So too, in the *Critique of Pure Reason*, Kant uses mathematical knowledge as a datum that both calls for explanation and shows that it is possible to have synthetic knowledge *a priori*. In other cases, philosophers have rejected this characterization of mathematical knowledge. Thus, Mill rejected 1 and maintained that mathematics is just an especially well-supported inductive science, while Carnap rejected at least part of 3 by insisting that mathematical statements are free of content.

6.1.1 Epistemology, Metaphysics, and Mathematics

It is striking that the ontological and epistemological questions raised by mathematical knowledge do not depend in any essential way on its *mathematical* character. There are other candidates for knowledge that exhibit the three features listed above. Thus, while the classic paper (Benacerraf 1973) raises epistemological questions pertaining to mathematics, the rejoinder by Katz (1998) concerns itself with logical and linguistic knowledge as well. What makes mathematics significant here is not that it presents philosophical puzzles *qua* mathematics, but that it serves as a case study for a number of philosophically interesting issues concerning, e.g., abstract objects,

¹Recall Frege's observation (1884: §9) that a chemist who mixes five units of one liquid with two units of another and winds up with six units will never conclude that $5 + 2 = 6$ but that some of the matter was lost in a chemical reaction.

epistemological certainty, and so on.

Our point is that the ontological and epistemological theses developed in light of the philosophically puzzling features of mathematics are independent of many of the details of the mathematical theories whose possibility they purport to explain. This is the case even for a much-discussed contemporary position in the philosophy of mathematics such as structuralism. As Shapiro, its chief proponent, describes it, structuralism is the thesis that “mathematics is the science of structure.”

The subject matter of arithmetic is a single abstract structure, the pattern common to any infinite collection of objects that has a successor relation, a unique initial object, and satisfies the induction principle. (Shapiro 2000: p. 258)

Other branches of mathematics study the structures appropriate to them. Furthermore, a structure is “the abstract form of a system,” a system being “a collection of objects with certain relations among them.” (*ibid.*, p. 259)

There are plenty of questions that can be asked about structuralism. For example, structures are supposed to be abstract entities. Given this, how do we have epistemic access to them? Structuralists are free to pursue this and similar issues, but the question we wish to ask here is whether we can give any precise mathematical content to the claims that they make. Are these structures mathematical entities of a certain kind? If so, can we prove mathematically that they exist? A natural candidate for a structure would be the kind of thing studied in model theory; on the other hand, since structures are the objects of all mathematical theories, this would mean that every branch of mathematics is really model theory in disguise. This is clearly not the case.

Or consider that many mathematical theories be interpreted within ZF and treated as the study of sets of a certain kind. This is similar to the claim that mathematics studies structures. Perhaps structures are kinds of sets. If so, where do they appear in the cumulative hierarchy? Structuralists do not seem to be concerned with these

questions; rather, their internal disputes are ontological and epistemological ones, special cases of the familiar debates about universals, abstract entities, etc.

While there is nothing wrong with metaphysics and epistemology *per se*, they usually proceed in isolation from mathematics, even when they purport to be studying mathematics. The kind of approach that structuralism represents in the philosophy of mathematics is removed from mathematics itself. We maintain that there is a place for philosophy that makes it continuous with mathematics and even capable of altering the way mathematics is done.

6.1.2 Pluralism

The idea that philosophy can have an impact on mathematics is perhaps not anathema today, but philosophers and mathematicians tend to regard it with suspicion. The general worry seems to be encouraged by two ideas:

1. new mathematical results (a new theorem, for example) are not *philosophical* achievements, but *mathematical* ones;
2. philosophers have no special authority when it comes to mathematics.

Both of these are clearly false. In the first case, one need only recall results like Gödel's Theorems or Church's Theorem, which are at least as important for their philosophical consequences as for their mathematical ones. That the second is false is clear if one consults a list of great philosophers, which will inevitably mention figures like Leibniz, Pascal, and Russell; it is only by attributing distinct philosophical and mathematical personalities to such luminaries that one can deny these philosophers mathematical authority.

Although claims 1 and 2 are false, they are widely accepted and understood jointly to entail that a philosopher who wants to have an impact on mathematics should just do some mathematics rather than trying to force his philosophical speculation on

mathematicians who have no special obligation to listen to the philosopher. This leaves room for the kind of ontological and epistemological inquiry discussed in the previous section, but it calls into question whether philosophical work can change mathematics itself. Such an attitude is expressed in no uncertain terms by David Lewis.

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. . . Can you tell them, with a straight face, to follow philosophical argument wherever it may lead? If they challenge your credentials, will you boast of philosophy's other great discoveries: that motion is impossible, that a Being than which no greater can be conceived cannot be conceived not to exist, that it is unthinkable that anything exists outside the mind, that time is unreal, that no theory has ever been made at all probable by evidence (but on the other hand that an empirically ideal theory cannot possibly be false), that it is a wide-open scientific question whether anyone has ever believed anything, and so on, and on, *ad nauseam*? (Lewis 1991: p. 59, original emphasis)

Lewis is challenging the idea that one should reject or reform mathematics because of a philosophical nominalism that denies existence to the abstract entities that mathematics purports to study, but the point can easily be generalized.

It is not so clear what Lewis's litany of counterintuitive philosophical theses is meant to show, since none of them has any obvious connection to mathematics, but presumably they are meant to support his claim that a philosophical challenge to the prevailing mathematics can be legitimately dismissed on grounds of presumption. He neglects to mention that Berkeley, the same philosopher who argued for the counterintuitive thesis that nothing exists outside the mind, also posed one of the more serious philosophical challenges to the prevailing mathematics of his day. Perhaps Berkeley should have been less presumptuous and stuck to his idealism and tar water, leaving the serious work of calculating with infinitesimals to the real mathematicians. For that matter, one wonders what Lewis would say about Brouwer, who rejected classical mathematics on philosophical grounds but waited to reconstruct it until he had

proved the invariance of dimension and thereby put classical topology on a sound footing.

In any case, the idea that philosophy can adjudicate between the various conflicting approaches to mathematics that are available is not a popular one today.

One response to this delimitation of the philosopher's task is mathematical pluralism. We mentioned pluralism in Chapter 3, but it is worth a more detailed discussion now. Strictly speaking, we should distinguish between logical pluralism and mathematical pluralism. Of course, intuitionism calls for a revision in both logic and mathematics, but it is possible to endorse the logical revision while minimizing the need for a mathematical one. This is the position adopted by Bishop and his followers, who advocate the use of intuitionistic logic but avoid asserting theorems that would contradict classical mathematics.

According to one statement of logical pluralism, “the notion of ‘*the* correct logic’ is simply a mistake, one which fails to take account of the purpose-relativity and language-relativity of logic.” (Hellman and Bell 2006: p. 68) The idea is that different canons of logical reasoning have their own uses, and we are free to pick and choose which one we will best suit our goals in a given setting. Beall and Restall (2006) suggest that our concept of logical consequence is not sufficiently precise to pick out a single logic (classical, intuitionistic, or relevance) as the correct one, and therefore we should avail ourselves of whichever kind of consequence aligns best with our purposes, which will generally vary from case to case.

Mathematical pluralism is similar, but it applies to the whole of mathematics and not just to logic. For example, there may be times when we are especially interested in the computational content of a given mathematical result. In these cases, we would do well to stick to constructive mathematical theories, in which every result is guaranteed to have computational content. In other settings, full-strength classical mathematics or some other alternative may be more appropriate.

So understood, pluralism is not the rather weak claim that it is not *yet* settled which of the various alternative logics and mathematics is the correct one, and until it is, we should use whatever we need to accomplish our other goals. Also, it is not merely the position that figuring out which logic and mathematics is the correct one requires taking account of applications in, e.g., physics. A commitment to pluralism amounts to an endorsement of a pragmatic theory of truth for logical and mathematical statements: on this instrumentalist view, there is nothing more to the truth, or even the meaning, of such a statement than that it helps us to accomplish the tasks we have set for ourselves. Otherwise, it seems that we should concede that there is one true logic, since it cannot be the case that the *tertium non datur* or *ex falso quodlibet* are both valid and invalid. We might take this logic to be one of the available options, we might take it to be something that is yet to be discovered, or we might take it to be the intersection of all the candidate logics available, but it is not open to us to accept conflicting accounts of logical consequence without making this acceptance a matter of pragmatic value.

Beall and Restall (*ibid.*, p. 92) consider the possibility that, on their view, the correct logic is the intersection of all candidate logics. Their response is twofold. First, they observe that this would probably lead to the conclusion that the correct logic is the one in which the only valid argument is from ϕ to ϕ and that this is objectionable. This begs the question against somebody who denies that classical, intuitionistic, relevant, etc., logics are all equally good codifications of logical consequence.

Second, they observe as an example that some arguments are valid according to classical second-order logic but invalid according to classical first-order logic, but they insist that these both capture legitimate consequence relations and therefore the argument is both valid (in second-order logic) and invalid (in first-order logic). We reject the premise of this argument: the two consequence relations are not equally legitimate, and to think that they are is to think that we have reached the end of

history in our investigation of logical consequence.

If one wants to adopt pluralism and endorse a pragmatic theory of truth for logic and mathematics, so be it, but then one should be ready to tell mathematicians that they are not in the business of discovering truths in the ordinary sense. The contrary view that we endorse treats mathematics and logic like other sciences whose practitioners' task is to investigate and discover truths. If two distinct mathematical or logical theories disagree about what these truths are, then it is incumbent on us as inquirers to do further work in order to determine which theory, if either, is correct.

6.1.3 Naturalism

Another version of the view that philosophers should not interfere with mathematics itself finds expression in naturalism. One version of this, which is associated with Quine and has been endorsed more recently by Maddy, makes the natural sciences, perhaps along with mathematics itself, the ultimate arbiter of what logical and mathematical truths or frameworks we should endorse. (Maddy 2000; 2007, Quine and Ullian 1970). As will become clear throughout this chapter, our own attitude toward the foundations of mathematics resembles naturalism in certain respects. In particular, we agree that the best route to a foundation for intuitionism is not through general metaphysics or epistemology.

Here, we merely note that one variety of naturalism, according to which the natural sciences alone, perhaps even physics alone, are the ultimate arbiters of which mathematics we should accept, goes too far and is irrelevant for our present purposes. It is irrelevant because there seems to be no scientific application that would privilege classical mathematics over intuitionistic mathematics or *vice versa*. All of the evidence that supports the use of classical mathematics in the sciences supports constructive mathematics as well. The chief argument in favor of this is based on the remarkable past success of scientific applications of mathematics. There seems to

be no reason to think that any explanation of this would depend on the technical details of the mathematical theories in question (in particular, whether they conform to classical or constructive standards).

Naturalism goes too far because it ties mathematics too closely to the natural sciences rather than respecting its status as an autonomous discipline. For example, the more extravagant parts of set theory, such as that concerned with large cardinals or even sets of very large cardinality within the cumulative hierarchy, seem not yet to have any special scientific application. Thus, they do not now form an essential component of our best overall theory of the world. Depending on one's naturalism, this could have various consequences. An ontological naturalist, who holds that we should attribute existence to mathematical entities only when they are indispensable for the sciences, would insist that we should refrain from believing that large cardinals exist. Methodological naturalism of a certain kind, one maintaining that only the method of the sciences is somehow authoritative for mathematics, are hard-pressed to say what could be valuable about research so detached from experience and empirical science.²

Sometimes, naturalists will point to a particular case where a mathematical theory developed independently of science became important for science later. This is the case with group theory, which initially seemed like a purely mathematical playground but turned out to have important applications in physics. If this is to be the model for a general policy, however, it would help if it was a more common occurrence. It is at least as common for a branch of mathematics to be developed in close conjunction with a new scientific theory (consider Newton and the calculus) or to be developed by generalizing on scientific or quotidian applications (whence the word 'geometry', literally, 'earth-measuring').

Naturalists who require mathematics to derive its legitimacy from the sciences

²Another kind of methodological naturalism is available, one that treats mathematics as a science in its own right and with its own methods and authority. The position we endorse in the following section is a version of this, so we do not address it here.

presume to alter mathematics for philosophical reasons just as much as those who call for mathematical revisionism out of purely philosophical motives. Furthermore, such naturalists present a misleading picture of what mathematics is. Such a view assumes that math is a matter of symbolic manipulation of syntax and that the task of the mathematician is to construct symbol-complexes that the natural scientist can appropriate for his own purposes. This ignores the meanings inherent in mathematical statements *per se*—meanings that mathematicians understand perfectly well on their own and without interpretation by philosophers or scientists. Our position is that mathematics is an autonomous discipline; its value is not exhausted by its scientific applications and there is something to be said for respecting its own methods in order to resolve mathematical disputes. That being said, an endorsement of the method of mathematics does not mean that philosophers should stand entirely apart from the activity of mathematicians.

6.1.4 Philosophy as First Mathematics

We observed above that the certainty of mathematical knowledge is one of its philosophically salient features and that part of this certainty derives from its methods, viz., deductive proof rather than inductive argument, inference to the best explanation, or other notoriously slippery ways of drawing conclusions. Of course, deductive arguments generally require premises, and it is not required that every premise be immediately capable of further demonstration. Any mathematical theory has its axioms, and when a theory like ZF is meant to serve as a foundation for the whole of mathematics, it is especially important that its basic principles stand up to scrutiny as axioms.

It is not plausible that an axiomatization of set theory should live up to the ideal according to which each axiom is self-evident and incapable of rational doubt. In the case of Euclidean geometry, an account of the purported self-evidence of the axioms

can take advantage of the spatial, visual character of the subject matter, but the subject matter of set theory is very general and includes things like infinite series of cardinal numbers increasing in size—objects far beyond what we can draw in detail or picture clearly in our minds. Indeed, Euclidean geometry did not even live up to its own ideal since it included the Parallel Postulate among its basic principles. The activity of evaluating candidate axioms can be messy; it is also where philosophers can contribute a great deal to the development of mathematics.

This idea, that the investigation of fundamental axioms involves philosophy as well as mathematics, is in the spirit of what Russell calls “mathematical philosophy” in his introduction to that subject.

Mathematics is a study which, when we start from its most familiar portions, may be pursued in either of two opposite directions. . . . The other direction, which is less familiar, proceeds, by analysing, to greater and greater abstractness and logical simplicity; instead of asking what can be defined and deduced from what is assumed to begin with, we ask instead what more general ideas and principles can be found, in terms of which what was our starting-point can be defined or deduced. It is the fact of pursuing this opposite direction that characterises mathematical philosophy as opposed to ordinary mathematics. (Russell 1920: p. 1)

In Russell’s case, the starting point is made up of familiar mathematical truths that nobody would doubt (such as the familiar truths of arithmetic). It is open to us to shift the starting point, however, so that the analysis begins with some of the controversial results of intuitionistic mathematics: the invalidity of the *tertium non datur*, for instance, or the continuity of every total function from \mathbb{R} to \mathbb{R} . We can then ask what might motivate one to accept these claims by examining what general principles would entail them.

More recently, Russell’s mathematical philosophy has found expression in the program of reverse mathematics, in which the goal is to determine the weakest set of assumptions required to prove a given result. Indeed, Simpson, who devotes much of his (1999) to reverse mathematics, observes that there is a family resemblance

between his foundationally sensitive development of mathematics and Bishop's constructivism. He also distinguishes five differences between the two (*ibid.*, p. 31):

1. "The constructivists believe that mathematical objects are purely mental constructions, while we make no such assumption.
2. "The meaning which the constructivists assign to the propositional connectives and quantifiers is incompatible with our classical interpretation.
3. "The constructivists assume unrestricted induction on the natural numbers, while in RCA_0 [second-order arithmetic with only recursive comprehension] we assume only Σ_1^0 induction.
4. "We always assume the law of the excluded middle, while the constructivists deny it.
5. "The typical constructivist response to a nonconstructive mathematical theorem is to modify the theorem by adding hypotheses or 'extra data'. In contrast, our approach in this book is to analyze the provability of mathematical theorems as they stand, passing to stronger subsystems of Z_2 [full second-order arithmetic] if necessary."

Of these, only the fourth should give the constructivist pause, but Simpson's RCA_0 uses only recursive comprehension, and worries about the unrestricted use of the *tertium non datur* are thereby mitigated to a degree. We have seen that ontological constructivism is neither a presupposition nor a consequence of mathematical constructivism and that the latter does not presuppose any special constructive meaning for the logical constants. Concerning point 3, constructivists ought not to object to the use of induction principles weaker than those they accept. Regarding point 5, there is a great deal of mathematical interest in results that reveal the necessary

presuppositions of essentially nonconstructive theorems; the constructivist can learn from such results and use them to distinguish his mathematics from his opponents’.

Thus, some of the differences between the reverse mathematics program and the one we are proposing here are merely apparent. We emphasize, however, that learning what axioms are necessary to prove a given theorem is not of merely academic interest. As Russell points out, this can often provide us data by which we can evaluate proposed axioms. If it turns out that a proposed axiom is necessary to prove a result that is intuitively obvious or indispensable for the development of a mathematical theory, then this constitutes evidence for the truth of the axiom.

A good particular example of the kind of development at which we are gesturing is given by Zermelo’s proof that every set can be well-ordered. According to Ferreirós (2011: §2), Cantor maintained that the Well-Ordering Theorem is “a fundamental and momentous law of thought.” Zermelo clearly must have disagreed because he set out to demonstrate the Theorem. Notoriously, his proof uses the Axiom of Choice, but this is no reason to dismiss the proof. For one thing, even an intuitionist, who rejects AC because it entails the validity of the *tertium non datur*, can concede that it is *prima facie* more plausible than the Well-Ordering Theorem. In addition, Zermelo’s proof shifts the foundational debate. Rather than arguing directly about whether every set can be well-ordered, mathematicians and philosophers can begin to look for reasons to accept or to reject AC; assuming that one accepts Zermelo’s proof, whatever reason one can give for or against its chief premise will extend to the Well-Ordering Theorem.

What kinds of reasons might be offered for accepting or rejecting an axiom? There seem to be at least three categories of argument that apply. Sometimes, it turns out that a proposed axiom can be deduced from more fundamental principles. Zermelo’s proof provides one example of this. So does Frege’s derivation of the Peano Axioms from Hume’s Principle. More recently, various axioms have been proposed that would settle the Continuum Hypothesis one way or another, thereby mitigating the question

whether CH *per se* should be accepted or rejected. In the case of intuitionistic and constructive mathematics, the rejection of the *tertium non datur* is a plausible candidate for further justification. Although some texts on the subject, e.g., (Heyting 1966: p. 3), present this rejection as quite obviously correct in itself, this is hardly satisfying to many mathematicians and philosophers. A good deal of the interest of Brouwer's strong counterexamples is that they motivate the rejection of classical logic.³

When no interesting deduction of a basic principle is forthcoming, one must resort to other means by which it might be justified. One of these is familiar, viz., appeals to intuitive plausibility coupled with thought experiments, heuristic arguments, and so on. Russell (*op. cit.*, pp. 126ff.) asks his reader to imagine selecting shoes and socks in order to understand the controversy surrounding AC. A justification for the ZF Axiom of Union might take the form of talk about dumping the contents of several boxes into one big box. These kinds of considerations are rarely decisive, but sometimes they are all that is available, particularly when demonstrative proof is not an option.

The third way of justifying a proposed axiom is by an appeal to its consequences. Here, mathematics exhibits its similarity with the natural sciences. Just as a physical theory provides an explanatory framework for empirical data, so too a mathematical theory can be understood as a way of explaining mathematical data. The data in question here are what Russell referred to in the quotation above as the "starting-point." In the case of mathematical foundations generally, it will include the core results of the various branches of mathematics. If a fundamental theory T entails that $1 + 1 = 2$ and an alternative theory T' does not, this is a point in favor of T . The idea that a mathematical principle can be justified via its consequences appears in the discussion of the Axiom of Reducibility in *Principia Mathematica*.

The reason for accepting an axiom, as for accepting any other proposi-

³Of course, one can always propose that this constitutes a *reductio* of the Brouwerian premises, but it would be excessively stubborn to insist *a priori* that there could never be a sound argument for the rejection of a classical principle.

tion, is always largely inductive, namely that many propositions which are nearly indubitable can be deduced from it, and that no equally plausible way is known by which these propositions could be true if the axiom were false, and nothing which is probably false can be deduced from it. (Whitehead and Russell 1910: p. 62)

More recently, (Maddy 2011: §V.4) has suggested that the kind of inductive evidence Russell and Whitehead mention (which she calls “extrinsic support”) is the *best* source for justification of an axiom. See (Koellner 2009: §5) for an example of how such a justification might look for a version of the Axiom of Determinacy. Common to all of these discussions, as well as our own, is that a foundational theory for mathematics is akin to a scientific theory in that it is to be evaluated in large part by how it explains mathematical data. As in the scientific case, the data underdetermine the theory, and so we are left to appeal to other theoretical virtues in selecting between the candidate theories available to us.

In the remainder of this chapter, we will consider some possible axioms for intuitionistic and constructive mathematics. The goal is twofold. First, this will serve to establish fully our claim that the place to look for intuitionistic foundations is within mathematics itself. Second, our discussion will canvass some of the foundational options available to the intuitionist. We do not purport either to establish what principles an intuitionist *must* endorse or to provide absolutely decisive reasons for accepting one or the other of the options we discuss. Rather, we set ourselves the task of shifting and focusing the debate, much as Zermelo did when he first made AC explicit as a foundational principle for classical set theory.

6.2 A Survey of Foundational Principles for Intuitionistic Mathematics

6.2.1 General Form of the Discussion

There are a few desiderata that we can lay down *a priori* on the candidate axioms for intuitionistic mathematics that we will discuss. Since we are presently interested in what distinguishes intuitionistic mathematics from its classical rival, one desideratum is that the axiom entail the invalidity of the *tertium non datur*. We have frequently cautioned against taking the rejection of this principle as the entire essence of intuitionism, but there can be no denying that it is an important intuitionistic doctrine. Furthermore, the technical development of intuitionistic mathematics is facilitated by the rejection of the *tertium non datur*: one can show that its validity is a consequence of other mathematical statements and infer the negations of these statements as a result. To see an example of this, consider

Theorem 1. *Not every subset of a finite set is finite.*

Proof. Assume for *reductio* that every subset of a finite set is finite. Consider the set $\{0|\phi\}$ where ϕ is an arbitrary mathematical statement. This is the set that contains 0 if and only if ϕ and contains no other elements. Note that $\{0|\phi\}$ is a subset of $\{0\}$, a finite set. Thus, by our assumption, $\{0|\phi\}$ is finite. This means that its cardinality is a natural number n . Regardless of the truth-value of ϕ , the only element that can possibly be in $\{0|\phi\}$ is 0, so $n = 0$ or $n = 1$. (Note that equality on the natural numbers is provably decidable by induction.) If $n = 0$, then $\{0|\phi\}$ is empty, so $0 \notin \{0|\phi\}$, from which $\neg\phi$ follows. If $n = 1$, then $0 \in \{0|\phi\}$, from which ϕ follows. Since ϕ was arbitrary, we have that

$$\forall\phi(\phi \vee \neg\phi),$$

i.e., the *tertium non datur* is valid. This is a contradiction, so not every subset of a

finite set is finite. □

Each of the three principles we discuss below has as a consequence that the *tertium non datur* is invalid; the proofs of this are given after each principle is introduced. After that, we mention some of each principle's other consequences, its consistency relative to other aspect of intuitionistic and constructive mathematics, and the intuitive motivation that can be given for it.

6.2.2 Brouwer's Continuity Principle

Brouwer's Continuity Principle (CP) says that

$$\forall \alpha \exists n. A(\alpha, n) \rightarrow \forall \alpha \exists m \exists n \forall \beta. (\bar{\beta}m = \bar{\alpha}m \rightarrow A(\beta, n)).$$

Here, α and β range over all infinite sequences of natural numbers, n and m range over the natural numbers, A is an arbitrary relation between $\mathbb{N}^{\mathbb{N}}$ and \mathbb{N} , and $\bar{\alpha}m$ is the m -length initial segment of the sequence α .⁴

Theorem 2. *CP entails the invalidity of the tertium non datur.*

Proof. Assume CP and assume for *reductio* that the *tertium non datur* is valid. By the latter assumption, we have that

$$\forall \alpha (\forall n. \alpha(n) = 0 \vee \neg \forall n. \alpha(n) = 0).$$

For each sequence, we can find a natural number that tracks the behavior of the sequence. That is,

$$\forall \alpha \exists m ((m = 0 \wedge \forall n. \alpha(n) = 0) \vee (m = 1 \wedge \neg \forall n. \alpha(n) = 0)).$$

⁴We assume throughout that identity between sequences is an entirely extensional matter. Extensionality is not always assumed in intuitionistic mathematics, but (van Atten 2006: p. 104) points out it does not interfere at all with the development of intuitionistic analysis.

Now, we can apply the CP to infer that

$$\forall\alpha\exists m\exists p\forall\beta(\bar{\beta}p = \bar{\alpha}p \rightarrow ((m = 0 \wedge \forall n.\beta(n) = 0) \vee (m = 1 \wedge \neg\forall n.\beta(n) = 0)))$$

(where p is a natural number).

Let α be $\lambda n.\alpha(n) = 0$. Then

$$\exists p\forall\beta(\bar{\beta}p = \bar{\alpha}p \rightarrow (m = 0 \wedge \forall n.\beta(n) = 0) \vee (m = 1 \wedge \neg\forall n.\beta(n) = 0)).$$

Fix p and let β be such that $\bar{\beta}p = \bar{\alpha}p$. Since equality on \mathbb{N} is decidable, we can consider two cases.

Case 1: $m = 0$. In this case, let β be such that $\beta(p+1) = 1$. Then $\neg\forall n.\beta(n) = 0$, contradicting the second conjunct of the relevant disjunct above.

Case 2: $m = 1$. In this case, let $\beta = \alpha$. Then $\forall n.\beta(n) = 0$, which is also a contradiction.

Since we have a contradiction in either case, we have completed our *reductio* and conclude that the *tertium non datur* is invalid. □

We have already discussed the CP when we looked at Brouwer's strong counterexamples to the *tertium non datur* by way of the Uniform Continuity Theorem (UCT). As the proof just presented shows, the UCT is not necessary to generate such counterexamples, since they can be obtained from the CP alone. This is as it should be; the value of the UCT, like much of intuitionistic mathematics, is not exhausted by its consequences for classical logic. Besides, the CP alone does not suffice to prove the UCT: one also needs the principle of Bar Induction. Brouwer attempted to prove this in (Brouwer 1927), but his proof begs the question. Instead, Bar Induction can be introduced as an axiom in its own right. As pointed out by (Kleene and Vesley 1965: p. 51), it appears to be independent of the other axioms of intuitionistic analysis. Furthermore, it is intuitively plausible and classically acceptable, so there seems

to be little reason to pursue it further here, where we are interested in nonclassical principles.

It is worth saying something more about the CP, where we are concerned not with its place in Brouwer's thought but with what might motivate it as a foundational mathematical principle. The CP will be immediately appealing to anybody who regards the notion of the actual infinite with suspicion, since it allows us to work with inherently infinitary objects (e.g., the continuum) without committing ourselves to the idea that these objects could ever be "completed." If there were no prospects on the horizon for a successful theory of the actually infinite, then intuitionistic analysis with the CP would be like an oasis in a desert. In light of classical analysis modeled in ZF, however, there is no good mathematical reason to worry about actual infinities.

Sometimes, attempts to argue for the CP start by assuming that it applies to a special kind of mathematical object, a *choice sequence*. Here, the idea is that choice sequences, even when some of them are extensionally identical with classical sequences, are governed by special principles that don't apply to classical sequences, in particular the CP. This is the approach taken by, e.g., van Atten (*ibid.*, ch. 6). One could take the CP as constituting either a definition of or a principle governing a choice sequence as distinct from other kinds of infinite sequence: the set of choice sequences, on this view, would be the set of all those sequences that must be treated in the finite manner characterized by the CP. Such an attitude would be especially amenable to mathematical pluralists, who can thereby maintain that intuitionistic analysis is the theory of a special kind of object, the choice sequence (or the continuum generated from these sequences), while classical analysis just studies something else. In order for CP to be useful, however, one must show that choice sequences *exist*. Classically, they cannot, since they violate the *tertium non datur*. Even without this difficulty, it is not enough in general to lay down a definition; one must also show that there exists an object to which it applies, and even if one could, it is not clear what advantage

this would give to intuitionistic analysis over its classical counterpart.

Our own view takes seriously the objection raised to the Brouwerian argument for the CP discussed in the previous chapter. Even if we should be worried about actual infinities in mathematics, the CP requires something stronger, namely, that *all* of our knowledge about infinite sequences must be based on finite initial segments of them. This is a very implausible claim, since we can give fully finite characterizations of some infinite sequences. In our proof of Theorem 2, for example, we referred to the constantly 0 sequence. All of the information we could want to know about this sequence's values is captured by a finite, easily understood description: they're all 0.

In order to develop intuitionistic analysis from the CP, however, it appears to be the case that the principle must hold for *all* sequences, even those capable of being given by a finite description. There seems to be little reason to accept this beyond a stubborn insistence that something is wrong with the notion of the actually infinite. In light of the existence of a successful classical theory of analysis, it is a mistake to take the CP as a foundational principle for intuitionistic mathematics. This does not mean that intuitionists necessarily should reject the CP entirely; as we will see below, it is a consequence of other principles that have a better claim to foundational justification.

6.2.3 The Uniformity Principle

The Uniformity Principle (UP) says that

$$\forall X \exists n A(X, n) \rightarrow \exists n \forall X A(X, n).$$

Here, X ranges over subsets of \mathbb{N} , n ranges over \mathbb{N} , and A is an arbitrary relation between $\mathcal{P}(\mathbb{N})$ and \mathbb{N} .

Theorem 3. *UP entails the invalidity of the tertium non datur.*

Proof. Assume UP and the validity of the *tertium non datur*. For $X \in \mathcal{P}(\mathbb{N})$, let $A(X)$ hold iff $0 \in X$. Let B be a relation on $\mathcal{P}(\mathbb{N}) \times \mathbb{N}$ such that $B(X, 1)$ holds if $A(X)$ holds and $B(X, 2)$ holds if $\neg A(X)$ holds.

In the remainder of this proof, X ranges over subsets of the natural numbers.. Since we are assuming that the *tertium non datur* is valid, we know that

$$\forall X(0 \in X \vee 0 \notin X).$$

So, we have that

$$\forall X(A(X) \vee \neg A(X)).$$

By definition of B , it follows that

$$\forall X(B(X, 1) \vee B(X, 2)),$$

from which we can infer

$$\forall X \exists n. B(X, n)$$

where n is a natural number.

Now, by the UP, we conclude that

$$\exists n \forall X. B(X, n).$$

By the definition of B , this number n must be either 1 or 2. So, either $0 \in X$ for all X or $0 \notin X$ for all X . The first disjunct is false since $\{1\}$ is a subset of \mathbb{N} but $0 \notin \{1\}$. The set $\{0\}$ similarly shows that the second disjunct is false. Thus, we have a contradiction, and we conclude that the *tertium non datur* is invalid. \square

Troelstra and van Dalen (1988: p. 241) report that the UP seems first to have been introduced by Troelstra (1973), although Kreisel (1971) gestures at it. A motivation

for accepting it, however, can be traced back much earlier. In Book VI of the *Physics*, Aristotle discusses the notion of a continuum in the course of responding to Zeno's paradoxes. There, he observes that there is something funny about thinking of a continuum as a collection of points.

For instance, a line, which is continuous, cannot consist of points, which are indivisible, first because in the case of points there are no limits to form a unity (since nothing indivisible has a limit which is distinct from any other part of it), and second because in their case there are no limits to be together (since anything which lacks parts lacks limits too, because a limit is distinct from that of which it is a limit). (Aristotle 1986: 231a21ff.)

This puzzle about treating the continuum as a collection of points appears in Brouwer's early work, e.g., in his 1908 dissertation. There, one finds him attempting to reconcile his ontological constructivism with the unavoidable datum that mathematics deals with continuous as well as discrete entities. His early solution was to treat the continuum as a *sui generis* entity about which little of a foundational nature could be said, but he soon realized that this was unsatisfactory. This dissatisfaction led him to introduce choice sequences and begin appealing to the CP in his proofs, but if our discussion above is correct, then the full-strength CP is hard to justify as a basic principle.

Brouwer might have considered using the UP rather than the CP. Why would a total relation on $\mathcal{P}(\mathbb{N})$ have to be uniformized in the way described by the UP? Precisely because an uncountable set like $\mathcal{P}(\mathbb{N})$ is uncountable, there is not any clear way to isolate each of its members from all of the others. In the case of a finite or countably infinite set S , we can do this by introducing a one-to-one total function from S to \mathbb{N} that serves to pick out each member of S as an individual object that stands apart from any other; the decidability of equality on \mathbb{N} carries over to S and induces a clear distinction between each element of the latter. In the case of an uncountable set, there is no such one-to-one total function.

Nevertheless, when a classical mathematician introduces relations over the whole of $\mathcal{P}(\mathbb{N})$ without something like the UP, she is assuming that the individual elements of $\mathcal{P}(\mathbb{N})$ can stand on their own in the way that blurs the distinction between countable and uncountable sets. The UP constrains this by placing restrictions on what kinds of total relations there can be on a set like $\mathcal{P}(\mathbb{N})$. As Troelstra and van Dalen put it, “the sets of natural numbers are a very ‘diffuse’ totality, at least from an extensional point of view.” (*op. cit.*, p. 234)

For a similar reason, one might treat the continuum as a set to which UP applies. This would be to deny that there could be a relation that serves to pick out distinct inhabited proper subsets of the continuum, which amounts to an endorsement of Brouwer’s result that the continuum is indivisible (*unzerlegbar*), i.e., that if A and B are disjoint subsets of \mathbb{R} such that $A \cup B = \mathbb{R}$, then $A = \mathbb{R}$ or $B = \mathbb{R}$. Brouwer deduced this in the course of proving the continuity theorems in his (1927), but one could take it as axiomatic for the continuum instead.

\mathbb{R} comes equipped with an apartness relation $\#$ such that $p\#q$ iff $|p - q| > 0$. In other words, two real numbers are apart from one another if there is a positive distance between them under the usual metric on \mathbb{R} . (Note that ‘ $p\#q$ ’ is stronger than ‘ $p \neq q$ ’.) It is a theorem of intuitionistic topology that a total function from a uniform set (i.e., a set to which UP applies) to an apartness space is constant. Thus, if \mathbb{R} is a uniform space, it follows that every total function from \mathbb{R} to \mathbb{R} is constant, and hence uniformly continuous. This shows that we can obtain the Uniform Continuity Theorem from a version of UP without having recourse to CP.

6.2.4 Constructive Church’s Thesis

The constructive version of Church’s Thesis (CT) discussed in this section maintains that every total function on the natural numbers is computable (or, what is equivalent, recursive). Note that this is not the same as the Church-Turing Thesis, according to

which every intuitively computable function is Turing computable. The idea that the Church-Turing Thesis does not admit of purely mathematical analysis because of the imprecision of the concept “intuitively computable” is not relevant here. Whether or not one accepts our version of CT, there should be no controversy about the precision of the terms or concepts needed to state it.

Theorem 4. *CT entails the invalidity of the tertium non datur.*

Proof. Assume CT and assume the *tertium non datur* for *reductio*. Let $H(n)$ be the halting predicate, i.e., $H(n)$ holds if and only if the machine with index n halts on input n . Since we have assumed the *tertium non datur*, we can say that

$$\forall n(H(n) \vee \neg H(n)).$$

The quantifier ranges over the entire set \mathbb{N} . Let f be the function such that $f(n) = 1$ if $H(n)$ holds and $f(n) = 0$ otherwise. This is a total function, so by CT it is computable. Thus, f computes a function that solves the Halting Problem, which is impossible. From this, we conclude that the *tertium non datur* is invalid. \square

A word of warning is in order about CT. Accepting it does not commit one to the existence of *no* noncomputable functions. One is free to endorse the definition of noncomputable functions as perfectly good definitions. CT commits one only to maintaining that noncomputable functions on the natural numbers are necessarily partial.

We will offer two observations that can be given in favor of CT. The first concerns ways by which one might motivate CT from without, so to speak. That is, what could an advocate of a form of intuitionistic or constructive mathematics based on CT say to a skeptical interlocutor? Consider first the ordinary Church-Turing Thesis, according to which every intuitively computable function is computable by a Turing machine. Even if this is not subject to precise mathematical demonstration, there are

considerations weighing strongly in its favor, particularly the equivalence of the large number of models of computation that have been proposed since (Turing 1937).

At the present stage of research, it is not hard to motivate the Church-Turing Thesis. In order to move from this to CT, the constructive form of Church's Thesis that interests us, one must argue that every total function on \mathbb{N} is intuitively computable. One might argue for this on the grounds that the computational capacity of any human being must be capable of finite characterization. (This is not to say that we should endorse a finitistic version of mathematics, since one could still maintain that the domain of possible inputs is not finite.) Furthermore, all of the familiar well-developed notions of finitely characterized computation (Turing machines, recursive functions, λ -calculus, etc.) are equivalent, and so we have reason to think that human computation is equivalent to them as well.

An advantage of this argument is that it leaves open the possibility of refining CT should a new notion of finite computation be developed that allows for more functions to be computed. Since any such method of computation would still be unable to solve its own Halting Problem, our proof of Theorem 4 would still apply and we would still be able to deduce the invalidity of the *tertium non datur*. Nevertheless, as long as we have reason to think that Turing computability and its equivalents capture something absolute and essential about computability in general, and that human beings are bound by some kind of finite limit on their computational abilities, then there is some reason to accept CT.

One might object to the starting point of this line of argument because it ties mathematics too closely to human capacities. Why should we think that mathematics is to be limited by restrictions placed on feeble humans? Here, the intuitionist has an advantage over the classical mathematician. The latter must insist that a noncomputable natural-number function f has a value for every input without being able to say anything about what these values are in general. In reply, the intuitionist can

point to his preferred mathematics as an acceptable alternative that doesn't require such extravagance.⁵

The other reason why an intuitionist should take CT seriously is internal to the constructive program in mathematics. CT is more frequently associated with Markov and the Russian school of constructivism rather than intuitionism. This is because free-choice sequences and intuitionism are frequently presented as going hand-in-hand, but the former are non-recursive by design. At the present stage of constructive mathematical development, this view defers too excessively to tradition. Brouwer introduced free-choice sequences because he thought that they were necessary to develop an intuitionistic theory of the continuum that could rival the powerful classical theory. Bishop (1967) showed that Brouwer was wrong about this. This paves the way for the intuitionist to adopt a principle like CT that would seem to restrict his mathematical ontology. There is no decisive mathematical reason against it, since CT is consistent with both second-order Heyting arithmetic and with intuitionistic ZF set theory. The only thing that the intuitionist must give up by adopting CT is the Fan Theorem, since these two principles are mutually inconsistent. (Beeson 1982: p. 22) Thus, the intuitionist may not have a way to prove the Uniform Continuity Theorem. One can still obtain a weaker Pointwise Continuity Theorem from CT, however. (Bridges and Richman 1987: p. 120) It is hardly clear that one gains extra intuitive clarity or practical benefit by insisting that this continuity must be uniform at all costs.

⁵Note that the intuitionist is not committed to being able to produce an input n such that $f(n)$ is undefined, even though he maintains that f is not a total function. Rather, he denies that $\neg\forall n\exists m.f(n) = m$ entails $\exists n\forall m.f(n) \neq m$. His logic allows this, since $\neg\forall x.A(x)$ does not entail $\exists x.\neg A(x)$.

6.3 Summary and Conclusion

We have seen that the best attempt to provide a semantic foundation for intuitionistic mathematics is unsuccessful. The antirealistic semantics proposed by Dummett does not validate all and only the laws of intuitionistic logic; thus, intuitionists, who reason in accord with these laws when they do mathematics, cannot endorse the Dummettian semantic theory.

Our other chief challenge to semantic antirealism concerns the success of classical mathematics. If, as Dummett and his followers claim, classical mathematicians routinely and systematically make assertions and inferences that are not faithful to the meanings of the statements they use. If this is so, then the success of classical mathematics is remarkable, even miraculous. We repeat this point here because there is a more general lesson to draw from it. It is a mistake to think that fundamental, apparently intractable disagreements in mathematics can be settled on the basis of the meanings of bits of language. If the parties on one side in the dispute are in violation of the very meanings of the words and statements they use, then we should expect this to show up on the inside of the practice—the entire edifice ought to collapse into incoherence. That this has not happened in the case of either intuitionistic or classical mathematics provides good evidence that there is nothing wrong with the linguistic meanings presupposed by the practitioners of either approach.

There is a lesson here for intuitionists. Intuitionistic mathematics, and constructive mathematics more generally, really does have the revolutionary character that Weyl attributed to it; it is a mistake to play down this element by insisting on a special constructive meaning for mathematical statements. A statement of the *tertium non datur*, the Uniformity Continuity Theorem, Constructive Church's Thesis, or any other mathematical principle or result has the same meaning in the mouth of a classical mathematician or a constructivist. The fact that the two disagree about the truth values of these statements shows merely that both sides have work to do

to convince the other. Classical mathematicians have popular opinion on their side, but that is no reason for the constructivist to slide into a mathematical, logical, or semantic pluralism that masks the existence of a genuine disagreement; if anything, constructivists should be emboldened by the prospect of struggling to effect such a radical change in a field as important as mathematics.

A more general lesson to draw from our discussion of candidate intuitionistic principles is that intuitionism does not need to have a sweeping philosophical foundation. Many intuitionists seem to think that their mathematics can be justified only by an appeal to a larger philosophical position, be it Dummettian semantic antirealism, Brouwer's neo-Kantian idealism, or some other grand, systematic view. What we have shown here is that the intuitionist can argue for his position by focusing on individual mathematical principles. This has practical benefit: it allows intuitionists to cease trying to justify their mathematics on the basis of the latest philosophy and instead to focus on showing what consequences their principles have for mathematics itself and how each principle might be justified individually. In other words, intuitionists are no longer beholden to philosophy to make their position legitimate; they can do this from their proper place as mathematicians who prove theorems and justify their fundamental principles in the ways canvassed above. Perhaps some philosophy creeps in at this foundational level, but on our proposal, mathematics takes its rightful place as queen with philosophy as its ancillary.

Bibliography

- Aristotle. *Physics*. Oxford University Press, 1986. Tr. by Robin Waterfield.
- JC Beall and Greg Restall. *Logical Pluralism*. Clarendon Press, Oxford, 2006.
- Michael Beeson. Problematic principles in constructive mathematics. In D. van Dalen, D. Lascar, and J. Smiley, editors, *Logic Colloquium '80*, pages 11–55. North-Holland, 1982.
- John L. Bell. *A Primer of Infinitesimal Analysis*. Cambridge University Press, 2nd edition, 2008.
- Paul Benacerraf. Mathematical truth. *Journal of Philosophy*, 70(19):661–680, 1973. Reprinted in (Benacerraf and Putnam 1983: pp. 403-420).
- Paul Benacerraf and Hilary Putnam, editors. *Philosophy of Mathematics: Selected Readings*. Cambridge University Press, 2nd edition, 1983.
- Errett Bishop. *Foundations of Constructive Analysis*. McGraw-Hill, 1967.
- Errett Bishop. Schizophrenia in contemporary mathematics. In Murray Rosenblatt, editor, *Errett Bishop: Reflections on Him and His Research*, volume 39 of *Contemporary Mathematics*, pages 1–32. American Mathematical Society, 1985.
- Douglas Bridges and Ray Mines. What is constructive mathematics? *The Mathematical Intelligencer*, 6(4):32–38, 1984.

- Douglas Bridges and Fred Richman. *Varieties of Constructive Mathematics*. Cambridge University Press, 1987.
- L.E.J. Brouwer. *Leven, Kunst en Mystiek*. Waltman, 1905. Translated into English as Brouwer (1996).
- L.E.J. Brouwer. *Over de grondslagen der wiskunde*. PhD thesis, Universiteit van Amsterdam, 1907. English translation in (Brouwer 1975: pp. 15-101).
- L.E.J. Brouwer. Die mögliche Mächtigkeiten. *Atti IV Congr. Intern. Mat. Roma*, 3: 569–571, 1908a. Reprinted in (Brouwer 1975: pp. 102-104).
- L.E.J. Brouwer. De onbetrouwbaarheid der logische principes. *Tijdschrift voor Wijsbegeerte*, 2:152–158, 1908b. English translation in (Brouwer 1975: pp. 107-111).
- L.E.J. Brouwer. *Intuitionisme en Formalisme*. Clausen, Amsterdam, 1912. English translation in (Benacerraf and Putnam 1983: pp. 77-89).
- L.E.J. Brouwer. Begründung der Mengenlehre unabhängig vom logischen Satz vom ausgeschlossenen Dritten. Ersten Teil: Allgemeine Mengenlehre. *KNAW Verhandelingen*, 1(5):1–43, 1918. Reprinted in (Brouwer 1975: pp. 150-190).
- L.E.J. Brouwer. Bewijs dat iedere volle functie gelijkmatig continu is. *KNAW Verslagen*, 33:189–193, 1924. English translation in (Mancosu 1998: pp. 36-39).
- L.E.J. Brouwer. Über Definitionsbereiche von Funktionen. *Mathematische Annalen*, 97:60–75, 1927. Partial English translation in (van Heijenoort 1967: pp. 457-463).
- L.E.J. Brouwer. Die Struktur des Kontinuums. Lecture delivered in Vienna on March 14th, 1928. English translation in (Ewald 1996: pp. 1186-1197)., 1928.
- L.E.J. Brouwer. Mathematik, Wissenschaft und Sprache. *Monatshefte für Mathematik und Physik*, 36(1):153–164, 1929. Reprinted in (Brouwer 1975: pp. 417-428). English Translation in (Ewald 1996: pp. 1170-1185).

- L.E.J. Brouwer. Essentieel negatieve eigenschappen. *Indagationes Mathematicae*, 10: 322–323, 1948. English translation in (Brouwer 1975: pp. 478-479).
- L.E.J. Brouwer. Historical background, principles and methods of intuitionism. *South African Journal of Science*, 49:139–146, 1952. Reprinted in (Brouwer 1975: pp. 508-515).
- L.E.J. Brouwer. Points and spaces. *Canadian Journal of Mathematics*, 6:1–17, 1954.
- L.E.J. Brouwer. *Collected Works I: Philosophy and Foundations of Mathematics* (ed. A. Heyting). North-Holland, 1975.
- L.E.J. Brouwer. Life, art, and mysticism. *Notre Dame Journal of Formal Logic*, 37 (3):389–429, 1996. Translated by Walter P. van Stigt.
- John Burgess. Dummett’s case for intuitionism. *History and Philosophy of Logic*, 5 (2):177–194, 1984.
- Rudolf Carnap. *Logische Syntax der Sprache*. Springer, 1934.
- Rudolf Carnap. *The Logical Syntax of Language*. Littlefield, Adams & Co., 1959. English translation of Carnap (1934).
- Lewis Carroll. What the tortoise said to Achilles. *Mind*, 4(14):278–280, 1895.
- Roy Cook. Should anti-realists be anti-realists about anti-realism? Forthcoming in a special issue of *Erkenntniss* on logical pluralism.
- Donald Davidson. Truth and meaning. *Synthese*, 17(3):304–323, 1967. Reprinted in (Davidson 2001: pp. 17-42).
- Donald Davidson. *Inquiries into Truth and Interpretation*. Oxford University Press, 2nd edition, 2001.

- E.B. Davies. A defence of mathematical pluralism. *Philosophia Mathematica*, 13(3): 252–276, 2005.
- H.C.M. de Swart. Another intuitionistic completeness proof. *Journal of Symbolic Logic*, 41(3):644–662, 1977.
- Richard Dedekind. *Was Sind und Was Sollen die Zahlen?* Friedrich Bieweg und Sohn, 2nd edition, 1893. English translation in (Ewald 1996: pp. 787–833).
- Emil du Bois-Reymond. Über die Grenzen der Naturerkenntnis. In *Reden von Emil du Bois-Reymond*, pages 105–141. Veit & Comp., Leipzig, 1886.
- Paul du Bois-Reymond. *Die allgemeine Funktionentheorie*. H. Laupp, 1882.
- Michael Dummett. The justification of deduction. *Proceedings of the British Academy*, 59:201–232, 1975a. Reprinted in (Dummett 1978b: pp. 290–318).
- Michael Dummett. The philosophical basis of intuitionistic logic. In H.E. Rose and J.C. Shepherdson, editors, *Logic Colloquium '73*, pages 5–40. North-Holland, 1975b. Reprinted in (Dummett 1978b: pp. 215–247).
- Michael Dummett. Wang's paradox. *Synthese*, 30:301–324, 1975c. Reprinted in (Dummett 1978b: pp. 248–268).
- Michael Dummett. Realism. In *Truth and Other Enigmas*, pages 145–165. Harvard, 1978a.
- Michael Dummett. *Truth and Other Enigmas*. Harvard University Press, 1978b.
- Michael Dummett. *The Logical Basis of Metaphysics*. Harvard University Press, 1991.
- Michael Dummett. *The Seas of Language*. Clarendon Press, 1993.
- Michael Dummett. *Elements of Intuitionism*. Clarendon Press, 2nd edition, 2000.

- William B. Ewald, editor. *From Kant to Hilbert: A Source Book in the Foundations of Mathematics*, volume 2. Oxford University Press, 1996.
- José Ferreirós. The early development of set theory. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. The Metaphysics Research Lab, Stanford University, Fall 2011 edition, 2011. <http://plato.stanford.edu/archives/fall2011/entries/settheory-early>.
- Gottlob Frege. *Die Grundlagen der Arithmetik*. Kuebner, Breslau, 1884.
- G. Gentzen. Untersuchungen über das logische Schliessen. *Mathematische Zeitschrift*, 39:176–210, 405–431, 1935.
- Kurt Gödel. Zum intuitionistischen Aussagenkalkül. *Anzeiger der Akademie der Wissenschaften in Wien*, 69:65–66, 1932.
- N. Goodman. A theory of constructions is equivalent to arithmetic. In J. Myhill, A. Kino, and R. Vesley, editors, *Intuitionism and Proof Theory*, pages 101–120. North-Holland, Amsterdam, 1970.
- G.F.C. Griss. Negationless intuitionistic mathematics. *Indagationes Mathematicae*, 8:675–681, 1946.
- Susan Haack. *Deviant Logic: Some Philosophical Issues*. Cambridge University Press, 1974.
- Geoffrey Hellman. Never say “never”! On the communication problem between intuitionism and classicism. *Philosophical Topics*, 17(2):47–67, 1989.
- Geoffrey Hellman and John L. Bell. Pluralism and the foundations of mathematics. In Shephen H. Kellert, Helen Longino, and C. Kenneth Waters., editors, *Scientific Pluralism*, number 19 in Minnesota Studies in the Philosophy of Science, chapter 4, pages 64–79. University of Minnesota Press, 2006.

- Arend Heyting. Die formalen Regeln der intuitionistischen Logik. *Sitzungsberichte der Preussischen Akademie von Wissenschaften*, 9:42–56, 57–71, 158–169, 1930.
- Arend Heyting. Die intuitionistische Grundlegung der Mathematik. *Erkenntnis*, 2: 106–115, 1931. English translation in (Benacerraf and Putnam 1983: pp. 52-61).
- Arend Heyting. *Mathematische Grundlagenforschung: Intuitionismus, Beweistheorie*. Springer Verlag, 1934.
- Arend Heyting. *Intuitionism: An Introduction*. North-Holland, 2nd edition, 1966.
- David Hilbert. Mathematische Probleme. *Archiv der Mathematik und Physik*, 3(1): 44–63, 213–237, 1901. Partial English translation in (Ewald 1996: pp. 1096-1105).
- Rosalie Iemhoff. Intuitionism in the philosophy of mathematics. In Edward N. Zalta, editor, *Stanford Encyclopedia of Philosophy*. The Metaphysics Research Lab, Stanford University, winter 2009 edition, 2008. <http://plato.stanford.edu/archives/win2009/entries/intuitionism/>.
- Immanuel Kant. *Critique of Pure Reason*. Cambridge, 1998. ed. and trans. by Paul Guyer and Allen Wood.
- Jerrold J. Katz. *Realistic Rationalism*. The MIT Press, Cambridge, MA, 1998.
- S.C. Kleene. *Introduction to Metamathematics*. North-Holland, 1950. 2009 Reprint by Ishi Press.
- S.C Kleene and R.E. Vesley. *The Foundations of Intuitionistic Mathematics Especially in Relation to Recursive Functions*. North-Holland, 1965.
- Peter Koellner. Truth in mathematics: The question of pluralism. In Otávio Bueno and Øystein Linnebo, editors, *New Waves in Philosophy of Mathematics*, pages 80–116. Palgrave Macmillan, 2009.

- A. Kolmogorov. O principe tertium non datur. *Matematicheskij Sbornik*, 32:646–667, 1925. English translation in (van Heijenoort 1967: pp. 416–437).
- A.N. Kolmogorov. Zur Deutung der intuitionistischen Logik. *Mathematische Zeitschrift*, 35:58–65, 1932.
- G. Kreisel. On weak completeness of intuitionistic predicate logic. *Journal of Symbolic Logic*, 27(2):139–158, 1962.
- G. Kreisel. Informal rigour and completeness proofs. In I. Lakatos, editor, *Problems in the Philosophy of Mathematics*. North-Holland, 1967.
- G. Kreisel. A survey of proof theory II. In J.E. Fenstad, editor, *Proceedings of the Second Scandinavian Logic Symposium*, pages 109–170. North-Holland, Amsterdam, 1971.
- Boris A. Kushner. The constructive mathematics of A.A. Markov. *American Mathematical Monthly*, 113(6):559–566, 2006.
- David Lewis. *Parts of Classes*. Blackwell, Oxford, 1991.
- Penelope Maddy. *Naturalism in Mathematics*. Oxford University Press, 2000.
- Penelope Maddy. *Second Philosophy: A Naturalistic Method*. Oxford University Press, Oxford, 2007.
- Penelope Maddy. *Defending the Axioms*. Oxford University Press, Oxford, 2011.
- Paolo Mancosu, editor. *From Brouwer to Hilbert: The Debate on the Foundations of Mathematics in the 1920s*. Oxford University Press, 1998.
- Charles McCarty. The coherence of antirealism. *Mind*, 115:947–956, 2006.
- Charles McCarty. Completeness and incompleteness for intuitionistic logic. *Journal of Symbolic Logic*, 73(4):1315–1327, 2008.

- Charles McCarty. Antirealism and constructivism: Brouwer's weak counterexamples. *Review of Symbolic Logic*, 6(1):147–159, 2013.
- D.C. McCarty. On theorems of Gödel and Kreisel: Completeness and Markov's principle. *Notre Dame Journal of Formal Logic*, 35(1):99–107, 1994.
- D.C. McCarty. Problems and riddles: Hilbert and the du Bois-Reymonds. *Synthese*, 147(1):63–79, October 2005.
- D.C. McCarty. Proofs and constructions. In Giovanni Sommaruga, editor, *Foundational Theories of Classical and Constructive Mathematics*, pages 209–225. Springer, 2011.
- J. Myhill. Formal systems of intuitionistic analysis I. In B. van Rootselaer and J. Stall, editors, *Logic, Methodology and Philosophy of Science III*. North-Holland, 1968.
- J. Niekus. The method of the creative subject. *Indagationes Mathematicae*, 90(4):431–443, 1987.
- W.V. Quine. *Philosophy of Logic*. Harvard University Press, 2nd edition, 1986.
- W.V.O. Quine and Joseph Ullian. *The Web of Belief*. McGraw-Hill, New York, 1970.
- Fred Richman. Interview with a constructive mathematician. *Modern Logic*, 6:247–271, 1996.
- G. Rose. Propositional calculus and realizability. *Transactions of the American Mathematical Society*, 75:1–19, 1953.
- Gideon Rosen. The shoals of language. *Mind*, 104(415):599–609, 1995. Critical Notice of Dummett (1993).
- Bertrand Russell. *Introduction to Mathematical Philosophy*. Macmillan, New York, 2nd edition, 1920.

- Stewart Shapiro. *Thinking about Mathematics*. Oxford University Press, Oxford, 2000.
- Stephen G. Simpson. *Subsystems of Second Order Arithmetic*. Perspectives in Mathematical Logic. Springer, Berlin, 1999.
- A. Tarski. Der Wahrheitsbegriff in den formalisierten Sprache. *Studia Philosophica*, 1:261–405, 1935. English translation in (Tarski 1983: pp. 152-268).
- A. Tarski. *Logic, Semantics, Metamathematics*. Hackett Publishing Co., 2nd edition, 1983.
- Neil Tennant. *Anti-Realism and Logic*. Clarendon Press, 1987.
- Neil Tennant. *The Taming of the True*. Oxford University Press, 1997.
- A.S. Troelstra. *Principles of Intuitionism*. Number 95 in Lecture Notes in Mathematics. Springer-Verlag, 1969.
- A.S. Troelstra. Notes on intuitionistic second order arithmetic. In A.R.D. Mathias and H. Rogers, editors, *Cambridge Summer School in Mathematical Logic*, number 337 in Lecture Notes in Mathematics, pages 171–205. Springer Verlag, Berlin, 1973.
- A.S Troelstra and D. van Dalen. *Constructivism in Mathematics*. North-Holland, 1988.
- A.M. Turing. On computable numbers, with an application to the entscheidungsproblem. *Proceedings of the London Mathematical Society*, 42:230–265, 1937.
- Toshio Umezawa. On intermediate propositional logics. *Journal of Symbolic Logic*, 24(1):20–36, 1959.
- Mark van Atten. *On Brouwer*. Wadsworth Philosophers Series. Thomson Learning, London, 2004.

- Mark van Atten. *Brouwer Meets Husserl: On the Phenomenology of Choice Sequences*, volume 335 of *Synthese Library*. Springer, Dordrecht, 2006.
- Mark van Atten. The development of intuitionistic logic. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. The Metaphysics Research Lab, Stanford University, summer 2009 edition, 2009. <http://plato.stanford.edu/archives/sum2009/entries/intuitionistic-logic-development/>.
- Dirk van Dalen, editor. *Brouwer's Cambridge Lectures on Intuitionism*. Cambridge University Press, 1981.
- Dirk van Dalen. From Brouwerian counterexamples to the creating subject. *Studia Logica*, 62:305–313, 1999.
- Jean van Heijenoort, editor. *From Frege to Gödel: A Sourcebook in Mathematical Logic, 1879-1931*. Harvard University Press, 1967.
- W.P. van Stigt. *Brouwer's Intuitionism*, volume 2 of *Studies in the History and Philosophy of Mathematics*. North-Holland, Amsterdam, 1990.
- W. Veldman. An intuitionistic completeness theorem for intuitionistic predicate logic. *Journal of Symbolic Logic*, 41(1):159–166, 1976.
- W. Veldman. On the continuity of functions in intuitionistic real analysis. Technical Report 8210, Department of Mathematics, Katholieke Universiteit Nijmegen, 1982.
- W. Veldman. Understanding and using Brouwer's continuity principle. Technical Report 0008, Department of Mathematics, University of Nijmegen, 2000.
- Hao Wang. *Reflections on Kurt Gödel*. MIT Press, 1987.
- Hermann Weyl. Über die neue Grundlagenkrise der Mathematik. *Mathematische Zeitschrift*, 10:37–79, 1921. English translation in (Mancosu 1998: pp. 86-118).

Alfred North Whitehead and Bertrand Russell. *Principia Mathematica*, volume 1. Cambridge University Press, Cambridge, 1910.

Andrew Wiles. Modular elliptic curves and Fermat's last theorem. *Annals of Mathematics*, 141(3):443–551, 1995.

Crispin Wright. Strict finitism. *Synthese*, 51(2):203–282, 1982. Reprinted in (Wright 1993: pp. 107-175).

Crispin Wright. *Realism, Meaning, & Truth*. Blackwell, 2nd edition, 1993.

Michael Randall Koss

Curriculum Vitae

Indiana University, Bloomington
Department of Philosophy
Sycamore Hall 026
1033 E. Third St.
Bloomington, IN 47405-7005, USA
Phone: (812) 855-9503
Fax: (812) 855-3777

942 E. Hillside Dr.
Bloomington, IN 47401
Phone: (248) 444-6242
Email: mrkoss@indiana.edu
Website: <http://mypage.iu.edu/~mrkoss/>

EDUCATION

Indiana University, Bloomington, IN:

Ph.D., Philosophy (2013)

Minor in History and Philosophy of Science

Dissertation: *Semantic and Mathematical Foundations for Intuitionism*

Committee: David Charles McCarty (chair); Gary Ebbs; Amit Hagar; Neil Tennant

University of Michigan, Ann Arbor, MI:

B.A., Philosophy and Mathematics (2006)

RESEARCH AREAS

Area of Specialization: Philosophy of Mathematics, Philosophy of Logic

Areas of Competence: Formal Logic, Philosophy of Language, History of Analytic Philosophy, Philosophy of Science, Ancient Philosophy, Medieval Philosophy, Kierkegaard

PRESENTATIONS

1. "Finding a Place for the Philosophy of Math," Indiana University Logic Seminar, Bloomington, IN, Oct. 10th, 2012.
2. "Some Obstacles Facing a Semantic Foundation for Intuitionistic Logic," University of Western Ontario, London, ON, Feb. 7th, 2012.
3. "Some Obstacles Facing a Semantic Foundation for Intuitionistic Logic," California State University San Bernardino, San Bernardino, CA, Feb. 3rd, 2012.
4. "The Coherence of Classical Mathematics from the Constructive Point of View", 2nd Midwest Conference on Philosophy of Science, Technology, Engineering, and Mathematics, Indiana University-Purdue University Fort Wayne, Fort Wayne, IN, Nov. 11th, 2011.
5. "The Coherence of the Incoherence", Indiana University Logic Seminar, Bloomington, IN, Oct. 19th, 2011.
6. "Wittgenstein on Meaning and Dummett's Antirealism", Indiana Philosophical Association Spring Meeting 2011, Ball State University, Muncie, IN, April 29th, 2011.
7. "Some Tarskian Reflections on Proof-Conditional Semantics", Indiana University Logic Seminar, Bloomington, IN, March 10th, 2010.
8. "Wittgenstein on Meaning and Dummett's Antirealism", Midsouth Philosophy Conference, University of Memphis, Memphis, TN, March 5th, 2011.
9. "Reexamining Brouwer's Earliest Challenge to Classical Logic", Indiana Philosophical Association Fall Meeting 2009, Indiana University, Bloomington, IN, Dec. 12th, 2009.

10. "A Comparison of 13th Century Theories of Supposition", The 12th Annual E.A. Moody Workshop in Medieval Philosophy: The Logic of Peter of Spain, University of California Los Angeles, Los Angeles, CA, March 7th, 2009.

BOOK REVIEWS

1. *Foundational Theories of Classical and Constructive Mathematics*, Giovanni Sommaruga, ed. Springer, 2011. Reviewed in *Philosophia Mathematica* 20(2), June 2012, pp. 267-271.

READING LANGUAGES

- German
- Latin
- Ancient Greek
- Danish
- Spanish

AWARDS, FELLOWSHIPS, & GRANTS

1. Louise B. McNutt Graduate School Dissertation Fellowship, Graduate School of Arts and Sciences, Indiana University, Bloomington, Academic Year 2011-2012.
2. Graduate Academic Excellence Award, Department of Philosophy, Indiana University, 2008
3. Outstanding Associate Instructor, Department of Philosophy, Indiana University, 2007.
4. Dean's Merit Scholarship, University of Michigan College of Literature, Sciences, and the Arts, 2002-2006.

SERVICE

- Graduate Student Representative to Graduate Curriculum Committee, Department of Philosophy, Indiana University, Academic Year 2011-2012.
- President, Graduate Association of Student Philosophers, Department of Philosophy, Indiana University, Academic Year 2010-2011.
- Graduate Student Representative to Graduate Admissions Committee, Department of Philosophy, Indiana University, Academic Year 2009-2010.
- Member of *ad hoc* Committee on Graduate Student Travel Fund Policy, Department of Philosophy, Indiana University, Academic Year 2009-2010.
- Referee: *Notre Dame Journal of Formal Logic*, *Polish Journal of Philosophy*
- Co-coordinator and webmaster (with Rega Wood and Carrie Swanson) for conference on *Aristotelian Logic and Metaphysics*, Indiana University, Bloomington, IN, October 20th-21st, 2012.

PROFESSIONAL MEMBERSHIPS

- American Philosophical Association
- Association for Symbolic Logic

TEACHING HISTORY

Assistant Instructor: Assisted teaching the course in coordination with primary instructor; planned and taught weekly discussion-section classes, met with students, organized class materials, graded papers and exams.

Indiana University, Bloomington

1. P100: Introduction to Philosophy, Spring 2007, Spring 2008, Spring 2009, Spring 2010

2. P105: Critical Thinking and Reasoning, Spring 2011, Spring 2013
3. P135: Introduction to Existentialism, Fall 2007
4. P150: Elementary Logic, Fall 2008
5. P201: Ancient Greek Philosophy, Fall 2009
6. P250: Introductory Symbolic Logic, Fall 2006

Primary Instructor: Solely responsible for all aspects of the course; developed course-packet for each class, constructed syllabus, delivered lectures, designed and graded assignments, papers and exams.

Indiana University, Bloomington

1. P100: Introduction to Philosophy, Summer 2010
2. P150: Elementary Logic, Fall 2010
3. P135: Introduction to Existentialism, Fall 2012