

# A Defence of Predicativism as a Philosophy of Mathematics

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# Preface

## Declaration & Statement of length

This dissertation is the result of my own work and includes nothing which is the outcome of work done in collaboration except where specifically indicated in the text. No part of this dissertation has been submitted for any other degree or qualification. This dissertation conforms to the word limits set by the Degree Committee of Philosophy. It is approximately 75,000 words in length.

## Acknowledgements & Dedication

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This thesis is dedicated to my teachers.



# Abstract

## **A Defence of Predicativism as a Philosophy of Mathematics**

**T. J. Storer**

A specification of a mathematical object is impredicative if it essentially involves quantification over a domain which includes the object being specified (or sets which contain that object, or similar). The basic worry is that we have no non-circular way of understanding such a specification. Predicativism is the view that mathematics should be limited to the study of objects which can be specified predicatively.

There are two parts to predicativism. One is the criticism of the impredicative aspects of classical mathematics. The other is the positive project, begun by Weyl in *Das Kontinuum*, to reconstruct as much as possible of classical mathematics on the basis of a predicatively acceptable set theory, which accepts only countably infinite objects. This is a revisionary project, and certain parts of mathematics will not be saved.

Chapter 2 contains an account of the historical background to the predicativist project. The rigorization of analysis led to Dedekind's and Cantor's theories of the real numbers, which relied on the new notion of arbitrary infinite sets; this became a central part of modern classical set theory. Criticism began with Kronecker; continued in the debate about the acceptability of Zermelo's Axiom of Choice; and was somewhat clarified by Poincaré and Russell. In the light of this, chapter 3 examines the formulation of, and motivations behind the predicativist position.

Chapter 4 begins the critical task by detailing the epistemological problems with the classical account of the continuum. Explanations of classicism which appeal to second-order logic, set theory, and primitive intuition are examined and are found wanting.

Chapter 5 aims to dispell the worry that predicativism might collapse into math-

emational intuitionism. I assess some of the arguments for intuitionism, especially the Dummettian argument from indefinite extensibility. I argue that the natural numbers are not indefinitely extensible, and that, although the continuum is, we can nonetheless make some sense of classical quantification over it. We need not reject the Law of Excluded Middle.

Chapter 6 begins the positive work by outlining a predicatively acceptable account of mathematical objects which justifies the Vicious Circle Principle. Chapter 7 explores the appropriate shape of formalized predicative mathematics, and the question of just how much mathematics is predicatively acceptable.

My conclusion is that all of the mathematics which we need can be predicativistically justified, and that such mathematics is particularly transparent to reason. This calls into question one currently prevalent view of the nature of mathematics, on which mathematics is justified by quasi-empirical means.

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# Chapter 1

## Introduction

### 1.1 Overview

This thesis presents a case for predicativism as a philosophy of mathematics. By predicativism, I will mean what is sometimes called predicativism given the natural numbers. The position can be briefly characterized as the acceptance of the natural numbers as a foundation for mathematics, and the rejection of impredicative methods in building up from that foundation. The view was first clearly put forward by Hermann Weyl in his short book of 1918, *Das Kontinuum*.

As its title suggests, the main object of Weyl's book was to develop a satisfactory account of the arithmetical continuum, i.e., the real-number system. There were two familiar accounts of the real numbers available to Weyl, which are essentially equivalent: the reals can be explained in terms either of Cauchy sequences (certain sequences of rational numbers) or in terms of Dedekind cuts (certain sets of rational numbers). Given that sequences can be implemented as sets, the question of what real numbers there are boils down either way to a question about what infinite sets there are. So what theory of infinite sets should we adopt? The dominant account when Weyl was writing — and to this day — was a full-blooded Cantorian set theory. Weyl found such an account unsatisfactory because of its use of a particular concept of set which was, he maintained, ill-defined. In its place, Weyl proposed a foundation based on the natural numbers, and a non-Cantorian 'logical' (i.e. predicative) concept of sets of natural numbers.

More specifically, Weyl's objection to Cantorian set theory, and to the classical

analysis which takes it as a foundation, was that they are viciously circular. The alleged circularity consists in the commitment to the existence of sets, including particularly sets of natural numbers, (or to the existence of real numbers) which are specified by means of a quantification over all sets (or over all real numbers): in short, impredicativity.

Impredicativity in this sense, then, is a matter of offending against the so-called Vicious Circle Principle (VCP), which states that no set can contain objects which are definable only in terms of that set. We say, then, that an instance of the general principle of set-abstraction or comprehension,

$$(\text{CP-}\epsilon) \quad \exists x \forall y (y \in x \leftrightarrow \phi(y))$$

is impredicative if  $\phi$  contains a bound variable ranging over a domain of sets which includes the very set introduced by that instance of comprehension; and it is predicative otherwise. In full generality, CP- $\epsilon$  is of course inconsistent; so if it is to be accepted at all, it must be restricted in some way. Standard ZF-style set theory, in which classical mathematics can be regimented, avoids paradox by requiring  $\phi$  to be of the form  $y \in z \ \& \ \psi(y)$ ; this yields the axiom scheme of Separation. Separation is still impredicative in that it places no restrictions on the quantifiers which may appear in  $\psi$ . To put the matter another way, although Separation only allows us to form subsets of sets which we already know to exist, it allows us to pick out the members of such a subset by a condition which may quantify over the entire set-theoretic universe.

But does offending against VCP really entangle us in vicious circles? Well, a set specification is supposed to pin down a set  $x$  by settling, for each  $y$ , whether or not  $y$  belongs to  $x$ ; it does this by means of the condition  $\phi(y)$ . In the impredicative case,  $\phi(y)$  features a quantified claim, one instance of which will concern the set  $x$  that we are trying to specify. The instance may, for example, return us to the question of whether or not  $y$  belongs to  $x$ . This is the reason for thinking that impredicative set specifications may be viciously circular. But we will need to return to the question whether this is right.

A positive motivation for a predicativity constraint on class comprehension is that, as Quine puts it,

it realizes a constructional metaphor: it limits classes to what *could* be generated over an infinite period from unspecified beginnings by using,

for each class, a membership condition mentioning only preexistent classes.<sup>1</sup>

As such, the sets of predicative set theory can be explicitly defined. This makes predicative set theory much less questionable than impredicative set theory; indeed, we will later see that the consistency (and indeed correctness) of predicative set theory is immediate.

The possibility of a predicative set theory then suggests a programme and a philosophy. Predicativism as a mathematical programme is the development of (as much as is possible of) classical mathematics in a predicative manner. This might be done, for example, within a predicative set theory or a predicative theory of second-order arithmetic. One striking difference between classical and predicative mathematics is that predicative mathematics does not countenance the Cantorian ‘higher infinite’: the only infinite objects that are recognized by predicative mathematics are countably infinite.

Predicativism as a philosophy of mathematics is the position that this is in some sense the right way to do mathematics, and that there is something wrong with classical mathematics. As with other revisionary philosophies of mathematics, most notably intuitionism and finitism, it argues that there is a privileged core of acceptable mathematics. Such a revisionist project gives urgency to the mathematical programme, and in particular the technical question of its scope: Exactly how much of the body of existing classical mathematics falls within the acceptable core?

We can distinguish two central philosophical claims in such revisionary positions: a positive claim, that good sense can be made of the privileged core; and a negative claim, that there is some problem with (justifying or making sense of) what goes beyond it.

Such revisionary programmes also come in two flavours: evangelical, and tolerant. Tolerant predicativism is the mild claim that there is some real added value in proofs that only use predicatively acceptable means, and some value in being explicit about when we are, and when we are not involved in impredicativity. Compare the attitude of most working mathematicians towards the Axiom of Choice: they suppose that you can assume it if you need it, but it’s poor etiquette to assume it without mentioning the fact; and a certain value is accorded to proofs which avoid the assumption. Similarly, everyone prefers to have a direct, rather than a *reductio*

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<sup>1</sup> Quine, *Set Theory and its Logic* p. 243.

proof of a disjunctive or an existential claim, because it tells us, at least in principle, more about the subject: namely which disjunct holds, or which item witnesses the existential. (Of course, proofs which are more constructive in these ways may well be longer and more complex, and so there may well also be a place for an elegant non-constructive proof, especially for textbook purposes.) The tolerant predicativist similarly argues that we should privilege predicative proofs when they can be had.

Evangelical predicativism pushes the stronger claim that only predicative mathematics is genuinely mathematics. Certainly, that part of mathematics which is predicative enjoys a significantly less problematic epistemology than the rest. But the evangelical claim is twofold: first, that impredicative maths is not really mathematics — that it is either an empty formal game, or an entirely mysterious ‘physics of abstract objects’;<sup>2</sup> and second, that predicative mathematics preserves the traditional epistemology of mathematics as (more or less) transparent to pure reason.

In this thesis, I argue for a weak form of evangelical predicativism. I also explore the scope of predicative mathematics, and suggest that adopting evangelical predicativism does not mean sacrificing as much mathematics as is often thought. In fact, the predicatively acceptable core of mathematics seems to include all of the mathematics which is used in the natural sciences.

## 1.2 Plan of the rest of the thesis

Chapter 2 sketches the historical origins of impredicative mathematics; it also highlights some of the objections and resistance to those developments, which form the pre-history of predicativism. The historical approach serves to introduce and explain some of the connections between many of the themes which will be explored in the rest of the thesis.

Chapter 3 explains more carefully what predicativism, and the predicativist programme, actually are. It finishes the introductory part of the thesis by presenting both the basic worry which underlies the predicativist objection to classical mathematics, and the predicativist response to that worry. The worry is that quantification over open-ended totalities, such as that of the sets of natural numbers, is unclear, and so unsuitable to serve in definitions. The proposal is that such definitions can

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<sup>2</sup> I owe the phrase to Coffa, *Semantic tradition*, though the same thought is expressed by Wittgenstein: ‘Arithmetic as the natural history (mineralogy) of numbers.’ (Wittgenstein, *RFM* p. 229, §IV.11.)

be avoided, and that we can instead develop mathematics using sets defined by unproblematic (in particular arithmetical) means.

Chapter 4 deals with the negative side of the predicativist project, making a case that the epistemology of classical mathematics is significantly more problematic than that for predicative mathematics. The focus is on the key distinction between classical and predicative mathematics, their treatment of the continuum. I discuss three broad styles of justification of the classical view of the continuum. The first route is based on the claim that our understanding is mediated by means of logic, and our understanding of the second-order consequence relation. The second endorses the set theoretic project as our route to understanding the continuum. The third simply takes the classical continuum as given to us in intuition. I suggest that none of these approaches is adequate to the task.

Chapter 5 aims to dispel the worry that predicativism might collapse into mathematical intuitionism. I assess some of the arguments for intuitionism, especially Dummett's argument from indefinite extensibility. Broadly speaking, I endorse Dummett's argument, though I disagree with him as to its upshot. I argue that the natural numbers are not indefinitely extensible, and that, although the continuum is, we can nonetheless make some sense of classical quantification over it. We need not reject the Law of Excluded Middle.

Chapter 6 is concerned with explaining the predicative conception of set, as it is developed in both *Das Kontinuum* and in *Principia Mathematica*. I criticize both Russell's arguments for the axiom of Reducibility (which undoes the mathematical effect of his predicativity requirement), and Gödel's arguments against the Vicious Circle Principle.

Chapter 7 explores appropriate formal theories for developing predicative mathematics, and examines how and where predicative mathematics fits into the 'Reverse Mathematics' programme. It discusses the scope of predicatively acceptable mathematics, and whether impredicative mathematics can be justified by appealing to its indispensability (either to the natural sciences, or to mathematics itself).

Chapter 8 concludes with some reflections on what has been achieved, and what remains to be done.



## Chapter 2

# History

One way in to the issues surrounding the predicativist programme is through the historical development of impredicative mathematics. The crucial moment in this story is Cantor's proof of 1874 of the uncountability of the set of the real numbers. But crucial moments can only be seen as such in their wider context, and the story starts rather further back, and runs somewhat further forward.

The main force driving the changes to mathematics was the programme of the rigorization of analysis: infinitary set theory and the modern conception of the continuum both arose from this programme. This story is a largely familiar one, and so it is presented here fairly briskly, largely by way of headline reminders: however, I try to draw out some aspects of the story which are relevant to predicativism, and which are not always stressed as much as they merit, or explained in quite the way that is done here.

This chapter falls into three sections. The first, §2.1, covers the back-story: the development of the mathematics of continuous magnitude into the modern discipline of analysis, and in particular the efforts to make analysis rigorous. It takes us up to Dedekind's account of the real numbers, which is where set theory, and in particular the idea of arbitrary infinite collections, enter the story.

§2.2 is devoted to Cantor's set theory, which arose from somewhat different considerations, but which developed into a general theory of (potentially arbitrary infinite) collections, and hence into a foundational theory for the real numbers.

§2.3 looks at some of the repercussions of Cantor's set theory: at its development into modern set theory, at the hands of Zermelo, and at the criticisms and concerns

caused by the set-theoretic paradoxes, and by the Axiom of Choice. These debates constituted the beginnings of the ‘crisis in the foundations’, and formed the intellectual background to the revisionary projects of Hilbertian finitism, Brouwerian intuitionism, and Weylian predicativism.

## 2.1 The back-story

### 2.1.1 Beginnings: Irrationals, the infinite, and the development of naive analysis

According to tradition, it was a member of the school of Pythagoras who first demonstrated the existence of irrational magnitudes: the length of the diagonal of a unit square,  $\sqrt{2}$ , is not precisely equal to any ratio of those units. This result gave the first suggestion that the scale of continuous magnitudes introduces complexities which are not found in the arithmetic of discrete quantity.

Aristotle distinguished between the two sorts of quantity, and claimed that it is in the continuous that we first encounter the infinite, through repeated (in theory, unlimited) division of an interval.<sup>1</sup> This provides a model: our idea of potential infinity is a description of some idealized process that can be carried on without limit.

Aristotle’s opinion on the infinite, as on much else, became the starting point for later thought. Most importantly, Aristotle claimed that the actual infinite could not exist: the only infinite domains which are possible are potentially infinite.

The infinite is not that of which *nothing* is outside, but that of which there is *always something* outside. [...] Nothing is complete which has no end, and the end is a limit.<sup>2</sup>

This Aristotelian injunction against the actual infinite was modified in medieval thought to the view that only God is actually infinite; but the effect was much the same, in keeping the infinite firmly beyond of the reach of mathematics. Augustine, for instance, insisted that although the infinite sequence of natural numbers is uncompletable for us, it is grasped as finite by God:

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<sup>1</sup> Aristotle, *The Physics* 200b17, ff.    <sup>2</sup> Ibid. 206b33–207a15.

Let us then not doubt that every number is known to him ‘of whose understanding,’ as the psalm goes, ‘there is no set number.’ [...] Wherefore, if whatever is comprehended by knowledge is limited by the comprehension of him who knows, assuredly all infinity is also in some ineffable way finite to God because it is not incomprehensible for his knowledge.<sup>3</sup>

The traditional attitude of extreme caution towards the (actually) infinitely large and the infinitely small was, in practice at least, cast aside when the infinitesimal calculus was developed by Leibniz and Newton. While Newton’s kinematic fluxions were very different from Leibniz’s infinitesimals, both accounts had similarly shaky foundations: they made essential use of a concept of limit which was not properly understood, and of series (infinite sums) without an understanding of whether or not they would converge. Certain quantities were regarded as at some times being equal to zero, and at other times as being legitimate divisors.

Analysis, the branch of mathematics which was based on these foundations, quickly grew to resemble the Wild West. The Bernoulli brothers, for instance ‘did extensive work concerning series, and they showed almost no awareness of any need for caution. Wrong results were described as paradoxes.’<sup>4</sup> The general ethos is well-expressed by the maxim attributed to d’Alembert: Go forward, and faith will come to you!<sup>5</sup>

This was not an unreasonable attitude. The methodology was to use whatever mathematical techniques were available, even though they were known sometimes to lead to nonsensical results, and to check the answers by calculation. This quasi-experimental method led to many important new results, and progress in mathematics went hand-in-hand with progress in physics, which both provided confirmation of the mathematics used, and suggested new mathematical problems to study. (Particularly fruitful for analysis was the consideration of the motion of a vibrating string.) Mathematics became, in part, a branch of physics:

The mathematics did not stand on its own. [...] Since nothing could be proved in a reliable way, one tried to confirm the results of a mathematical derivation on some independent grounds. If a result was contrary

<sup>3</sup> Augustine, *City of God* XII, xix; p. 91 of vol. IV (quoting Psalm 147:5). Cantor seems to have been much taken with this idea, and with the idea that his theory of the infinite could give us knowledge of the divine. See Hallett, *Cantorian set theory* p. 35.    <sup>4</sup> Lavine, *Understanding the Infinite* p. 22.

<sup>5</sup> Quoted Coffa, *Semantic tradition* p. 25.

to expectations, it was often dismissed.<sup>6</sup>

### 2.1.2 The rigorization of analysis

The conceptual chaos did not go entirely unchallenged. Berkeley's criticisms of the calculus,<sup>7</sup> in particular his sardonic characterization of Newton's fluxions as 'the ghosts of departed quantities', are now well-known among philosophers; but they had little influence on the mathematical community of the time.<sup>8</sup>

However, towards the end of the eighteenth century, the worries seem to have become more prevalent. One instance of this is the mathematical prize essay competition announced by the Berlin Academy in 1784:

It is well known that higher mathematics continually uses infinitely large and infinitely small quantities. Nevertheless, geometers, and even the ancient analysts, have carefully avoided everything which approaches the infinite; and some great modern analysts hold that the terms of the expression 'infinite magnitude' contradict one another. The Academy hopes, therefore, that it can be explained how so many true theorems have been deduced from a contradictory supposition, and that a principle can be delineated which is sure, clear — in a word, truly mathematical — which can appropriately be substituted for 'the infinite'.<sup>9</sup>

Lagrange attempted to meet the challenge: his *Theorie des fonctions analytiques* (1797) attempted to found calculus algebraically, avoiding the problematic notions of limit and continuity. But the attempt proved to be unworkable.<sup>10</sup>

The Academicians were not seeking rigour for the sake of it. (Indeed, rigour is rarely pursued for its own sake. Bolzano and Frege are notable exceptions to this rule; the reward for their supererogation was to have their work largely ignored by

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<sup>6</sup> Lavine, *Understanding the Infinite* p. 28.    <sup>7</sup> Berkeley, *The Analyst. Or a Discourse [...]* (1734); the title of the pamphlet is worth giving in full: *The Analyst. Or a Discourse Addressed to an Infidel Mathematician. Wherein It Is Examined Whether the Object, Principles, and Inferences of the Modern Analysis Are More Directly Conceived, or More Evidently Deduced, than Religious Mysteries and Points of Faith. 'First Cast the Beam Out of Thine Own Eye; and Then Shalt Thou See Clearly to Cast Out the Mote Out of Thy Brother's Eye.'* (The reference is to Matthew, 7:3.)    <sup>8</sup> Lavine, *Understanding the Infinite* p. 24 suggests that the earlier criticisms of Bernard Nieuwentijdt were more widely read: certainly he prompted a reply from Leibniz. Mancosu, 'Metaphysics of the calculus' discusses some other early criticisms of the calculus.    <sup>9</sup> Quoted in Grabiner, *The Origins of Cauchy's Rigorous Calculus* pp. 41–2.

<sup>10</sup> Ibid. pp. 36, 44.

the mathematical community.) The call for rigorous explanation of mathematical talk of the infinite came largely for reasons internal to mathematics. In particular, it was the research programme which began with the vibrating-string problem (and Fourier's mathematically very similar problem of heat flow) which created the need for an increase in rigour; or to be more precise, it was the fact that this research programme was being pursued with the limited computational resources of the time. Euler had been interested in series of *numbers*, where the problematic results caused by divergent series can easily be spotted by summing the first few terms: but Cauchy, developing his work on Fourier series, was investigating series of *functions*. Here calculation was simply unfeasible: the only way to tell computationally if a series of functions  $f(x) = \sum_{i=0}^{\infty} f_i(x)$  is convergent was to pick a few points to try. But even if each point one tried could be seen to converge quickly, the possibility remains that one had simply picked the wrong points. Clearly a better way was needed to know when limits can safely be taken, and it was above all this problem that prompted Cauchy's ground-breaking work of the 1820s.<sup>11</sup>

Bolzano's proof of the Intermediate Value Theorem is also particularly noteworthy, despite its very limited immediate impact: it was published as his *Rein analytischer Beweis des Lehrsatzes, dass zwischen je zwei Werthen, die ein entgegengesetztes Resultat gewähren, wenigstens eine reelle Wurzel der Gleichung liege* (1817). Bolzano's aim was to avoid geometric intuition, and instead to prove the result by calling only on very basic assumptions about numbers and functions. The object was not just an epistemological one — to achieve greater certainty for the results — but a semantic or a logical one: the object was to expose interrelations and better to systematize mathematics, to clarify the meaning of mathematical claims, and to identify what their grounds were.<sup>12</sup> And similarly, this question of the grounds of the truth of a mathematical statement is not so much an epistemological question (how do we come to know?), as a metaphysical one: In what does the truth of a statement consist? Bolzano's desire to expel intuition from analysis (like Frege's desire to expel intuition from arithmetic) needs to be understood as taking issue with Kant's doctrine that the statements of mathematics are synthetic.

The work on functions and series of functions in eighteenth and nineteenth-century analysis went hand-in-hand with a broadening of the class of functions which

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<sup>11</sup> Lavine, *Understanding the Infinite* p. 32.    <sup>12</sup> For discussion of some of the issues here, see Coffa, *Semantic tradition* Ch. 2.

were considered. Euler, for instance, defined a continuous function to be one which was given by a single analytic expression, i.e. an algebraic expression built up from polynomials and trigonometric functions. But he also considered a broader class of functions, given by possibly different analytic expressions on different intervals, as giving possible descriptions of the initial state of a vibrating string. (The main reason why the vibrating string problem and the problem of heat-transfer in a bar were such fertile research programmes was that in both cases, the initial condition can be any piecewise continuous function.)

Integration was traditionally understood as the inverse of differentiation, but this definition did not apply smoothly to piecewise analytic functions (as the differential may not exist at certain points), and was not of much use when considering functions which were not the differentials of known functions. Such considerations motivated Cauchy to give (in 1823) an independent definition of the integral, as the limit of a sum of rectangles.<sup>13</sup> This definition provided a justification for Fourier's use of integrals of functions which were not given by analytic expressions.

In 1829, Dirichlet gave the function

$$f(x) = \begin{cases} 0 & \text{for } x \in \mathbf{Q} \\ 1 & \text{for } x \in \mathbf{R} - \mathbf{Q} \end{cases}$$

as an example of a function which could not be integrated, an explicit case of allowing wholly non-analytic means to define a function. And in 1837, Dirichlet gave the first truly modern definition of a continuous function as any association of values to arguments that is single-valued and varies continuously. Later, Riemann extended Cauchy's methods for integration, and gave an example of a function that was integrable, but had infinitely many discontinuities in every interval, however small.

This broadening of the concept of 'function' is often described as a move away from the idea of a function as given by some sort of rule or definition and towards the idea of a function as an arbitrary correlation of values to arguments.<sup>14</sup> But we should be wary of taking this to mean that Dirichlet's concept of function included *unspecifiable* correlations of argument and value, rather than just leaving it open how such a correlation was to be given. I will return to this important point in Ch. 3.3.6 below.

<sup>13</sup> Grabiner, *The Origins of Cauchy's Rigorous Calculus* pp. 140–145. <sup>14</sup> E.g. Lavine, *Understanding the Infinite* again.

### 2.1.3 The arithmetization of the reals — Dedekind

One object of the rigorization of analysis was to pull the discipline away from its reliance on geometric intuition; and one result of this was the discovery of some of the close ties between analysis and arithmetic. This sentiment was well-expressed by Kronecker:

Thus arithmetic cannot be demarcated from that analysis which has freed itself from its original source of geometry, and has been developed independently on its own ground; all the less so, as Dirichlet has succeeded in attaining precisely the most beautiful and deep-lying arithmetical results through the combination of methods of both disciplines.<sup>15</sup>

A notable example was Dirichlet's proof of 1837 that any arithmetic progression containing two relatively prime terms must contain an infinite number of primes. This was remarkable for using essentially analytic methods (continuous variables and limits), and also for its non-constructive character. It can be taken as the birth of analytic number theory

On the other hand, algebraic methods were also applied with great success to geometry. Most notably, the investigation of the real numbers by means of (what became known as) Galois theory led to impossibility proofs for the famously elusive Euclidean problems of squaring a circle, doubling a cube and trisecting an angle; and also to positive results such as Gauss's construction for the 17-gon.<sup>16</sup>

It was discovered by these methods that as well as the surds, irrational quantities known as such to the Greeks, there are also real numbers which are *transcendental*, that is, which are not roots of rational polynomials. (Those reals which are roots are called algebraic reals.) Liouville's somewhat artificial example of  $\sum_{n=1}^{\infty} 10^{-n!}$  was followed by proofs that the familiar numbers  $e$  and  $\pi$  were transcendental (due, respectively, to Hermite in 1873 and Lindemann in 1882). It was in this context (in 1874, in fact) that Cantor published his first proof of the uncountability of the real numbers generally. Given that the algebraic numbers are easily shown to be countable, this can be taken as a proof of the existence of transcendental reals; indeed, as a proof that almost all of the reals are transcendental. (We will return to this in §2.2 below.)

<sup>15</sup> Kronecker, *Vorlesungen über Zahlentheorie* I p. 5; quoted in Stein, 'Logos, logic, and *logistiké*'.

<sup>16</sup> Stewart, *Galois Theory* is an excellent textbook for this material, and gives some of the historical background.

The discovery of transcendentals presses the question: What are the reals? It came to be seen that the most fundamental part of the programme of the rigorization of analysis was the task of giving a rigorous definition of the real numbers from which all of their properties could be deduced. It was Dedekind who first tackled the job in 1872.<sup>17</sup>

Dedekind's objective can be seen as an extension of Bolzano's: to give a foundation for the real number system that did not appeal to geometrical intuition. Instead, the basis was to be arithmetical, and Dedekind could appeal to good authority here:

it appears as something self-evident and not new that a theorem of algebra and higher analysis, no matter how remote, can be expressed as a theorem about natural numbers, — a declaration I have heard repeatedly from the lips of Dirichlet.<sup>18</sup>

The crucial step in Dedekind's account was the construction of the real number system from the rationals. Dedekind introduced the notion of a 'cut' in the rationals, that is, a partition of the rationals into two classes, such that the first class is closed downwards (any rational less than any member of the first class is also in the first class), and the second class is closed upwards. Some of these cuts, such as  $\left\{ \left\{ q \in \mathbf{Q} \mid q < \frac{1}{3} \right\}, \left\{ q \in \mathbf{Q} \mid q \geq \frac{1}{3} \right\} \right\}$ , are produced by rational numbers, in the sense that there is either a smallest rational in the upper class, or a largest in the lower class; but others, such as  $\left\{ \left\{ q \in \mathbf{Q} \mid q < 0 \vee q^2 < 2 \right\}, \left\{ q \in \mathbf{Q} \mid q > 0 \ \& \ q^2 \geq 2 \right\} \right\}$  are not produced by any rational. We can define in the obvious ways operations on cuts which are analogous to the arithmetical operations on rationals. For example, if we have cuts  $\alpha = \langle A_1, A_2 \rangle, \beta = \langle B_1, B_2 \rangle$ , we can define  $\alpha + \beta$  to be  $\langle C_1, C_2 \rangle$ , where  $C_1 = \{ q \in \mathbf{Q} \mid \exists r \in A_1 \exists s \in B_1 . r + s \geq q \}$ , and  $C_2 = \mathbf{Q} - C_1$ . Similarly, an obvious order relation can be defined on the cuts. The domain of all possible cuts of the rationals, equipped with these arithmetic-like operations and the order relation, forms a system of 'numbers' which can be identified with the reals. (Dedekind famously did not make this identification, instead regarding the reals as new items corresponding to the cuts, and formed from them by a process of abstraction; but this ontological shuffle does not affect anything here.) Dedekind succeeded in proving some of the fundamental theorems of analysis, such as the convergence of bounded monotone

<sup>17</sup> Dedekind, *Stetigkeit und irrationale Zahlen*. <sup>18</sup> First preface to Dedekind, *Was sind und was sollen die Zahlen?* p. 35.

sequences, from the fact that the reals are complete, in the sense that considering cuts of real numbers does not lead outside the reals.

Lurking unnoticed was the crucial assumption, that the infinity of possible cuts of the rationals, including those that cannot be finitely specified, can legitimately be considered as a definite domain. As Kanamori has argued, the work here was seminal not just for the classical understanding of the continuum, but also for the set-theoretic considerations which were used:

The formulations of the real numbers advanced three important predispositions for set theory: the consideration of infinite collections, their construal as unitary objects, and the encompassing of arbitrary such possibilities.<sup>19</sup>

Lipschitz objected<sup>20</sup> that Dedekind's statement of the 'continuity' (i.e. connectedness) of the real line was unnecessary, as no-one could conceive of a line without that property. Dedekind flatly rejected this: 'If space has at all a real existence it is *not* necessary for it to be continuous.'<sup>21</sup> The point was demonstrated by Dedekind's example of the existence of models (to use modern terminology) of Euclidean geometry where the co-ordinates of all of the points are algebraic numbers: 'the discontinuity of this space would not be noticed in Euclid's science, would not be felt at all.'<sup>22</sup> Dedekind's response here makes clear not only his commitment to the programme of rigorization, but also suggests a theme that will be picked up in chapter 7 below: that for scientific purposes, we do not in fact need to assume as much as the classical continuum.

#### 2.1.4 The arithmetization of the reals — Kronecker's programme

Kronecker was the most prominent early critic of the kind of set-theoretic account of the real numbers championed by Dedekind. Kronecker emphasized the importance, to all of mathematics, of construction or computation, founded ultimately on the natural numbers. He was therefore happy to work with specific irrational numbers,

<sup>19</sup> Kanamori, 'Cantor to Cohen' p. 3. Cantor also developed a theory of the real numbers, based on Cauchy sequences rather than cuts. However, the difference of implementation is relatively superficial.

<sup>20</sup> Dedekind, *Gesammelte mathematischen Werke* vol. III. <sup>21</sup> (I.e. connected.) Dedekind, *Stetigkeit und irrationale Zahlen* §3 = Dedekind, *Essays on the Theory of Numbers* p. 12 <sup>22</sup> First preface to Dedekind, *Was sind und was sollen die Zahlen?* = Dedekind, *Essays on the Theory of Numbers* p. 38

but regarded the consideration by Dedekind and Cantor of the domain of reals, or of arbitrary reals, as an illegitimate departure from this foundation.

Kronecker was wholly in the tradition of the rigorization of analysis; but he thought that this tradition had taken a wrong turning. To put the matter neutrally, we can see that there was at least a fork in the path: one path forward had been forged by Dedekind and Cantor, and was followed by most mathematicians; but Kronecker took a different path.

Kronecker is sometimes taken to have been opposed to all use of irrationals in analysis.<sup>23</sup> In fact, his view was that

one should only work with specific irrational numbers; starting to talk about the totality of irrational numbers would mean leaving the foundations [i.e. arithmetical laws] on which the irrational number was constructed.<sup>24</sup>

Kronecker is often caricatured as an obstructive conservative — or reactionary — in mathematics; but his work can instead be seen as the start of a positive programme in foundations — to rigorize analysis and abstract algebra not set-theoretically, as Dedekind and Cantor were doing, but arithmetically and algorithmically.

Kronecker can be seen in some respects as a forerunner of Weyl's predicativism; but his constructivism applied also to arithmetic. One example of this is Kronecker's 'completion' of the proof of Dirichlet's theorem on arithmetical progressions mentioned above: what Kronecker did was to make the proof constructive, though he took himself to have filled in gaps in the proof. Sieg has suggested that the recent results of the Simpson school of Reverse Mathematics concerning systems conservative over Primitive Recursive Arithmetic should be seen as partial realizations of Kronecker's Programme (rather than Hilbert's).<sup>25</sup> As we shall see, Weyl was happy to take a more full-bloodedly realistic attitude towards arithmetic.

## 2.2 Cantor's theory of the infinite

Dedekind's work on the foundations of the real numbers (and indeed more explicitly his work on the foundations of the natural numbers) had appealed at crucial places

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<sup>23</sup> See Edwards, 'Kronecker's views' for rebuttal of this. <sup>24</sup> Ibid. Material in brackets mine. <sup>25</sup> Sieg, 'Relative consistency', with reference to Simpson, 'Partial realizations'.

to what we now see as set theory. In essence, Dedekind needed to quantify over all infinite sets of a certain sort; but he seems initially not to have recognized any need for caution here.<sup>26</sup> As the set theoretic paradoxes show, however, caution, and an adequate theory of infinite sets, are needed. The theory which came to be accepted as filling the gap was Cantorian set theory, and it is to this that we turn now.

The starting point for Cantor's investigations into the transfinite was a problem from analysis: the representation of analytic functions as Fourier series. (This area of mathematics grew from Fourier's investigations of heat-flow, mentioned above.) Cantor showed that such a representation is unique if a certain convergence condition holds everywhere on the interval; but also if the condition holds everywhere except on a finite set of points. Indeed, Cantor showed that the representation is unique for a function even under the weaker condition that the set  $A$  of points where the convergence condition fails is such that  $A'$  is an isolated set.<sup>27</sup>

Cantor thus began to consider the operation of going from a set of points  $P$  to the set of its limit points, its 'derived set'  $P'$ . This is a monotone operation (if  $P \subseteq Q$ , then  $P' \subseteq Q'$ ), and it can be repeated indefinitely (write  $P^{(0)}, P^{(1)}, \dots, P^{(n+1)}$  for  $P, P', \dots, (P^{(n)})'$ ). Cantor showed that the uniqueness theorem continues to hold if the set of exceptional points  $P$  has  $P^{(n)} = \emptyset$  for some natural number  $n$ ; and he then considered what he (initially) wrote as  $P^{(\infty)}$ , where

$$P^{(\infty)} =_{\text{def}} \bigcap_{n \in \mathbb{N}} P^{(n)},$$

i.e. the set of those points which are in all of  $P, P', P'', \dots$

And of course we can continue applying the operation to obtain  $P^{(\infty+1)}, P^{(\infty+2)}, \dots, P^{(\infty.2)} = \bigcap_{n \in \mathbb{N}} P^{(\infty+n)}, \dots$ . Initially, Cantor described these superscripts as 'symbols of infinity': they were simply a way of indexing infinite iterations of the operation of taking limit points. However, the treatment of infinite point-sets as objects to which operations could be applied, rather than merely as collections, was a significant move.

<sup>26</sup> Dedekind's later response to the paradoxes ('he almost arrived at doubts, whether human thinking is completely rational', Bernstein reported), and his consequent reluctance to republish *Was sind und was sollen die Zahlen?* are discussed in Sieg, 'Relative consistency'. <sup>27</sup> A member  $x$  of a subset  $A$  of the real line is an *isolated point* if there is an open interval  $(a, b)$  which contains  $x$  and no other member of  $A$ . Conversely, a point  $y$  is a *limit point* of  $A$  (whether or not  $y \in A$ ) if every open interval which contains  $y$  also contains some point of  $A$  other than  $y$ . A set which consists entirely of isolated points is *isolated*. If a set  $A$  is isolated, the set of limit points of  $A$ , written  $A'$ , is disjoint from  $A$ , i.e.  $A \cap A' = \emptyset$ .

Cantor realized that what was important for the uniqueness theorem was how ‘concentrated’ the set of exceptional points was, and that the derived set operation — or rather, how far it could to be iterated before the set vanished — gave a measure of this.

For the elaboration of his proof of the uniqueness theorem, Cantor needed to make the real numbers of the interval amenable to mathematics, and he therefore gave a definition of the real numbers (in terms of Cauchy sequences, rather than cuts as Dedekind did). Cantor and Dedekind both recognized the need for an axiom that asserted that to each of their real numbers, there corresponded a point on the line. Kanamori describes this as ‘a sort of Church’s Thesis of adequacy for the new conception of the continuum as a collection of objects.’<sup>28</sup>

The event which marks the beginning of set theory in something like the modern sense is Cantor’s proof of the uncountability of the reals. His first proof, of 1874, was not the famous diagonal proof, which came some years later, but was instead analytic in nature. It is a *reductio* of the hypothesis that there is an enumeration of the unit interval. The idea is to develop in parallel two monotone subsequences from this enumeration, one increasing and one decreasing, and then to show that the limit(s) of the sequences cannot be in the enumeration.

On the other hand, Cantor showed that both the rational numbers, and the algebraic numbers, can be enumerated. Cantor’s proof was therefore a new proof for the existence of transcendental numbers.

It should be noted that both this analytic proof, and Cantor’s later diagonal proof of the uncountability of the reals, are entirely constructive: given any sequence of reals (e.g. an enumeration of the algebraic reals), each proof gives a method for constructing a real which is not in that sequence.

The crucial step, however, was Cantor’s interpretation of the uncountability result: he *assumed* that the real numbers were a definite domain, and so that they formed a set, and then took his proof as showing that it was a set of a larger infinite size than that of the natural numbers. Equinumerosity of sets — the existence of a bijection between them, uniquely pairing off their elements — was taken as the criterion of the sets being of the same size (whether or not the sets were infinite). But it was the understanding of the continuum as a definite domain which really opened the way to set theory, and it was unargued for. Hallett<sup>29</sup> discusses Cantor’s

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<sup>28</sup> Kanamori, ‘Cantor to Cohen’ p. 3.    <sup>29</sup> Hallett, *Cantorian set theory*.

commitment to what motivated the assumption: Cantor believed that any potential infinity is always secondary to a completed (definite) infinite domain. But this is of course no less problematic.

A particular spur to the development of set theory was the continuum problem, which immediately arose from this interpretation of the uncountability proof. If there is more than one 'size' of infinite set ('power', or later 'cardinality', when Cantor came to regard these sizes as a new sort of number), then it is natural to ask how many there are. In particular, are there any cardinalities between that of natural numbers ( $\aleph_0$ , as Cantor called it) and that of the continuum? Cantor thought not: this is (the initial form of) his Continuum Hypothesis. Cantor's attempts to prove CH continued throughout his career. (We return to the open status of CH in Ch. 4.1.3: here we just review — necessarily very briskly — some Cantorian background.)

Cantor's approach to the Continuum Hypothesis involved the consideration of the derived set operation. In order to explore the continuum, the 'infinite symbols' which index the iterations of this operation needed to be defined independently of the point sets, and in a way which would make clear how far such iterations could go, rather than simply being introduced ad hoc. This meant setting up the ordinals as numbers in their own right, and was marked by the notational change from  $\infty$  (suggesting potentiality) to  $\omega$  (the last letter of the Greek alphabet, suggesting a completed infinity).

The ordinals were justified, for Cantor, by their use as the order types of well-ordered sets. (A well-ordered set is a set ordered so that any non-empty subset of it has a least element.) Cantor suggests that the ordinal numbers are formed by abstraction: by taking a well-ordered set, and abstracting away from the particular elements it contains, leaving only the structure of the well-ordering.

Ordinals differ only in length, and are themselves well-ordered by length. As Cantor seems to have realized quite early on, this gives rise to a paradox, now known as the Burali-Forti paradox: the set  $\Omega$  of all ordinals, being well-ordered, must have an ordinal measuring its length; but this ordinal must be greater than any of the ordinals in  $\Omega$ ; which contradicts the initial assumption that  $\Omega$  contains all of the ordinals. Cantor seems not to have been troubled by this: he simply concluded that  $\Omega$ , the collection of all ordinals, is not a set: it is an 'inconsistent multiplicity', part of the 'Absolute' infinite, as opposed to the (mathematically tractable) transfinite.

The connection between ordinals and the concept of 'power' (or cardinal size)

was an important strategy for Cantor in his efforts towards proving the Continuum Hypothesis. Cantor recognized that the ordinals could be classified according to the power of the sets of which they were well-orderings. Indeed, his endorsement of the cardinals as a sort of number came from the idea that they arose from well-ordered sets by a second process of abstraction: the first disregarded the elements, but kept the ordering, to give the ordinal corresponding to the set; and the second disregarded even the ordering, to give a cardinal number that would be shared by any two sets which were equinumerous.

Cantor's classification of the ordinals was into 'number classes'. The first number class comprised the finite ordinals, which he identified with the ordinary natural numbers. The second number class comprised the countable ordinals: those ordinals (such as  $\omega, \omega + 1, \dots, \omega \cdot 2, \dots, \omega^\omega, \dots$ ) which were the order-types of well-orderings of the natural numbers (or any other equinumerous set). But Cantor claimed that this did not exhaust the ordinals: he came to think that the second number class formed a set which was not itself countable.

The argument he gave for this is that no countable sequence  $s$  of countable ordinals can include every countable ordinal. First extract a strictly increasing subsequence  $s'$  from  $s$  (take  $s'_0 = s_0$ , and then set  $s'_{n+1}$  to be  $s_m$ , with  $m$  minimal such that  $s_m > s'_n$ , if one exists). If  $s'$  is finite, then its last element,  $\alpha$ , is the greatest element in  $s$ , and so its successor  $\alpha^+$  is a countable ordinal not in  $s$ . But if not, the supremum of  $s'$  is a countable ordinal not in  $s$ . Like the arguments for the uncountability of the reals, this argument is perfectly constructive; the questionable assumption is simply that the domain of countable ordinals forms a set. Cantor was aware that the domain of all ordinals is 'an inconsistent totality' (as he called it), but he does not seem to have worried whether the domain of countable ordinals was inconsistent, or whether the only ordinals are countable ordinals.

These number classes gave rise to Cantor's alephs as a scale of cardinalities. The first number class is a set consisting of the natural numbers, and has cardinality  $\aleph_0$ . The second number class comprises the countable ordinals, and is of a greater infinite cardinality, which Cantor called  $\aleph_1$ . The third number class comprises those ordinals with cardinality  $\aleph_1$ , and is itself a set of cardinality  $\aleph_2$ ; and so on. The continuum problem then becomes the question of where the cardinality of the continuum fits into this scale. Despite his best efforts, Cantor was unable to prove his Continuum Hypothesis.

## 2.3 After the paradoxes

As we have seen, although Cantor was never very precise about which collections formed sets, he certainly did recognize that not all of them could. Lavine suggests that Cantor's idea of set was fundamentally a combinatorial or essential notion; the guiding intuitive idea being that a set is built up by the successive addition of elements to it.<sup>30</sup>

Frege, in contrast, developed a set theory based on the idea of sets as the extensions of concepts. A concept carves the universe of objects into two: those which fall under the concept and those which lack it. The symmetry between the two cases means that it is natural, on this view of sets, to take the sets to be closed under complementation; whereas this is certainly not the case on a combinatorial view. Frege famously made the assumption that there is a set corresponding to every concept in Axiom V of the logical system of his *Grundgesetze*.

It was Russell's paradox which showed conclusively that the assumption was false: not every concept determines a set. It was in the aftermath of this that modern axiomatic set theory began to be built, primarily by Zermelo, on the basis of Cantor's ideas. The debate around the paradoxes (Russell's, and the others which shortly afterwards became known) prompted a great deal of critical thought on the foundations of set theory, and it was in this context that the notion of impredicativity was first formulated.

### 2.3.1 The Axiom of Choice

If Russell's paradox showed conclusively that not every concept determines a set, Zermelo's Axiom of Choice made more explicit the assumption that there are sets which are not determined by any concept (at least, by any concept finitely built up from some unproblematic base). The axiom appeared first in Zermelo's proof of 1904 of the Well-Ordering Principle, which formalizes Cantor's principle that every set can be 'counted' by (i.e. put into bijective correspondence with) an ordinal.<sup>31</sup>

In 1900, Hilbert had listed various issues concerning the continuum as the first two items in his famous list of open problems in mathematics. One of these was to settle the status of Cantor's Continuum Hypothesis. But in order for the question

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<sup>30</sup> Lavine, *Understanding the Infinite*. <sup>31</sup> The story is told in depth by Moore, *Zermelo's Axiom*; but the brisk account here will pick out some relevant points.

of the Continuum Hypothesis to be meaningful, we need to know first that the continuum — the collection of real numbers — is in fact a set.

Although Cantor had recognized that not every property determined a set — or, as he put it, that not every multiplicity is consistent — Hilbert noted that Cantor had given no criterion for distinguishing consistent from inconsistent multiplicities. Hilbert therefore asked for a proof of the ‘existence of the totality of real numbers or — in the terminology of G. Cantor — the proof of the fact that the system of real numbers is a consistent (complete) set’.<sup>32</sup> Because of his general belief that in mathematics, consistency entails existence, Hilbert suggested that this could be achieved by showing an axiomatic characterization of the reals to be consistent.

Another problem which needed to be dealt with before the Continuum Hypothesis could be tackled is that of showing that the continuum can be well-ordered: if not, then it is not equinumerous with any of the alephs.<sup>33</sup> Hilbert therefore asked for a specific well-ordering of the reals.

In 1904, Julius König delivered an address to the International Congress of Mathematicians containing a proof that the continuum cannot be well-ordered.<sup>34</sup> The proof was quickly shown to be fallacious; although König’s reasoning was valid, it relied on a lemma (due to Bernstein) which did not hold in full generality. But the episode apparently caused a nasty shock to Cantor and his followers, and brought home the need for more secure foundations for Cantorian set theory. It was this which prompted Zermelo to seek out an assurance against any possible repair of König’s proof: Zermelo proved rigorously the principle that any set can be well-ordered.<sup>35</sup>

Cantor had used the Well-Ordering Principle freely, apparently relying on the intuitive argument that any set can be well-ordered simply by an informal recursion on the ordinals, at each successor stage making an arbitrary choice from among the elements not already chosen. (Some form of the Axiom of Replacement is actually needed to ensure that anything for which this process fails to terminate is not a set; though as mentioned above, this view of the boundary between set and ‘inconsistent

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<sup>32</sup> Hilbert, ‘Über den Zahlbegriff’    <sup>33</sup> If there is a bijection between the continuum and some aleph, then that clearly induces a well-ordering on the continuum.    <sup>34</sup> ‘Julius König’ was the name under which the Hungarian mathematician Gyula König published his work in German journals. (König’s Lemma is named after his son, the pioneering graph theorist Dénes König.)    <sup>35</sup> König withdrew his claim in a note published in 1905, but subsequently made another attempt to show that the continuum could not be well-ordered, using an argument based on Richard’s Paradox: see Ch. 3.3.2 below.

totality' was certainly a part of Cantor's thinking.)<sup>36</sup>

Cantor's argument was unfit for the purpose: it is questionable whether an infinite succession of arbitrary choices is even coherent. Zermelo's proof made explicit appeal to the Axiom of Choice, which avoided all objectionable appeal to intuitive psychological ideas of 'choice' or iterating a process through time. The new axiom made a bald assertion of commitment to the existence of a choice set for any disjoint family of non-empty sets: such a choice set would contain one element of each member of the family.

Zermelo's proof was published in 1904,<sup>37</sup> but was very heavily criticized. Zermelo responded four years later<sup>38</sup> with an article which included a revised proof and a reply to some of the criticisms.

The Axiom of Choice is very different from the other axioms, which Zermelo laid out in his second paper of 1908<sup>39</sup>: the axioms of the Empty set, Pair-set, Separation, Union, Powerset, and Infinity all assert the existence of sets by specifying exactly which members they are to have. In contrast, the Axiom of Choice is a bald existential: a choice set could only be 'constructed' by making an infinity of arbitrary choices.<sup>40</sup> As is characteristic of modern set theory, a problematic intuitive idea of construction was replaced by the simple existential assumption that a set with the desired properties exists.

The story of the introduction and reception of the Axiom of Choice is a familiar one. Here, let's just emphasize that the way in which the Axiom of Choice was justified represents a curiously radical anti-foundational move by Zermelo. A very similar move was also made by Russell in his justification of the Multiplicative axiom (the form of Choice used in *Principia*), but especially (because it is essential to the programme of *Principia*) in his justification of the Axiom of Reducibility.<sup>41</sup>

This new step was the adoption and advocacy of a 'regressive' or 'quasi-inductive' methodology for mathematics: axioms were justified on the basis that they had desirable consequences and no known undesirable ones; or that they had been used (though perhaps implicitly) by many mathematicians without leading to contradiction. This move is presumably at least in part a result of the idea that the set-theoretic

<sup>36</sup> See Cantor, 'Letter to Dedekind [1899]', and Zermelo's comments on it (p. 117 of the English translation). <sup>37</sup> Zermelo, 'Beweis (1904)'. <sup>38</sup> Zermelo, 'Neue Beweis (1908)'. <sup>39</sup> Zermelo, 'Untersuchungen'.

<sup>40</sup> Zermelo's first axiom, Extensionality, has a special status in a different way: it is definitional or analytic in character. See Boolos, 'The iterative conception' pp. 27–28 for discussion.

<sup>41</sup> See Ch. 6.2 below for a discussion of the content of Reducibility.

paradoxes had shown intuition to be ‘bankrupt’, and invalidated appeals to obviousness.

As Zermelo wrote,

[in deciding on axioms] there is at this point nothing left for us to do but to proceed [...] starting from set theory as it is historically given, to seek out the principles required for establishing the foundations of this mathematical discipline. In solving this problem we must, on the one hand, restrict these principles sufficiently to exclude all contradictions and, on the other, take them sufficiently wide to retain all that is valuable in this theory.<sup>42</sup>

The regressive method, or rather what it represents, the break with the ultimate reliance on self-evidence, is perhaps the most distinctive part of the modern attitude to mathematics.

### 2.3.2 Poincaré on the paradoxes

Perhaps the most prominent of the critics of the direction mathematics had taken was Poincaré. Poincaré anticipated some of the basic ideas of Weylian predicativism, and indeed is responsible for the concept of predicativity. Unfortunately, his writings are somewhat fragmentary, and his (changing) views are nowhere set out very clearly.<sup>43</sup> Here, a lightning tour will have to suffice.

Poincaré’s views on set theory seem to have developed mainly in the course of a debate with Russell on the paradoxes. Russell’s initial diagnosis of the paradoxes was that they involved ‘self-reproductive’ concepts: any set of items all of which fall under such a concept gives rise to a more extensive collection of such items. With the Burali–Forti paradox, for example, the supremum of any set of ordinals is itself an ordinal not in that set. So the attempt to form a set of *all* such items is doomed to failure.<sup>44</sup> But Russell seems to have been happy to think of some infinite collections as completed or actual, in contrast to Poincaré.

Although Poincaré was not a systematic philosopher, and, as mentioned, his writings on set theory present his views as they developed, certain key ideas remained

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<sup>42</sup> Zermelo, ‘Untersuchungen’ p. 200. <sup>43</sup> A helpful overview of Poincaré’s thought on mathematics is given by Folina, *Poincaré*. <sup>44</sup> Russell, ‘Some difficulties’. Russell’s diagnosis is closely related to Dummett’s notion of indefinite extensibility, to which we will return in Chapter 3.2.2 and in Chapter 5.1.2.

constant, and the most important of these was his rejection of actual infinity. ‘There is no actual infinite; the Cantorians forgot that, and they fell into contradiction.’<sup>45</sup> By ‘Cantorian’, Poincaré seems to have meant the whole tendency of modern set-theoretic mathematics. And while his pronouncement is clearly overstated — there is no obvious link between acceptance of actual infinity and paradox — Poincaré was pointing to what he saw as a deep-rooted malaise within this tendency, of which the paradoxes were a symptom.

It was in the debate of 1905–6 between Russell and Poincaré on the paradoxes that the concept of predicativity first appeared. It was clear that the comprehension principle needed to be restricted to avoid paradox, and various ways of doing this were entertained. Russell described a membership condition as predicative, for a given approach to set theory, if a class corresponded to it. Poincaré argued that the paradoxes arose from ‘vicious circles’, and proposed this as a way to distinguish the ‘predicative’ (admissible) classes, which obeyed it, from the ‘impredicative’ classes which did not. Obeying the Vicious Circle Principle is the sense of the word ‘predicative’ which has stuck.<sup>46</sup>

Poincaré viewed the mathematics of the infinite as being concerned not directly with infinite objects such as sets — he did not believe that there were actual infinities — but with quasi-linguistic descriptions or rules for generating or constructing an unending sequence of objects: for instance, the natural numbers.

Because of this stress on the infinite as being always potential, many of Poincaré’s statements involve talk of collections growing or classifications being disrupted. One difficulty with taking the notion of the potential infinite as primary is knowing what to make of such reference to temporal notions: presumably the idea of sets being constructed in time is metaphorical, but it is unclear how or for what we could cash the metaphor in. The temporal notions could of course be replaced with modal ones, but that is not obviously any better.

What is clear, despite the somewhat vague and metaphorical nature of some of Poincaré’s statements, is his rejection of impredicative quantification. While the temporal metaphor of the continuing formation of infinite domains drives Poincaré’s thought, the distinction between predicative and impredicative classes arises from this:

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<sup>45</sup> Poincaré, ‘Les mathématiques et la logique [1906b]’ §XV. <sup>46</sup> Russell’s ideas will be discussed more fully in chapter 6.1—2 below.

[...] there emerges a distinction between two types of classification applicable to the elements of infinite collections, *predicative* classifications, which cannot be disrupted by the introduction of new elements, and *non-predicative* classifications for which the introduction of new elements requires constant reshaping.<sup>47</sup>

For example, if we imagine the natural numbers being generated one by one, each can be classified as odd or even, and obviously the classification of a number as odd will not be disturbed by the numbers which are generated later. On the other hand, a classification of natural numbers according to whether or not they belong to all of an indefinitely extensible class of sets of naturals — that is, the formation of the intersection of that class — is liable to be disrupted by the formation of new members of that class. (An example of this, to which Poincaré often turned, is Richard's paradox, which is discussed in Ch. 3.3.2 below.)

Poincaré linked belief in the actual infinite with the acceptance of impredicative definitions:

It is the belief in the existence of the actual infinite which has given birth to those non-predicative definitions. Let me explain. [...] In these definitions the word 'all' figures [...]. The word 'all' has a very precise meaning when it is a question of a finite number of objects; to have another one, when the objects are infinite in number, would require there being an actual (given complete) infinity. Otherwise *all* these objects could not be conceived as postulated anteriorly to their definition, and then if the definition of a notion *N* depends upon *all* the objects *A*, it may be infected with a vicious circle, if among the objects *A* are some indefinable without the intervention of the notion *N* itself.<sup>48</sup>

This focus on the problematic meaning of quantification over certain infinite domains is something to which we will frequently return.

The rejection of the actual infinite means that infinite collections can only be specified by giving rules for indefinitely continuing some construction. For Poincaré, the prohibition on impredicative definitions comes as a consequence of what constructions are possible. It is impossible to *construct* an object according to an

<sup>47</sup> Poincaré, 'La logique de l'infini [1909]' p. 463 = *Dernières pensées*, p. 10 = *Mathematics and Science*, p. 47. <sup>48</sup> Poincaré, 'Les mathématiques et la logique [1906b]' §XV.

impredicative specification, because such a specification refers to the object itself, or to sets or domains of quantification to which the object belongs. For the specification to have the intended meaning, therefore, the object must already exist. (This view of the predicativity constraint is to be distinguished from Russell's. Russell thought of VCP as a logical principle.)<sup>49</sup>

Such construction rules are essentially linguistic, and Poincaré insisted that the only mathematical objects which should be admitted are those which are definable in a finite number of words. This 'definitionism' (or 'pragmatism', as he termed it) will be discussed in Ch. 3.3.1 below.

It might be expected from all this that Poincaré would be systematically predicativist. But in fact, Poincaré seems to have been happy to accept classical analysis, as based on the impredicative least upper bound principle: because the (geometrical) continuum is antecedently existent, we can pick out elements however we like. (See Ch. 4.4 for more discussion of this position, which takes the continuum as given to us in intuition.)

Overall, Poincaré did not provide a clear anti-classical programme for mathematics, or a well-developed philosophy which might underpin such a programme, so much as a collection of ideas and suggestions. Drawing out those ideas would be a very worthwhile task, though not an easy one. It is, however, not a task which can be attempted here.

### 2.3.3 *Das Kontinuum* and Brouwer's 'revolution'

The predicativist programme proper was begun by Hermann Weyl's monograph of 1918, *Das Kontinuum*. The exploration and development of the ideas it presented will be my main concern in the rest of this thesis; here, I will restrict myself to a few remarks to place the book in its historical context, by way of introduction.

To state the obvious, that context is the explosion of work on the foundations of mathematics which began with Frege and Cantor and lasted until the 1930s. And one part of that, as we have just seen, is the radical shift in the understanding of the nature of mathematics which is entailed by the conscious adoption of the regressive method. The fork in the path which Kronecker had first noticed was now clearly visible. Weyl's predicativist programme needs to be understood as being an attempt to vindicate and return to the traditional conception of mathematics as based on

<sup>49</sup> Steiner, 'Review of Folina (1992)' p. 254.

rationally or intuitively compelling first principles.<sup>50</sup>

The attempt is a rejection of the counsel of despair that motivated the regressive method. *Das Kontinuum* begins with a general philosophical discussion on the nature of judgement, and the enterprise is deeply foundational. One example of this is Weyl's endorsement of the traditional characterization of the axioms as self-evident truths:

Mathematics concerns itself with *pertinent, general, true* judgments. Among these are a few which are immediately recognized as true, the *axioms*, say  $U_1, U_2, U_3, U_4$ , which are such that all other pertinent, general true judgments are logical consequences of these few [...]<sup>51</sup>

(Weyl was of course writing well before the Incompleteness theorems.)

Weyl described how he saw his foundational and revisionary programme in the preface to *Das Kontinuum*:

It is not the purpose of this work to cover the 'firm rock' on which the house of analysis is founded with a fake wooden structure of formalism [...]. Rather, I shall show that this house is to a large degree built on sand. I believe that I can replace this shifting foundation with pillars of enduring strength. They will not, however, support everything which today is generally considered to be securely grounded. I give up the rest, since I see no other possibility.<sup>52</sup>

The 'sand' on which classical analysis was built was the idea of arbitrary infinite sets and functions:

The notion that an infinite set is a 'gathering' brought together by infinitely many individual arbitrary acts of selection, assembled and then surveyed as a whole by consciousness, is nonsensical; 'inexhaustibility' is essential to the infinite.<sup>53</sup>

However, as Weyl later pointed, a key assumption of *Das Kontinuum* was that 'one may safely treat the sequence of natural numbers as a closed sequence of objects.'<sup>54</sup>

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<sup>50</sup> Weyl does not himself use the vocabulary of 'predicative'; though in the section of *Das Kontinuum* where he acknowledges the similar work of Russell and the Vicious Circle Principle, he adds: 'Of course, Poincaré's very uncertain remarks about impredicative definitions should also be noted here.' Weyl, *The Continuum* p. 47. <sup>51</sup> Ibid. p. 17. <sup>52</sup> Ibid. p. 1. <sup>53</sup> Ibid. p. 23. <sup>54</sup> Weyl, *Philosophy of mathematics* p. 60.

From this arithmetical foundation, Weyl built up a family of explicitly definable properties, sets and functions, which he contrasts with the

*completely vague* concept of function which has become canonical in analysis since Dirichlet and, together with it, the prevailing concept of set.<sup>55</sup>

It might be helpful at the outset to position the predicativist programme in relation to the two other revisionary programmes which arose out of the foundational crisis: Hilbertian finitism, and Brouwerian intuitionism.

Weyl's aim was to found mathematics predicatively, while Hilbert's aim, at least on a certain reading, was to found mathematics finitistically.<sup>56</sup> So Weyl's programme was more modest than Hilbert's in that it took more for granted, as a legitimate part of the ground — Weyl took all of classical arithmetic as unproblematic, while Hilbert restricted himself to finitary arithmetic. (With the benefit of philosophical hindsight and a great deal more mathematical logic than was available to Weyl and Hilbert, we may identify Weyl's base theory as first-order Peano Arithmetic, and Hilbert's as Primitive Recursive Arithmetic.) Weyl's programme was also less ambitious than Hilbert's in that Hilbert was determined to justify classical mathematics in its entirety (including Cantor's 'paradise'), whereas Weyl was happy to jettison those parts of mathematics which could not be justified predicatively. Perhaps the most fundamental difference, though, is that Weyl did not envisage any sort of proof-theoretic reduction: this was Hilbert's great innovation. The justification of higher mathematics that Hilbert sought did not involve attributing any meaning to its statements, but was purely formal: the justification for a calculus of higher mathematics was to be that it might lead to quicker proofs of contentful results, and could not lead the mathematician into a (contentful) falsehood. For Weyl, in contrast, all of mathematics was to be contentful.

Another comparison is with Brouwerian intuitionism. It is particularly important to get straight on the relationship between Weylian predicativism and intuitionism because Weyl himself came, just a few years after *Das Kontinuum*, to reject his earlier views in favour of intuitionism. (Predicativism has ever since been tainted with guilt by association.) As Weyl bombastically wrote in 1921, in the paper credited with starting serious debate over intuitionism:

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<sup>55</sup> Weyl, *The Continuum* p. 23. <sup>56</sup> It is in fact arguable that Hilbert was rather less of a 'foundationalist' than he is often taken to have been.

So I now abandon my own attempt and join Brouwer. In the threatened dissolution of the state of analysis, which is beginning, even though as yet few recognize it, I tried to find solid ground without leaving the order upon which it rests, by carrying out its fundamental principle purely and honestly. And I believe this succeeded — as far as it could succeed. For *this order is itself untenable*, as I have now convinced myself, and Brouwer — that is the revolution!<sup>57</sup>

(In fact, Weyl himself admitted to not fully understanding certain parts of Brouwer's thought, and Majer has suggested that Weyl's 'intuitionism' is better understood as a form of finitism.)<sup>58</sup>

The fundamental difference between intuitionism and Weyl's predicativism lies in their divergent attitudes to the Law of the Excluded Middle. Both schools of thought agree that LEM is not generally valid; but while intuitionism develops mathematics without LEM, predicativism can be seen as the restriction of mathematics to areas for which (the predicativist believes) LEM can be justified. Crucially, first-order arithmetic (with classical quantification over the natural numbers) is one such area. (See below (Ch. 5.2.3; Ch. 6.5), where I argue that Gödel's negative translation shows that LEM for arithmetic can be justified.) The predicativist approach to the continuum, then, is to avoid the real indefinitely extensible continuum as much as possible, and instead to work with a surrogate: the arithmetically definable real numbers.

## 2.4 Conclusions

The story briskly presented above has covered the development of modern mathematics, together with some of the dissenting voices who expressed misgivings about that development and began to suggest other ways of proceeding.

In particular, the focus has been on the infinite. The infinity of the natural numbers has presented no serious *conceptual* problems to mathematics (though there are of course very fundamental philosophical questions around arithmetical truth, and very deep open questions in number theory). In contrast, the continuous infinite — the real line — has been highly problematic.

<sup>57</sup> Weyl, 'Grundlagenkrise'. (Mancosu's translation, modified.) For the history of the foundational crisis around intuitionism, see Hesselning, *Gnomes in the Fog*. <sup>58</sup> Majer, 'Differenz zwischen Brouwer und Weyl'.

The modern conception of the continuum developed hand-in-hand with the set theory which underlies it. The theory was strikingly ad hoc; this was only brought out clearly by the explicit adoption of the regressive method, but was present before. Particularly striking is the anomalous position of the continuum in Cantor's set theory.

The suggestions of Kronecker and Poincaré led up to Weyl's eventual proposal of a coherent predicativist alternative to the 'classical' story. It is to the philosophical motivations behind Weylian predicativism that we will now turn.



## Chapter 3

# Predicativism

The purpose of the current chapter is to set out the stall for predicativism. I clarify what I mean by predicativism, and develop some of the motivation behind the position. However, those motivations will be further developed later on in the thesis. In the next two chapters, which discuss in greater depth the classicist position (Chapter 4) and the intuitionist and finitist positions (Chapter 5), the predicativist alternative is the foil. In Chapter 6, the basics of the predicativist view — the predicativist concepts of property and set — are established. The technical implementation of predicativism within formal systems for mathematics is only possible after this work, in Chapter 7. What follows in this chapter is therefore somewhat programmatic.

The fundamental point which emerges is that the predicativist's rejection of classical set-existence principles stems from, and is justified by, a conception of certain mathematical domains as open-ended (or indefinitely extensible, as we will say). Most importantly, these include the domain of sets of natural numbers, and the domain of real numbers. (These two are in fact equivalent, given a certain amount of coding, and I shall often talk about them interchangeably as 'the continuum'.)<sup>1</sup> The domain of natural numbers, however, is definite, that is, not open-ended.

Quantification over open-ended (i.e. indefinitely extensible) domains raises serious issues. The predicativist programme is to avoid such quantification, at least

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<sup>1</sup> The domain of characteristic functions of sets of natural numbers,  $2^{\mathbb{N}}$ , is clearly also the domain of all binary fractions, and so all real numbers, in the interval  $[0, 1]$ . This closed interval is cardinally equivalent to the real line, and if the end-points are removed, the resulting open interval is homeomorphic (i.e. topologically equivalent) to the real line. In Simpson, *Subsystems of Second Order Arithmetic* pp. 9–12, real numbers are represented as certain sets of naturals via several layers of coding.

in the crucial context of set-definitions, and to see how much mathematics can be developed without it.

In §3.1, I discuss the meaning of predicativity and the basic worry behind problematically impredicative definitions: those definitions that involve quantification over open-ended domains. §3.2 presents the argument that quantification over open-ended domains is insufficiently determinate in its meaning to serve in definitions. In §3.3, I argue that certain domains, including that of the sets of natural numbers, are open-ended in the relevant sense. I explain how this thought can be motivated by ‘definitionism’, and how this leads the predicativist to reject uncountable totalities (more precisely: to view uncountable domains as indefinitely extensible). Finally, §3.4 clarifies the form of predicativism I am presenting, and positions it in relation to other views which have been given similar labels.

Before we launch in, a note on terminology is in order. First, the word ‘domain’. As I will use it, it is no more ontologically committing than the use of the plural idiom. ‘The domain of natural numbers’ is just a way of referring to all of the natural numbers at once. (The words ‘collection’ and ‘totality’ are used similarly by others.)

It is important to stress that there is no assumption that such a ‘domain’ must be some single, set-like object: I do not assume the ‘all-in-one principle’.<sup>2</sup>

Second, the phrase ‘indefinitely extensible’. This is a Dummettian term of art which I have borrowed, and we will be particularly concerned to pin the notion down in Ch. 5. For now, I will also use the more impressionistic term ‘open-ended’. The primary bearers of indefinite extensibility are concepts; but the ‘indefiniteness’ that is at issue concerns the *extensions* of those concepts, and I will generally speak of the domains themselves as indefinitely extensible. Roughly, an indefinitely extensible concept is one such that if we have a clear conception or characterization of a domain of items all of which fall under that concept, then we can, by making reference to that domain, give a characterization of a more extensive domain of items all of which fall under the original concept.<sup>3</sup> The process by which we move from one characterization to another is called the ‘principle of extension’. The characterization is rough in that it is left vague just what ‘a clear conception or characterization of a domain’ is. I do not have a fully general account of this to offer; but it will become clear in the course of the thesis (and especially of Ch. 5) why I think that we can credit ourselves with a clear conception of some domains and not of others. (Note, though,

<sup>2</sup> The term derives from Cartwright, ‘Speaking of Everything’. <sup>3</sup> Cf. Dummett, ‘What?’ p. 441, quoted on p. 115 below.

that ‘concept’, as used here and throughout this thesis, is the (approximate) equivalent of Frege’s *Begriff* or Russell’s ‘propositional function’, whereas a ‘conception’ is a more general view one might have about how things are in some respects, in some region of reality.) I use ‘definite’ as an antonym to ‘indefinitely extensible’.

Note that ‘indefinitely extensible’ is primarily concerned with what we can and cannot do or conceive of, rather than with how things are independently of us. However, I will argue in Ch. 5.3.1 that in mathematics, what there is cannot be wholly independent of what we can conceive of.

It is often thought that there is a set corresponding to each definite domain: the set which has as members just those things. I have no quarrel with this, but it is not important for my purposes.

### 3.1 Motivations

At its narrowest, predicativism is the doctrine that mathematical definitions must be predicative, and that mathematics can or should concern itself only with objects which are so definable. Impredicative definitions are those which fall foul of the Vicious Circle Principle: that is, they quantify over domains which include (or depend upon) the item being defined. I am here concerned only with what has been called ‘predicativism given the natural numbers’; worries about the impredicativity of the natural numbers themselves<sup>4</sup> will be addressed only very briefly, in §3.4.1 below.

The predicativist programme, as initiated by Weyl in *Das Kontinuum* and developed more recently by Feferman, among others,<sup>5</sup> is to develop as much of classical mathematics as is possible in a predicatively acceptable manner. The programme has an epistemological pay-off, in that predicative methods are more constructive than others; and it has an ontological pay-off, in that it dispenses with the extravagant ontology of (standard, impredicative) set theory.

One motivation for predicativism was well expressed by Quine (quoted in §1.1 above): ‘it realizes a constructional metaphor’. The value of this is not so much that it makes the consistency of predicative mathematics obvious; it is that it ensures that the content of the claims of predicative mathematics is clear, because the objects

<sup>4</sup> Such as those which have been expressed e.g. in Parsons, ‘The impredicativity of induction [1983]’.

<sup>5</sup> See in particular Feferman, ‘Systems of predicative analysis [I]’ and the papers collected in Feferman, *In the Light*.

dealt with are all capable of explicit definition. The importance of this definability of all mathematical objects, or ‘definitionism’, is discussed in §3.3.1 below.

A standard modern account of impredicativity is the following:

a definition ... is impredicative if it defines an object which is one of the values of a bound variable occurring in the defining expression [...].<sup>6</sup>

This makes clear the crucial role of quantification. As already mentioned, we are helping ourselves to the natural numbers, and so need not worry about their definition; so the first problematic case, on which we will mainly focus, is the definition of sets of natural numbers. Such definitions are impredicative if they contain quantifiers ranging over all of the sets of natural numbers (or of course if they contain quantifiers ranging over even ‘worse’ domains, such as the sets of the sets of the natural numbers); and conversely, a definition of a set of naturals is predicative if it contains no quantification at all, or only quantification over the natural numbers themselves.

As Poincaré, Ramsey, and Quine have all stressed,<sup>7</sup> the issue is not the legitimacy of impredicative specifications, but rather the legitimacy of the assumption that there exist objects which satisfy those specifications. If we have already been given an adequate specification of some objects, then an impredicative re-specification of one of those objects by means of a quantification over that domain is entirely unobjectionable, even though the domain of quantification includes the very object being specified. Ramsey’s example of such a case of harmless impredicativity is ‘the tallest man in the room’. A common mathematical example is a specification of a natural number as the smallest satisfying a certain condition. If we say that  $a$  is the least  $F$ ,  $a = \mu x Fx$ , then the definition is  $\forall x(x = a \leftrightarrow (Fx \& \forall y(Fy \rightarrow x \leq y)))$ , and the bound variable  $y$  in the definition ranges over the natural numbers, of which one is  $a$ , the definiendum. But this is unproblematic, regardless of whether or not we know which number  $a$  is, because the quantification is over the natural numbers, and each of the naturals has a predicative canonical specification in terms of zero and the successor function.

So when is an impredicative specification problematic? Well, suppose that we are trying to specify a set which consists of objects of some previously recognized sort; for example, natural numbers. Our set will contain those natural numbers which

<sup>6</sup> Fraenkel, Bar-Hillel and Lévy, *Foundations of set theory* p. 38.    <sup>7</sup> See especially Quine, *Set Theory and its Logic* p. 242.

satisfy some property. For our specification to be legitimate, the property needs to be well-defined and unambiguous. We will demand that it is given by a formula  $\phi(x)$  in some mathematical formal language, which contains one free variable. The set  $A$  can be written as  $\{x \in \mathbf{N} \mid \phi(x)\}$ . The simplest case is where  $\phi$  is a quantifier-free formula of arithmetic. Less trivial is the case where  $\phi$  contains quantifiers which range over the natural numbers.

We might imagine forming  $A$  by going through the natural numbers one by one, putting in 0 just in case  $\phi(0)$ , 1 just in case  $\phi(1)$ , and so on. But when  $\phi(x)$  is complex, there is no guarantee that we would be able to do this, even in principle: it may require the solution of as-yet unsettled problems in number theory. If we assume, though, that there is a fact of the matter, independent of our knowledge, as to whether or not  $\phi$  holds of any particular natural number, then the specification makes sense.

Now consider specifications which feature quantification over the sets of natural numbers. For example, consider the impredicative specification:  $A$  is the intersection of all those sets of naturals which satisfy condition  $B$ , i.e.  $A = \{x \in \mathbf{N} \mid \forall X(B(X) \rightarrow x \in X)\}$ . To determine whether  $0 \in A$ , we need to know whether 0 is in every  $B$ -set. But if  $A$  is itself a  $B$ -set, then we are led in a circle.<sup>8</sup>

But is this just an epistemological problem? Is the problem just a problem for us? We cannot use the obvious route to determine which numbers the set contains; but that in itself does not mean that the set is ill-defined.

If we suppose that the sets of natural numbers are just as determinate as the naturals themselves, then there is indeed no reason to be worried. On this supposition,  $\forall X(B(X) \rightarrow x \in X)$  is a determinate property of natural numbers, and so determines the set  $A$  of those numbers which satisfy it.

The predicativist, however, is not willing to make the supposition of determinacy for the sets of natural numbers: instead she sees them as open-ended, as will be explained below. It is this open-endedness that means that quantification over the sets does not always have a determinate meaning, and that therefore makes the circularity of an impredicative set-specification into something viciously circular. The simplest way to respect this scruple is to require that the specification of a set of natural numbers may not contain quantification over the sets of natural numbers.

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<sup>8</sup> A real example of just this sort is Kleene's set  $O$  of notations for recursive ordinals, which is defined by a  $\Pi_1^1$  formula. See Feferman, 'Systems of predicative analysis [I]' for discussion.

(This is the simplest, but not the only way for the predicativist to go; see §3.3.4 below on ramification.)

## 3.2 Open-endedness and quantification

Why does the open-endedness of a domain raise problems for the meaningfulness of classical quantification over that domain? This is an issue which will be dealt with in more detail Chapters 5 and 6; but the main arguments are presented here by way of introduction.

### 3.2.1 ‘Any’ and ‘all’

One expression of the open-endedness thought is the distinction between generalizations made with ‘any’ and those made with ‘all’. This distinction was first explicitly drawn by Russell. (Russell in fact attributes the idea to Frege, but this attribution is dubious given Frege’s absolutism about the domain of quantification.)

Intuitively, ‘any’ expresses schematic generality, whereas ‘all’ expresses the logical product of its instances. The meaning of ‘all’ is therefore dependent on a determinate range of instances. The use to which Russell put the distinction was to generalize over domains which are not determinate: indefinitely extensible domains, such as the domains of propositions and properties.

In the case of such variables as propositions or properties, ‘any value’ is legitimate, though ‘all values’ is not. Thus we may say: ‘ $p$  is true or false, where  $p$  is any proposition’, though we cannot say ‘all propositions are true or false’. The reason is that, in the former, we merely affirm an undetermined one of the propositions of the form ‘ $p$  is true or false’, whereas in the latter we affirm (if anything) a new proposition, different from all the propositions of the form ‘ $p$  is true or false’. Thus we may admit ‘any value’ of a variable in cases where ‘all values’ would lead to reflexive fallacies; for the admission of ‘any value’ does not in the same way create new values.<sup>9</sup>

Russell refers to the sort of variable which features in an ‘any’ generalization — what are now called *seen as either free or schematic variables* — as a ‘real variable’; and to

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<sup>9</sup> Russell, ‘Theory of types’ p. 158.

the variable in an ‘all’ generalization — a bound variable — as an ‘apparent variable’.

From the standpoint of modern formal logic, free first-order variables are a minor technicality. Some systems of first-order logic permit them, but some do not, and without any loss of expressive power. If we do allow formulae with free variables, then the usual semantic treatment for them is straightforward: a formula featuring a free variable,  $\phi(x)$ , comes out as true on all interpretations just in case  $\forall x\phi(x)$  comes out true.

However, schematic letters which appear in predicate position are more interesting. In the standard presentation of first-order Peano Arithmetic, the induction scheme is an example of such schematic generality. In the formal context, it is of course laid down what the legitimate substitution instances are; but the informal idea behind it is an open-ended one: we endorse the induction scheme because we are happy to commit ourselves to accepting induction not only for all of the predicates in our current language, but also for any predicate we may in the future come to consider meaningful.

(Why should we be happy to commit ourselves to this? Why is induction so compelling? Just because the antecedent of an instance of induction,  $\phi(0) \& \forall x(\phi(x) \rightarrow \phi(sx))$ , gives us, by repeated modus ponens, that  $\phi(x)$  holds of 0, of  $s0$ , of  $ss0$ , . . . — and these are all of the natural numbers.)

This is more or less the opposite of Kreisel’s view of axiom schemes: Kreisel claims that the only possible reason for asserting a scheme is a commitment to the corresponding second-order axiom.<sup>10</sup> On the second-order view, there is some fixed and all-encompassing domain of properties (or sets) of natural numbers, and what we really want to do is to say that induction holds for all of the members of that domain.<sup>11</sup> But Kreisel’s second-order view demands more of the asserter than a genuinely schematic understanding does: namely, an understanding of that domain.

The two sorts of generalization — schematic and logical product — behave differently. Most notably, ‘any’ generalizations cannot be embedded in the scope of other logical operators: as Russell expressed it, ‘The scope of a real variable can never be less than the whole propositional function in the assertion of which the said variable occurs.’<sup>12</sup> A striking consequence of this is that, as negation must have

<sup>10</sup> For more discussion of this point, see e.g. Parsons, *Mathematical Thought and its Objects* p. 270.

<sup>11</sup> Feferman, ‘Reflecting on incompleteness’ gives a formal analysis of the power of schemata intended to apply to expansions of the language. <sup>12</sup> Russell, ‘Theory of types’ p.159.

a narrower scope than the generalization, there are ‘any’ generalizations which can neither be endorsed nor rejected. As Russell put it:

When we assert *any* value of a propositional function, we shall say simply that we assert the *propositional function*. .... Similarly we may be said to deny a propositional function when we deny any instance of it. We can only truly assert a propositional function if, whatever value we choose, that value is true; similarly we can only deny it if, whatever value we choose, that value is false. Hence in the general case, in which some values are true and some false, we can neither assert nor deny a propositional function.<sup>13</sup>

The unembeddability of schematic generality means that quantification which is so understood cannot appear in definitions.

### 3.2.2 Indefinite extensibility

A different expression of the same basic thought, that quantification must be understood differently when its domain is open-ended, is to be found in Dummett’s argument that quantification over indefinitely extensible domains must be intuitionistic rather than classical.<sup>14</sup> Of course, intuitionistic quantification is considerably more flexible than schematic quantification: it can be meaningfully negated and embedded.

Sullivan uses the striking analogy of a checkerboard to make vivid just what it is about some domains — such as the domain of objects in physical space — that makes them definite rather than indefinitely extensible. The idea is that each square of the checkerboard — each position in space — may or may not contain a star, and that the squares are the only positions where a star may be. If we are interested in the question of whether any of the stars has a certain property  $\phi$ , then we know in advance that either there exists a particular square which contains a star which is  $\phi$ ; or on the other hand if there is no such square, then we know that every star is  $\sim \phi$ .

What the checkerboard makes vivid is the idea of reality settling the matter when we put to it any well-defined predicate. There is, so to say, a canonical presentation of each star as the star occupying such-and-such a square of the checkerboard. This

<sup>13</sup> Russell, ‘Theory of types’ p.157.    <sup>14</sup> This argument is the ‘new argument’ for intuitionism, in Dummett, *Frege: Philosophy of Mathematics*. Sullivan, ‘Dummett’s case’ gives an excellent exposition.

canonical presentation means that we know in advance what it would be to have a witness for an existential claim; and, conversely, we know what it would be for there to be no such witness.

The contrast is with cases when reality is not determinate in this way, when the domain of quantification is in some sense open-ended, and there is no uniform way of defining all of the items in that domain. What is characteristic of such cases is that there is no scheme of canonical presentation of all of the potential witnesses. As Dummett writes,

If we choose to explain the concept real number in a Dedekindian manner ... by saying that a real number is required to have determinate relations of magnitude to rationals, we say nothing about the manner in which an object having such relations is to be specified, but simply leave any purported specification to be judged on its merits when it is offered.<sup>15</sup>

The thought is that with an existential quantification over such a domain, although it is quite possible to have a good witness for an existential claim, and quite possible that we could find a general proof of the converse universal, in the absence of either, it is unclear just what the quantification means.

As is well-known, the conclusion drawn by intuitionists is that it is unjustified to claim that the quantified sentence must be either true or false; they therefore propose a whole-scale revision of logic which avoids commitment to bivalence. But a more immediate conclusion is that, as quantification over open domains does not, in general, have a determinate meaning, such quantification cannot appear in legitimate definitions.

The predicativist proposal is twofold. First, that the natural numbers are a definite domain, with a canonical notation in terms of zero and the successor operation serving as a checkerboard. Second, that the best response to the open-endedness of certain domains (such as the domain of the sets of natural numbers) is not to try to find weakened logical principles which can justifiably be applied to indefinitely extensible domains, but rather to require quantification in definitions to be over definite domains. These claims are explored and defended in Chapter 5 below. What we turn to first, though, is why we should think that the domain of sets of natural numbers is open-ended.

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<sup>15</sup> Dummett, *Frege: Philosophy of Mathematics* p. 319.

### 3.3 The indefinite extensibility of $\mathcal{P}(\mathbb{N})$

There are two sorts of reason for thinking that the continuum — i.e. the sets of natural numbers — is indefinitely extensible. The negative sort of reason is that the alternative is so problematic. The problems that are faced by that alternative — the classical view of the continuum as a definite domain — are the subject of Chapter 4. But there is a positive reason as well: definitionism.

#### 3.3.1 Definitionism

A strand of thought which runs through Poincaré, the French analysts (notably Borel, Baire, and Lebesgue), and Weyl is what might be called definitionism: the doctrine that mathematics should concern itself only with objects which can be defined.<sup>16</sup>

Poincaré's definitionism is expressed in his comment on the set-theoretic paradoxes:

I think for my part, and I am not the only one, that the important point is never to introduce objects that one cannot define completely in a finite number of words.<sup>17</sup>

In *Das Kontinuum*, Weyl's commitment to definitionism is shown by his insistence that infinite sets must be given by rules, and the detailed way in which such rules are built up from basic arithmetical properties:

I contrast the concept of set and function formulated here in an exact way with the *completely vague* concept of function which has become canonical in analysis since Dirichlet and, together with it, the prevailing concept of set.<sup>18</sup>

By 'the completely vague concept of function', Weyl means allowing arbitrary and unspecifiable correlations of values with arguments to count as legitimate functions.

It is definitionism which is the basic motivation for Poincaré's and Weyl's rejection of impredicative specifications; however, definitionism is plainly in need of elaboration, raising as it does the questions of what a legitimate definition is, and what it means for mathematics to 'concern itself' with certain objects. Predicativism is one way of fleshing out the definitionist thought into a philosophy of mathematics.

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<sup>16</sup> Maddy uses the word 'definabilism' for the position. <sup>17</sup> Quoted in Kline, *Mathematical Thought* p. 1003. <sup>18</sup> Weyl, *The Continuum* p. 23.

Definitionism can be motivated by a whole range of considerations. For Poincaré, the doctrine seems to have been motivated by a rejection of the idea of the completed infinite, and by his conviction that the only things which can be the subject of (contentful) mathematics are finite objects such as definitions. However, we need not go as far as atheism about the completed infinite: we could just content ourselves with agnosticism. The definable sets are manageable objects to reason with, and (as we shall see in Chapter 7) such reasoning is surprisingly powerful. Why, then, should we want to worry ourselves with the mathematics of undefinable sets, which raises such thorny epistemological problems?

### 3.3.2 Richard's Paradox

Richard's paradox of definability, published in a one-page paper of 1905,<sup>19</sup> shows that there is an essential open-endedness to the notion of legitimate definition. When combined with definitionism, this vindicates the predicativist view that certain domains — notably the real numbers, and the sets of natural numbers — are themselves open-ended.

Richard points out that the set  $E$  of real numbers which can be defined using finitely many words (from a fixed vocabulary) is clearly countable, and that we can therefore diagonalize to define, again using only finitely many words (again from that vocabulary), but this time making reference to  $E$ , a real number which is not in  $E$ :

Let  $p$  be the  $n$ th decimal of the  $n$ th number of the set  $E$ ; we form a number  $N$  having zero for the integral part and  $p + 1$  for the  $n$ th decimal, if  $p$  is not equal either to 8 or 9, and unity in the contrary case. This number  $N$  does not belong to the set  $E$  because it differs from any number of this set, namely from the  $n$ th number by the  $n$ th digit. But  $N$  has been defined by a finite number of words. It should therefore belong to the set  $E$ . That is a contradiction.<sup>20</sup>

Richard's paradox is standardly seen as being a genuine paradox, on a par with the Liar, for natural languages, which allow semantic reflection. 'Definable' is as problematic as 'true', and for similar reasons: both are standardly classed as semantic,

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<sup>19</sup> Richard, 'Principes des mathématiques'. <sup>20</sup> Ibid.

rather than set-theoretic paradoxes. As such, the standard resolution is to discern in the reasoning an equivocation on the concept *definable in a finite number of words*.

A properly formalized version of the argument might consider a language  $L_0$  with no means of semantic reflection; in this case, the reasoning about what is ‘definable in  $L_0$ ’ would need to take place in a metalanguage,  $L_1$ , and the paradox is of course avoided, because the diagonal number  $p$  is definable in  $L_1$ , but not in  $L_0$ .

Or we might instead have a series of partial definability predicates  $D_0, D_1, \dots$  (analogous to partial truth predicates) in a single fixed language  $L'$ , in which  $D_0$  would intuitively mean ‘definable by an open sentence of  $L'$  not containing any  $D_i$ ’,  $D_1$  would mean ‘definable by an open sentence of  $L'$  not containing any  $D_i$  for  $i > 0$ ’, and so on.

Either way, the argumentation, when repeated, generates a hierarchy, either of metalanguages  $L_1, L_2, \dots$ , or of partial definability predicates  $D_0, D_1, \dots$ ; and in both cases, of nested sets of ‘definable reals’  $E_0 \subsetneq E_1 \subsetneq \dots$ . This is what is meant by saying that the definable reals (or, of course, the definable sets of naturals) are open-ended.

### 3.3.3 König and Cantor on Richard’s paradox

A telling side-line to the story is the use König made of Richard’s reasoning in a new argument he gave against the possibility of well-ordering the continuum.<sup>21</sup> The finitely definable real numbers are countable, and so if the continuum were well-ordered, there would be a first real in the ordering which is not finitely definable; but that is a definition of it. Cantor’s response to König is instructive here. Cantor seems to have accepted the definitionist demand that every mathematical object must be finitely definable; but as the continuum is uncountable, there must be an uncountably infinite number of basic (undefined) concepts which can feature in such definitions. In a letter to Hilbert, he wrote:

Every definition is essentially *finite*, that is, it explains the concept to be determined through a *finite* number of *already understood* concepts  $B_1, B_2, B_3, \dots B_n$ .

‘Infinite definitions’ (which are not possible in finite time) are absurdities.

<sup>21</sup> König, ‘Mengenlehre und das Kontinuumproblem’. König’s earlier argument was mentioned in Ch. 2.3.1 above.

If König's claim that the 'finitely definable' real numbers formed a totality with cardinality  $\aleph_0$  was correct, it would imply that the whole continuum is countable, which is certainly wrong.

The question then is what error lies behind the *apparent proof* of this false statement?

The error (which also occurs with emphasis in Poincaré's note in the last issue of the *Revue de Métaphysique et de Morale*), seems to me to be this:

It is assumed that the system  $\{B\}$  of concepts  $B$ , which must be used in the definition of individual real numbers, is finite, or at most countably infinite.

— This assumption *must be an error*, as otherwise we would have the *false theorem* that the continuum is of power  $\aleph_0$ .

Am I mistaken, or am I right?<sup>22</sup>

The modern consensus is that Cantor was indeed mistaken. We surely cannot claim to 'already understand' uncountably many primitive concepts. A language which contained an uncountable number of primitive predicates would clearly be unlearnable and unusable — at least by finite creatures such as us. The fact that Cantor was forced to accept such 'definitions' shows the tension in his idea of a universe of sets that is uncountable but not arbitrary.

### 3.3.4 Ramification

The hierarchy of definable sets (or reals) that Richard's paradox gives rise to leads to a choice for predicativist mathematics: either to consider only those sets definable in a fixed initial language, or instead to ramify. To ramify is to consider, as well as these ('level 0') sets, further levels of definable sets, each of which can include in their definitions quantification over the sets of earlier levels.

Consider again the case of specifying a set of natural numbers. The predicativist's worry is that the domain of all of the sets of natural numbers is not definite, and that quantification over that domain is not legitimate, at least in set specifications. Suppose instead that we settle on some restricted means of formal set specification,

<sup>22</sup> Cantor, letter to Hilbert, 8.8.1906. From Cantor, *Briefe* p. 178. (My translation.)

which do not allow us to specify a set of natural numbers by a formula which involves quantification over the sets of natural numbers. Now it seems that the domain of sets of naturals which are specifiable by these means is a definite domain, over which we can legitimately and meaningfully quantify. There is, therefore, no objection on the predicativist view to the following procedure of ramification. Call those sets of naturals level 1 sets; and then say that level 2 sets can be specified by means which include quantification over the level 1 sets. Continue similarly to form level 3 sets, and so on.

However, the decision as to whether or not to ramify a system of predicative mathematics turns out not to be a matter of any great philosophical significance. It is always legitimate to ramify, and as we shall see in chapter 7, ramification can bring with it a certain increase in mathematical power when it is carried on into the transfinite. But for most of this thesis, we will focus the discussion on unramified predicative theories of the natural numbers, for this will suffice to illustrate the conceptual security and the mathematical power of predicative mathematics.

### 3.3.5 Uncountability

The predicativist view, then, is that the only definite domains are those which consist of members which can be completely defined by finitary means. As legitimate definitions are finite constructions in a finite alphabet, there are only countably many definitions, and so such domains can be at most countably infinite.

The question then arises of how the predicativist is to understand what are normally taken as demonstrations of the existence of uncountable domains. The most famous case for the existence of uncountable domains is of course Cantor's Diagonal Argument.

The Diagonal Argument can be seen as a reductio of the assertion that there is a counting of the powerset of the natural numbers, i.e. a surjective  $f : \mathbf{N} \mapsto \mathcal{P}(\mathbf{N})$ . Consider the set of naturals  $D_f = \{n \mid n \notin f(n)\}$ : then the assumption that  $D_f = f(m)$  for some natural number  $m$  leads to the conclusion that  $m \in D_f \leftrightarrow m \notin D_f$ , which is absurd. What is to be noted is that the argument is constructive relative to  $f$ : given any function  $f : \mathbf{N} \mapsto \mathcal{P}(\mathbf{N})$ , it gives us an explicit set  $D_f$  which is not in its range.

The position that Cantor's argument threatens is what we might call 'strict countablism': the view that every domain is (finite or) countable. The strict countablist

would be forced into the awkward position of denying some part of the (very elementary) reasoning in the argument.

The predicativist, on the other hand, accepts the conclusion that the domain of sets of natural numbers is not countable; but this conclusion is understood as meaning that the domain is indefinitely extensible, rather than that it is a definite domain of some larger infinite size. It should be noted that this is just what the classical set theorist is forced to say about the domain of ordinals, or of the domain of sets in general. If the domain of all of the sets is ‘too big’ to form a set, might not the same be true of the continuum? The predicativist challenge to the classicist can be simply put as the question: What reason is there for believing in the principle that entails the existence of the continuum — the powerset axiom?<sup>23</sup>

For the predicativist, sets of natural numbers are given by means of principles of definition or set-construction. Any fixed list or collection of such means will give rise to only countably many sets, and therefore the means of definition can always be expanded — the process of diagonalizing will always give rise to more sets. What is unjustifiable is the classical assumption that there is some sort of a limit to this process: a completed totality or definite domain of all of the sets of natural numbers, a totality of which the diagonal set of any sequence of members is already a member.<sup>24</sup> Poincaré nicely expresses the situation:

Richard’s proof teaches us that, whenever I break off the process, then there is a corresponding law, while Cantor proves that the process can be continued arbitrarily far.<sup>25</sup>

And what goes for sets of natural numbers also goes for real numbers. For the predicativist, then, the continuum is not a mathematical object: we cannot unproblematically employ classical quantification over it. The ‘Weylian number system’, comprising those reals which are definable by the arithmetic means that Weyl outlines in *Das Kontinuum*, is obviously incomplete, in that we can diagonalize out of it: that is to say, we can define in terms of it a real number which it does not include. But what recommends it is that it is a definite domain, over which we

<sup>23</sup> This question is the germ of the whole of the negative part of this thesis. It is given further force by the morass which surrounds the Continuum Hypothesis, which will be discussed in §4.1.3 below. The question is further discussed from various angles by Hallett, *Cantorian set theory*. <sup>24</sup> See § 4.3.4 below for further discussion of the countablist construal of the Diagonal Argument. <sup>25</sup> Poincaré, ‘Über transfinite Zahlen’ pp. 46–47.

can meaningfully quantify classically, and that it is rich enough to allow the natural development of large parts of classical analysis.

Although the idea of an infinite powerset is perhaps the obvious route to the idea of uncountability, another route seems to be given by considering the set of all countable ordinals. The predicativist's objection here is, again, simply to ask: Why should we suppose that there is any such set?<sup>26</sup> The objection is given substantial force by the paradoxes; in particular the Burali-Forti paradox, which tells us that there is no set of *all* of the ordinals. Unless we already know that there are uncountable ordinals, we have every reason to be wary of the assumption that there is a set of all of the *countable* ordinals. But of course the only grounds for the belief that there are uncountable ordinals are the very arguments in question. It seems that there is no non-circular route to the Cantorian higher infinite.

### 3.3.6 The arbitrary infinite

The history of the broadening of the concept of 'function' in the course of the eighteenth and nineteenth centuries is often told as a move from the idea of a function as some sort of 'analytic expression' — a function described by means of an expression built up from polynomials and trigonometric functions — and to the modern idea of a function as any association of arguments with unique values. And this was what was suggested in the potted history of chapter 2 above.

But the discussion of definitionism above allows us to see that the important development as far as predicativism is concerned is not the broadening in permissible methods of function definition, so much as the consideration of a domain of functions, of which some are not definable at all; it is that which led to impredicative specifications, that is, the use of quantification over that domain of functions in a specification of one of its members. I will use the phrase 'arbitrary function' for the concept of a correlation of arguments with unique values, regardless of whether it can be given by a finite rule.

While historically it was with the consideration of *functions* that these issues first arose, it should be kept in mind that the situation with other infinite objects — most importantly, arbitrary real numbers and arbitrary sets of natural numbers — is exactly the same.

The move to arbitrary functions has nothing to do with the move out from

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<sup>26</sup> See the discussion in §2.2 above.

functions defined by nice algebraic expressions to a broader class of functions which are defined by increasingly ad hoc means. The example given by Riemann of a (Riemann-) integrable function which has infinitely many discontinuities in every interval is not, in the relevant sense, an arbitrary function, *precisely because it is an example*. That is to say, it is not arbitrary, because it is completely defined by a finite rule.

But in general, to specify an arbitrary function — or to switch to the simpler case, a real number — requires an infinite amount of information. In certain cases this is not so: there clearly are real numbers which are finitely specifiable, such as  $\frac{2}{3}$ ,  $\pi$ , and the real between 0 and 1 with binary digits given, at each place, by whether or not the Turing machine with that number halts.<sup>27</sup> But we have every reason to believe that systems of notation or description which we could use would only get us countably many real numbers.

The important conceptual leap is the leap to the idea of a domain of quantification which includes reals, or functions, regardless of whether or not they are definable. This leap takes us from an understanding of quantification over functions which is compatible with schematic quantification ('if you give me a function, I'll give you a Fourier series for it'), to the classical understanding of such a quantification, as the logical product of all of the members of the domain — in this case, the function space  $\mathbf{R}^{\mathbf{R}}$ . It is the leap from talk of 'any' function to talk of 'all' functions.

On the older conception, the domain of functions was open-ended. Hypothesized generalizations about any function could be established on the basis of schematic reasoning, or refuted by producing a counterexample; but in the absence of either, such a claim had no determinate content.

Of course, it is quite coherent for a constructivist to suppose that all of the functions that there are are definable by certain specified means; and in this case there is no difference between schematic and classical universal quantification over all of the functions. But as a matter of fact, the definable functions were understood as being open-ended. The discussion of Richard's paradox above shows why: given the domain of items which are defined by any given formally laid down means of definition, there will be a diagonalization which is not definable in the original sense, but clearly is a legitimate definition in the informal sense.

An implementation of the positive predicativist project will involve placing

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<sup>27</sup> The point of the last example is to show that the definable reals are not just the Turing computable reals.

certain restrictions on the definition of functions and sets: such definitions must be predicative. But this requirement follows from the basic criticism of the Cantorian approach, which predicativists and intuitionists share. It is not that the Cantorian is over-liberal in the function or set definitions she countenances, that she allows in particular items that are too gruesome to be stomached; the objection is that the Cantorian takes herself — extravagantly — to be considering the set of *all* real-valued functions, *including those without any definition at all*. (Which is, on the classical view, almost all of them.)

The Cantorian picture is of a definite domain which includes all arbitrary functions, as well as those given by rules. The way in which this picture makes itself felt is through the assumption that quantifications over such a domain have determinate meaning; most egregiously, for the predicativist, in the assumption that such quantification may legitimately feature in definitions.

Consider the following argument. We can imagine an early nineteenth-century analyst, who is aware that there are functions with infinitely many discontinuities. He claims that nonetheless, for every function and every interval, there is at least one point in that interval where the function is continuous. He is then confronted with Dirichlet's example of a totally discontinuous function. He protests: But that isn't the sort of thing I meant! That isn't a proper function!

For such a dispute to be possible, there must be a difference in opinion of the meaning of the quantifier phrase 'all functions'. Such disputes were in effect prevented by the adoption of a completely general definition of a function: any single-valued correspondence of reals with reals is a function. Which is to say: anything goes. But is this explicit dropping of restrictions enough to determine a univocal meaning for the phrase 'all functions'? The Cantorian assumption is that mathematical reality is enough to settle the matter. There is obviously much that can be said on the matter; but the modest predicativist point is simply that there is an assumption here which the classicist needs to make and which the predicativist does not.

### 3.4 Predicativism

As discussed in the introduction, the position I put forward in this thesis is a form of evangelical predicativism. My central criticism of impredicative classical mathematics is not that it is inconsistent or incoherent: it is that I do not see any reason to

believe it to be true. Nothing that I have to say gives any reason to think that a system such as ZF is inconsistent. I do raise doubts as to the clarity of the intuitive model of ZF — the iterative hierarchy of sets. As the only positive reason to believe that a fundamental mathematical theory is consistent is that we have a clear intuitive model of it, if such doubts can be sustained, mathematicians should be less confident in the consistency of ZF than they currently are. But such considerations are inevitably rather nebulous; all that can ultimately be said is that, despite my best efforts, I have not found the explanations classicists have offered to be satisfactory.

I am aware that I cannot hope to make a wholly compelling case for the evangelical position given current constraints of time and space. I hope, however, to do enough to show that the evangelical position is worthy of consideration, and so to shift at least some of the burden of proof onto the defender of the classical status quo. In particular, I will argue that one currently popular line of defence for the realistic acceptance of classical mathematics, the scientific indispensability argument, fails to support impredicative mathematics.

The indispensability argument derives from Quine. The idea is that mathematics is an essential part of our total scientific theory, and that our reason for holding mathematics true is of the same sort as our reason for holding true any (other) scientific belief: mathematical truth, like the rest of scientific truth, is ultimately empirical. I will deal more fully with the subject of indispensability arguments in §7.2 below; but, to briefly anticipate that discussion, I will argue that the Quinean line, when augmented with some consideration of the extent of predicative mathematics, leads to the conclusion that impredicative mathematics does not earn its keep. It is not a working part of our scientific machinery, but, to use Quine's words, 'mathematical recreation', and as such is 'without ontological rights.'<sup>28</sup>

At this point, it is as well to clarify how my use of the label 'predicativism' relates to that of others. Feferman<sup>29</sup> notes that intermediate between the predicative and the Cantorian conception of sets is what he calls the 'constructive' conception, which permits, besides predicative set definitions, generalized inductive definitions of various sorts, allowing one to form the smallest sets satisfying certain closure conditions. Feferman cites Lorenzen, Wang and Myhill as writers who have explored this broader conception, and regrets that Lorenzen and Wang used the term 'predicative' to refer to it.

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<sup>28</sup> Quine, 'Reply to Parsons' p. 400.    <sup>29</sup> Feferman, 'Systems of predicative analysis [I]' pp. 4–5

It should be noted that Feferman himself seems to endorse such an intermediate position: he describes himself not as a predicativist, but as an antiplatonist with predicativist sympathies. He accepts the legitimacy of certain formal systems which go well beyond the limits of predicative mathematics, because they can be justified proof-theoretically by means of iterated inductive definitions such as Lorenzen and Wang studied.

I shall follow Feferman's narrower use of 'predicative', but not his use of 'constructive': 'constructive' is useful and now well-established as a broad-brush term for the whole tendency within foundations which is sceptical of modern classical mathematics, and is prepared to consider revisions.

### 3.4.1 'Given the natural numbers'

The position I am exploring and promoting in this thesis is predicativism given the natural numbers. The significance of the qualification is that the position takes for granted the arithmetic of the natural numbers. I should make it clear at once that I mean first-order classical arithmetic: I will argue in Ch. 4.2 that the second-order consequence relation is not something we can claim to have a purely logical grasp of. The legitimacy of this 'taking for granted', and what in fact it amounts to, are explored in Ch. 5.2; but my concern in this thesis is primarily with the philosophy of analysis (and higher mathematics more generally), and not with the philosophy of arithmetic.

Taking arithmetic for granted is the route advocated by Poincaré and followed by Weyl; it contrasts, of course, with the logicist position of Russell and Whitehead in the *Principia*. Russell and Whitehead wanted to explain arithmetic as a body of logical truths, though to carry this out they found themselves forced to assume, as a supposedly logical axiom, the existence of an infinity of individuals. One might well think that this is not substantially better than simply postulating the existence of the natural numbers themselves.<sup>30</sup>

The reason that Poincaré and Weyl gave for taking the natural numbers for granted, as their starting point, was essentially the same reason that Kant gave: the truths of arithmetic are synthetic a priori, and our knowledge of them derives from

<sup>30</sup> It is in fact *somewhat* more modest: *Principia*'s axiom of infinity assumes that the individuals are simply infinite, and it is then proved (without the use of any Choice assumptions) that the system's natural numbers, two types up, are Dedekind infinite. See Boolos, 'Honest toil'.

intuition. In particular, Poincaré and Weyl both lay great stress on the intuition of iteration as our route to the natural number concept, and therefore on arithmetical induction as primitive.<sup>31</sup>

It has been argued by various writers that the natural numbers can only be fully characterized by impredicative means.<sup>32</sup> It is induction that is blamed for the impredicativity: the natural numbers are supposed to be characterized axiomatically; that is, the Peano axioms serve as a sort of implicit definition of the concept of natural number. But among those axioms are instances of induction which involve quantification over the natural numbers. (This is why the definition of the natural numbers in *Principia* depends essentially on the Axiom of Reducibility, which effectively licenses impredicative set specifications.)

I will argue in Ch. 5.2.2 that instances of the first-order induction scheme are not in fact formally impredicative. But the broader question is whether the natural numbers stand in need of the sort of characterization which is being offered. The role that induction plays in the characterization is to weed out non-standard elements. But we do not arrive at our conception of the natural numbers by first thinking of some larger domain, and then throwing elements away until we are left with the minimal collection which contains zero and is closed under the successor operation. In reality, the process is quite the reverse: we build up from zero. The natural numbers are zero, the successor of zero, the successor of the successor of zero, and so on. And after coming to that conception of the natural number structure, we can then see that the Peano axioms hold of it.

There is of course much more to be said about how we come to understand the natural number structure; the simple point being made here is that our grasp of the natural numbers comes from understanding the iteration of the successor operation, and that they can therefore be seen as being built up in parallel with the numerals. What this gives us is a disanalogy between the natural numbers, on the one hand, and the sort of sets which the predicativist rejects, on the other. Those sets are given by impredicative specifications, and in general there is no way of building them up from below in a finitary step-by-step fashion in the way that the naturals can be generated. This disanalogy is a reason to think that the naturals are a legitimate starting point, and so also a reason to think that the position I am advocating, which

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<sup>31</sup> Goldfarb, 'Poincaré against the logicians'. <sup>32</sup> See e.g. Parsons, 'The impredicativity of induction [1992]', Nelson, *Predicative Arithmetic*, Dummett, 'Gödel's theorem'.

takes the naturals for granted and then proceeds in accordance with a predicativity requirement, is one that is indeed worth exploring.<sup>33</sup>

### 3.4.2 Conclusion

This chapter completes the introductory part of the thesis: as such, I have issued several promissory notes which I will try to redeem later on. To help make clearer what has and what hasn't been achieved so far, I shall here briefly reiterate the main points of the chapter.

Predicativism is driven by the concern that quantification over open-ended domains may not be determinately meaningful. Domains for which we have a uniform system of notation (which gives a name to each of the elements of the domain) are not open-ended in this way. The paradigmatic example of such a domain is the domain of natural numbers, for which we have the numerals as a notational system. All such domains are at most countably infinite; they lack the arbitrariness which is the hallmark of the uncountable. We cannot simply wish away the uncountability of the continuum: but as quantification over the continuum is highly problematic, we must avoid such quantification in our definitions.

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<sup>33</sup> Isaacson has influentially argued that there is a distinct sort of arithmetical knowledge which grounds first-order Peano arithmetic. See Isaacson, 'Arithmetical truth and hidden higher-order concepts', Isaacson, 'Some considerations on arithmetical truth and the omega-rule'.

## Chapter 4

# Classicism about the continuum

The thesis of this chapter is that there are serious *prima facie* problems with the classical view of the continuum. These problems comprise a good case that it is worth at least exploring possible alternatives to classicism.

The charge I make against classicism is not that it is incoherent; it is simply that it is very difficult to explain in detail what the position really is, and very difficult to see either how anyone could come to such a conception, or why it should be thought true.

The structure of this chapter is as follows. §§4.2–4.4 deal with three different routes to classicism: *second-orderism*, according to which our grasp of the classical continuum flows from our grasp of the natural numbers together with a logical understanding of the subsets of the naturals; *Cantorianism*, according to which it flows from a mathematical understanding of the powerset of the naturals; and *intuitivism*, which takes the continuum as primitively given to us in intuition. §4.1 deals with some preliminaries: I explain the taxonomy of classicism that I have adopted; and I explore the issue of the Continuum Hypothesis, which to some extent cuts across the taxonomy.

### 4.1 Preliminaries

What are the commitments of contemporary mathematical practice? Here is a list of some commitments that concern the infinite: There are actually infinite combinatorial collections, most centrally some set that will serve as the natural numbers and some set that will serve as the

power set of that one, which gives us the real numbers. Those collections exist in some sense that licenses reasoning about them impredicatively.<sup>1</sup>

The above quotation nicely expresses the standard view of the ontology behind arithmetic and analysis — what I will call the classical view. On this view, both the collection of natural numbers, and the arithmetical continuum (the collection of its subsets) are definite domains. In keeping with the Kreisel–Dummett dictum that the important philosophical issue is the semantics of mathematics, not the ontology, I will focus on the cash-value of this: the classicist takes statements involving arbitrary quantification over  $\mathbf{N}$  and  $\mathcal{P}(\mathbf{N})$  to be meaningful and to have determinate truth-values (even though in many cases we may not be able to discover them).

On the classical view, then, every sentence in the language of second-order arithmetic (analysis, as logicians call it) is, when given the standard interpretation, determinately either true or false, quite independently of us, and of our ability to know which it is.

The predicativist view, which I am expounding, opposes this: it takes there to be an important difference between the domains  $\mathbf{N}$  and  $\mathcal{P}(\mathbf{N})$ . The predicativist agrees with the classicist that the natural numbers are a definite domain, and so agrees that arithmetical statements (statements in the language of first-order arithmetic) are meaningful and have determinate truth-values; but she rejects this for the arithmetical continuum, i.e. the powerset of the natural numbers.

The predicativist contention is of course more general, in that it rejects the conception of the classical powerset of any infinite set. But, again, I focus here mostly on the first and simplest case, which is also the most important for mathematics.

Anti-classicism — that is, scepticism about the classical view — may or may not be the result of predicativist sympathies. In most cases, though, the dialectic between the classicist and the anti-classicist runs something like this. The anti-classicist demands that the classicist explain what exactly it is that the classicist believes, or that she explain how the uninitiated (or sceptical) could ever get to a classical conception of the continuum.

The classicist perhaps tries to respond to these question, but, as we will see, some common responses do not get very far. But a very popular alternative move for the classicist is a *tu quoque* style of argument: to insist that, although classicism might seem a little mysterious, and although these questions are very hard to answer to the

<sup>1</sup> Lavine, *Understanding the Infinite* p. 157.

sceptic's satisfaction, as a matter of fact, everyone (or almost everyone) *already has* a classical conception of the continuum; and that therefore the sceptical view really is sceptical, in the sense of being a falsehood universally acknowledged as such (though it may be difficult to know just what is wrong with the position or how to refute it). The existence of this shared classical conception of the continuum is shown (the classicist claims) by various shared, largely unproblematic practices which require such a conception for their underpinning (or at least require for their underpinning foundations which can also support classicism). Popular candidates for practices which supposedly reveal classical commitment include our free use of the concept of the ancestral of a relation, and our ready acceptance of classical arithmetic, as well as aspects of language use such as the plural idiom and quantification into predicate position. I will attempt to undermine such arguments by showing either that the practices appealed to are more problematic and questionable than is assumed by the classicist, or that they do not in fact lend the support for classicism about the continuum that is claimed for them.

#### 4.1.1 Taxonomy: three routes to classicism

A defence of classicism about the arithmetical continuum might take one of three broad paths: one path is to take second-order logic seriously as logic, and so to see the truths of analysis as logical consequences of (sentences expressing) the structure of the natural numbers. I will call this the second-orderist path. The second approach is conceptually more economical, but is ontologically somewhat profligate (at least in its usual form): it views analysis as a special branch of a more general set theory. I will call this the Cantorian path. The third approach is to take the classical continuum as in some sense intuitively given. An obvious route would be to appeal to our geometric intuition of the continuity of space (or of lines in space), and to explain the real numbers on that basis. I will call this approach 'intuitivism' (though with some reluctance).

Second-orderists view the classical domain of sets of natural numbers as something which is logically given when the natural numbers are: analysis is just arithmetic plus logic. While second-order logic is very often seen as (at least a part of) our route to a grasp of the natural numbers, second-orderism is independent of, and not to be confused with logicism: logicism is the position that the natural numbers themselves are given to us logically. Second-orderists need not (and generally do

not) take such a view: it is now very widely held that it is not a *logical* truth that there are an infinite number of objects. What distinguishes second-orderists is instead their view that what there is (mathematically speaking) suffices to completely determine the truth-value of sentences featuring second-order quantification over what there is. In the case of the arithmetical continuum, the second-orderist could have a (first-order) domain consisting just of the natural numbers, and justify or analyse classical analysis as the second-order theory of these objects. (Such a view may sidestep worries about the Continuum Hypothesis, as we will see below.)

On the Cantorian view, there are not really two sorts of quantification, one over natural numbers and one over sets of natural numbers: there are just two sorts of objects, natural numbers and sets of natural numbers. What sets of natural numbers there are is a consequence of general principles of set existence; but the sets of naturals are abstract objects in the world, just as the numbers are; and as such it is at least *logically* possible that there were fewer sets of naturals than there actually are. (It is not important for my purposes to distinguish between set-theoretic reductionist Cantorians who take the natural numbers actually to be sets, and non-reductionist Cantorians who take them to be urelemente.) In brief: for the Cantorian, analysis is given by arithmetic together with the set concept. As such, the Cantorian views both the arithmetical continuum and the natural numbers as mathematical structures to be investigated by means of a standard (first-order) mathematical theory.

In this, the Cantorian agree with the intuitionist, who appeals to (perhaps geometrical) intuition to justify a mathematical theory of the classical real numbers. However, the intuitionist route also has similarities to second-orderism, in terms of its (apparent, relative) modesty: it does not obviously commit itself to the existence of sets such as  $\aleph_{17}$ .

My three-fold distinction is, to repeat, among classicists about the arithmetical continuum: that is, those who believe that every sentence  $\phi$  of the language of second-order arithmetic,  $L_{2A}$ , has a determinate truth value. Second-orderists believe this because they take themselves to have a logical understanding of what it is for something to be a full model of a second-order theory, and because on this assumption, Dedekind's proof of the categoricity of second-order Peano arithmetic ( $PA_2$ ) tells us that there is, up to isomorphism, only one such model of  $PA_2$ , which will of course settle the truth-value of any sentence of  $L_{2A}$ .

Cantorians and intuitionists are not prepared to take second-order logical con-

sequence as being unproblematic or fundamental in this way: they instead defend the meaningfulness and bivalence of  $\phi \in L_{2A}$  with reference to a pair of mathematical structures  $\mathbf{N}$ ,  $\mathcal{P}(\mathbf{N})$  over which, respectively, the first- and second-order variables of  $L_{2A}$  range. (Most commonly, these structures are identified with certain sets — the von Neumann ordinal  $\omega$  and its powerset — in a background set theory such as ZFC, though this, too, is an optional extra. I will, however, sometimes make this convenient identification to smooth the exposition.)

The appeal to direct geometrical intuition of the continuum is clearly different from the other two approaches. However, the distinction between second-orderists and Cantorians should perhaps not be taken too seriously: I make the distinction here primarily for expository convenience.<sup>2</sup> My concern in this chapter is to criticize the argument that we have sufficient grasp of the continuum to justify impredicative mathematics, and my discussion falls into three parts, according as my opponents account for their grasp by logical, by mathematical, or by intuitive means.<sup>3</sup>

#### 4.1.2 Taxonomy: first-order and second-order

The second-orderist is committed to *full* second-order logic. But as the adjective suggests, other ‘logics’ are also sometimes called second-order, and with good reason. For instance, the subsystems of second-order arithmetic discussed in Simpson’s book of that name are not theories in a background of full second-order logic.<sup>4</sup> Such theories are indeed often called two- (or many-) sorted first-order theories.

There are (at least) three notions of second (or higher) ‘order’ which are run together in this terminological morass; and it is as well to try to distinguishing these as a preliminary to our discussion. A theory may be:

- (a) *interpretationally* second-order in that it has, as its intended model, two (or more) essentially different sorts of objects: urelemente, and sets or collections of urelemente (and perhaps also sets or collections of those, and perhaps beyond);
- (b) *presentationally* second-order, in that, in addition, the language of the theory distinguishes between these two (or more) sorts of object by featuring two (or more) sorts of variables;

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<sup>2</sup> Shapiro, for instance, notes that the acceptance of what he calls ‘working realism’ in set theory comes too much the same thing as acceptance of second-order logic: see Shapiro, *Foundations* p. 255. <sup>3</sup> See Moore, ‘Beyond first-order logic’ for discussion. <sup>4</sup> Simpson, *Subsystems of Second Order Arithmetic*.

- (c) *fully* second-order, in that, further, a structure is to count as a model of the theory only if it has the second-order variables ranging over the full classical powerset of the domain.

It is precisely because the Cantorian (unlike the second-orderist) does not take the idea of the full classical powerset of an infinite domain to be unproblematically logical, and so cannot accept a fully second-order theory of the continuum, that she needs a *formal* theory of the continuum. Such a formal theory may not, on pain of circularity, be fully second-order; but it may well be presentationally second-order.

An example of a formal theory which is presentationally second-order is the standard, recursively axiomatized, formal second-order theory of arithmetic  $PA_2$ .<sup>5</sup> Iterative set theories such as ZF and its relatives are presented in single-sorted languages; though they could perhaps be argued to be interpretationally transinitely higher-order, based as they are on an idea of rank. (In contrast, for example, to type-free systems such as Quine's NF.) But it is not necessary to take a view on this here. Set-class theories, such as NGB and MK, are straightforwardly interpretationally second-order, given the distinction they draw between sets and classes; again, they may or may not be presented in a two-sorted language.

### 4.1.3 The Continuum Hypothesis

One problem which seems at first to confront classicists of whatever stripe is the Continuum Hypothesis, CH, and so, before turning to discuss the three routes to classicism, I want to pause here to briefly note that a classicist's judgement on its status can depend on the route to classicism which she takes. As we saw in Ch. 2, CH is the hypothesis, made by Cantor soon after he recognized the uncountability of the continuum, that its cardinality is  $\aleph_1$ , the smallest it could be.

But as is familiar, the standardly accepted principles of set theory fail to settle the matter: they are compatible with both the truth of CH (as Gödel showed in 1940),<sup>6</sup> and with its falsity (as Cohen showed in 1963).<sup>7</sup>

The fact that CH continues to be 'wide open' is something of a scandal given its fundamental position. As Scott writes:

<sup>5</sup> However, a variant presentation of this theory can be given in a one-sorted language. In such a case one requires predicates meaning (intuitively) '... is a number', '... is a class.' <sup>6</sup> Gödel, *The Consistency of the Axiom of Choice*. <sup>7</sup> Cohen, 'Independence of CH'.

Is it not just a bit embarrassing that the currently accepted axioms for set theory (which could be given — as far as they went — a perfectly natural motivation) simply did *not* determine the concept of infinite set even in the very important region of the continuum?<sup>8</sup>

Cantor, famously, regarded CH as hugely important and worked at it incessantly, on several occasions taking himself to have found a proof. Cantor of course believed CH to be true;  $\aleph_1$  is certainly the tidiest cardinality for the continuum, and so this would be in keeping with Cantor's deeply held (and religiously motivated) belief that the realm of the transfinite was not in the least arbitrary.

Received opinion among present-day set theorists seems to be against CH, if only on the grounds that a larger continuum makes life more interesting. But a more fundamental question is whether CH has a truth-value at all. In the wake of Cohen's proof, many suggested that set theory would bifurcate into 'Cantorian' (where CH holds) and 'non-Cantorian' set theories (where the negation of CH holds), just as geometry bifurcated into Euclidean- and non-Euclidean geometries after it was shown that the parallel postulate and its negation were both consistent with Euclid's other axioms. As Scott wrote,

Cohen's ideas created so many [consistency] proofs that he himself was convinced that the formalist position in foundations was the rational conclusion.<sup>9</sup>

Kreisel has famously defended the meaningfulness of CH on second-orderist grounds.<sup>10</sup> Cohen's proof shows that different models of first-order set theory have 'continua' of different sizes. But we have known since Skolem that first-order theories with infinite models cannot be categorical. And Zermelo showed, in modern terms, that second-order set theory, ZF2, is quasi-categorical: any two models differ only in respect of their height, that is to say in how many ordinals they contain. In all of them the referent of ' $\mathcal{P}(\omega)$ ' is the real powerset of  $\omega$ ; and it is the case that, either this set has cardinality  $\aleph_1$  in all of the models, or it has some other cardinality  $\kappa$  in all of the models. The only trouble is that we do not know which. Kreisel's position is a (particularly strong) form of second-orderism.

CH can be formulated as a sentence of pure second-order logic:

$$\forall X(\text{CONT}(X) \leftrightarrow (\text{ALEPH-1}(X))),$$

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<sup>8</sup> Scott, 'Foreword' p. xv.    <sup>9</sup> Ibid.    <sup>10</sup> Kreisel, 'Informal Rigour'.

where  $\text{CONT}(X)$  abbreviates the claim that  $X$  is continuum-sized, and  $\text{ALEPH-1}(X)$  says that  $X$  is of cardinality  $\aleph_1$ .<sup>11</sup>

So the classicist, who by definition believes in the determinacy of truth-value for sentences of  $L_{2A}$  and so *a fortiori* of sentences of pure second-order logic, seems to be committed to the determinacy of the meaning and truth-value of CH.

But this appearance is misleading, at least for some second-orderists. For a second-orderist whose domain consists just of the natural numbers, CH is vacuously true: the second-order variables range only over properties of natural numbers, and obviously their extensions — sets of natural numbers — are either finite or countably infinite. There is nothing continuum-sized or  $\aleph_1$ -sized in the range of the variable  $X$ , and so the biconditional is vacuously fulfilled.

CH *can* be formulated in a way meaningful to those who accept only countably many objects: but this formulation requires third-order arithmetic:

$$\forall \Xi (\text{CONT}(\Xi) \leftrightarrow \text{ALEPH-1}(\Xi)).^{12}$$

To take stock, then: CH is not a worry for the strictly arithmetical second-orderist, i.e. the second-orderist whose first-order mathematical ontology is limited to the natural numbers. (This is on two assumptions. First, that her non-mathematical ontology is also finite or countable, or at least not obviously of the power of the continuum. Second, that she is not tempted into accepting the meaningfulness of third-order logic.) The question of CH does not arise on such a view, because there are no (first-order) objects corresponding to the real numbers, and the only second-order items are the sets of natural numbers.

Weston, in contrast, defends the meaningfulness and univocality of CH on non-logical grounds.<sup>13</sup> He rejects Kreisel's second-orderist approach for various reasons (some of which are not very different to those I present below). But he claims that the continuum is a determinate object, on the grounds that it is physically realized,

<sup>11</sup>  $\text{CONT}(X)$  can be  $\exists C \exists R (\text{ALEPH-o}(C) \ \& \ \forall x \forall y (Rx y \rightarrow (Xx \ \& \ Cy)) \ \& \ \forall Y (Y \subseteq C \rightarrow \exists x \forall y (Rx y \leftrightarrow Yy)) \ \& \ \forall x \forall y ((Xx \ \& \ Xy \ \& \ \forall z (Rzx \leftrightarrow Ryz)) \rightarrow x = y))$ ;  $\text{ALEPH-1}(X)$  abbreviates  $\forall Y. Y \subseteq X \rightarrow (\text{FIN}(Y) \vee \text{ALEPH-o}(Y) \vee Y \sim X)$ ;  $\text{FIN}(X)$  is  $\sim \text{INF}(X)$ , where  $\text{INF}(X)$  is the obvious second-order statement that  $X$  is Dedekind infinite,  $\exists Y (Y \subsetneq X \ \& \ Y \sim X)$ ;  $X \sim Y$  abbreviates the statement that there is a bijection between  $X$  and  $Y$ , say  $\exists R [\forall x (Xx \rightarrow \exists y (Yy \ \& \ Rx y \ \& \ \forall z (Rzx \rightarrow y = z))) \ \& \ \forall y (Yy \rightarrow \exists x (Xx \ \& \ Rx y \ \& \ \forall z (Rzy \rightarrow x = z)))]$ ;  $\text{ALEPH-o}(X)$  abbreviates the statement  $\text{INF}(X) \ \& \ \forall Y (Y \subseteq X \rightarrow (Y \sim X \vee \text{FIN}(Y)))$ ; and finally  $X \subseteq Y$  abbreviates  $\forall x (Xx \rightarrow Yx)$ . See Shapiro, *Foundations* for further details and discussion.

<sup>12</sup> The abbreviations here can simply be the third-order versions of what was given in the note above.

<sup>13</sup> Weston, 'Kreisel and CH'.

and that not only classical mathematicians, but also constructive mathematicians of Bishop's school share the same conception of it.

The claim that the classical continuum is physically realized seems to me to be highly problematic. While the real numbers are used ubiquitously in physics, this does not seem to commit physicists to the Dedekind completeness of the set of points in a spatial interval any more than the use of continuous supply and demand curves in economics commits economists to the claim that goods and money are continuously divisible and can be exchanged in arbitrarily small quantities.

For his claim that Bishop-style constructivists share the classical conception of the continuum, Weston gives as evidence the fact that Bishop's definition of a real number is in terms of sequences which satisfy a constructive convergence criterion; and he suggests that a classicist can see that all and only the real real numbers will satisfy such a condition.

But all that this shows is that the classicist can agree with Bishop's definition of the real numbers. The question of what real numbers there are comes down to what sequences of rationals there are which will satisfy Bishop's convergence criterion. And there is every reason to think that the classicist believes in more sequences of rational numbers (ones which have impredicative definitions, for example) than Bishop does (or indeed, than the predicativist does).

The conclusion I would suggest from this highly problematic status of CH is that there is a general reason to be suspicious of the classicist's claim to have succeeded in pinning down the (classical) continuum, in whatever way she purports to have done this. However, as we shall see, this conclusion needs to be qualified somewhat.

## 4.2 Second-orderism

An illustration of received philosophical opinion on the nature of the second-order logical consequence relation is given by Michael Dummett (in his discussion of one part of the platonist philosophy of mathematics he opposes):

That there may be mathematical facts that we shall be forever incapable of establishing [...] is normally admitted on the ground that our inferential powers are limited: there may be consequences of our initial assumptions that we are unable to draw. If these are first-order consequences, we could 'in principle' draw them [...]. If they are second-

order consequences, we may be unable even in principle to see that they follow.<sup>14</sup>

The picture is that although there is not and cannot be an effective method for drawing out all of the second-order consequences of a set of axioms, what those consequences are is nonetheless entirely determinate. In the matter which concerns us here, the second-orderist stance is that our grasp of the meaning of second-order quantification is what underwrites a determinate conception of the classical continuum.

In opposition to this, I argue that the second-order consequence relation is highly problematic, and that insofar as we do understand second-order quantification, it cannot be plausibly claimed that this is logical understanding: it is instead by (implicit) appeal to some sort of set theory.

#### 4.2.1 The meaning of the second-order quantifiers

The standard second-orderist view (for example, as outlined in Shapiro, *Foundations*) is that the monadic second-order universal quantifier  $\forall X$  simply means *for all classes*; or *for all concepts*, or *properties*, or *propositional functions*, or *subsets of the domain*; or whatever. The metaphysical nature of the items referred to by the predicates, and over which the predicate variables range is not the point here. To vindicate classicism, the important thing is quantity, not quality: there need to be sufficiently many extensionally distinct items; there needs to be at least one item for each possible extension.

But which *are* the possible extensions? This is the very question the second-orderist is trying to answer. What we can say is that the domain of extensions should not be an extensible one:<sup>15</sup> the collection of extensions should be closed under diagonalization. As we will see in a moment, this requirement can only be formulated because the case in which we are interested here is one where the natural numbers are first-level objects, and so the classes we are concerned with are classes of natural numbers.

(It is worth noting in passing that another consequence of insisting that the natural numbers are present is that we can limit our discussion of second-order

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<sup>14</sup> Dummett, 'What?' p. 431.    <sup>15</sup> 'Domain', here as elsewhere, is not meant to be ontologically committing to a third-order entity, a class of classes or suchlike: it is used simply as the most convenient form of expression.

logic to monadic second-order logic with no loss in power or generality. This is because primitive recursive pairing functions are available, so quantification over polyadic predicate variables or function variables can be eliminated. This makes the discussion somewhat smoother than it would otherwise be.)

By saying that we require the classes to be closed under diagonalization, what I mean is that we require that for any sequence of classes  $X_1, X_2, \dots$ , the domain will also contain the extensions  $\{i \in \mathbf{N} \mid X_i(i)\}$ ,  $\{i \in \mathbf{N} \mid \sim X_i(i)\}$ , and so forth. And what does ‘and so forth’ mean? Again, this is more or less the whole point at issue. One of the predicativist’s objection to the second-orderist is the narrower point about the problematic impredicativity of classes such as the diagonal ones just defined; but the broader and more basic problem is that the second-orderist is simply incapable of fully stating what it is that she believes in. The best we can do is to say that the second-orderist’s domain of extensions should be large enough to validate the impredicative class-abstraction principle,

$$\exists X \forall x (Xx \leftrightarrow \phi(x)),$$

where  $\phi$  is any formula of the full second-order language (as usual, not containing  $x$  or  $X$  free). (If there are parameters in  $\phi$ , i.e. free variables other than  $x$  or  $X$ , the principle is to be understood as being prenex universally quantified.)

But insisting on the abstraction principle is not enough to capture the second-orderist position. This is because from the second-orderist’s perspective, there will be countable models of this principle; or indeed of any principle that can be written down and is open to interpretation. The second-orderist is forced simply to bang the table: to say, loudly, that all possible extensions just means ALL POSSIBLE EXTENSIONS.

The standard semantics explains the second-orderist view more fully. In the semantics for a first-order language, an interpretation  $I$  with domain  $D$  assigns to each variable  $x$  an element of  $D$ ; to each  $n$ -place predicate a subset of  $D^n$ ; and a quantified formula  $(\forall x)p$  is true on  $I$  iff all  $x$ -variants of  $I$  make  $p$  true. (An  $x$ -variant of  $I$  is an interpretation which is either  $I$  itself, or differs from  $I$  only by assigning to  $x$  a different element of the domain.) Similarly, for second-order languages, an interpretation assigns to each monadic predicate-variable  $X$  a subset of  $D$ ; and the formula  $(\forall X)p$  is true on  $I$  iff all  $X$ -variants of  $I$  make  $p$  true.

As the last clause shows, the notion of all of the subsets of the domain — the classical powerset  $\mathcal{P}(D)$  — is central to full second-order logic.

The full second-order semantics just sketched can of course be seen as a special case of so-called first-order semantics for monadic second-order languages: here the range of the second-order quantifiers is stated explicitly as part of the interpretation. As well as  $D$ , a set  $\mathcal{S}_D$  of subsets of  $D$  is given; and the  $X$ -variants considered are those which (may) assign a different member of  $\mathcal{S}_D$  to the variable  $X$ .

The Cantorian makes sense of the second-order quantifiers by choosing, for their range  $\mathcal{S}_D$ , the full powerset  $\mathcal{P}(D)$ . The Cantorian's understanding of second-order logic is mediated by her supposed mathematical knowledge of the powerset. The second-orderist, on the other hand, credits herself with understanding of the quantifiers by virtue of immediate, logical knowledge of their range. For this knowledge of the domain of classes to be logical knowledge, it must presumably be based on an understanding of a general notion of property or logical class, as opposed to a mathematical notion of combinatorial collection, which is the supposed basis of the Cantorian's knowledge.

#### 4.2.2 Second-order logic as logic

It is often said in favour of second-order logic that there are many arguments we find intuitively valid which are most naturally formulated in second-order terms. (A toy example is this: John is tall; Mary is tall; therefore there is something that John and Mary have in common. In fact, the conclusion of this argument is a logical truth on the second-order story. The argument is of course valid; but then so too is the argument: John is bald; Mary is tall; therefore there is something that John and Mary have in common. There is always something that John and Mary have in common, namely the property of being either John or Mary.)

It should be emphasized that such arguments are not relevant to our present concern. Whatever moves in such arguments we find logically compelling are simply candidate axioms or inference rules for a logical calculus which is to represent our (idealized) inferential practice.

If we believe that our idealized inferential practice must be recursively axiomatizable, then it is clear that these candidate axioms can never serve as evidence that the appropriate semantics for our logical practice is the full second-order semantics, because no recursively axiomatizable logical calculus can be sound and complete for that semantics. (This familiar fact follows from the Dedekind's Categoricity Theorem for second-order arithmetic, together with Gödel's first Incompleteness Theorem.)

It is this that suggests that full second-order semantics is a part of meaning which would necessarily transcend our use of language.

I would argue that there are good reasons to think that our idealized inferential practice must be recursively axiomatizable; but to pursue this would take us too far afield. But even those who are sceptical of the claim should recognize that the set of all of the candidate axioms which have actually been proposed is a recursively enumerable set. (In fact, it is presumably just the union of a small finite number of axioms, and the instances of a small finite number of axiom schemes.) Similarly, the inference rules which have been proposed as candidates are recursively specified and recursively based.

It might be objected that logicians do consider theories such as ‘true arithmetic’ (the set of sentences of the language  $L_A$  of first-order arithmetic (with addition and multiplication) which are true on the standard interpretation), and non-effective rules of inferences such as the  $\omega$ -rule for arithmetic (which allows one to infer  $\forall xFx$  from the infinite set of premises  $F_0, F_1, F_2, \dots$ ). But in practice, as a matter of fact, they do so from within some recursively axiomatizable meta-theory — normally ZF set theory or something similar. Reasoning with non-formal systems is done by working inside some formal system. Of course, mathematics is done informally; but it is an important regulative ideal for mathematics that if any stage of a proof were to have doubt cast on it, it could be formalized to remove such doubt.

It might be suggested that although the current basic principles of mathematics are formal, in the future, there may be proposals of candidate axioms or rules which are not. I am unworried by this apparent possibility, though: I believe that it is only apparent, and that our idealized inferential practice must remain formal. But if our concern is just with logico-mathematical practice as it exists, it is enough for our purposes here to note that this practice is, as a matter of fact, formalizable.

Boolos has given a related argument that an adequate account of our inferential practice would be a second-order account.<sup>16</sup> He gives an example of a ‘curious inference’ which (unlike the arguments just mentioned) is first-order valid; which has concisely stated premises and conclusion; but which would require for its proof (in standard systems of first-order logic) more symbols than there are leptons in the observable universe. Alternatively, the conclusion can be proved from the premises in a few dozen lines by using a standard deductive system for second-order logic.

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<sup>16</sup> Boolos, ‘A curious inference’.

Boolos comments:

Since Skolem's discovery of non-standard models of arithmetic, it has been well known that there are simple and fundamental logical concepts, e.g., the *ancestral*, that cannot be expressed in the notation of first-order logic. It is also well known that there notions of a logical character expressible in natural language that cannot be expressed in first-order notation. And it is increasingly well understood that it is neither necessary nor always possible to interpret second-order formalisms as applied first-order set theories in disguise. Thus although the existence of a simple first-order inference whose validity can be feasibly demonstrated in second- but not first-order logic cannot by itself be regarded as an overwhelming consideration for the view that first-order logic ought never to have been accorded canonical status as *Logic*, it is certainly one further consideration of some strength for this view.<sup>17</sup>

It should be pointed out, however, that the 'curious inference' is one which can straightforwardly be interpreted as applied first-order set theory. Boolos is quite right to point out that the reasoning we use to see the validity of the curious inference is clearly not the unfeasibly long pure first-order argument. But again, any candidate principle for reasoning which we might propose, to account for our finding the inference compelling, is just that: another candidate for inclusion in our idealized inferential practice. As such, it cannot be evidence that our logical practice requires the assumption of full second-order semantics. The other items on Boolos's charge-sheet are discussed below.

### 4.2.3 Arbitrary concepts

The basic objection to second-orderism is the notion of concept that it requires; in particular, it needs a notion of concept which includes arbitrary concepts. To be clear: 'arbitrary concepts' are those which need not be given by any finite rule. The question is where we can get a notion of concept suitable for the second-orderist's purposes.

Clearly the Tractarian or Armstrongian view of concepts, on which they are appropriate components of states of affairs (revealed by logical analysis or by physics)

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<sup>17</sup> Boolos, 'A curious inference' pp. 379–380. Emphasis in original.

will not do. On such a view of concepts, there would presumably be only finitely, or at most countably many concepts, and so would not be closed under diagonalization; and so the concepts could not be the basis for a classical continuum. The second-orderist needs a notion of concept that is ‘thinner’, more pleonastic than this.

As mentioned earlier, it is a logical truth, on the second-order account, that John and Mary have something in common: namely, the concept of being John or being Mary. And that obviously generalizes: we can construct a concept true just of those objects given in a finite list. But of course it is the infinite case that we are interested in. Here too, some concepts will be given by a rule. (For instance, the concepts with co-finite extensions can be given by listing the objects of which they are false; the concepts *even number* and *prime number* can be given by simple arithmetical expressions.) But if our rules are to be rules for us, things that we could (perhaps only in principle) understand or follow, they must be finite; for example, expressions formed of a finite number of symbols from a finite alphabet. And there can only be countably many such finite rules, while there are supposed to be uncountably many concepts in the domain of the second-order variables. Therefore, almost all of these concepts are undefinable.

The simple fact that we do not have symbols for these concepts does not in itself mean that it is not legitimate to quantify over them all. After all, we do not have a name for every grain of sand in the desert, but that does not stop us from being able to quantify over them.

What we do have, though, is the ability in principle to get acquainted with and name any particular object, including any particular grain of sand. (So, at least, we classically suppose.) Whereas it is much more questionable that we could, even in principle, become acquainted with wholly arbitrary concepts, conceived of as corresponding to arbitrary subsets of an infinite domain.

Why is there a disanalogy here between unnamed objects and unnamed concepts? I suggest that this is because our understanding of concepts derives from our understanding of the compositional structure of complete propositions, in a way that our understanding of objects does not.<sup>18</sup> As such, the concepts are indefinitely extensible, just as the propositions are: we can form new propositions by quantification, and so new concepts from those propositions. In contrast, the objects are not

<sup>18</sup> To put the matter in Kantian terms, we have a robust ‘concept of object in general’, but no similarly robust concept of concept in general. See Parsons, *Mathematical Thought and its Objects* Ch. 1 for some discussion of the issues here.

indefinitely extensible in this way, unless we accept impredicative functions from concepts to objects, as are entailed by Hume's Principle, for example, or Axiom V.

A proposition can be decomposed by seeing it as functionally dependent on one of the objects which features in it; that function is a concept. For Frege, a concept is a function from objects to truth-values; for Russell, a concept (or 'propositional function', as he called it) is a function from objects to propositions. On the level of language, we get a predicate by taking a declarative sentence and replacing a singular term in it with a variable.<sup>19</sup>

But substituting a variable for a singular term only gives us the idea of an 'objective predicate': that is, an  $F$  such that  $Fa$  says about  $a$  what  $Fb$  says about  $b$ .<sup>20</sup> The idea that the second-orderist needs is not an 'objective predicate' but rather something more like Ramsey's idea of a propositional function in extension: this is a purely arbitrary correlation of objects with propositions. In Ramsey's example,  $\phi(\text{Socrates})$  may be: Queen Anne is dead; while  $\phi(\text{Plato})$  may be: Einstein is a great man.<sup>21</sup> Or, if we are working in the Fregean framework, what the second-orderist needs is the idea of a purely arbitrary correlation of objects with truth values.

Either way, the problem with Ramsey's propositional functions in extension or with arbitrary Fregean functions from objects to truth values is that we do not have a genuine functional connection between the argument and the value; we just have a

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<sup>19</sup> Of course, as Ramsey famously pointed out in 'Universals', thus far there is perfect symmetry between predicates and names: a name can be viewed as what is left after taking a predicate out of a simple sentence. But a restriction to simple sentences is question-begging, as 'simple' here means subject-predicate. And in the general case, as Ramsey recognized ('Universal and the "Method of analysis"') there is a difference: 'Wise, like a  $\phi x$  in Mr Russell's system, determines the narrower range of propositions " $x$  is wise" and the wider one " $f$  wise," where the last range includes all propositions whatever in which wise occurs. Socrates, on the other hand, is only used to determine the wider range of propositions in which it occurs in any manner; we have no precise way of singling out any narrower range.' Ramsey then continues, somewhat mysteriously: 'Nevertheless this difference between Socrates and wise is illusory, because it can be shown to be theoretically possible to make a similar narrower range for Socrates, though we have never needed to do this.' I do not know what Ramsey had in mind here; but I suggest that his second thought is the right one, and that there is an important difference in the combinatorial behaviour of the words 'Socrates' and 'wise,' and in the worldly items to which they refer. <sup>20</sup> The phrase 'objective predicate' is due to Sullivan, 'Wittgenstein on *FoM*'. In the presence of impredicative set theory, the arbitrary predicates which the second-orderist needs are of course also 'objective predicates' expressing set-membership claims. (This recalls Russell's argument for the Axiom of Reducibility: see Ch. 6.2 below.) But to appeal to set theory here is to give up on second-orderism in favour of Cantorianism. <sup>21</sup> Ramsey, 'Foundations' p. 215.

table listing, for each argument, what the value is. And if we have an infinite domain of arguments, it is unclear what sense we can make of such a table.<sup>22</sup> Whether or not we can find some way of making sense of the idea of such arbitrary correlations, what seems highly questionable is that they have anything to do with *logic*: that these apparently ungraspable concepts play an essential role in an adequate explanation of our inferential practice.

#### 4.2.4 The plural alternative

So far, I've cast doubt on the idea that full second-order quantification (that is, quantification over a definite domain of all arbitrary sets of objects) is a genuinely logical notion. It might be objected, however, that this notion is one that we are committed to by our use of the plural idiom in natural language, or by our understanding of the ancestral of a relation, or of arithmetic. In this and the next few subsections, I will argue that these objections are misplaced.

George Boolos, as we have already seen, has been a great champion of second-order logic, and his work has been responsible for much of the increase in philosophical acceptance of second-order logic from the 1970s onwards. Perhaps his most important contribution has been in developing the interpretation of second-order logic as a logic of plurals. This interpretation is used to great effect in the sort of *tu quoque* gambit mentioned above: Boolos argues that we are already committed to second-order logic by our use of the plural idiom in natural language.<sup>23</sup>

The standard account of the intuitive meaning of the first-order existential quantifier (applied to a predicate) is in terms of there being an object in the domain:  $\exists x\phi(x)$  says that there is an object  $x$  which satisfies the open sentence  $\phi$ . Boolos's suggestion is that the (monadic) second-order existential quantifier can be understood precisely analogously by talking of there being *some* objects:  $\exists X\phi(X)$  say that there are some things,  $X$ , which satisfy the open-sentence  $\phi$ .<sup>24</sup> We might talk of

<sup>22</sup> Wittgenstein seems to be making the same criticism when he writes: 'Ramsey's theory [...] makes the mistake that would be made by someone who said that you could use a painting as a mirror as well, even if only for a single posture. If we say this we overlook that what is essential to a mirror is precisely that you can infer from it the posture of a body in front of it, whereas in the case of the painting you have to know that the postures tally before you can construe the picture as a mirror image.' Wittgenstein, *PG* p. 315. <sup>23</sup> Boolos, 'To be is to be a value of a variable'; Boolos, 'Nominalist platonism'. <sup>24</sup> On the plural account, the universal second-order quantifier needs to be introduced as an abbreviation for  $\sim\exists\sim$ .

$X$  as ‘a plurality’; but this is loose talk, and potentially misleading. Second-order logic does not commit us to objects such as ‘pluralities’ (or classes or concepts): all it commits us to is what we are already committed to by our acceptance of first-order quantification, namely the objects of the domain themselves.

Boolos points out that this interpretation of second-order logic is potentially autonomous, in the sense that a model-theoretic explanation of the satisfaction of a sentence (of second-order logic) by a structure can be given on the basis of second-order logic, rather than piggy-backing on set theoretic concepts in the meta-theory.

According to Boolos, all of this shows, first, that accepting second-order logic does not commit us to a domain of classes or sets over which the second-order variables range; and second, that the presence of plural quantificational idioms in natural language (‘There are some horses all of which are faster than Zev’, and so forth) shows that, like Monsieur Jourdain, we have, without knowing it, been talking in second-order logic all along.

If the first, ontological claim is sustained, then a good deal that I have written so far about second-order logic might seem objectionable, or indeed misleading. For I have repeatedly said that the classical understanding of the second-order quantifiers is as ranging over the powerset of the domain of objects. But in fact the difference between conventional set talk and Boolosian plural talk is something of an irrelevance here. As mentioned above, the key issue is not whether, in talking of pluralities of objects, we should or should not treat these as further entities over and above the original objects. The issue is whether we can make sense of the idea of arbitrary pluralities of objects. So even if second-order logic can be shown to have no new ontological commitments, that does not seem to make any easier the task of explaining the meaning of the second-order quantifiers.

The second point is more to our present concern. But suppose we accept that natural language involves primitive plural quantificational idioms. That does not in itself show that in using these idioms we are already committed to *arbitrary* pluralities of the worrying kind. In fact, as we’ll see, predicatively specified pluralities seem to suffice for all of the plausible cases. So there is no quick route from claims about plural idioms in natural language to the conclusion that we have the sort of understanding of pluralities that could underpin a construction of the classical continuum.

### 4.2.5 Arithmetic and the ancestral

Dedekind's categoricity theorem for second-order arithmetic can be combined with the non-categoricity of first-order arithmetic to give another instance of the '*tu quoque*' gambit. The argument runs like this: *of course* we have a grip on the natural numbers (up to isomorphism, at least, i.e., with regard to their mathematical properties); but a first-order understanding couldn't give us that; so evidently we do grasp second-order logic.

As is familiar, what allows arithmetic to be axiomatized categorically in second-order logic is that the ancestral relation can be defined: this lets us form the transitive closure of the successor relation. And a variant of the argument is simply to say that the ancestral is something which we all clearly understand.

The argument fails. Second-order logic is substantially more powerful than is needed to specify the naturals up to isomorphism: there are various weaker logics (weak second-order logic, ancestral logic, logic with a infinity quantifier, and  $\omega$ -logic) all of which allow arithmetic to be axiomatized categorically, and none of which requires a grasp of the domain of all arbitrary subsets of the domain of objects.<sup>25</sup>

These logics all effectively have a notion of finitude 'built in'; this is the source of the increase of expressive power which they have over first-order logic, which cannot express finitude in general. And it is our grasp of the primitive idea of finite iteration, I suggest, that is the source of our knowledge of the natural numbers. (Lavine<sup>26</sup> follows Poincaré and Weyl in arguing that grasp of the concept 'finite' cannot, on pain of circularity, be explained by means of a formal mathematical theory, because the notion of finite is needed to give an account of what a proof (or even a sentence) in such a theory is. This seems at least debatable: surely it is not necessary to have a theory of our syntax, even an implicit one, before we can work with it? Surely it is enough that the sentences and proofs we construct and consider are, as a matter of fact, all finite in the absolute sense?<sup>27</sup>)

What is important to note, though, is that the cluster of notions here — *finite*,

<sup>25</sup> A useful summary discussion is in Shapiro, *Foundations* § 9.1. To speak in terms of the intuitive interpretations: weak second-order logic has class variables which range only over finite subsets of the domain of objects; ancestral logic has a primitive operator which gives, for any relation, the ancestral (i.e. the transitive closure) of that relation; logic with an infinity quantifier is self-explanatory; and  $\omega$ -logic has a primitive binary relation  $<$  which is order-isomorphic to the natural numbers.

<sup>26</sup> Lavine, *Understanding the Infinite* pp. 161–2. <sup>27</sup> See Goldfarb, 'Poincaré against the logicians' for discussion of Poincaré's argument of this sort.

*ancestral, natural number* — are considerably more primitive than the notion of all arbitrary classes; and that this primitiveness is formally reflected in the fact that the logics based on these notions are provably weaker than full second-order logic. (Boolos recounts a question once put to him: ‘Do you mean to say that because I believe that Napoleon was not one of my ancestors, I am committed to such philosophically dubious entities as *classes*?’<sup>28</sup> The answer is of course no. But I suggest, contra Boolos, that our philosophical doubts should focus not on the ontological price of classes, but on the conceptual problems which afflict full second-order logic, regardless of whether it is explained in terms of arbitrary classes or in terms of arbitrary pluralities.)

I am not here proposing that any of these intermediate logics should be deemed ‘logic’ in the honorific sense of universally rationally compelling, or should be taken as an appropriate background system in which to formalize our ordinary mathematical practice. These logics are model-theoretic logics,<sup>29</sup> i.e. they are non-formal. They are characterized by their semantic features, and the semantics is, in each case, strong enough to deliver the categoricity of the natural numbers. By Gödel’s Incompleteness Theorems, semantic entailment which is that strong can never be captured by a formal deductive consequence relation. There is simply no such thing as working (giving a proof) in such a non-formal logic. They are of interest as objects of study in mathematical logic (noting their relative expressive capabilities, for instance); but that is to view them from outside. To the extent that such study is rigorously formalized, it is formalized in first-order terms (in practice, almost always in a standard first-order set theory such as ZF).

However, the moral is that the *tu quoque* argument fails: there is a large and important gap between the power of conceptual resources which could account for our grasp of the natural number structure, and those which could account for a grasp of the full second-order consequence relation. To concede that we do understand arithmetic does not mean also conceding that we understand second-order quantification.

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<sup>28</sup> Boolos, ‘Nominalist platonism’ p. 73.

<sup>29</sup> In the sense of Barwise and Feferman, *Model-theoretic Logics*.

### 4.2.6 The Full Induction Axiom

A variant of the argument considered above cites our commitment to the full second-order axiom of arithmetical induction.<sup>30</sup> If we are committed to that, the argument goes, then we must be committed to full second-order logic.

If we are working in pure number theory (intending, that is, that the domain contain nothing but the natural numbers), the Induction Axiom may be formulated thus:

$$\forall X([X0 \ \& \ \forall y(Xy \rightarrow Xsy)] \rightarrow \forall xXx).$$

In words: every subset of the domain which includes zero and which is successor-closed is the whole domain. The Induction Axiom can indeed be seen as an inductive definition of the natural numbers, and it may seem wholly unobjectionable. It is certainly very hard to see how room could be found for disagreeing with it. Any property of natural numbers which we consider, if it applies to zero, and if it is successor-closed, clearly applies to all of the natural numbers.

The sceptic's objection is not so much to the induction axiom as to the classical interpretation of it: in particular, to the interpretation of the initial quantifier as ranging over every member of the classical powerset of the domain. The sceptic — or the wary — does not think that there might be some collection of natural numbers which would be a *counterexample* to the induction axiom; just that we cannot make sense of the domain of collections of natural numbers, and so that we cannot legitimately quantify over them all. (This is the worry which Gentzen's consistency proofs for arithmetic are designed to soothe.)<sup>31</sup>

In terms of formal mathematical practice, there are predicatively acceptable formal systems which contain the full induction axiom in its form above; for example, the system  $ACA_0$  with which we will be much concerned later.<sup>32</sup> What is not predicatively defensible is to insist upon a classical interpretation of the second-order quantifiers.  $ACA_0$  is predicatively acceptable because it has a predicative class-comprehension axiom scheme. The only sets to which the induction axiom can be applied in  $ACA_0$ -proofs are those which have been given by a previous application of the comprehension scheme; and these are just the extensions of arithmetical

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<sup>30</sup> Kreisel, 'Informal Rigour' is a *locus classicus*. <sup>31</sup> Gentzen, 'Neue Fassung'. <sup>32</sup> See especially Ch. 7. In brief, though,  $ACA_0$  is a system which conservatively extends PA by the addition of predicative classes.

formulae.<sup>33</sup> The point is that commitment to the Induction Axiom does not entail commitment to the full second-order consequence relation.

To sum up the discussion of second-orderism, we have found no good reason crediting ourselves with a logical understanding of the full second-order consequence relation. We have also found some reasons — the dubious notion of arbitrary property that is required, as well as the severing of logic from formal reasoning — for actively thinking that we do not have such an understanding. Second-orderism does not seem to be a promising route for a justification of the classical view of the continuum.

### 4.3 Cantorianism

The Cantorian approach, let's recall, seeks to explain how we can have a classical understanding of the continuum by appealing to a general grasp of the set concept (where sets are conceived as combinatorial collections, rather than as extensions of properties). It is a mathematical project, and aims to develop a formal mathematical theory of the continuum: it therefore cannot be fully second-order (in the terms of §4.1.2 above).

The Cantorian position is committed to developing a formal theory of the natural numbers and the arithmetical continuum. Since the First Incompleteness Theorem, it has been clear that there is no hope of developing such a formal theory which will logically entail all of the truths even among just the  $\Pi_1^0$  sentences of arithmetic. The Cantorian position is that notwithstanding these limits to our *knowledge* of arithmetic (and all the more so of the limits to our knowledge of the continuum), our concept of set is sufficiently determinate that (at least) every sentence of the language of second-order arithmetic does succeed in being either true or false about a determinate part of the world: the natural numbers and the continuum.

This conception — alone, or as part of a broader conception of the set-theoretic universe — justifies the adoption of certain axioms. In particular, and distinctive of Cantorianism, for my purposes here, the conception of the continuum (and perhaps also of further infinite powersets) is taken to justify the axioms of (some system of)

<sup>33</sup> In fact, because the comprehension axiom of  $ACA_0$  is restricted to arithmetical formulae (those without bound class variables), the induction axiom of the system is weaker than the induction scheme: the theory  $ACA$ , which has the full induction scheme for any formula of the language, is stronger than  $ACA_0$ .

formally impredicative set theory.

One notable example of such an axiom is the impredicative class comprehension axiom, in the context of a formal theory of second-order arithmetic such as  $PA_2$ , or of a set-class theory such as MK.<sup>34</sup> Others are the set-theoretic axioms of Separation and Replacement.

#### 4.3.1 Coming to grasp $\mathcal{P}(N)$

The now-standard justification for Cantorian set theory is the so-called iterative conception of the set-theoretic universe.<sup>35</sup> Crudely put, the story is that we start off with some urelements, or indeed (to keep things tidier) nothing at all; at each stage, we consider all of the sets we can form from the objects we already have; these are new objects; and then we keep going. A little less crudely (and taking the pure set route), as a transfinite recursion we have:

$$\begin{aligned} V_0 &= \emptyset \\ V_{\alpha+} &= \mathcal{P}(V_\alpha) \cup V_\alpha \\ V_\beta &= \bigcup_{\alpha < \beta} V_\alpha \text{ for } \beta \text{ a non-zero limit.} \end{aligned}$$

Spelling out  $V$  as built by a transfinite recursion makes it clear that the iterative story relies, effectively, on the ordinals. This suggests that the story is not autonomous, but instead relies on our already knowing about all of the ordinals. However, if we start off only knowing about the ordinals up to  $\omega + \omega$ , say, we will find at  $V_{\omega+2}$  sets which are uncountable von Neumann ordinals: so the set theory itself witnesses larger ordinals than those we started out assuming. And we can use *these* ordinals to continue iterating the hierarchy a very long way indeed.<sup>36</sup>)

To justify standard Cantorian set-theory, there are two particularly important steps we need to justify: the stages indexed by  $\omega$  and by  $\omega + 1$ .  $\omega$  is the first limit-stage, which comes immediately after all of the stages indexed by natural numbers. The

<sup>34</sup>  $PA_2$  is also frequently referred to as  $Z_2$ , after the use in Hilbert and Bernays, *Grundlagen*. I avoid this terminology because of the possible confusion with set theories. (Shapiro, for instance, uses ‘Z2’ to mean (the axioms for) ZFC2, a notational variant of MK.) <sup>35</sup> Standard accounts are to be found in Wang, ‘The concept of set’, Shoenfield, ‘Axioms of set theory’. <sup>36</sup> In fact, by a trick due to Scott, ‘Axiomatizing set theory’, the construction can be performed without invoking any ordinals at all. Potter, *Set Theory and its Philosophy* gives an introductory account of set theory based on Scott’s idea.

legitimacy of this step is considered problematic by finitists: it is a step with no immediately preceding step, and so the construction of  $V_\omega$  is a supertask.

It is stage  $\omega + 1$  which is relevant to our concern here, the justification of the classical continuum. Indeed, it makes no difference for our purposes if, instead of the hierarchy of  $V$ , the pure sets, as is standardly considered, we consider instead starting with the natural numbers as urelements; in this case, our problem comes at the first level. Either way, the problematic step is the first infinite powerset we are to take, and therefore the first point at which we need to consider *arbitrary* subsets of what we have so far.

In looking for an intuitive route to the concept of the collection of every arbitrary subset of the naturals, a natural place to start is by considering a supertask that can give us one such arbitrary subset. One such supertask would be to run through the natural numbers one by one, deciding, of each one, whether or not it is to be included by some random process such as tossing a coin.

However, an infinite sequence with a random choice at each step is something that we might well scruple at; and Russell did scruple at it, in his discussion of the Axiom of Choice. Consider the famous millionaire who has  $\aleph_0$  pairs of boots and  $\aleph_0$  pairs of socks. How many socks does he have?

The *pairs* are given as forming an  $\aleph_0$ , and therefore as the field of a progression. Within each pair, take the left boot first and the right second, keeping the order of the pair unchanged; in this way we obtain a progression of all the boots. But with the socks we shall have to choose arbitrarily, with each pair, which to put first; and an infinite number of arbitrary choices is an impossibility.<sup>37</sup>

The thought is that the only grip finite creatures like us can get on the infinite is by means of some sort of a rule. And Russell is by no means alone in subscribing to this: Poincaré and Weyl certainly agreed. The acceptance of an arbitrary infinite collection such as is produced by such a supertask is a — perhaps *the* — mark of the acceptance of the infinite as actual, rather than merely potential; and it is very difficult to see what could be said in favour of the actual infinite which might persuade anyone with doubts.

But there is a larger obstacle to overcome in order to justify the stage  $\omega + 1$ . Even if we grant the intelligibility of such a supertask of infinite arbitrary selection, and

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<sup>37</sup> Russell, *Introduction to Mathematical Philosophy* p. 126.

hence the acceptability of *one* such arbitrary set of naturals, we still do not get a grip on the domain of *all* sets of natural numbers. That would come if we were to credit ourselves with a conception of *all* possible outcomes of the supertask; but here the ‘all’ is precisely the problematic concept which wanted explaining. Even repeating the supertask countably many times (not counting any duplicates!) is of no help: as the constructive part of Cantor’s Theorem shows, a countable collection of subsets of  $\mathbf{N}$  is not closed under diagonalization: so no such countable collection can be the classical continuum.

We are not forced to countenance an uncountable infinity as actual, even if we are persuaded by the consideration of step-by-step supertasks to accept the countable infinite as such.

It is worth noting that even hard-nosed Cantorians must accept that there is a somewhat mysterious character to the powerset of the natural numbers: in technical terms, it is non-absolute, meaning that it cannot be specified except by means of quantification over the whole set-theoretic universe. The undeniable indefinite extensibility or open-endedness of the universe of sets is therefore reflected in the unlimited richness of the continuum.<sup>38</sup>

#### 4.3.2 $V = L$

There are, as we have just seen, serious problems with the supposition that we have a clear conception of the powerset operation as applied to infinite sets. It might be thought that Gödel’s Axiom of Constructibility ( $V = L$ ) offers an alternative justification of impredicative set theory. Gödel proved that the constructible universe,  $L$ , is a model of the axioms of ZF. And so it would seem that a grasp of  $L$  is enough for the Cantorian’s purposes.

The real powerset operation is arguably objectionable because it is supposed to contain *all* of the subsets of a given set  $x$ , and because the axiom scheme of Separation (which specifies which subsets there are) is impredicative, in that its instances assert the existence of subsets of  $x$  which are defined by means of formulae involving quantification over the whole set-theoretic universe, including  $x$ .

In contrast, the constructible hierarchy,  $L$ , is built by a sort of constructive analogue to the powerset operation. This operation,  $\mu$ , takes a set  $x$  to the set of

<sup>38</sup> See Hallett, ‘Putnam and the Skolem paradox’ for discussion of the interrelation between impredicativity, non-absoluteness and unrestricted quantification in set theory.

all of those subsets of  $x$  which can be defined by means of formulae of which the parameters and quantifiers are restricted to  $x$ .

$$L_0 = \emptyset$$

$$L_{\alpha^+} = \mu(L_\alpha) \cup L_\alpha$$

$$L_\beta = \bigcup_{\alpha < \beta} L_\alpha \text{ for } \beta \text{ a non-zero limit.}$$

Gödel showed that the constructible hierarchy  $L$ , the structure produced by this transfinite recursion, is an ‘inner model’ of the axioms of ZF, including the Powerset axiom and the (full) Separation scheme.

We write  $\Xi(y)$  to say that  $y$  is constructible, i.e. is in  $L$ . To show that the Separation scheme holds, what needs to be established is that for any formula  $\Phi(y)$ , there is an ordinal  $\rho$  for which  $\Phi^{L_\rho}(y) \leftrightarrow \Phi^\Xi(y)$ : that is, that there is an ordinal level  $\rho$  of the constructible hierarchy which reflects  $\Phi$  in the sense that the restriction of the quantifiers and parameters in  $\Phi$  to that level is equivalent to the restriction of  $\Phi$  to the whole of  $L$ .<sup>39</sup>

This requires that we go far enough in building the constructible hierarchy: we need to make use of the uncountable ordinals from our background set theory. Gödel himself put the matter this way: with the constructible universe,

all impredicativities are reduced to one special kind, namely the existence of certain large ordinal numbers (or well-ordered sets) and the validity of recursive reasoning for them.<sup>40</sup>

The construction of an ‘inner model’ is not autonomous: we need to have accepted the axioms of ZF before we can construct  $L$ , a model of those axioms. The fact that the method of building  $L$  is so ‘constructive’ means that  $L$  can be shown to be well-ordered, which proves the consistency of the Axiom of Choice with the other axioms of ZF; and in fact  $L$  is so well-behaved and non-arbitrary that the Continuum Hypothesis also holds in it. But the inner model is of no use if we want to justify the axioms of ZF themselves. In the absence of an independent route to the uncountable ordinals which are needed in the construction, the constructible hierarchy is no help at getting the uninitiated (or the predicativist sceptic) to a classical conception of the continuum.

<sup>39</sup> See Devlin, *Constructibility* for details. <sup>40</sup> Gödel, ‘Russell’s mathematical logic [1944]’ p. 147.

### 4.3.3 Countable models of set theory

A natural first thought is that Cantorianism is doomed to failure. Any formal theory of the continuum will be inadequate because of the Downward Löwenheim-Skolem Theorem: such a first-order theory will have a countable model, and so is a miserable failure as a theory of the continuum.

I will argue that the upshot of this is broadly correct, though the details are a little more complicated. The mere existence of unintended models for a theory does not, I claim, show that theory to be inadequate as a description of the intended structure; but, in general, the *smallest* model of a theory cannot be said in any real sense to be unintended as a model of that theory. It is, on the contrary, precisely what the axioms explicitly guarantee to exist, and no more. A countable model will of course be unintended by a Cantorian, in that it is unfaithful to the informal understanding of the continuum that motivated the axioms in the first place: but for the Cantorian to argue thus is for her to give up on the Cantorian project of justifying a mathematical theory of the continuum, and instead, as we will now see, to retreat into the obscurity of second-orderism.

To see a countable model of ZF set theory as being unintended is to bring to bear a conception which goes well beyond the axioms. The conception most often appealed to here is of course second-orderism: the informal notion of unintended model is replaced by the notion of ‘non-standard model’ as formalized by Montague. Montague defined a non-standard model of a first-order theory axiomatized by (a finite number of axioms and) a single axiom-scheme to be a structure which does not satisfy (on the standard semantics, of course) the corresponding second-order theory (that is, the theory which has a single  $\Pi_1^1$  axiom instead of the first-order scheme).<sup>41</sup>

Such an appeal to the second-order consequence relation to fix the intended interpretation may well be coupled with a continued use of first-order set theory, perhaps because of the practical benefits of a compact and complete logic; but the underlying philosophical justification makes it clearly a species of second-orderism; as such, it is subject to my criticisms in §4.1 above.

<sup>41</sup> Montague, ‘Set theory and higher-order logic’. Familiar examples of such theories are PA and  $PA_2$ ; ZF and  $ZF_2$ ; and for analysis, the first-order axioms for an ordered field together with a scheme for Dedekind-completeness, and the corresponding theory with the second-order completeness axiom. See Shapiro, *Foundations* p. 110 for further details.

If she is not to retreat into second-orderism, the Cantorian must take the view that our knowledge of the powerset operation is not logical knowledge, but is nonetheless clear and direct: rather than flowing *from* the axioms, our pre-formal understanding of the powerset operation is what the axioms try (imperfectly) to express.

It is familiar that no formal theory with infinite models can be categorical. (To repeat: By ‘formal’, I mean that we are restricting ourselves here to first-order, axiomatizable theories.) The Löwenheim-Skolem theorem tells us that there will be models of every infinite cardinality. And so in particular, there are countable models of set theory.

But the familiarity of all this should not blind us to the unsatisfactory situation with regard to set theory in particular. Mathematicians normally work as if they had a firm grasp of the concepts of countability and uncountability, but this of course derives from their understanding of set theory. It is, after all, in a background set theory that model theory is done, and that results such as the Löwenheim-Skolem theorem are proved.

In response to such worries, Cantorians tend to fall back on the ‘*tu quoque*’ gambit. An analogy is drawn between the case of set theory and the case of arithmetic, and it is suggested that doubts which non-standard models may raise about the former apply just as well to the latter, where they are clearly unacceptable; and so we should dismiss such doubts altogether. I would suggest that this is rather too quick. For one thing, the unacceptability of such doubts about arithmetic is a fact which cuts both ways. The anti-Cantorian may well suggest that it is simply much more plausible to table-bangingly claim that we have a clear intuition of the natural number sequence (which formal systems attempt to capture, though of course incompletely) than it is to claim that we have such an intuition of the continuum. (Not to mention the rest of the set-theoretic hierarchy.)

But more substantially, there is also a significant limitation to the analogy between the cases. There is an important difference in the nature of the non-standard models: all models of arithmetic include the natural numbers; non-standard models of arithmetic have additional elements. On the other hand, models of set theory do not, in general, have full powersets; it is only standard models of set theory which do.

The significance of this contrast becomes clear when we remind ourselves that such models are infinite mathematical objects; as such, they are only encountered or

dealt with by us indirectly, mediated by description. And a description or conception of a non-standard model of arithmetic requires a prior grasp of the standard model of arithmetic, the real natural numbers: one has to say to oneself, there's 0, 1, 2 and so on, and there are other elements, not in that series. A non-standard model of arithmetic is not something that one could get hold of by mistake when being introduced to the infinite by means of the first-order Peano Axioms (or indeed of the axioms of induction-free arithmetic): either one remains a strict finitist and so refuses to accept the axioms, or one comes to an understanding of the natural number series.<sup>42</sup> But in the case of set theory, a description of a non-standard model need not require a grasp of the problematic concept of the full powerset. Non-standard powersets fall short of the real thing; and so the Cantorian position gives rise to the sceptical worry that in aiming for the real continuum, we might not have made it all of the way; or indeed that different people might have got hold of different ersatzes.

The upshot of this is that a formal system of set theory is of no help in giving us a conception of the continuum. The axioms may be motivated by such a conception: but they are not adequate to express it.

But does all of this leave open the possibility of combining standard (impredicative) set theory with the view that there are no real uncountable infinities? Wright has suggested it does. I will argue not.

#### 4.3.4 Wright's Skolemism

Wright<sup>43</sup> has sketched a line of argument based on the Löwenheim-Skolem Theorem to the effect that a formally impredicative set theory such as ZF can be coherently adopted without thereby incurring a commitment to the classical continuum, or indeed to any uncountable sets at all. While the philosophical position advanced in 'Skolem and the skeptic' is certainly not a classical view of the continuum, I discuss it here because it is intended as a justification of a classical mathematical theory of the continuum.<sup>44</sup> Examining Wright's criticisms of Cantorianism will, I hope, bring out more clearly the predicativist alternative.

Wright's brand of Skolemism is to be sharply distinguished from traditional

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<sup>42</sup> This point is made forcefully by Dummett in 'Gödel's theorem'. <sup>43</sup> 'Skolem and the skeptic'.

<sup>44</sup> It is unclear whether or not Wright's position is classical in my official sense of vindicating bivalence for  $L_{2A}$ , but it would seem not. The theorems of ZF do not settle the truth-values of all of the first-order sentences of arithmetic, never mind all of those of analysis (second-order arithmetic).

Skolemism, the set-theoretic relativism about countability allegedly propounded by Skolem himself. Skolemist relativism was supposed to threaten the coherence of the idea of uncountability by suggesting that it might always be possible to find a perspective from which any given, supposedly uncountable set can be seen in fact to be countable. This line of argument is now generally agreed to be confused. Why? Well, the usual thought these days<sup>45</sup> is, roughly, that not everything is up for grabs; that is, not everything is a candidate for reinterpretation; or at least, not everything all at the same time. From whatever perspective is our current resting place, if we accept the axioms of ZF, then we are committed to uncountable sets. We may always be able to reflect on our previous mathematical practice; this will involve a move to a new perspective, from which, perhaps, we may look back at the set we previously called ‘the continuum’, and see that this set can in fact be put into bijective correspondence with the natural numbers: but this possibility does nothing to impugn the coherence of our current background notion of uncountability.

Wright’s form of Skolemism is based on a different line of thought from that of the traditional Skolemist, and is supposed to lead to a somewhat different conclusion. Wright’s conclusion is that the intended interpretation of ZF (an uncountable universe of sets), while perhaps not internally incoherent, is at least not *captured* — or not *forced upon us* — by the axioms; it is open to us to accept ZF and yet remain countabilist.

In some passages, Wright seems to be arguing merely that the axioms of ZF are not adequate as an introduction to the uncountable universe of the intended model; and I think that this point is well taken. If the ZF axioms are to be taken seriously and accepted on their own merits (rather than taken in a formalistic spirit), then they stand in need of justification. Cantorianism is one attempt at such a justification. Wright gives a different justificatory story, which does not involve commitment to uncountable sets. I have already discussed the difficulties facing the Cantorian attempt; and I think that Wright’s attempt faces equally serious difficulties.

The Cantorian claims to have a conception of the iterative hierarchy — an uncountable universe of sets — and argues that the ZF axioms are true of this universe. (I assume for simplicity that the axioms are presented in a form with the quantifiers which appear explicitly restricted so as to range only over the sets. The Cantorian believes that all of the sets are in the iterative hierarchy, and so holds the

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<sup>45</sup> As expressed, for example in Benacerraf, ‘Skolem and the skeptic’.

axioms true *simpliciter*.) Wright attempts to describe a countable universe of sets, of which the ZF axioms are true.

It is the countable submodel form — henceforth, SMT — of the Löwenheim–Skolem theorem that Wright appeals to: this asserts that any uncountable model of a first-order theory has a countable submodel. If we apply this theorem to the case of a standard (transitive, uncountable, set-) model of ZF, SMT gives us a countable submodel with a domain of real sets such that all of the members of the sets in the domain are also sets in the domain, and that the relation-symbol ‘ $\in$ ’ is interpreted in the submodel as the real membership relation.<sup>46</sup>

SMT tells us that, if ZF has an uncountable set model, then there are countable such models. (Note that the hypothesis is significantly stronger than the mere consistency of the axioms: the hypothesis requires the axioms to be *sound*, that is, true of an (uncountable) universe of sets.) What Wright needs to show is that such a countable model can coherently be seen as a universe of set theory, i.e., as all of the sets that there are. Only this would show that the axioms of ZF can be justified by a story on which the universe of sets is countable.

Wright claims that there is a perspective from which the Diagonal Argument serves to show merely ‘that there is no recursive enumeration of all recursively enumerable infinite decimals.’<sup>47</sup> It seems that the countable universe which Wright is suggesting as a possible model for ZF consists just of effectively enumerable sets. He regards the restriction to such sets as something that is not ruled out by the explicit content of the axioms of ZF — though this is surely wrong, as I shall show — and can therefore only be ruled out by informal explanation. Wright claims that in order to make sure that a trainee set theorist gets hold of a standard interpretation of set theory, complete with uncountable sets, the Cantorian must give such an informal explanation; and Wright then criticizes such explanations as obscure and inadequate to the propaedeutic task.

As Wright says:

[...] before the Diagonal Argument [...] can lead us to a conception of the intended range of the individual variables in set theory which will allow us to regard any countable set model as a non-standard truncation, we need to waive the restrictions [to an effective listing of effectively

<sup>46</sup> The proof of SMT requires the axiom of Choice; I concede this to Wright for the sake of argument.

<sup>47</sup> Wright, ‘Skolem and the skeptic’ p. 134.

computable infinite decimals]. And in order to understand the waiver, we need to grasp the notion of a *non-effectively enumerable denumerably infinite subset* of natural numbers.<sup>48</sup>

Here, Wright pinpoints the key problem in understanding Cantorian set theory: How are we to make sense of the idea of wholly arbitrary infinite sets? And he is quite correct that the Diagonal Argument does not help us in this task: whatever countable family  $\mathcal{A} = \{A_i \mid i \in \mathbb{N}\}$  of sets of natural numbers we start with, the diagonal method gives us a new set of natural numbers, not in  $\mathcal{A}$ : but it is of course not an *arbitrary* set: it has a perfectly good finite definition, in terms of  $\mathcal{A}$ .

Wright's mistake is his suggestion that there is a coherent standpoint on which one can endorse ZF and yet remain a countabilist.

Wright focuses his attention on how to interpret, in a countable universe, Cantor's Diagonal Argument, as it is formalized in ZF. The proof shows that  $2^{\mathbb{N}}$ , the set of functions from the natural numbers to the set  $\{0, 1\}$ , is not equinumerous (cannot be put into bijective correspondence) with the natural numbers. To be accurate, Wright actually first considers the proof of the uncountability of the infinite decimal fractions; but as  $2^{\mathbb{N}}$  is obviously equinumerous with the (infinite decimal) fractions between 0 and 1, I'll concentrate on the simpler case,  $2^{\mathbb{N}}$ .

The problem for Wright's approach to the Diagonal Argument is that whatever countable family of sets of natural numbers we started off believing in, the argument gives us a new one. For the Diagonal Argument shows, for any countable set  $A$  of sets of natural numbers, that  $A$  is not the powerset of the naturals.

For instance, if we start off as 'recursivists', that is, believing only in recursive sets, then what the Diagonal Argument shows us is that any recursive set  $A$  of recursive sets of natural numbers is incomplete: there is a recursive set of naturals not in  $A$ , and so  $A$  is not the full powerset. If we are committed to the powerset axiom, then the Diagonal Argument forces us to move beyond recursivism: to accept that the powerset of the naturals is not a recursive set.

Wright's countabilist standpoint was supposed to be one on which the ZF axioms and *only* sets of a limited sort were countenanced. In fact, there is no such stable conception: the Diagonal Argument genuinely is a principle of extension no matter what countable range of subsets of the naturals we may have accepted: that is why the set of subsets of  $\mathbb{N}$  — or equivalently the set of reals — is considered by predicativists

<sup>48</sup> Wright, 'Skolem and the skeptic' p. 134.

to be an ‘open-ended’ (indefinitely extensible) domain. The alternative is to accept the Cantorian claim that there is a domain of subsets of  $\mathbf{N}$  which is closed under the diagonalization: and then the Diagonal Argument goes to show that this domain must be uncountable.

If we assume, for the sake of argument, the existence of an uncountable standard model of the axioms, and enough Choice to prove SMT, then SMT tells us that there will be a countable  $\mathcal{M}$  which satisfies the axioms (when the quantifiers in the axioms are restricted to the domain  $|\mathcal{M}|$ ). Let’s also suppose that a trainee set theorist somehow gets hold of this model. But this does not give Wright what he wants: in  $\mathcal{M}$ , the set, call it  $\alpha$ , which is the referent of ‘ $\mathcal{P}(\mathbf{N})$ ’ satisfies (the formal version of) the predicate ‘is uncountable’. So Wright faces a dilemma: either the novice sees  $\alpha$  as really being countable, or she does not.

If she does, the function that counts  $\alpha$  (the bijection between  $\alpha$  and the natural numbers) gives rise to a diagonal set of natural numbers which is not itself in  $\alpha$ , or indeed in the model  $\mathcal{M}$ . The novice sees  $\mathcal{M}$  merely as a part of the set-theoretic universe. The fact that  $\mathcal{M}$  satisfies the ZF axioms does not mean that those axioms are true.

On the other horn, if she believes that the domain of  $|\mathcal{M}|$  is the real, full, uncountable universe of sets, then the standpoint Wright offers is not a countabilist one: the novice will sincerely assert that  $\alpha$  is uncountable. Of course, because of the way in which we set up the example, *we* see her as being wrong about this; but that is because we are on the first horn, so to speak, looking down at  $\mathcal{M}$  from outside.

The part of Cantor’s Diagonal Argument which Wright discusses is the constructively acceptable part: this part of the argument is a principle of extension, which when applied to a countable collection  $x_0, x_1, \dots$  of sets of naturals, gives us a new such set of naturals,  $\{n \mid n \notin x_n\}$ , which is not in the collection. This part of the argument is irreproachable, and does not rely on any questionable assumption of ZF. (The corresponding result in second-order arithmetic can in fact be proved in very weak systems, which have only a quantifier-free comprehension axiom.<sup>49</sup>)

The part of Cantor’s argument which the sceptic of uncountability should object

<sup>49</sup> The second-order form says that for any enumeration  $A_0, A_1, \dots$  of classes of natural numbers, there is a class which is not  $A_i$  for any  $i \in \mathbf{N}$ . A sequence of classes of naturals is implemented in second-order arithmetic as a class  $B$  of pairs  $\{\langle m, n \rangle \mid m \in A_n\}$ . The proof is the usual diagonal construction: the class  $\{n \mid n \notin A_n\}$  can be proved to exist, but obviously cannot be in the enumeration. See Feferman, ‘What rests on what?’ pp. 198–199 for discussion.

to is the other part: the assumption that there is a collection of *all* of the subsets of the natural numbers. This is, in effect, the Powerset Axiom: it is this premise that takes us from the constructive part of Cantor's argument to the conclusion that there is a set of all of the subsets of  $\mathbb{N}$ , which must be an uncountably infinite set.

To sum up, Wright has given us good reasons not to be a Cantorian, but he has not given us what we were promised: a coherent conception on which we can remain countabilist, and yet accept the axioms of ZF. Rather than leave matters there, though, it would be nice to find a diagnosis of what has gone wrong. I suggest that the root cause is (to borrow Whitehead's phrase) the 'fallacy of misplaced concreteness' for the case of models of set theory

Wright argues that:

if the intuitive concept of set is indeed satisfactorily explicable — and how else could it be communicable? — the explanation has to be, at least in large part, informal; and it will not suffice informally to explain the set membership relation and then to stipulate e.g. that the ZF-axioms are a correct digest of the principles of set existence. If the Cantorian wishes it to follow from his explanation that there are all of the sets which, intuitively, he believes that there are, he has to do something more. What?

He has to say something which entails that there are uncountably many sets. And that is not the same thing as stipulating an axiomatic framework in which Cantor's theorem may be proved, since the difficulty is exactly that if his preferred set theory can take its intended interpretation at all, it can take a *set-theoretic* interpretation under which Cantor's theorem cannot be interpreted as a result about uncountability.<sup>50</sup>

The problem here is precisely the one considered in §4.3.3 above. The countable models which the Löwenheim–Skolem theorem gives us are infinite mathematical objects; their availability to a mathematician depends on what infinite objects she believes exist; these beliefs will in turn depend on what mathematical theories she accepts. It is absurd to think of models as if they were concrete: as if a countable submodel is something which a trainee might somehow get hold of or stumble over, and then recognize to be correctly described by the axioms she has just been given.

<sup>50</sup> Wright, 'Skolem and the skeptic' pp. 131–2.

The Cantorian believes ZF to be sound; that is, true of the real, uncountable universe of sets. So SMT tells her that there exist countable transitive set models of the axioms. But it is not clear what, if anything, SMT tells the trainee. The trainee might well doubt whether the axioms are true of anything at all, or even whether they are consistent; and in that case the theorem tells her nothing.

It is, I suppose *possible* that a trainee could somehow get hold of an autonomous description of the contents and structure of a countable transitive set model of ZF. ('Autonomous' here means a description which is not parasitic on one of the Cantorian's intended models; if it is parasitic in this way, then such a structure is accessible to a trainee only if she first accepts that there is an uncountable universe of sets — that is, only if she first becomes a Cantorian.) This would not be a vindication of Wright's claim: the dilemma mentioned above still stands. But such a description would also constitute a consistency proof of ZF. The prospects for such a thing seem dim, to say the least: the second Incompleteness Theorem tells us that such a proof could not be given in any theory mutually interpretable with ZF; or of course in any weaker system.

One route to such a proof would be arithmetical insight. Indeed, in a sense, this is the only route: any countable model is, modulo coding, a model in the natural numbers. On such a model, the axioms of ZF would be seen to be (not merely consistent, but also) true of the natural numbers under this interpretation. All of this, we are supposing, is for reasons which are accessible to the novice purely on the basis of reflection on the natural numbers. This seems a tall order, to put it mildly.<sup>51</sup>

## 4.4 Intuitively based classicism

### 4.4.1 Cantor again

As Lavine argues at length, and as has been mentioned in Ch. 2.2 above, it seems that the early views of Cantor are in some respects importantly different from those

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<sup>51</sup> Harvey Friedman, building on the famous Paris–Harrington result, has tried to give arithmetical reasons for believing in higher set theory: he has presented various example of combinatorial principles about the natural numbers, but which can be proved only in strong systems of set theory. As I understand it, though, the principles are consequences of the theories, but not conversely: that is, the evidence provided for the consistency of ZF is merely 'regressive' in character. It is of course highly questionable whether the combinatorial principles can be recognized as true by means other than the set theory used to prove them.

views of modern set theorists which I have been discussing under the name of ‘Cantorianism.’<sup>52</sup>

In his pre-1891 writings on set theory, Cantor seems to have simply assumed that the continuum of real numbers was a set.<sup>53</sup> In 1891 came an important change in Cantor’s thinking,<sup>54</sup> marked by the invention of the Diagonal Argument, which depends on the idea of characteristic functions and of power sets. The assumption changed from the (unjustified) postulation of a set of real numbers to the assertion of a new axiom — effectively, the powerset axiom: for every set, there is a set of all of its subsets.<sup>55</sup>

The previous section, §4.3, has discussed and criticised the attempt to justify a classical conception of the continuum on the basis of a set theory with a powerset axiom; it remains to assess Cantor’s first thought, and to consider the possibility of taking the classical continuum as primitively given to us in intuition.

#### 4.4.2 Postulating or intuiting the classical continuum

Why did Cantor think that it was legitimate to postulate the continuum as a set? Lavine attributes this to a fundamental (though very vague) general belief, the Domain Principle: this states that every domain of a mathematical variable is a set.<sup>56</sup>

The obvious point is that the real numbers, and real-valued functions, were the stock-in-trade of nineteenth-century mathematicians. Cantor was well aware that not all domains could consistently be seen as sets: those which could not, he called the Absolutely Infinite (and he associated them with the unknowability of the divine in his theological reflections). But it seems to have been almost unthinkable to Cantor that the real numbers could be such.

Cantor was not alone, of course. Poincaré famously viewed transfinite set theory as a disease, and rejected impredicative set definitions; but he endorsed the least upper bound principle for real numbers, on the grounds that the reals were an unproblematic pre-existent mathematical object. Poincaré viewed most infinite

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<sup>52</sup> Lavine, *Understanding the Infinite*. <sup>53</sup> At one point, Cantor did in fact worry that perhaps the reals could not be well-ordered: see Hallett, *Cantorian set theory* pp. 42, 73, 76–77. <sup>54</sup> Cantor, ‘Über eine elementare Frage’. See Lavine, *Understanding the Infinite* pp. 93–98 for discussion. <sup>55</sup> ‘Effectively’ because, to be precise, Cantor considered not subsets of a given set, but the set of functions from a set to a pair of arbitrarily chosen items. See Cantor, ‘Über eine elementare Frage’. <sup>56</sup> Lavine, *Understanding the Infinite* p. 90; Hallett, *Cantorian set theory* attributes to Cantor a similar Domain Principle, and for similar reasons.

sets as potential, as being constructed, and it is this that makes impredicative set specification illegitimate: to construct a set which has been specified impredicatively is a viciously circular task, which requires us to have finished before we can begin. But the real numbers, on Poincaré's view, are there to begin with, given to us by means of intuition. As such, specifying a real as the least upper bound of some others is as harmless as pointing out the tallest man in the room.

Clark takes a dim view of Poincaré's postulation of a set of the reals:

Poincaré is trying to have his cake and eat it. He is prepared to recognize the real line and the plane as sets or collections, at least as entirely legitimate mathematical objects, but he is not prepared to think of those collections as obtained from the natural numbers in any way. Thus he regards the plane and line as independent mathematical objects and our knowledge of them as given synthetic a priori independently of any knowledge of the natural numbers. [...] it is certainly worth noting that this sort of semi-intuitionism as it has come to be called does leave the impression of theft over honest toil.<sup>57</sup>

What is unclear is whether there is honest toil which can be done to justify the classical view of the continuum; in this chapter I have attempted to show that there is not.

Insofar as the Domain Principle is compelling, it is so because of its conservatism: it licenses us to take as a set any domain which mathematicians have traditionally been happy to deal with by considering functions over. Traditional (early nineteenth-century) analysis deals with functions on the real numbers; so the Domain Principle tells us that the real numbers are a set. But what this glosses over is the question of just what real numbers there are. As we saw above, discussing Weston's assimilation of Bishop's continuum to the classicist's, the fact that everyone agrees that every Cauchy sequence of rationals has a limit in the reals (or that every lower Dedekind cut of the rationals has a least upper bound in the reals) does not show that everyone's continuum is the same. That would only follow if everyone agreed on what Cauchy sequences or sets of rationals there are. But that is more or less precisely the question at issue: modulo coding, it comes down to the question: What sets of natural numbers are there? And that is of course the same as the question: How big is the continuum?

<sup>57</sup> Clark, 'Logicism, the continuum and anti-realism' pp. 131–2.

The argument from tradition for the definiteness (and hence sethood) of the continuum overlooks the fact that traditional mathematics is actually *countable*, in a certain extended sense. As Simpson puts it:

The distinction between set-theoretic and ordinary mathematics corresponds roughly to the distinction between ‘uncountable mathematics’ and ‘countable mathematics’. This formulation is valid if we stipulate that ‘countable mathematics’ includes the study of possibly uncountable complete separable metric spaces. (A metric space is said to be separable if it has a countable dense subset.) Thus for instance the study of continuous functions of a real variable is certainly part of ordinary mathematics, even though it involves an uncountable algebraic structure, namely the real number system. The point is that in ordinary mathematics, the real line partakes of countability since it is always viewed as a separable metric space, never as being endowed with the discrete topology.<sup>58</sup>

As Simpson’s remarks suggest, and as I argued in Ch. 2, the modern conception of the continuum is a recent one: it is not older than set theory. (My choice of the word ‘classicism’ for it is perhaps somewhat unfortunate.) While it certainly has been accepted as a natural development, and indeed as the rational reconstruction or explication of the earlier conceptions of the continuum, it has not gone wholly unchallenged. And of course those mathematicians who challenged it tended to be just those mathematicians who were also unhappy with Cantorian set theory as a justificatory basis for the continuum. It follows that the intuitive route to the classical continuum that I am discussing here was taken by hardly anyone: the earlier Cantor is the only clear example I am aware of.

Intuition is of course an extremely popular justification for non-classical accounts of the continuum: the obvious examples are the Brouwerian intuitionists, but the semi-intuitionists should also be counted, and perhaps also other constructivists of various stripes. In the case of Poincaré, his commitment to the least upper bound principle does not suffice to make his conception of the continuum classical, unless he also accepts all of the sets of real numbers that the classicist does: but Poincaré did not accept all of those sets — he rejected impredicatively defined sets.

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<sup>58</sup> Simpson, *Subsystems of Second Order Arithmetic* pp. 1–2.

And intuition was also the obvious justification for those who considered the continuum before the development of set theory. Here matters become difficult: it would be very difficult to substantiate the claim that Kant (to take an obvious example) had a view of geometrical intuition which has as its best rational reconstruction what I have called classicism about the continuum. I suspect that in fact Kant's views are better represented as being anti-classicist, but this would not be much easier to substantiate.

#### 4.4.3 Arithmetic and geometric continua

I have taken the assumption that sentences of second-order arithmetic all have determinate truth-values to be constitutive of classicism about the continuum. So we need to examine how this arithmetical conception relates to the assumption of the definiteness of the (classical) continuum based on analytic or geometrical intuitions.

Classical analysis can be formulated in second-order arithmetic, as has been long been familiar from the work of Hilbert and Bernays.<sup>59</sup> One careful recent development of classical analysis in the language  $L_{2A}$  is Simpson's:<sup>60</sup> Simpson represents real numbers as sets of naturals, using several steps of coding.<sup>61</sup> The equivalence between analysis and second-order arithmetic is therefore a well-established mathematical result. In order to assess the claim that our analytic (or geometrical) intuitions can justify classicism about the reals, what needs closer inspection is those intuitions.

The crucial intuition of the continuum is gaplessness, and this is made precise in the notion of the completeness of the field of real numbers, i.e. its obeying the least upper bound principle. To take an example from a textbook:

The real numbers are complete in the sense that there are no 'holes' in the real line. Informally, if there were a hole in the real line (see Figure 5.1), the set of numbers to the left of the hole would have no least upper

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<sup>59</sup> Hilbert and Bernays, *Grundlagen*. <sup>60</sup> Simpson, *Subsystems of Second Order Arithmetic*. <sup>61</sup> The details of the coding may seem rather arbitrary, but some of them turn out to be of importance to the Reverse Mathematics project being pursued. The natural way to develop analysis in  $ACA_0$  (as presented, *ibid.*, § I.4) is different from his official account (*ibid.*, § I.8) which is less logically complex, and is therefore suitable for use in the weaker system  $RCA_0$  (*ibid.*, § I.8). For example, while in  $ACA_0$  real numbers would most naturally be represented as Cauchy sequences of rationals, in  $RCA_0$  the reals need to be represented by sequences of rationals which satisfy a more restrictive convergence criterion.

bound.<sup>62</sup>

So our attention turns to the least upper bound principle, which asserts that every non-empty set of real numbers which is bounded above has a least upper bound. So to characterize the real numbers, we need to know just what sets of real numbers there are.

A typical modern approach<sup>63</sup> is simply to *define* the real numbers to be some appropriate structure (standardly  $\langle A, 0_A, 1_A, +_A, \cdot_A, \leq_A \rangle$ , with  $A$  as the domain, the real numbers, and the other items corresponding to the non-logical vocabulary of the language of analysis) which satisfies (the first-order axioms for an ordered field and) the least upper bound principle. Then we stand in need of assurance that such a structure exists. Standard set theory of course gives that assurance; but to appeal to set theory here is simply Cantorianism. The intuitionist proposal we are considering now is that we have geometric intuition of one such structure. However, the crucial point is that for this approach to be an alternative to Cantorianism, we need to formulate the least upper bound principle without smuggling in the Cantorian notion of set.

We could take it as a second-order sentence of the language of analysis (with first-order variables ranging over the reals, and second-order variables ranging over sets of reals),

$$\begin{aligned} & \forall X \{ \exists x \forall y (Xy \rightarrow y \leq x) \\ & \rightarrow \exists x [ \forall y (Xy \rightarrow y \leq x) \ \& \ \forall z (\forall y (Xy \rightarrow y \leq z) \rightarrow x \leq z) ] \}; \end{aligned}$$

or as a first-order schema,

$$\begin{aligned} & \exists x \forall y (A(y) \rightarrow y \leq x) \\ & \rightarrow \exists x [ \forall y (A(y) \rightarrow y \leq x) \ \& \ \forall z (\forall y (A(y) \rightarrow y \leq z) \rightarrow x \leq z) ]. \end{aligned}$$

The second route can quickly be ruled out as giving what the classicist wants: the resulting theory is in fact rather trivial: it is a *complete* theory, as Tarski showed; and its most natural countable model is the set of algebraic numbers (with the usual operations and relations on them). But the first alternative seems to throw us back either on Cantorianism (if we try to explain the set quantification in terms of some

<sup>62</sup> Johnsonbaugh and Pfaffenberger, *Foundations of Mathematical Analysis* p. 16. Figure 5.1 helpfully shows the reader a line with a hole in it. <sup>63</sup> And the one followed by Johnsonbaugh and Pfaffenberger.

general theory of sets), or on second-orderism (if we refuse to, and instead say that it is a logical matter).

There is, though, another way to understand the least upper bound principle for sets. Rather than directly appealing to a theory to tell us what sets there are for the principle to apply to, we might instead be prepared, on intuitive grounds, to endorse the principle in an open-ended fashion, as applying to whatever sets of points we come to recognize. (Compare induction over the natural numbers.) ‘I don’t know just what reals there are, and I don’t know just what sets of reals there are,’ one might say, ‘but I do know that any set of reals which is bounded above has a least upper bound.’ (This seems to have been more or less Poincaré’s view of the matter.) Does this give us classicism about the continuum? No!

The simplest way to see this is to consider forming a completion  $\mathbf{Q}^*$  of the rationals which satisfies the least upper bound principle, in the sense that any bounded set  $X \subset \mathbf{Q}^*$  has a least upper bound. To avoid appealing to Cantorianism, we cannot assume that we have any general idea of what subsets of  $\mathbf{Q}^*$  there are; but we can of course consider those sets that we can define. These will be either explicitly countable sets (the range of a definable sequence of elements in  $\mathbf{Q}^*$ ) or intervals with endpoints in  $\mathbf{Q}^*$ . But this is not enough to give us a classical continuum: as we shall see in Ch. 7, the arithmetical reals — those given by predicative sets of natural numbers — satisfy sequential completeness, and also interval completeness.

It is interesting to note that in the classical context, the completeness principle for bounded intervals is enough — it is equivalent to the full completeness principle.<sup>64</sup> One might at first have thought that the problem with the least upper bound principle comes with its application to ‘nasty’ sets of reals — those with complex structure, such as fractals or non-measurable sets; but the equivalence between the principle restricted to intervals and the principle in full generality shows that this is not the case. However, this does not change the main point, as the classical context in which the equivalence holds is one where all of the real numbers (which can serve as end-points to specify the intervals) are already present.

What I suggest is that it is extremely difficult to defend the view that our intuition of the continuum is rich enough to ground what I have been calling classicism about it: while the least upper bound principle is extremely compelling for those collections

<sup>64</sup> Proof sketch: Given a bounded set of reals  $X$ , consider the set  $Y = \{x \mid \exists y, z \in X. (y \leq x \leq z)\}$ . The set of upper bounds for  $Y$  is clearly the same as the set of upper bounds for  $X$ , so  $Y$  will have a l.u.b. just in case  $X$  has.

of real numbers that we can envisage or intuit, it is simply question-begging for the classicist to claim that we can intuit all of the sets of real numbers, and on that basis see the truth of the least upper bound principle in its full classical generality.<sup>65</sup> (I will return to the subject of completeness principles in the discussion of what the predicative continuum is like, in chapter 7.1.9 below.)

More generally, I suggest that we have not been given a compelling argument to seriously endorse uncountable mathematics by any of the routes considered — second-orderism, Cantorianism and intuitivism. And those routes are general enough that it is hard to know where else the classicist could turn. Of course, none of this suggests that the classicist conception is actually inconsistent or even just incoherent. All I have been trying to show is that there is no particular reason to think classicism *true*.

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<sup>65</sup> It is an interesting question just how far such geometric intuition *can* take us in the development of the real line. However, that is a question which I cannot attempt to answer here.

## Chapter 5

# The stability of the predicativist position

The purpose of this chapter is to address concerns that one might have about the internal stability of predicativism as a philosophical position. Is it in danger of collapsing into some more radically revisionary philosophy of mathematics, such as intuitionism or finitism? Do the concerns with classical mathematics that were raised in Ch. 4 cut so deeply as to warrant the rejection of predicativist mathematics too? I shall argue not.

Intuitionism can claim to be the most influential and popular of anti-classical approaches to mathematics. It is particularly important for my defence of predicativism to distinguish it clearly from intuitionism, as Weyl himself renounced his predicativist project after falling under Brouwer's spell;<sup>1</sup> as a result, there has been a persistent suspicion of a slippery slope from predicativism to intuitionism.

As originally presented by Brouwer, intuitionism was motivated by a strongly anti-platonist view of mathematical reality, according to which the objects of mathematical study are mental constructions. The intuitionistic mathematics that Brouwer developed is constructive, in the sense that to be intuitionistically acceptable, a proof of an existential statement  $\exists x Fx$  must be direct, by actually constructing (or at least by showing how to construct) an instance, and not just by refuting  $\forall x \sim Fx$ . This constructivity requirement on proofs led Brouwer and his followers to reject certain logical principles, and so to adopt instead a non-classical logical calculus, which no-

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<sup>1</sup> In fact, it is arguable that Weyl's later position is better seen as a form of finitism. See especially Majer, 'Differenz zwischen Brouwer und Weyl'.

toriously does not validate the classical Law of the Excluded Middle. Later advocates of intuitionism, most notably Michael Dummett, have also argued for non-classical logic, but have not always based their arguments on Brouwerian metaphysics.

In their criticism of classical mathematics, predicativism and intuitionism share a number of philosophical concerns, but the two schools have developed very different bodies of mathematics. The fundamental differences between predicativism and intuitionism lie in their divergent attitudes towards the natural numbers and towards the continuum. With respect to the natural numbers, predicativists take the view that the number sequence is fully determinate, and so their mathematics is fully classical with respect to first-order arithmetic; intuitionistic arithmetic, by contrast, does not validate LEM even for quantification over the natural numbers. With respect to the continuum, the difference is even more fundamental, in that while predicativistic analysis differs from classical analysis only in terms of which definitions for real numbers (or sets of natural numbers) are permissible, intuitionistic analysis is based on a very different idea of the real numbers — that they are given by choice sequences.

The structure of this chapter is as follows. First, in §5.1 I consider two arguments for intuitionism: one based on the Brouwerian premise that the objects of mathematics are mental constructions; and one based on Dummett's doctrine that certain mathematical domains are indefinitely extensible. I find the first to be inconclusive, but the second to be highly suggestive of a problem with classical mathematics. I then examine what the upshot of that argument is in the two cases which are crucial to the predicativist programme. In §5.2 I examine whether the natural numbers exhibit the phenomenon of indefinite extensibility. I argue that they do not. And in §5.3, I argue that the real numbers are indefinitely extensible, and I investigate how this can be accommodated mathematically. I argue that the predicative approach here compares favourably with intuitionistic analysis, and that in both arithmetic and analysis, the Law of the Excluded Middle turns out to be both less problematic and less important than has often been thought.

## 5.1 Arguments for intuitionism

### 5.1.1 Brouwer: Mathematical objects as mental constructions

On Brouwer's view, mathematics was a mental activity based on pure intuition. Brouwer was determined to avoid both platonism, which hypostasized the objects

of that intuition into quasi-concrete mind-independent inhabitants of some abstract realm, and naive formalism, which denied that intuition, and so reduced mathematics to the manipulation of empty symbols.

As such, Brouwer came to think of mathematical truth as being a wholly mental and experiential matter: as he put it, ‘there are no non-experienced truths.’<sup>2</sup> And of course this leads immediately to a rejection of bivalence, and of the corresponding logical Law of the Excluded Middle: for any open mathematical problem, there is currently no proof that can lead the mathematician to experience its truth, nor a refutation that can lead her to experience its falsity. If all there is to the truth or falsehood of a mathematical proposition is our experiencing it as such, then we are not currently warranted in saying that an open proposition is either true or false.

It is interesting to note that Weyl seems to have been attracted to some form of idealism throughout his career, as is suggested by his approving mention of Fichte in *Das Kontinuum*, and his later comment that:

the numbers are to a far greater measure than the objects and relations of space a free product of the mind and therefore transparent to the mind.<sup>3</sup>

It may well have been the idealistic metaphysics of Brouwer’s intuitionism that first attracted him to the position.

Brouwer suggested that the laws of classical logic had their origins in our reasoning about finite collections, and that the naive extension of those laws to infinite domains by the classicist went beyond the range in which those laws were valid. As the later Weyl put it (with the zeal of a convert):

According to his [Brouwer’s] view and reading of history, classical logic was abstracted from the mathematics of finite sets and their subsets. [...] Forgetful of this limited origin, one afterwards mistook that logic for something above and prior to all mathematics and finally applied it, without justification, to the mathematics of infinite sets. This is the Fall and original sin of set-theory, for which it is justly punished by the antinomies.<sup>4</sup>

It is not obviously the case, however, that viewing mathematical objects as mind-dependent necessarily threatens either the bivalence or the objectivity of the truth

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<sup>2</sup> Brouwer, *Collected Works* 1 p. 488. <sup>3</sup> Weyl, *Philosophy of mathematics* p. 22. <sup>4</sup> Weyl, ‘Mathematics and logic’ p. 276.

values of the mathematical propositions about those objects. Feferman is an example of someone who combines the two views:

[Hellman] points out that I am even an anti-platonist regarding the natural numbers but that I accept classical logic concerning arithmetical statements on the basis of the objectivity of truth values for them. My reason for doing so is that I regard the mind-dependent conception of the natural number sequence as intersubjectively robust, just like various other human conceptions...<sup>5</sup>

Even if we grant the constructivist the ontological premise that the natural numbers are our mental creations, it does not seem to follow that the only facts about them are those that are immediately available to us. Presumably the mental creation of mathematical objects is by some sort of stipulation: but our stipulations may very well have consequences which we did not foresee.

Brouwer's remark quoted above, that there are no non-experienced truths, is a very strong one. If taken seriously, it seems to lead to an extreme 'decisionist' view, such as that associated with (a certain reading of) the later Wittgenstein, on which a stipulation made in the past has no power to bind us now to go on one way rather than the other; at every point, a fresh decision is required.

But this is not the way that intuitionism has gone. This is most apparent in its attitude to infinity. However we spell out 'experience', it seems that there are only finitely many of the natural numbers which we have experienced in the past, or will experience in the future. We are, after all, finite creatures. But intuitionism is not the same as strict finitism: crucially, intuitionists accept that the natural number sequence is potentially infinite, and therefore that there is something in our grasp of the sequence that serves as 'rails to infinity'. When the natural number sequence was created, we determined how things would go all of the way down the line. So intuitionists are willing to idealize away from some of our limitations, and therefore to open themselves to the phenomenon that stipulations or constructions we have performed in the past in some sense already contain consequences which we are yet to unpack.

The question then becomes: how far (and in what directions) should we idealize?

Intuitionists go as far as endorsing bivalence for the case of decidable predicates applied to natural numbers. For every individual number, there is a fact of the matter

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<sup>5</sup> Feferman, 'Comments on Hellman' p. 317.

as to whether it has the Goldbach property (being either odd or the sum of two primes). It is in principle decidable by a mere computation, and that is enough for the determinacy of its truth value, even if that computation would be too long and too large to be physically carried out in our universe. But the intuitionist is not prepared to endorse bivalence for the universal generalization, that is, for Goldbach's Conjecture that every number has the property. That, it seems, is an idealization too far. But why?

What makes arithmetical sentences true or false is the way the numbers are: this, the mentalist insists, is transparent to reason, as the numbers are products of reason. But it is the elementary (quantifier-free) arithmetical facts which are transparent to reason; they are decidable by mere calculation. And on the classical view, it is the infinity of those instances which make quantified arithmetical statements true or false.

The intuitionist thought seems to be precisely a rejection of this last move: a universal generalization is of course false if it has a counterexample, and true if it has a proof, but these sorts of finite reasons are not obviously exhaustive of the possibilities. The real force of Brouwer's remark that there are no non-experienced truths seems to be to insist that such finite reasons — reasons that we can experience — for the truth or falsity of mathematical statements are the only reasons that there are; and so in the absence of such a finite reason, we have no warrant for thinking that a statement must be either true or false. But it is the infinity of the natural numbers, and not their status as mind-dependent objects, that makes the difference here — that gives us reason to worry about the bivalence of statements quantifying over them.

It seems that while a belief in the mind-dependent nature of mathematical objects may suggest a rejection of bivalence for the propositions of mathematics, there is no immediately compelling argument from the first to the second. If we want a reason to be intuitionists, we need to look harder at the problems associated with the infinite.

### 5.1.2 Dummett: Indefinite extensibility

Michael Dummett has influentially looked for such a reason in general considerations about meaning. He draws on the striking difference between the standard semantics for the intuitionistic logical connectives — the so-called Brouwer–Heyting–

Kolmogorov semantics, which are in terms of abilities and operations on proofs — and the semantics for the classical connectives, which are of course truth-conditional. The thought is that this classical semantics asks more of us than we can deliver.

Dummett has suggested two requirements that an adequate theory of the meaning of language must meet: the implicit knowledge of meaning that it credits us with must be knowledge which can be both *acquired* and *manifested* through actual language use. The basic thought is that the assertion conditions for a sentence should be such that we can always know, in principle, when they obtain, that they obtain. And the meaning of the intuitionistic connectives is supposed to be given by the BHK clauses, which give assertion conditions for sentences where those connectives appear in the main position, in terms of the assertion conditions for the principal subformulae. In contrast, the classical logical connectives have a meaning which is explained in terms of their truth conditions — the truth-function that takes the truth-values of the principal subformulae, and gives the truth-value of the whole formula; and the classical quantifiers are explained in terms of the truth values of the bound formula when it is instantiated by every element of the domain. And in contrast with assertion conditions, it is obviously not the case that we are always in a position to know that such truth conditions obtain when they do.

Consider again Goldbach's Conjecture: a universal quantification of unknown truth value. It seems clear that we know what it means; but if the quantifier is a classical one, then grasp of that meaning is something that we cannot plausibly acquire or manifest through behaviour, because it is only through a general proof — which in this case we don't have — that we could come to acquire or to manifest the belief that *all* of the instances obtain. On the other hand, an intuitionistic understanding of the quantifier makes its meaning a matter of our abilities to recognize and (in some cases) produce proofs of, or counter-examples to such universal claims: and these are abilities which clearly are acquired and manifested in the education and practice of mathematicians. The suggestion is that intuitionistic logic therefore meets the acquisition and manifestation challenges, whereas classical logic does not.

Dummett's meaning-theoretic argument has spawned a vast literature, but it has certainly not met with universal acceptance.<sup>6</sup> The reasonableness of the acquisition and manifestation challenges has been questioned (might not an appeal to shared human nature obviate the need for absolute publicity of our meaning?), as has the

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<sup>6</sup> See, for instance, McDowell, 'Truth conditions'.

claim that a classical, truth-conditional account of meaning cannot meet those challenges, when they are properly construed. I cannot begin to do justice here to the depth of the argumentation on both sides. Stepping back from the argument, though, we might worry that an area of philosophy as contentious as the theory of meaning is not currently a promising source for a genuinely *suasive* argument for a revisionary project in mathematics such as intuitionism.

I will therefore focus instead on Dummett's other, specifically mathematical, argument for intuitionism: the argument from indefinite extensibility. This line of argument does not rely, as the Brouwerian case for intuitionism does, on imagery of our creation of mathematical reality. And while it is meaning-theoretic, it is much more localized in its ambition than the general meaning-theoretic argument just considered. It turns on the specific issue of what is needed for quantification over a domain to be meaningful. We first met the argument in Ch. 3.2.2 above.

For a domain which we are happy to view realistically — the domain of *F*s, say, there is no general requirement to characterize the extent of the domain in order to meaningfully quantify over it. To ensure that a sentence which quantifies over the *F*s has determinate truth-conditions, we need of course to have sharp criteria of application and of identity for the predicate '*F*'; to know, that is, of any given item, what it takes for it to be one of the *F*s, and to know of any two given *F*s what it takes for them to be the same *F*. (If we do not have sharp criteria of application and identity, then we are unable to give any substance to the standard explanation of the quantifiers as infinite truth-functions: there is no question of 'mentally running through' all of the *F*s if we are unsure of what counts as an *F* or whether we have checked them all.) But there is no further work that needs to be done in the way of saying what *F*s there actually are: the realist thought is that reality will take care of that. As Dummett writes,

In order to confer upon a general term applying to concrete objects — the term '*star*', for example — a sense adequate for its use in existential statements and universal generalizations, we consider it enough that we have [...]: a criterion of application and a criterion of identity. The same indeed holds true for a term, like '*prime number*', applying to mathematical objects, but regarded as defined over an already given domain.

The last clause, the requirement that the domain is already given, is crucial, though;

for Dummett continues:

It is otherwise, however, for such a mathematical term as ‘natural number’ or ‘real number’ which *determines* a domain of quantification. For a term of this sort, we make a further demand: namely that we should ‘grasp’ the domain, that is, the totality of objects to which the term applies, in the sense of being able to circumscribe it by saying what objects, in general, it comprises — what natural numbers, or what real numbers, there are.<sup>7</sup>

Why do we — or why should we — make this further demand? Why is it that in the case of mathematical object, unlike concrete objects, ‘reality cannot be left to blow all haziness away’?<sup>8</sup>

Dummett’s answer seems to be that the need to make this demand is the lesson of the set-theoretic paradoxes. The paradoxes revealed that some mathematical concepts are ‘self-reproductive’ or ‘indefinitely extensible’. The simplest example is the concept of ‘non-self-membered set’. Consider a set  $W$  of objects which fall under the concept;  $W$  will itself fall under the concept, and so naturally gives rise to another set,  $(W \cup \{W\})$ , of objects all of which fall under the concept, and which is more extensive than the original set.

Dummett endorses Russell’s analysis of the paradoxes: that they all involve quantification over an ‘illegitimate totality’ (i.e. domain) — that is, a domain which contains all of the objects falling under a self-reproductive concept:

it is impossible coherently to understand individual variables as ranging over all objects, or even over all sets, all ordinal numbers. [...] What is meant [...] is that it is not possible to suppose that, by specifying the range of some style of individual variables as being over ‘all objects’, or ‘all sets’, or ‘all ordinals’, we have thereby conferred a determinate truth-value on all statements containing quantifiers binding such variables (even given that the other symbols occurring in these statements have been assigned a determinate sense). Any attempt to stipulate senses for the predicates, relational expressions and functional operators that we shall want to use relative to such a domain will either lead to contradiction or will prompt us to concede that we are not, after all, using

<sup>7</sup> Dummett, ‘What?’ p. 438. (My emphasis.) Cf. Dummett, *Frege: Philosophy of Mathematics* pp. 314–5.

<sup>8</sup> Ibid. p. 315.

the bound variables to range over absolutely everything that we could intuitively acknowledge as being an object, a set, or an ordinal number.<sup>9</sup>

Dummett's definition of indefinite extensibility draws on this:

- [IE] An indefinitely extensible concept is one such that, if we can form a definite conception of a totality all of whose members fall under that concept, we can, by reference to that totality, characterize a larger totality all of whose members fall under it.<sup>10</sup>

The moral we are to draw from all this is the central thesis of the argument from indefinite extensibility:

- (\*) Classical quantificational logic is legitimate only over mathematical domains whose extent can be given a definite characterization.

So far, so Russellian. But Russell's response to this was to impose a predicativity constraint on mathematics, and avoid quantification over the domains which caused the trouble. Dummett, however, thinks that we need not give up on such quantification altogether; we only need to make sure it is not *classical* quantification. And what makes Dummett's argument from indefinite extensibility into an argument for mathematical intuitionism is that Dummett also accepts the additional thesis:

- (\*\*) Intuitionistic logic is legitimate for quantification over any domain (including indefinitely extensible domains).

The thought behind this second thesis is one that we saw at the heart of the general meaning-theoretic argument considered above: very roughly, it is that classical quantification is explained truth-conditionally, and so in a way that makes essential reference to the domain of discourse, whereas the meaning of intuitionistic quantification is explained in terms of our abilities — in the mathematical case, our abilities to give and understand proofs. Of course, the meaning of the quantified statement in both cases depends on the domain; but in the intuitionistic case, that dependence is fully captured by the axioms drawn on, and the methods used in the proofs themselves — by things which are graspable by us. In contrast, the meaning of a classical quantification is an infinitary truth-function over the domain itself, and so

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<sup>9</sup> Dummett, *Frege: Philosophy of Language* p. 568–9 (cf. pp. 476, 530). <sup>10</sup> Dummett, 'What?' p. 441.

the ungraspability of the extent of the domain renders the meaning of quantification over it hazy or mysterious: such statements may lack determinate truth conditions.<sup>11</sup>

Now Dummett recognizes that intuitionistic logic is not a panacea: after all, the derivation of Russell's paradox does not rely on LEM. As Dummett notes,

Abandoning classical logic is not, indeed, sufficient by itself to preserve us from contradiction if we maintain the same assumptions as before; but, when we do not conceive ourselves to be quantifying over a fully determinate totality, we shall have no motive to do so.<sup>12</sup>

The fundamental mistake in Frege's set theory was to suppose that we could be given a once-and-for-all recipe for forming the sets. We see that this is impossible when we realize that the domain of sets is indefinitely extensible, with the sets stretching off into the hazy distance. Dummett's claim is that intuitionistic quantification over all of the sets can be understood, because our concept *set* is fixed, even as we see its extension expand; but he admits that our specifically set-theoretic assumptions also need to be changed in the light of the concept of indefinite extensibility. (Changed, that is, from the assumption of naive set comprehension.) And this raises a question: if we make appropriate changes, could classical logic perhaps be legitimately applied after all?

Our discussion so far has focused on the set-theoretic paradoxes, and one might be tempted to think indefinite extensibility is restricted to set-theoretic concepts, and so that the argument from indefinite extensibility applies only to set theory. But Dummett has claimed that the concepts 'natural number' and 'real number' are also indefinitely extensible (or at least can be seen or argued to be so). And it is these cases, of course, which are the important ones for the project of this thesis.

The argument in these cases is necessarily somewhat different from the case for set theory, because it seems clear that no paradox results from assuming either that the naturals are a definite domain, or that the reals are. (In fact, no paradox results merely from the assumption that there is a universal set. Other set-theoretic principles are needed to derive a contradiction, and indeed there are various more or less well-motivated set theories which drop those principles, and suppose that there is a universal set. The same goes for a set of all ordinals.<sup>13</sup> The concept at the

<sup>11</sup> Dummett, 'What?' pp. 438–442. <sup>12</sup> Dummett, *Frege: Philosophy of Mathematics* p. 316. Cf. Dummett, 'What?' pp. 441–2. <sup>13</sup> See for example Forster, *Set Theory with a Universal Set*, Church, 'Set theory with a universal set'.

root of Russell's paradox, however, 'non-self-membered set', cannot have a set as its extension, as a matter of pure logic.) It has been suggested by both Clark and Oliver, for instance, that the case for the indefinite extensibility of the real numbers and of the natural numbers is weaker than for the sets.<sup>14</sup>

In §5.2, I argue that the natural numbers are not indefinitely extensible, and therefore that arithmetical classicism (the application of classical logic to arithmetic) is not vulnerable to a Dummettian argument from indefinite extensibility. While it may not be possible to give a characterization of the natural numbers which will satisfy the most resolute of sceptics, such resolute sceptics are not entitled to as much as intuitionistic arithmetic: they should be strict finitists.

In §5.3, I discuss the continuum. I argue that here we do have an indefinitely extensible domain, and I explore the notion of indefinite extensibility, and some criticisms of it, and explain why it is that, in my view, it enters the story where it does: with the real numbers, but not with the naturals. Further, I argue that despite this indefinite extensibility, classical logic can legitimately be used in quantifying over the continuum, as long as we respect the predicativity requirements which indefinite extensibility motivates.

## 5.2 Are the natural numbers indefinitely extensible?

### 5.2.1 Characterizing the natural numbers

In 'The philosophical significance of Gödel's theorem', first published in 1963, Dummett argues that we can give a definite characterization of the concept *natural number*, but not of the domain of the properties of the natural numbers. As we will shortly see, in later writing, such as 'What is mathematics about?', Dummett changes his position, and suggests that the natural numbers themselves are indefinitely extensible.

A definite characterization of the natural numbers can be given in terms of zero and the successor function: zero is a natural number and is not a successor; every natural number has a unique successor, which is another natural number; and there is no natural number which cannot be reached from zero by means of the successor function. In other words, a natural number is the result of finitely many applications of the successor function to zero.

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<sup>14</sup> Clark, 'Dummett's argument', Oliver, 'Dummett and Frege'.

This characterization is of course somewhat circular, in that it uses the concept ‘finite’. But for whom is that a problem?

This circularity is a problem for the *strict* finitist — by which I mean someone who believes that there is a greatest natural number. (Of course, strict finitists tend to say that we don’t know — and can’t — which that greatest natural number is.)

It seems, though, that everyone else actually subscribes to this characterization of the natural number concept. In particular, the finitist does. By finitism, I mean the view championed by Hilbert that meaningful mathematics is concerned solely with singular statements about finite objects, and that full-blown quantification over the natural numbers is therefore not meaningful. Such finitists do allow schematic reasoning, however, to establish conclusions about an arbitrary natural number; and it is just this concept which is at issue. Although the finitist is unwilling or unable to reflect on the concept of natural number, she knows one when she sees it, and unlike the *strict* finitist, she does not think that they give out somewhere further down the line. Certainly the concept is one which the intuitionist accepts.

As Dummett points out (and as we noted in §4.3.3 above), a non-standard model of the elementary Peano axioms (by which I mean just the axioms concerning zero and the successor function: we need not worry about induction at this stage) is a possible way of (mis)interpreting the vocabulary in the axioms, but it is not a possible way of *misunderstanding* the characterization they give. A non-standard model is an infinite mathematical object, and as such it can only be given as a description. And such a description will necessarily be in terms which presuppose a grasp of the (standard) natural numbers.<sup>15</sup> It is not merely that a non-standard model of the basic arithmetical axioms is a perverse interpretation; it is that such perverse interpretations are only available to those who have already helped themselves to the notion of the natural number sequence.

The early Dummett does say that there is some sort of indefinite extensibility connected with the concept *natural number*: the means of proof of arithmetical statements that we find acceptable are always incomplete: that, Dummett suggests, is the philosophical significance of Gödel’s theorem. But in 1963, Dummett was quite clear (and, I would add, quite right) that ‘there is really no vagueness as to the

<sup>15</sup> Dummett, ‘Gödel’s theorem’; cf. also Putnam, ‘Models’ on the ‘verificationist’ (or perhaps better: internalist) understanding of model theory. A formal version of this point is given by Tennenbaum’s theorem: any recursive model of Peano Arithmetic is isomorphic to the standard one. See Odifreddi, *Classical Recursion Theory* p. 24.

extension of “natural number”.”<sup>16</sup>

At some point after this, however, Dummett changed his mind, and began to talk of the domain of the natural numbers as indefinitely extensible. The later Dummett draws an analogy between the concept ‘natural number’ (where the ‘principle of extensibility’ is the successor function) and the concept ‘property of natural numbers’ (where it is diagonalization). Just as the predicativist refuses to accept a definite domain of properties or sets of natural numbers which is closed under diagonalization, the finitist finds it absurd to suppose that there is a definite domain of natural numbers closed under the successor function. And there seems to be no non-circular way of getting a rational being to make the leap to accepting the totality in either case. Dummett offers this reflection on the situation:

A natural response is to claim that the question has been begged. In classing *real number* as an indefinitely extensible concept, we have *assumed* that any totality of which we can have a definite conception is at most denumerable; in classing *natural number* as one, we have assumed that such a totality will be finite. Burden of proof controversies are always difficult to resolve; but in this instance, it is surely clear that it is the other side that has begged the question.<sup>17</sup>

But the concept *natural number* only fits Dummett’s official account of indefinite extensibility (see the quotation (IE) on p. 115 above) when it is explained as Frege does, defining each natural number to be the cardinality of its predecessors. The principle of extension here takes us from a (finite) domain (for Frege, strictly: concept) of natural numbers to the cardinality of that domain; when the domain is an initial segment of the naturals, the resulting cardinal will be a natural not in that segment. But the more obvious principle of extension is simply the successor operation, and here we do not have the same move from a finite domain of naturals to a more extensive finite domain; we just have a natural number, and then a bigger natural number.

There is indeed a ‘striking resemblance’, as Dummett puts it, between Frege’s reasoning for the infinity of the naturals, and the indefinite extensibility of the set concept — in short, their impredicativity. But this impredicativity is not an essential feature of the concept *natural number*, whereas it does seem to be essential to the

<sup>16</sup> Dummett, ‘Gödel’s theorem’ p. 196. Emphasis in original.    <sup>17</sup> Dummett, ‘What?’ p. 443. Emphasis Dummett’s.

concept *set*. If we do not share Frege's determination that the infinity of the natural numbers should be a logical truth, then we can instead appeal, for example, to a Hilbertian symbolic intuition of stroke strings to deliver the basic truths of arithmetic: that every stroke string has a unique successor (formed by the concatenation of another stroke), that no two distinct stroke strings have the same successor, and that such concatenation will never take us back to the single stroke with which we started.

Dummett's paradigm example of indefinite extensibility is the concept of non-self-membered set. In contrast to Lear, for example, who argued that the open-endedness of the sets arises from some underdetermination of the set concept,<sup>18</sup> Dummett is surely correct in diagnosing indefinite extensibility, not in any equivocation, but rather in our coming to recognize more and more instances of *the same concept*, by reflecting on that concept. But it seems that while this analysis is correct for sets of natural numbers, and for sets in general, it is grossly inappropriate for the case of the natural numbers. If someone has any concept of natural number worth the name, she must recognize that the sequence of numbers goes on indefinitely; that we can keep on counting and so keep on getting new numbers. But this is not the result of *reflection* on the concept natural number: it is merely the straightforward application of it.

### 5.2.2 The alleged impredicativity of the natural numbers

At this point, it is as well to return to an issue raised in Ch. 3.4.1 above: the argument that in fact there *is* an essential impredicativity of the natural number concept, which is shown in the induction principle.<sup>19</sup>

This issue seems to be intertwined, at least in Dummett's work, with concerns about the indefinite extensibility of the natural numbers. But the induction principle is of course unrelated to any principle of extension, and therefore to any possibility that the natural numbers might be indefinitely extensible: if anything, its contribution to the concept is quite the opposite, ruling out non-standard elements. What is indefinitely extensible, as Dummett argued in 'The philosophical significance of Gödel's theorem', is the collection of epistemically legitimate axioms (or methods of proof) for arithmetic.

<sup>18</sup> See Lear, 'Sets and semantics'. <sup>19</sup> The argument has been put forward by Parsons, Nelson, and Dummett himself. The most recent version is given in Parsons, *Mathematical Thought and its Objects* §50.

The alleged impredicativity of the concept ‘natural number’ consists in the fact that the validity of induction, with respect to all well-defined properties, is supposedly an integral part of the concept. And among these properties are those which quantify over the natural numbers.

Recall that according to the Vicious Circle Principle, a set specification is impredicative if it involves quantification over a domain which contains the set being specified. First-order instances of the induction principle however, taken as part of an implicit characterization of the concept ‘natural number’, do not involve quantification over any domain which *contains* the *set* of natural numbers (or the property of being a natural number); at worst, they quantify over a domain which *includes* the natural numbers. In this respect, instances of induction seem to be in the same boat as the axiom that every number has a successor. Such quantification does *not* fit our official definition of impredicativity. Indeed, as Alexander George has noted, there are adequate specifications of the set of natural numbers which quantify only over finite sets.<sup>20</sup>

It should come as no surprise that, in contrast to PA (by which I mean, as always, first-order Peano arithmetic), full second-order Peano arithmetic, which contains the second-order induction axiom, *is* officially impredicative when taken as a characterization of the natural numbers. When the axioms are framed so as to introduce a natural number predicate  $N$ , then the extension of that predicate is in the range of the second-order quantifier which features in the induction axiom.

The situation seems to be this. A basic understanding of the natural numbers is that they are zero and its successors and nothing else. The extremal clause, ‘and nothing else’, can be read in various ways; it certainly need not be read as presupposing the full second-order induction axiom. As we noted in Ch. 4.2.5 above, full second-order logic is more than is needed to pin the natural numbers down (up to isomorphism); there are weaker logics, such as logic with a built-in ancestral operator, that will do the job. But it is not clear quite what purpose is served by such non-formal formalizations, and I suggest we leave this characterization as it is, a sentence of informal language. What this basic understanding of the natural numbers entails, though, is that induction is valid for any meaningful, well-defined

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<sup>20</sup> George, ‘Imprecision’. The fullest and most modern version of this idea has been carried out by Feferman and Hellman, ‘Predicative foundations of arithmetic’.

predicate of the naturals.<sup>21</sup> But this basic understanding of the naturals leaves entirely open a crucial question: Which predicates of the natural numbers *are* meaningful?

A conservative answer to this is given by Hilbert-style finitism, which denies the meaningfulness of predicates which contain quantification over the natural numbers. Hilbert motivated this view by stressing the role of symbolic intuition as the only real basis for meaning in mathematics. Such intuition could only present individual finite objects: so statements about all of the natural numbers, or predicates involving such quantification, could not be given finitary meaning. The finitist will therefore not be willing to endorse induction for predicates featuring such quantification. On the other hand, the finitist does allow that sentences of arithmetic with free variables are meaningful, because they can be understood schematically; so induction for such predicates is legitimate. The systems PRA and  $IA_0$  are formalizations of this sort of view of arithmetic.

A more generous answer would correspond to a more full-bloodedly realist view of the natural numbers as a completed infinite domain. On such a view, a predicate featuring quantification over the naturals is taken to be meaningful and to give rise to a statement with a determinate truth value when it is predicated of any natural. And so induction is valid for complex predicates containing such quantification. Such embedded quantification over the natural numbers (unlike the intitial universal quantification which features in the elementary Peano axioms and which is acceptable to the finitist) cannot be understood as schematic quantification. To meaningfully embed quantifiers in this way, we need to take the natural numbers as already given.

The sort of circularity that is described as the ‘impredicativity’ of induction is that these instances of the induction scheme with embedded quantifiers feature in axiomatizations (most notably PA) which are also sometimes presented as characterizations of the natural number structure. And here we might object: if we don’t already assume that we know what the natural numbers are — if that is, the characterization is supposed to be an adequate introduction to the concept — then how could we make sense of these problematic instances?

This problem is a real one. It is a real conceptual leap to go from the sort

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<sup>21</sup> Indeed, perhaps the best way to semi-formally express the basic understanding of the naturals is through natural deduction inference rules for the introduction and elimination of a natural number predicate, where the induction principle is an elimination rule. See Parsons, *Mathematical Thought and its Objects* §31, §47 for presentation and discussion of this.

of finitistic understanding of the natural numbers embodied in PRA, say, to that embodied in PA. Finitism is a conceptually stable position.

However, the problem is not one of impredicativity; at least, not in the official sense that is at work in the Vicious Circle Principle, and that is my concern in this thesis. (It may well be that there is some broader concept — perhaps a necessarily imprecise, family-resemblance concept — of impredicativity that might encompass both formal arithmetic and impredicative set comprehension.<sup>22</sup> But again, such a concept is not my present concern.) As we have noted, the sort of embedded quantification that features in the instances of induction of PA is quantification over the natural numbers themselves, and not over any domain of objects which includes the set of natural numbers, or over a domain of properties which includes the property of being a natural number. So the axioms of PA are not formally impredicative.

And the sort of circularity that we diagnosed occurs only if the axioms of PA are taken all at once as an introductory characterization of the natural numbers. If instead we give first the elementary Peano axioms as a characterization of the basic grasp of the natural numbers — that the natural numbers are zero and its successors — then the instances of the induction principle can be presented subsequently, as further facts about the already understood structure. As we have noted, the stubborn finitist will reject these (purported) further facts as being ungrounded. And I do not propose here to try to persuade her otherwise. What is important for my purposes here is that those of us who have taken the leap and endorsed PA are not vulnerable to an argument, based on impredicativity, to the conclusion that we should not have made that leap.

### 5.2.3 The negative translation

It's here worth returning to the comparison between intuitionism and finitism. While finitism genuinely is a more modest position to take on arithmetic than predicativism (given that predicativists accepts PA), it seems that intuitionism is not. And it quite is clear that neither the Dummettian, nor the orthodox Brouwerian intuitionist is any longer in a position to doubt the *consistency* of PA.

The reason is as follows. The standard formalization of intuitionistic number theory is Heyting Arithmetic (HA). As the first-order Peano axioms are obviously

<sup>22</sup> George, 'Imprecision' makes more or less this suggestion.

true of the natural numbers from an intuitionistic perspective just as much as on a classical perspective, the only difference between PA and HA is in the background logic. However, Gödel's 'negative translation' gives a uniform mapping of sentences of arithmetic which carries PA-proofs into HA-proofs, and which leaves numerical equations unchanged. An inconsistency in PA would allow us to construct a PA-proof of ' $0 = 1$ '; but this could then be converted by the negative translation into an HA-proof of the same result, and would therefore mean that HA was also inconsistent.<sup>23</sup>

Formal systems for predicativism which are conservative over PA, such as  $\text{ACA}_0$ , are therefore guaranteed by intuitionistically acceptable means to be consistent; and so an intuitionist should have no scruples about making use of such a system in an instrumental spirit, as an 'ideal' mathematical theory (in Hilbert's sense).<sup>24</sup>

The intuitionist objection to LEM (and to classical logic in general) in the context of arithmetic cannot be that the assumptions lead to a contradiction; it must instead be about meaning. The charge must be that the classicist has inadvertently lapsed into nonsense; or that the explanations (implicit as well as explicit) of the meaning of mathematical sentences which the classicist can give are incoherent, or do not meet certain *a priori* requirements. (The obvious example of such requirements are Dummett's acquisition and manifestation challenges that we discussed earlier.)

But as Potter has argued,<sup>25</sup> even this more modest attack is called into question by the negative translation: if we look at the matter from above, as it were, it seems that the intuitionist will struggle to demonstrate an internal incoherence in the pattern of language use in classical mathematics, given that just those patterns are wholly replicated within intuitionistic mathematics. HA is a proper subtheory of PA, and so, on the face of the matter, intuitionist arithmetic is a part of classical arithmetic. The intuitionist might argue that HA is a privileged part — fully meaningful or justified in a way that classical arithmetic as a whole is not, or (more modestly) maintaining distinctions (such as that between a statement and its double-negation) that PA collapses. But what the negative translation gives is an injection of PA into HA that is not a surjection; hence the title of Potter's paper. And this seems to entirely undercut the claim that there is any merit particular to HA.

<sup>23</sup> See Gödel, 'Intuitionistische Arithmetik [1933e]'. <sup>24</sup> The conservativity must of course be proved in PA for this to hold. In fact, Shoenfield's proof of the conservativeness of  $\text{ACA}_0$  over PA is effectively in PRA: see Shoenfield, 'Relative consistency proof'. <sup>25</sup> Potter, 'Classical arithmetic is part of intuitionist arithmetic'.

If the intuitionist wants to resist this line of argument, it seems that she is forced to say that the logical constants are not up for reinterpretation in this way: that they have some special meaning which goes beyond what can be cashed out by describing patterns of legitimate inference. Such a desperate resort to incommunicable meanings seems to drive the intuitionists back into the Brouwerian solipsism from whence they came. This position is perhaps invulnerable, but also profoundly unappealing; and it is completely powerless to explain one of the most distinctive phenomena of mathematics, its objectivity.

Of course, the logical constants are used throughout language, not just in arithmetic. So the Dummettian, who defends intuitionistic logic for arithmetic but also insists on the publicity of meaning, can claim that it is the patterns of legitimate inference throughout language as a whole that pin down the meaning of the constants, and that intuitionistic logic is then applied to the subject of arithmetic to give us HA. But the problem with this tack, as Potter argues, is that the negative translation can be re-packaged as an intuitionistically acceptable, step-by-step introduction of the classical logical constants as used in arithmetic. So for there to be any real difference between HA and PA, it needs to reside in a difference in the understanding of the constants *as used in arithmetic*. And here we should note that HA and PA agree, not only on the atomic sentences, but also on the  $\Pi_1$ ,  $\Sigma_1$ , and even  $\Pi_2$  sentences. So the intuitionist will need to point to more complex sentences of arithmetic to find any special virtue in HA. If we had a firm pre-formal understanding of hypothetical conditionals, then that could serve as a point of a difference:  $\exists x Fx$  classically entails that one of  $F_0, F_1, \dots$  will be true, whereas if the premise is understood via the negative translation as  $\sim \forall x \sim Fx$ , no such implication holds intuitionistically. But an understanding of hypothetical conditionals with an infinite consequent is surely unavailable unless we have already settled the meaning of quantification over the numbers, which is precisely the point at issue: so an appeal to this as the value of intuitionistic logic would seem to be circular.

In the paper in which he announced the result of the negative translation, Gödel interpreted the situation thus:

the system of intuitionistic arithmetic and number theory is only apparently narrower than the classical one, and in truth contains it [...]. Intuitionism appears to introduce genuine restrictions only for analysis and set theory; these restrictions, however, are due to the rejection, not

of the principle of the excluded middle, but of notions introduced by impredicative definitions.<sup>26</sup>

The challenge which the predicativist faces from the intuitionist (Brouwerian or Dummettian) — to justify the Law of Excluded Middle for sentences featuring quantification over the natural numbers — seems to be adequately met. Unlike with finitism, it is very hard to find a compelling philosophical position which could motivate arithmetical intuitionism.

### 5.3 The indefinite extensibility of the continuum

So far, I have argued that if we set aside finitist scruples, we can credit ourselves with a determinate conception of the natural numbers. This means that we can be Dummettian realists about arithmetic, committed to bivalence for arithmetical statements, and can straightforwardly endorse classical Peano arithmetic as true.

What about analysis and the continuum? The predicativist agrees with the Dummettian intuitionist that the continuum is indefinitely extensible; but unlike the Dummettian, she attributes this to an important disanalogy between the natural numbers (which, contra the later Dummett, are a definite domain), and the real numbers. For the predicativist, then, the indefinite extensibility of a concept cannot be an immediate consequence of the infinity of its instances, or even of its ‘intrinsic infinity’. (This is Dummett’s term for a domain’s being infinite by virtue of the concept which gives rise to the domain: so ‘natural number’ is intrinsically infinite, whereas ‘star’ would not be, even if there happened to be an infinite number of stars.)<sup>27</sup> To draw this disanalogy between the case of the natural numbers and the case of the reals, we need first to get clearer on what indefinite extensibility is.

#### 5.3.1 Indefinitely extensibility again

As I am committing myself to the predicativist position that the continuum is indefinitely extensible, that is, that it is indeterminate just what real numbers exist, I will pause here to consider some objections to the coherence of this idea, and to make clearer why it is that I think that indefinite extensibility arises with the real numbers, but not with the naturals.

<sup>26</sup> Gödel, ‘Intuitionistischen Arithmetik [1933e]’ p. 37. <sup>27</sup> Dummett, *Frege: Philosophy of Mathematics* p. 318.

Clark and Oliver both complain of the obscurity in Dummett's use of the idea of indefinite extensibility.<sup>28</sup> Oliver says that

the failure to lay down precisely which objects fall under an indefinitely extensible concept must, for Dummett, be reflected in an indeterminacy in the existence of those objects.<sup>29</sup>

He then protests that this is tantamount to vague existence, 'an idea one cannot understand.'

Similarly, while Clark seems to accept that *set* is an indefinitely extensible concept, he objects to the inference he finds in his reconstruction of Dummett's argument that 'just because we have admitted that the concept of set is indefinitely extensible it is indeterminate as to what sets there are.'<sup>30</sup> Clark suggests that unless we are already (metaphysical) constructivists, or at least have some quasi-constructive idea of the sets evolving through time, then this inference is unwarranted. Further, Clark seems to suggest that there is an appeal here to Dummett's general meaning-theoretic argument: 'the argument for that claim is one which must lie within the theory of meaning: it must turn on what it is in general to have a "definite conception of everything falling under a concept", and why failure to have it could affect the issue of what objects there are.'<sup>31</sup> (Oliver certainly sees an appeal to the meaning-theoretic argument when the argument from indefinite extensibility is applied to concepts not threatened by set-theoretic paradoxes.)<sup>32</sup>

The clearest proposal for what Dummett could possibly mean is given by Sullivan.<sup>33</sup> Sullivan presents, as exegesis of Dummett, an argument that I will call a version of the argument from indefinite extensibility, although Sullivan himself does not use the notion of indefinite extensibility as a premise; rather he suggests it as 'an importantly illuminating way of formulating the intended conclusion.'<sup>34</sup> As Sullivan presents it, moreover, the argument is wholly independent of Dummett's meaning-theoretic considerations.

As Sullivan presents the argument, the key premise is what he calls Dummett's 'logicism'; and Sullivan describes this premise as 'clearly and importantly right.'<sup>35</sup> For the case of arithmetic — though it is explicitly claimed to hold for all mathematical domains — the premise is that

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<sup>28</sup> Clark, 'Dummett's argument', Oliver, 'Hazy totalities'. <sup>29</sup> Ibid. p. 48. <sup>30</sup> Clark, 'Dummett's argument' p. 61. <sup>31</sup> Ibid. <sup>32</sup> See Oliver, 'Hazy totalities' pp. 33-4, Oliver, 'Dummett and Frege' pp. 387-8. <sup>33</sup> Sullivan, 'Dummett's case'. <sup>34</sup> Ibid. p. 755. <sup>35</sup> Ibid. p. 760.

It is incoherent to suppose that anything might make for the truth of any statement of arithmetic save the conception we have of the objects it concerns. Arithmetical truth, that is, cannot outrun what is settled as true by our conception of the numbers.<sup>36</sup>

And of course the same must go for the existence of the numbers.

The load-bearing part of the premise is not of course that ‘It is incoherent to suppose’, but rather that nothing makes for the truth of a statement of arithmetic save our conception of the numbers. But the forceful support Sullivan gives for the premise is indeed that the alternative — what Sullivan calls ‘analogical platonism’ — is incoherent. Analogical platonism is so-called because the most familiar way of putting the view is to say that the objects of mathematics are real in just the same way as concrete objects. But as far as the argument goes, what counts as analogical platonism is just the belief that there is anything other than our conception of the mathematical objects which makes for the truth of any statement about those objects. It follows that ‘logicism’ and analogical platonism are collectively exhaustive and mutually exclusive positions.

Platonism in general is often cashed out by appeal to a faculty of intellectual intuition of mathematical objects which is (in some sense) analogous to sense-perception of physical objects. What the argument for ‘logicism’ (or equivalently, against analogical platonism) does is to place a limit on how far such an analogy can be taken.

In the case of concrete objects, a correct characterization of a region of concrete reality will allow us to draw conclusions about that region by purely deductive reasoning. But, by the nature of the case, such reasoning is only able to take us so far: a concrete object has a ‘dark side’, as Sullivan puts it, aspects which are not transparent to our reasoning on the basis of our conception of such objects. And the only way to fill in such gaps in our knowledge is by further empirical enquiry. There is no analogue to this in the mathematical case.

If the continuum hypothesis, say, is determinately true, that can only be because it follows from principles not yet formulated by us, but *already* inchoately present in our intuitive conception of the intended model of set theory. If that conception were a kind of blurred perception, on the

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<sup>36</sup> Sullivan, ‘Dummett’s case’ p. 757.

other hand, it might be that it could be filled out, with equal faithfulness to our present grasp of it, however implicit, both so as to verify and to falsify the continuum hypothesis, which nevertheless possessed a determinate truth-value according to the way things happened to be [...]. This supposition is manifestly absurd [...].<sup>37</sup>

The absurdity with analogical platonism is the thought that beyond faithfulness to our *conception* of mathematical objects, there is any further question of faithfulness to the objects themselves. In the case of the Continuum Hypothesis, *either* our conception of the sets is determinate enough already to settle its truth value (and it is just that we have not yet succeeded in expressing enough of that conception in set theoretic axioms); *or* nothing settles its truth value (and perhaps we will come to recognize that our present conception of the sets is really two distinct conceptions of sets blurrily superimposed; or perhaps even that it is really no conception at all). There is no third possibility, of the sets settling the truth value of CH behind our backs, as it were, by being arranged thus-and-so, despite the fact that their being so arranged is in no sense a consequence of our conception of them.

As Dummett writes,

if the analogy between physical and ideal objects were sound, our uncertainty about the continuum hypothesis need show no haziness in our *concept* of a set, but only in our knowledge of what sets God has chosen to create; for presumably ideal objects are as much God's creation as physical ones.<sup>38</sup>

The point is that the analogy is clearly not sound, but 'lame'. If it were up to God's creative will what things there are and how they are arranged in mathematical reality, then proof would not have the special role that it does in mathematics as the *unique* source of knowledge. There would then be an extra-mathematical fact about what God had actually done that would settle the truth value of CH. Conversely, as proof *is* the unique source of mathematical knowledge, it cannot be that mathematical reality is ultimately answerable to anything other than our conception of mathematical objects.

To put the matter another way: if the *F*s are concrete physical objects, and if our theory of the *F*s doesn't settle whether or not some statement  $\phi$  holds of the *F*s, then

<sup>37</sup> Dummett, *Frege: Philosophy of Mathematics* p. 310.    <sup>38</sup> Ibid. p. 302.

we need to do some empirical enquiry to find out, in this respect, how things stand with the  $F$ s — whether  $\phi$  is true or not. But if the  $F$ s are mathematical objects, and if our conception of the  $F$ s really doesn't settle whether or not  $\phi$  holds, then there is no question of anything else (the  $F$ s themselves) settling the matter. It must be that our conception of the  $F$ s was not univocal, and that there are in fact two sorts of  $F$ s: the  $\phi$ -sort and the  $\sim\phi$ -sort. (And this is precisely what is said by those set theorists who accept ZF but reject the idea that CH is settled by some (as-yet unformalized) part of our conception of the sets. And it is more or less how things turned out with Euclidean and non-Euclidean geometry.)

Two points should be noted here before we move on. First, Dummettian 'logicism' does not already entail that all mathematical truths are knowable by proof; what it does entail, rather, is that all knowable mathematical truths are knowable by proof. In one place, Dummett leaves open the possibility that second-order consequences of our initial mathematical assumptions might be genuine consequences, even though 'we may be unable even in principle to see that they follow.'<sup>39</sup> Second, Sullivan overstates the case in saying that if any truth about a subject area is obtainable by proof, then all obtainable truths must be so obtainable.<sup>40</sup>

As we discussed in Ch. 3.2.2 above, Sullivan explains the role that empirical reality plays in settling the meaning of empirical quantification through his striking metaphor of a checkerboard. When considering a quantified statement such as  $\exists xFx$  for an empirical concept  $F$ ,

The role of the checkerboard [...] is to provide positions, not only for objects that might fall under the concept, but at the same time for other viewpoints that might ground alternative ways of referring to those same objects. It is because it provides both of these that it is a framework both for differentiating and for identifying these objects.<sup>41</sup>

So to settle the criteria of application and identity of an empirical concept is thereby

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<sup>39</sup> Dummett, 'What?' p. 431. In Ch. 4.2, I argued against the idea that the full second-order consequence relation really is a consequence relation. Does this mean that I am committed to the view that all mathematical truths *are* in principle knowable by proof? I think that it does, and that this is not unacceptable. As the Incompleteness Theorems teach us, however, not all mathematical truths can be proved from any fixed collection of principles. There is clearly much more to be said; but I cannot attempt to say it here. <sup>40</sup> See Dummett, 'Reply to Sullivan' p. 788 for Dummett's rejection of Sullivan on this point. <sup>41</sup> Sullivan, 'Dummett's case' p. 777.

to fix its extension — or rather, to do what is needed to make it that reality fixes its extension. On the other hand,

In the absence of a checkerboard [...] our understanding of what it is for any given object to be *F* does not include any circumscription of the ways in which a candidate for being *F* might be given. In the mathematical case, then, knowing how to answer a question, ‘Is so-and-so an *F*?’ does not give us even a general grip on which such questions there are.<sup>42</sup>

Or, therefore, on which *F*s there are; and so we are in the dark about the meaning of quantifications over all of the *F*s. But it is not just that *we* haven’t exactly determined the range of our variables: it is that nothing has.

The naive realist picture, the analogical platonism that we have rejected, is of all of the objects, among them the mathematical objects, lying about the place in their respective regions of reality, and, by their existence, their properties and their arrangement, making our quantified sentences true or false. And were this picture right, then of course any problems associated with our ignorance of which *F*s there are and how they are arranged, would be just our problems; our sentences would still be meaningful and have determinate truth values, even though we often wouldn’t know what those truth values were, or perhaps even (in one sense) quite what our quantified sentences meant. But we have argued that analogical platonism is absurd: there cannot be any facts about what mathematical objects there are which are not consequences of our conception of those objects.

What this argument for Dummettian ‘logicism’ does is to answer Clark’s challenge: Dummett’s logicism is a weak form of constructivism on which the indefinite extensibility of a mathematical concept entails not merely our ignorance of, but the *indeterminacy* of the meaning, and so perhaps of the truth values of sentences which quantify over the objects which fall under that concept. Whether this is, as Oliver suggests, ‘an idea we cannot understand’ is another matter. The Russellian response to indefinite extensibility is indeed to view such sentences as nonsense, and to try to do without them altogether. Dummett suggests that we can make some sense of them, and I agree. First, though, is the question of where the continuum fits in.

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<sup>42</sup> Sullivan, ‘Dummett’s case’ p 778.

### 5.3.2 The continuum

In the course of his discussion, Sullivan asks why it is that there is no analogue of the checkerboard in the mathematical case. He considers how we might explain the notion of a real number, and notes that our explanations in no way settle how we might be presented with an instance: we ‘simply leave any purported specification to be judged on its merits when it is offered.’<sup>43</sup> There is something intrinsically open-ended here. However, Sullivan notes that this

turn[s] essentially on the intrinsically infinite character of the real numbers — of *each* real number, that is, and not only of the domain of the real numbers. And that makes it far from clear that any genuinely parallel source of open-endedness could afflict generalization over the natural numbers.<sup>44</sup>

I argued above that there is indeed no such open-endedness with the natural numbers. While the domain of natural numbers is infinite, it is built up by a uniform step-by-step process, and can therefore legitimately be considered to be a definite domain. The numerals provide a system of canonical notation which is, in Sullivan’s terms, an adequate checkerboard.

The sets of natural numbers are not uniformly generated in this way. Indeed, there can only be countably many intelligible definitions of sets of naturals, while Cantor’s Theorem shows that any countable collection of sets of naturals is incomplete and can be extended. Moreover, the non-absoluteness of the continuum means that it is infected by the indefinite extensibility of the domain of sets.

The argument of Ch. 4 was against the classical conception of the continuum as a definite domain. And we can now put this together with the notion of indefinite extensibility. With the sets of natural numbers, or the real numbers, there can be no equivalent of the checkerboard, because in the general case, we need an infinite amount of information to specify each one, and that is of course something that we can never have. No conception we can have of them can give us a grasp of more than a countable infinity of them. As we have renounced analogical platonism, it is our conception of the real numbers that determines what real numbers there are. And as

<sup>43</sup> Dummett, *Frege: Philosophy of Mathematics* p. 319; quoted Sullivan, ‘Dummett’s case’ p. 778.

<sup>44</sup> Ibid. p. 779. Do not confuse this ‘intrinsically infinite character’ with Dummett’s sense of ‘intrinsic infinity’, mentioned above.

Cantor taught us that the real numbers cannot be countable, it follows that the real numbers *in themselves* are indefinitely extensible: that statements which quantify over the reals have indeterminate meaning, and may have indeterminate truth value.

### 5.3.3 Predicativist analysis

Because of the indefinite extensibility of the continuum, predicativists and intuitionists agree that sentences which quantify over the continuum, such as, in particular, sentences of the language of second-order arithmetic, may lack a truth-value.

The intuitionistic approach to analysis is to reject the classical idea of a real number (which is in a sense a completed infinite sequence, and therefore intuitionistically illegitimate) and instead to base analysis on the idea of real numbers as given by choice sequences, about which only a finite amount of information is supposed to be available at any one time. While with arithmetic the intuitionists apply a more restricted logic to premises that are classically acceptable to give a subtheory of classical arithmetic, here the wholly original concepts used lead to results which actually contradict classical theorems of analysis. The predicativist response is much more modest; as we shall see in the following chapters, it simply involves a restriction, relative to classicism, of the principles by which real numbers (or sets of natural numbers) can be defined. The domain of quantifiers which range over the reals is not definite, and so bivalence is not guaranteed; but as we shall see, (bearing out Gödel's remark, quoted above), the assumption of LEM is nevertheless legitimate and does no harm.

The definable sets are a definite sub-collection of the full, indefinitely extensible continuum. They can be viewed as Weyl advocated, as built up genetically, by means of operations such as union, complementation, recursion, and so on, applied to a base-class of singletons; or they can be viewed (as is more common today) as arising from a comprehension principle applied to open sentences of arithmetic — which, in view of the way logically complex sentences are built up recursively from atomic formulae, comes to much the same thing. The important point for the predicativist position is that the means of defining (first-level) sets of natural numbers must not involve any quantification over the sets of natural numbers. (Ramified predicative systems allow second-level sets which may be defined using quantification over first-level sets, and then third-level sets defined by quantifying over those, and so on. However, the division of the sets (and therefore the real numbers) into levels

makes it much more difficult to give an intuitive interpretation to the theorems of ramified analysis, and I will not attempt to do so here.)

The genetic viewpoint, if taken seriously, provides one justification for this prohibition: we cannot permit set definitions to quantify over all of the sets because the sets are still being built (by means of their definitions). Outside the context of set definitions, quantification over the sets is perfectly legitimate. However, there is normally no suggestion that the only sets which exist are those which can be defined by the means permitted in the particular formal system, and this is in accordance with the intuitive picture: we can always ‘diagonalize out’ to define a set which cannot be defined by the means permitted. And so the ‘openness’ of the continuum is preserved, and the only universal conclusions about the sets which we are able to establish will be, in a sense, general: they will be claims which are true of *any* set, for general reasons, by the nature of the sets.

As Wang wrote, about the analogous case of general set theory:

If we adopt a constructive approach [to set theory] then we do have a problem in allowing unlimited quantifiers to define other sets. Even then there remains the possibility of accepting the law of excluded middle. The difficulty is rather in establishing universal conclusions because we cannot survey all permissible operations.<sup>45</sup>

To be predicatively acceptable, an axiom system for the continuum needs to be open-ended: that is, essentially existential. Most naturally, these existential axioms will correspond to methods of intuitively constructing sets of natural numbers — to some of Wang’s ‘permissible operations,’ for constructing sets either from natural numbers themselves, or from other sets of naturals. (Such as the arithmetically definable sets just mentioned, where the methods are recursion, union, intersection, complementation, and so on.)<sup>46</sup> What are not acceptable are limitative axioms, which say that only sets of a certain sort exist. Such an axiom may be true of some determinate part of the continuum (such as the arithmetically definable sets); but it is clearly not true of the full indefinitely extensible continuum. Also unacceptable are axioms which assert the existence of sets defined by means of quantification over the continuum: such axioms breach the Vicious Circle Principle.

<sup>45</sup> Wang, ‘The concept of set’ p. 560. <sup>46</sup> Such axioms will be of  $\forall\exists$  form: for every set  $X$ , there is a set  $Y = \mathbb{N} - X$ , and so on.

For a given axiom system, certain results will be provable about the continuum. In general, we may say that most of these results will be existential, and only rather trivial ones will be universal. The predicativist can recognize that the axioms are true of some delimited and definite part of the continuum, and that therefore the theorems derivable from the axioms will be true when the quantifiers are interpreted as ranging over this model of the continuum; but also that, because they are essentially existential, the axioms — and so also the theorems — will be true of the continuum as a whole, which is the intended interpretation.

The point of the prohibition on set quantification in set definitions (or set existence principles) is that the sets corresponding to such definitions will not, in general, be the same in all acceptable models of the continuum. If we want to define a set  $X$  as the intersection of all of the members of a class of sets with a certain property  $\Phi$ , then this condition may pick out one set  $X_{\mathcal{M}}$  in the model  $\mathcal{M}$ , and a smaller set  $X_{\mathcal{N}}$  in the richer model  $\mathcal{N}$ . On the classical point of view, of course, there is the assumption that the continuum is definite, and so that the definition succeeds in picking out the set  $X$  that we want, the intersection of all of the  $\Phi$  sets that there really are. But for the predicativist, all that we have are the successively richer models of the continuum, what we might call approximations from below,  $\mathcal{M}, \mathcal{N}, \dots$

The indefinite extensibility of the continuum is represented formally by the fact that formal predicative systems of analysis are not intended to be categorical as regards the range of their set variables. With systems of second-order arithmetic, where the first-order variables are intended to range over the natural numbers, and the second-order variables range over the sets of natural numbers, we consider  $\omega$ -models as standard, that is, those with a standard first-order part. These  $\omega$ -models differ in how rich their ‘continuum’ is: how many sets of natural numbers are contained in the second-order domain of the model. And therefore some statements involving second-order quantification will receive different truth values in the different models. Of course, for any two  $\omega$ -models,  $\mathcal{M}, \mathcal{N}$ , if  $\mathcal{N}$  contains a set that  $\mathcal{M}$  does not, then  $\mathcal{M}$  is inadequate — it cannot be the full continuum. But, if it is countable, neither can  $\mathcal{N}$ . As the continuum is indefinitely extensible, we can always keep extending it — or rather, revealing more of it. But for the purpose of actually doing mathematics, we need to choose some axioms to work with; we know in advance that they will not fully capture the continuum, but if we choose wisely, we can make sure that they capture enough of it for whatever mathematical purpose we have in mind. As any

model of the continuum will give determinate truth values to all of the sentences of the language of second-order arithmetic, there does not seem to be any good reason to forbid our theories from making the assumption of bivalence, as embodied in the Law of Excluded Middle.

To make good on this, we need to look more closely at the reasons given by Dummett (and others) as to why LEM might be problematic for indefinitely extensible domains.

#### 5.3.4 LEM and analysis

Dummett's position is that statements which quantify over an indefinitely extensible domain do not necessarily have determinate truth-conditions; but that does not make them meaningless, or indeed rule out warranted assertion of such statements:

if we have a clear grasp of any totality of ordinals, we thereby have a conception of what is intuitively an ordinal number greater than any member of that totality. Any definite totality of ordinals must therefore be so circumscribed as to forswear comprehensiveness, renouncing any claim to cover all that we might intuitively recognise as being an ordinal. It does not follow that quantification over the intuitive totality of all ordinals is unintelligible. A universally quantified statement that would be true in *any* definite totality of ordinals must be admitted as true of all ordinals whatever, and there is a plethora of such statements, beginning with 'Every ordinal has a successor'. Equally, any statement asserting the existence of an ordinal can be understood, without prior circumscription of the domain of quantification, as vindicated by the specification of an instance, no matter how large. Yet to suppose all quantified statements of this kind to have a determinate truth-value would lead directly to contradiction by the route indicated by Burali-Fori. [Footnote:] Abandoning classical logic is not, indeed, sufficient by itself to preserve us from contradiction if we maintain the same assumptions as before; but, when we do not conceive ourselves to be quantifying over a fully determinate totality, we shall have no motive to do so.<sup>47</sup>

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<sup>47</sup> Dummett, *Frege: Philosophy of Mathematics* p. 316.

As stated, this cannot be quite right. As Oliver notes, the domain of ordinals  $\{\alpha \mid \alpha \leq \omega\}$  is surely a definite domain (if any infinite domain is). But Dummett's generalization, 'every ordinal has a successor' is false in this domain.<sup>48</sup> Presumably, however, such domains do not even reach the starting-line: no-one would have such a domain as her intuitive model of the ordinals, precisely because it doesn't satisfy the obvious closure principle which is Dummett's example.<sup>49</sup>

And while Dummett's footnote concedes that abstaining from the use of classical logic is not sufficient to avoid contradiction, it also seems that it is not necessary. Whether or not we are led to a contradiction surely depends on the assumptions that we make about the ordinals, and on the richness of the language we use to talk about them. As Boolos points out, the (classical) elementary theory of the ordinals is even decidable: so every statement in that language clearly has a determinate truth-value.<sup>50</sup>

Dummett's thought can naturally be extended, for example to disjunction: a disjunctive statement which involves quantification over an indefinitely extensible domain should presumably be endorsed if every acceptable model makes true one disjunct or the other. But then we seem to have opened the way to a vindication of LEM: second-order instances of the Quantifier Law of Excluded Middle,  $\forall X \Phi(X) \vee \exists X \sim \Phi(X)$ , are true in all models. Can we not also admit these as true of all sets of natural numbers whatsoever?

Peter Clark (considering instead the case of general set theory, and drawing on Parsons' suggestion of relativizing the distinction between set and proper-class) has suggested that we can.<sup>51</sup> Dummett elaborates on the suggestion somewhat:

[Clark] disagrees with my opinion that a theory whose variables range over the objects falling under such a [sc: indefinitely extensible] concept must have an intuitionistic, not a classical logic. His justification is that we can view such a theory as systematically ambiguous, taking its variables as ranging over any one of a (necessarily indeterminate) range of definite sub-totalities. There will be many sentences of the theory that are not determinately true or false, because (say) false in some sub-totalities but true in larger ones; but the law of excluded middle

<sup>48</sup> Oliver, 'Dummett and Frege' p. 384. <sup>49</sup> The principle  $\forall \alpha \exists \beta. \beta = \alpha^+$  is therefore an appropriate axiom to adopt for partial characterizations of the ordinals. It is essentially existential. <sup>50</sup> Boolos, 'Whence?' p. 222. <sup>51</sup> Clark, 'Basic Law V' p. 247.

will hold good in the theory, because every sentence is determinately true or false in each definite sub-totality.<sup>52</sup>

As Dummett comments, “The proposal raises the question when we *do* have a definite conception of the objects over which our variables range.”<sup>53</sup> My suggestion — which builds on my earlier claim that we do have a definite conception of the natural numbers — is that we can count as definite any conception that is given by a (finitely-based) canonical system of notation for the objects.

For the continuum, then, the suggestion is that the predicativist can legitimately work with appropriate (formally predicative) unramified systems of second-order arithmetic, with a classical background logic (that is, assuming LEM throughout). Examples of such systems are  $\text{RCA}_0$ ,  $\text{WKL}_0$ , and  $\text{ACA}_0$ .<sup>54</sup> These three theories are nested: all of the theorems of  $\text{RCA}_0$  are theorems of  $\text{WKL}_0$ , and all of the theorems of  $\text{WKL}_0$  are theorems of  $\text{ACA}_0$ . The natural interpretation of the set quantifiers in all three systems is that they range over the intuitive continuum: all of the sets of natural numbers. But there are also perfectly determinate sub-domains which can serve as domains for the second-order variables in the theories. (The recursive sets comprise the smallest  $\omega$ -model of  $\text{RCA}_0$ , and the arithmetical sets are the smallest  $\omega$ -model of  $\text{ACA}_0$ .)

The indefinite extensibility of the continuum can therefore be reconciled with the meaningfulness and bivalence of quantification over the continuum. Such quantification is to be understood as systematically ambiguous, with the variables ranging over any definite collection of sets of natural numbers which is a model (strictly speaking, the second-order part of a model) of the theory that is being used at the time.

As with Clark’s interpretation of set theory, there will be many distinct

models for what is said about sets (the theory of sets) by any particular speaker on any particular occasion, so the range of quantifiers employed in making such statements must be systematically ambiguous. No systematic elaboration by laying down more and more claims about sets could serve to eliminate totally this ambiguity from a speaker’s discourse.<sup>55</sup>

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<sup>52</sup> Dummett, ‘Chairman’s address’ p. 250. <sup>53</sup> Ibid. <sup>54</sup> Simpson, ‘Predicativity’. <sup>55</sup> Clark, ‘Basic Law V’ p. 247.

Imagine a mathematician who first adopts  $\text{RCA}_0$ , and proves various theorems in the language of analysis, and who then comes to recognize the existence of more sets of natural numbers, and to accept the axioms of  $\text{ACA}_0$ . Formally speaking, she can recognize that the axioms of  $\text{RCA}_0$  are simple consequences (actually just special cases) of her new axioms, and so her old proofs carry over to the new context verbatim. But this is not merely a formal similarity: there is a clear sense in which she is talking about the same things as she was before — the continuum, that is, the sets of natural numbers. She has of course recognized more of its complexity; she now asserts existential statements which she did not previously. And she can now see all of the mathematics she did before as true of (true when interpreted in, by restricting the set quantifiers to range over) some definite sub-continuum. Formally, she can come to grasp the class of recursive sets, and see that that is a model of her old theory.

What she meant before by a universal second-order claim was that every set is thus-and-so. This was something of a wave of the hand or a shot in the dark of course, as it always is when we quantify over an indefinitely extensible domain: we will never know just what sets there are. But it would not be quite right to describe this as equivocation. Some interpretations of her words were adequate for everything that she said before, but are no longer adequate. But our words do not have meaning in virtue of formal interpretations. What she meant by ‘all sets’ is what she still means by ‘all sets’; it is just that now she knows about more of them.

### 5.3.5 Conclusion

Intuitionistic analysis stands in contrast to the predicativist approach to the indefinite extensibility of the continuum that I have just sketched. To put the matter impressionistically, the intuitionist agrees with the predicativist that the continuum is open-ended; but unlike the predicativist, the intuitionist tries to build that open-endedness into her mathematics once and for all. The way that this is done is to consider real numbers as given by choice sequences, either lawlike (given by a rule), or free (supposed to be given by a succession of arbitrary choices). The intuitionist rejection of completed infinite totalities is accommodated by assuming that at any point in time, the real number has only been determined by a finite number of choices, and so can only be said to lie within a finite (rational) interval.

The Brouwerian approach to analysis has been considered somewhat bizarre by

many, and has attracted the scorn even of a constructivist such as Bishop:

In Brouwer's case there seems to have been a nagging suspicion that unless he personally intervened to prevent it the continuum would turn out to be discrete.<sup>56</sup>

I cannot give an assessment of the intuitionistic approach to analysis here. Intuitionistic analysis is based on fundamentally different ideas from classical analysis, and this is why, unlike both Bishop's constructive analysis, and predicativistic analysis, intuitionistic analysis produces results which, at least when read naively, actually contradict theorems of classical analysis. The most famous is of course Brouwer's theorem that every total real-valued function is continuous.

What I have tried to show, however, is that we are by no means forced to adopt such an approach, even if we agree with Dummett that the continuum is indefinitely extensible. Predicativism accommodates that insight, but does not require us to abandon either the classical notion of what a real number is, or classical logic. Nor is intuitionistic logic a relevant alternative for arithmetic. In short: Gödel was right.

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<sup>56</sup> Bishop, *Foundations of Constructive Analysis* p. 6.

## Chapter 6

# The metaphysics behind predicativism

In this chapter, I argue that the view of mathematical ontology which motivates predicativism is an intensional one. There are extensional mathematical objects, such as sets and functions-in-extension (which are standardly represented as sets of ordered argument–value pairs); but these are not primitive — they are derived from intensional objects, namely properties and functions-in-intension.

Functions are to be understood as idealized procedures for producing an output from an input; and properties can be understood as Frege suggested, as functions which take objects as arguments and give True or False as values. Both properties and functions are on the sense side of Frege’s distinction between sense and reference, and it is the idea that senses come first which motivates the Vicious Circle Principle. In brief, an impredicative function-definition is illegitimate because it fails to give the function-symbol a sense; it gives a ‘rule’ which cannot be followed, because it includes itself.

Predicativism has traditionally given a privileged role to the natural numbers. Poincaré and Weyl were both hostile to reductionist accounts of the natural numbers, whether set-theoretic (such as that initiated by Dedekind), or logicist (such as the failed attempts of Frege and Russell). Instead, they suggested that the natural numbers are the fundamental objects of mathematics, and that these are given to us by the intuition of iteration. This is not to say that a position which combined predicativism with set-theoretic reductionism about numbers would be incoherent: but such a

position would lack some of the appeal of the more usual form of predicativism; it would also be further removed from the well-developed and technically convenient systems of higher-order arithmetic which have been the basis of foundational studies since Hilbert.

The natural numbers are the appropriate foundation of mathematics: they are the bedrock beneath which we will not succeed in digging. The natural numbers are understood in what is, in Dummett's terms, a robustly realist way, in that statements concerning them have a fully determinate truth-value. This does not necessarily commit the predicativist to ontological platonism, however: Feferman defends just this combination of anti-platonism with a belief in the determinacy of truth values for arithmetical statements.<sup>1</sup>

The most important feature of the natural numbers is that they obey the principle of mathematical induction with respect to every well-defined property. (To take this as a *definition* of natural number, as the logicians did, is to go too far, according to Poincaré and Weyl; but the applicability of induction is certainly an essential feature of the natural numbers, which follows from their origin in iteration.) The difficult ontological question is therefore: What are the well-defined properties of natural numbers?

It is common to re-frame that question so as to ask instead what *sets* of natural numbers exist. That is, for example, how Simpson phrases the question driving his research into the existential requirements of mathematics.<sup>2</sup> As well as making the technical case that the predicatively specifiable sets are all of the sets which we need, the predicativist should also, if possible, explain why it is that those are all of the sets which there are. The answer which both Russell and Weyl gave to this is that sets are not ontologically basic. They owe what existence they have to the properties from which they are abstracted. Abstraction, here, is not some mysterious mental faculty; it is simply that co-extensionality is taken as deciding identity claims about sets. The ontological issues around sets therefore devolve onto those around properties.

In this chapter, I will explore the ontological issues around properties and sets, with particular attention to Weyl (in his predicativist phase) and Russell (in the period around the time of the first edition of *Principia*). Russell and Weyl agreed that properties are ontologically basic to mathematics, and are subject to a Vicious Circle

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<sup>1</sup> See the quotation on p. 110 above.    <sup>2</sup> 'We are especially interested in the question of which set existence axioms are needed to prove the known theorems of mathematics.' Simpson, *Subsystems of Second Order Arithmetic* p. 1.

Principle. Russell was unable to accept the consequence of this, that a predicativity constraint applies to the whole of mathematics, as this would dash his hopes for a logicist justification of classical mathematics, by preventing both the derivation of arithmetic from logic, and the derivation of classical analysis from arithmetic. Russell therefore postulated the Axiom of Reducibility, which has the effect of side-stepping the predicativity requirement in extensional contexts such as mathematics.

In the first two sections of this chapter, I discuss Russell, and the criticisms of ramification and of the Axiom of Reducibility. I then turn to Weyl's views on properties, and on sets. §6.5 draws on some of the ideas of Ch. 5 in discussing the impact of this on quantification; and §6.6 explores the idea that sets are dependent on their defining properties.

## 6.1 Russell and ramification

Weyl's predicativism is in many ways very close to that of Russell in the *Principia*. *Das Kontinuum* can be seen as a successful attempt to carry out honestly a modest part of the ambitious programme which *Principia* had set itself and had managed to fulfil only by the most unsatisfactory means. Weyl regarded the central part of the logicist programme, the attempt to give a purely logical foundation to arithmetic, as unnecessary and unworkable, and so he took the natural numbers as given; he saw Russell's vindication of classical analysis to be vitiated by the unjustified Axiom of Reducibility, and so he set out to see how much of analysis could be won by honest toil.

The fundamental similarity is that Russell and Weyl are both intensionalists, in that they take items which are intrinsically meaningful to be primary. The basic ontology of both consists, first, of a domain of non-logical individuals, which are wholly objective (and, presumably, not meaning-bearing items); and then of properties of and relations between those individuals; and then of higher-order properties and relations. Properties and relations are called 'propositional functions' by Russell, 'judgement schemes' by Weyl. They are unlike Frege's 'concepts' in that what might be called their sense — the condition which an object (or sequence of objects) must satisfy, in order for the property (or relation) to be true of it (or them) — is internal

to them.<sup>3</sup> They are therefore not extensional, as different properties may happen to apply to just the same objects; and they are not all logically simple, as some will involve specific objects or (other) properties, and some will involve quantification over objects or properties of some order. This ‘involvement’ will necessarily be well-founded. The meaning of a property which involves quantification is dependent on the range of the quantifier, and from this follows immediately the fact that properties obey the Vicious Circle Principle. The VCP is a requirement of *sense*.

Goldfarb argues that this is what motivates Russell’s acceptance of the VCP, and what unifies his apparently distinct phrasings of it:

One formulation [of VCP] is ‘no totality may contain members that are definable only in terms of that totality’; the others use ‘presuppose’ and ‘involve’ instead of ‘are definable only in terms of’. Gödel points out that, *prima facie*, these are three distinct principles, and he claims that only the first yields ramification, whereas only the second and third are plausible without recourse to constructivism [...]. But if definitions are not external to the entities under consideration, as they are to classes but are not [...] to propositions and propositional functions, then the distinction among these formulations seems to collapse.<sup>4</sup>

Gödel was of course aware that Russell’s project was about intensional entities; but the views Gödel expresses about such properties are robustly realist, and he explicitly rejects the idea that definitions are internal to them: ‘concepts may [...] be conceived as real objects [...] as the properties and relations of things existing independently of our definitions and constructions.’<sup>5</sup>

Gödel explicitly adopts the word ‘concept’ for properties understood in this strongly realist way, and uses the word ‘notion’ for other purposes, particularly for Russell’s ‘constructivist’ conception of property. It should also be mentioned that Gödel’s use of terms such as ‘construction’ and ‘constructivism’ can mislead: in the note added to reprints of his paper in 1964 and (more fully) in 1972, Gödel sought to clarify that he attributes to Russell ‘a strictly nominalistic kind of constructivism [...] (which might better be called fictionalism)’, and which is quite distinct ‘from both

<sup>3</sup> The ‘value’ of a propositional function is for Russell a proposition, rather than a truth-value; and propositions are an independent part of Russell’s ontology; however, this will not be of importance for my purposes. <sup>4</sup> Goldfarb, ‘Russell’s reasons for ramification’ pp. 32–33. The article Goldfarb refers to is Gödel, ‘Russell’s mathematical logic [1944]’. <sup>5</sup> Ibid. p. 128.

‘intuitionistically admissible’ and ‘constructive’ in the sense of the Hilbert school.<sup>6</sup> Goldfarb therefore misrepresents Gödel in writing that Gödel ‘make[s] Russell out to have a general vision of what the existence of abstract entities comes to, and thus to be adopting constructivism as a fundamental stance toward ontology. That does not seem accurate to Russell.’<sup>7</sup> Gödel is well aware that Russell had (as Goldfarb puts it) only one, ‘full-blooded’ conception of existence. The issue is Russell’s desire for nominalistic reductions, which would limit the class of entities for which full-blooded existence must be assumed.

Gödel seems to suggest that Russell’s position in *Principia* was nominalistic, not only about classes, but also about properties. Gödel makes it clear that his strongly realist notion of ‘concept’, in contrast to Russell’s propositional functions, permits impredicative specifications:

Since concepts are supposed to exist objectively, there seems to be objection neither to speaking of all of them [...] nor to describing some of them by reference to all (or at least all of a given type). But, one may ask, isn’t this view refutable also for concepts because it leads to the ‘absurdity’ that there will exist properties  $\phi$  such that  $\phi(a)$  consists in a certain state of affairs involving all properties (including  $\phi$  itself and properties defined in terms of  $\phi$ ), which would mean that the vicious circle principle does not hold even in its second form for concepts or propositions? There is no doubt that the totality of all properties [...] does lead to situations of this kind, but I don’t think they contain any absurdity. [fn omitted] It is true that such properties  $\phi$  [...] will have to contain themselves as constituents of their content (or of their meaning) [...]; but this only makes it impossible to construct their meaning (i.e. explain it as an assertion about sense perceptions or any other non-conceptual entities), which is no objection for one who takes the realistic standpoint.<sup>8</sup>

A comparison suggests itself here with Ramsey’s endorsement of propositional functions understood extensionally, as possibly infinite truth-functions. Ramsey

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<sup>6</sup> Gödel, *Collected Works* II, p. 119.    <sup>7</sup> Goldfarb, ‘Russell’s reasons for ramification’ p. 26.    <sup>8</sup> Gödel, ‘Russell’s mathematical logic [1944]’ p. 139.

compared such self-containing propositional functions with a conjunction such as:

$$\bigwedge \{p, q, p \& q, p \vee q\}$$

which can be seen to evaluate harmlessly to one of its constituents, the third conjunct  $p \& q$ .<sup>9</sup> But of course the crucial word here is ‘evaluate’: Ramsey is urging us to look at functions (truth-functions and propositional functions) extensionally. In that case, it is not that the function contains itself, it is merely our expression for it which does so; or rather, one of our expressions for the function contains another expression for the function. Gödel’s suggestion seems to be instead that a concept, as an item of objective reality, may contain itself, which seems to prevent our explaining its meaning in any non-circular terms.

While it is clear that Russell’s outlook was not extensional, *Principia* is unforthcoming about just what propositional functions are supposed to be. Because *Principia* is an attempt to show that there is nothing more to mathematics than logic, it is not seen as relevant to the project to tell us what the elementary propositional functions are: that, presumably, is a task for a more general philosophical enquiry (or Tractarian ‘analysis’), and may well depend on empirical facts about what there happens to be in the world. Similarly, the individuals at the bottom of Russell’s ramified hierarchy are the furniture of the real world, whatever that happens to be; though there had better turn out to be enough of it for Russell’s purposes, a need which prompted the (quasi-empirical) Axiom of Infinity. This is all a far cry from Weyl’s explicitness on these matters, as we shall see: on his view, the intuition of iteration gives us the natural numbers as a domain of objects by means of the primitive relation of immediate succession, and allows the definition of further relations by recursion.

Non-elementary propositional functions are distinct from elementary functions in that they involve quantification. But this is not to say that propositional functions are linguistic items, which may have quantifiers as syntactic elements. Indeed, the Axiom of Reducibility (to which we turn in the next section) would lead to the heterological paradox if it were further assumed that to every propositional function there corresponds an individual which names it.<sup>10</sup> Moreover, since Russell’s development of his theory of definite descriptions, he was well aware that surface syntax may conceal hidden quantifiers.

<sup>9</sup> Ramsey, ‘Foundations’ p. 204. <sup>10</sup> Chwistek presents just this as a contradiction in the system of *Principia* with the Axiom of Reducibility: Ramsey shows the dependence of this argument on the assumption of nameability, and concludes that this assumption is unwarranted.

Russell famously thought that the proposition *that Mont Blanc is more than 4000 metres high* has the mountain itself as a constituent; and Goldfarb suggests that the requirement of predicativity is to be understood by Russell's seeing 'the variable' as a real constituent of quantified propositions or propositional functions:

[...] Russell takes a variable to presuppose the full extent of its range. Now, since the identity of a proposition or propositional function depends on the identity of the variables it contains, and the variables presuppose their range of variation, even the weakest form of the vicious-circle principle suffices to yield ramification. Indeed, Russell may perhaps even think of the variables as *containing* all the entities over which it ranges; in that case, the only principle needed is that a complex entity cannot contain itself as a proper part.<sup>11</sup>

As we saw above, this principle is not one which Gödel saw a need to endorse for his 'concepts'. But it is very hard to see how the obtaining of states of affairs which were ill-founded in this way could be knowable (or even entertainable) by finite creatures. While Gödel's realism may not be *absurd*, it is clear that there is a strong motivation to restrict ourselves first to the study of that part of the world which can potentially be grasped by minds like ours. Whether we can intelligibly go further remains in doubt; but whether we need to go further, in order to obtain a satisfactory system of mathematics, is something which an investigation of the scope of predicative mathematics will be able, at least in part, to settle. We will return to this matter in chapter 7.

## 6.2 Russell and Reducibility

One difference to be noted between *Principia* and *Das Kontinuum* is that Weyl's conception of sets as constructions or abstractions from properties is very different from Russell's conception of 'classes.' Whereas for Weyl, sets were pleonastic, for Russell, classes — if they existed at all — were full-bloodedly real. The 'no-class' position officially taken in *Principia* is one of agnosticism as to the existence of classes rather than a denial of their existence. Symbolism for class abstraction and membership features prominently in *Principia*, but this is not to be taken at face-value: class symbolism is explained away by the doctrine of incomplete symbols, and

<sup>11</sup> Goldfarb, 'Russell's reasons for ramification' p. 37, emphasis in original.

the meaning-in-use they are given is identical with that of Weyl: such symbolism is an extensional way of talking about intensional objects, the properties.

But Russell maintained that there was a further, substantive question as to whether classes really existed. This is brought out most clearly when Russell discusses the Axiom of Reducibility. The axiom has the effect of undoing, for mathematical (or other extensional) purposes, the ramified predicativism of the *Principia*, by asserting that every propositional function is extensionally equivalent to a predicative propositional function (that is, one which does not involve quantification over a type equal to or higher than its argument). For example — the crucial example for the logicist programme — the principle of arithmetical induction, when formulated so as to obey the ramified type restrictions, applies only to the predicative properties; but if we assume Reducibility, any property is coextensive with some predicative property, and so induction is a strong enough principle to allow the *definition* of the natural numbers as those objects which satisfy it. Russell writes that the hypothesis of the existence of classes would be a justification of Reducibility:

we must find, if possible, some method of reducing the order of a propositional function without affecting the truth or falsehood of its values. This seems to be what common sense effects by the admission of *classes*. Given any propositional function,  $\phi x$ , of whatever order, this is assumed to be equivalent, for all values of  $x$ , to a statement of the form ‘ $x$  belongs to the class  $\alpha$ ’. Now this statement is of the first order, since it makes no allusion to ‘all functions of such-and-such a type’. Indeed its only practical advantage over the original statement  $\phi x$  is that it is of the first order. There is no advantage in assuming that there really are such things as classes, and the contradiction about the classes which are not members of themselves show that, if there are classes, they must be something radically different from individuals. I believe the chief purpose which classes serve, and the chief reason which makes them linguistically convenient, is that they provide a method of reducing the order of a propositional function. I shall, therefore, not assume anything of what may seem to be involved in the common-sense admission of classes, except this: that every propositional function is

equivalent, for all its values, to some predicative function.<sup>12</sup>

The argument here is that the existence of a class  $\alpha$  of the objects satisfying a propositional function  $\phi\hat{x}$  would entail that there is an elementary propositional function, namely  $\hat{x} \in \alpha$ , which is extensionally equivalent to  $\phi\hat{x}$ . Therefore, if there is a class corresponding to every propositional function, then the Axiom of Reducibility holds.

But Russell does not argue for Reducibility by claiming that there really are classes corresponding to every propositional function, and then applying modus ponens to get the desired conclusion. It seems that the line of thought is something more like: we have good regressive reason to believe Reducibility to be true; one explanation of that would be the existence of classes; but it is ontologically more modest to take Reducibility as an axiom than it is to assume the existence of classes and derive Reducibility. We seem to have here an example of what might be called inference to a more modest explanation, rather than to the best; or indeed, sceptics might suggest, of inference to a non-explanation.

Russell's position has not been well-received by subsequent writers. Church sums up the critical consensus thus:

[...] as many have urged, [fn omitted, citing Ramsey, Chwistek and Carnap] the true choice would seem to be between the simple [i.e. extensional, impredicative] functional calculi and the ramified functional calculi without axioms of reducibility. It is hard to think of a point of view from which the intermediate position represented by the ramified functional calculi with axioms of reducibility would appear to be significant.<sup>13</sup>

The point of view which seems to have motivated Russell's intermediate position is that propositions and propositional functions are complex, structured entities, the structure and components of which are tightly bound up with our ability to grasp or judge them; whereas classes (if there are any) are something like individuals — ontologically weighty, simple items which may be among the ultimate constituents of propositions. It is wrong to think that a class which we specify as the extension

<sup>12</sup> Russell, 'Theory of types' p. 167. This passage appears almost unchanged in Whitehead and Russell, *Principia Mathematica* I, p. 166. <sup>13</sup> Church, *Introduction to Mathematical Logic* p. 355.

of an impredicative propositional function somehow contains or hides that logical complexity within it: as the class is a wholly extensional object, it is a simple fact that the class has the members it does; or, more precisely, it is an elementary proposition (an atomic fact) that a given object belongs to the class.

So it would be theft, not honest toil, for Russell to postulate the existence of classes; whereas propositional functions, being ontologically thinner, could be postulated without undue immodesty. Or so we might at first think. But of course the thinness of propositional functions comes from their being complexes of pre-existing entities. And what Russell needs to justify Reducibility is that there are enough of such complexes — enough realized combinatorial possibilities. If we think of such complexes as language-like, even in a broad sense, then they will be finite sequences of a finite (or at most countably infinite) alphabet of objects: in which case there will not be enough to validate Reducibility. But what else could they be?

There is something curiously half-hearted about Russell's attitude to classes and the Axiom of Reducibility: he adopts the axiom, and provides regressive arguments for it (it has desirable consequences which cannot otherwise be obtained so simply); he also considers an explanation for the axiom (namely the existence of classes) which he does not endorse, but the *possibility* of which is supposed to provide additional evidence for the axiom. It is hard to see why regressive reasoning, if it is able to take us as far as the axiom, is unable to take us a little further, to an explanatory basis for it; except that, as Russell remarks in the passage quoted above, the contradictions show that developing a theory of real classes is not a wholly straightforward matter. In any case, it seems that Russell's primary reasons for adopting the Axiom of Reducibility are regressive, and so can be fully assessed only when we have considered how much of classical mathematics — in particular, how much of that part which is applied in the natural sciences — can be justified by predicatively acceptable means.

The ramified hierarchy was Russell's solution of the paradoxes; and it provides a unified explanation of what were later seen as two distinct sorts of paradox: the semantic, and the set-theoretic. Ramsey was prominent in arguing for their distinct natures:<sup>14</sup> he suggested that mathematics was thoroughly extensional in character, and so that the mathematical (i.e. class-theoretic) paradoxes were best dealt with in the simplest way, by a simple (unramified) theory of types, which is legitimate (on Ramsey's view, as well as Russell's) if we take it that classes are real objects.

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<sup>14</sup> Ramsey, 'Foundations'. Peano had suggested such a distinction earlier.

The semantic paradoxes relied on notions such as ‘truth,’ ‘meaning,’ and ‘definition,’ which were alien to mathematics and should be dealt with by a distinct philosophical examination of those problematic notions (as in, e.g. Tarski’s subsequent work).

Ramsey’s case was a strong one, because, as mentioned, Russell found it necessary to assume the Axiom of Reducibility in order to allow the definition of natural number and the derivation of analysis in *Principia*: as it is ramification which causes the problems that Reducibility then has to solve, it seems that we are first splitting propositional functions into different levels, and then lumping them (strictly speaking, their extensional equivalents) all together again in the lowest level. Ramsey suggested that this is an unnecessary shuffle which we can avoid by not ramifying in the first place.

Now it should be noted that the Axiom of Reducibility, while serving to collapse the effect of ramification in many contexts, does not vitiate the solution to the semantic paradoxes. As Russell explains,

The essential point is that such results [consequences of the Axiom of Reducibility] are obtained in all cases where only the truth or falsehood of values of the functions concerned are relevant, as is invariably the case in mathematics. [...] It might be thought that the paradoxes for the sake of which we invented the hierarchy of types would now reappear. But this is not the case, because, in such paradoxes, either something beyond the truth or falsehood of values of functions is relevant, or expressions occur which are unmeaning even after the introduction of the axiom of reducibility. For example, such a statement as ‘Epimenides asserts  $\psi x$ ’ is not equivalent to ‘Epimenides asserts  $\phi!x$ ’, even though  $\psi x$  and  $\phi!x$  are equivalent.<sup>15</sup>

But as Ramsey pointed out (and, as the above quotation shows, Russell seems to have been well aware), the *mathematical* project could be carried out without the detour, by simply postulating classes. Russell’s procedure is to build up the ramified hierarchy of propositional functions in accordance with the constraints of meaning, but also to assume that there are uncountably many predicative propositional functions, with who knows what *meanings*, but with, it just so happens, the extensions of every one of the functions further out in the hierarchy. The procedure is, as Quine

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<sup>15</sup> Russell, ‘Theory of types’ p. 168.

comments, ‘oddly devious’.

The Axiom of Reducibility was undoubtedly forced on Russell: his regressive argument for it is that its consequences (in the presence of the other axioms of the *Principia*), are the theorems of ordinary arithmetic and analysis, which we took to be true all along, but for which we lacked a satisfactory axiomatic foundation. But of course for Russell, ‘satisfactory’ here must mean something like: plausibly logically true (and therefore necessarily containing no non-logical vocabulary). And Russell is such a strong realist about logic that not only the Axiom of Infinity (which asserts the infinitude of the individuals) but also the Axiom of Reducibility (which entails the existence of all of the uncountably many predicative propositional functions) can count as logical truths.

Russell later came to recognize, at Wittgenstein’s urging, the need to distinguish between a logical truth, and a truth which can be expressed in purely logical vocabulary.<sup>16</sup> Ramsey advocated Wittgenstein’s conception of tautology as an analysis of the idea of logical truth, against which the success of the logicist programme could be judged. A tautology, for Wittgenstein, is a degenerate (constant) truth function of atomic propositions. The Axiom of Reducibility does not meet this test, as Ramsey showed by constructing a countermodel.<sup>17</sup> Given all this, there seems little reason to disagree with Church’s assessment.

### 6.3 Weyl’s account of properties

During the period in which he wrote *Das Kontinuum* (1918) and *Der circulus vitiosus* (1919), Weyl was a predicativist; a constructivist of a sort, but not one with an objection to classical propositional logic. He believed that mathematical properties are at root intensional items: that their sense is logically prior to extension. (In this, he cites Husserl and Fichte as forebears. Frege’s ‘concepts,’ by contrast, are extensional items; senses of concepts serve only to pick out the concepts themselves.) And he saw modern analysis as having gone against this, in adopting a wholly extensional viewpoint which concealed a circularity just as vicious as that in Frege’s set theory. For a sense to be a sense for us, graspable by finite minds like ours, it must itself be finite; and that means constructed by a finite process from an intuitive foundation.

<sup>16</sup> Russell, *Introduction to Mathematical Philosophy* p. 205. <sup>17</sup> See Potter, *Reason’s Nearest Kin* pp. 160–161 for discussion of Wittgenstein’s obscure countermodel, and Ramsey’s clearer example.

That intuitive foundation consists of what Weyl calls primitive relations.

Weyl takes it that there are certain basic categories of objects given to us in intuition, and certain 'immediately given' properties of and relations between these objects. These relations are 'immediately given' to us in the sense that given such an  $n$ -place relation and  $n$  objects, we can tell non-inferentially whether or not the relation holds between the objects.

Weyl's main example of such a domain is the category of the natural numbers, with which is given the single immediately given relation of immediate succession. (Every branch of mathematics is, he says, concerned with this category.) Worth noting about the naturals is the fact that they are individuals; they are not homogeneous (as, for example, the points in Euclidean space are): each natural number is distinguishable from the others by means of the primitive relation of immediate succession. Names for the naturals are therefore eliminable in favour of canonical descriptions. (Zero is that which is not an immediate successor of anything; one is the immediate successor of zero; and so on.) There is nothing more to the natural numbers than is given by these canonical descriptions. (Weyl suggests that mathematics may be characterized as the branch of science dealing exclusively with general propositions, i.e. those not involving names.)

The 'primitive relations' are the immediately given relations, together with identity. It is interesting both that Weyl thinks that identity between the objects of a basic category is not (in general) immediately given; and that he thinks that, nonetheless, it can serve as one of the primitive relations from which mathematics is built up. Weyl's classification of identity is presumably motivated by the thought that it is a logical relation, and that the axioms of identity should count as logical truths, applicable to every category of objects we might study. (It might be thought that identity claims for a category of homogeneous objects are problematic; but if we are 'given' an object from such a category, then we ought (for a reasonable definition of 'given') to be able to tell if a second given object from that category is distinct. It would seem reasonable to say, for instance, that if we are to be 'given' two points in Euclidean space, they should be given to us in such a way that it is immediately clear whether or not they are distinct; and this is so even though there is no geometrical property to single out either one.)

'The mathematical process' is the building up of judgement schemes (i.e., judgements, properties and relations) from these primitive properties; and also the forma-

tion of sets as extensional object-correlates of properties and relations. By building up, Weyl means what we would now call recursion or induction: he gives a base class of primitive judgement schemes, and some operations or construction principles which turn judgement schemes into (more complex) judgement schemes. Everything reached from the base class by a finite process of applying these operations to primitive judgement schemes is a judgement scheme; and nothing else is.

Weyl's construction of judgement schemes comes to much the same thing as the inductive characterization of first-order formulae that we see in logic textbooks today, on the basis of atomic formulae. The difference is that the now-common way of viewing such an inductive definition as a transitive closure is quite alien to the predicativist enterprise. Inductive definitions are often explained today by saying that the set we want to characterize, the 'closure' of the base under the operations, is the intersection of all sets which contain the base and are closed under the operations. This presupposes a meta-theory, in which the 'construction' takes place, which would already be strong enough to do most of mathematics. To formalize the idea of taking the intersection of all sets closed under the operations, we need a background either of set theory with an axiom of infinity, or of full (impredicative) second-order logic. For Weyl, the construction process is based on the primitive intuition of iteration which underlies our arithmetical knowledge; it certainly does not, in his view, require operations such as taking the intersection of infinite sets. Weyl's stance here is supported by the observation that such inductive definitions can be formalized in systems weaker than full second-order logic, such as ancestral logic.<sup>18</sup> Once again, it seems that we can get what we need with less than we might at first think.

The base for Weyl's construction consists of the primitive relations. The formation rules are the standard logical operations (conjunction and disjunction, negation, the identification of free variables, the substitution of a function-term or the name of an object for a free variable, and the binding of a free variable with an existential quantifier), together with the 'principle of iteration' (i.e. the definition of new properties from old by means of recursion over the natural numbers).

This last is significant. The inclusion of definition by recursion as one of the ways of forming new properties shows clearly that, as mentioned above, Weyl's view

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<sup>18</sup> See e.g. Shapiro, *Foundations*, and also Ch. 4.2.5 on the ancestral.

of arithmetic is at odds with the axiomatic approach introduced by Dedekind: like Poincaré, Weyl was scornful of the idea that arithmetic could in any meaningful sense be 'reduced' to logic or set theory:

A set-theoretic treatment of the natural numbers such as offered in Dedekind (1888) may indeed contribute to the systematization of mathematics; [fn omitted] but it must not be allowed to obscure the fact that our grasp of the basic concepts of set theory depends on a prior intuition of iteration and of the sequence of natural numbers.<sup>19</sup>

It is interesting in its own right, and I think also helpful in understanding Weyl's views on properties to look at a little of the historical context of those views. Weyl tells us<sup>20</sup> that his search for a precise characterization of the concept of a mathematical property, which culminated in the predicativist view of *Das Kontinuum*, was initially prompted by a deficiency in Zermelo's 1908 axiomatization of set theory. Zermelo framed the Separation scheme by saying that for any given set  $a$ , and any 'definite *Klassenaussage*' ('definite property') we care to consider, there exists a subset containing just those elements of  $a$  which satisfy that property. Zermelo's explanation of what it is for a property to be *definit* was rather hazy:

A question or assertion  $\mathfrak{E}$  is said to be definite [*definit*] if the fundamental relations of the domain, by means of the axioms and the universally valid laws of logic, determine without arbitrariness whether it holds or not. Likewise a 'propositional function' [*Klassenaussage*]  $\mathfrak{E}(x)$ , in which the variable term  $x$  ranges over all individuals of a class  $\mathfrak{K}$ , is said to be *definit* if it is *definit* for *each single* individual  $x$  of the class  $\mathfrak{K}$ . Thus the question whether  $a \varepsilon b$  or not is always definite, as is the question whether  $M \varepsilon N$  or not.<sup>21</sup>

While this might be adequate for practical mathematical purposes, it was certainly not precise enough for the purpose of metamathematical investigations, nor does

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<sup>19</sup> Weyl, *The Continuum* I.§ 4, p. 24. (References in this and the following sections are to *The Continuum* unless otherwise stated.) The principle of iteration in the form stated by Weyl, applied to functions from sets to sets, in fact takes us outside the arithmetic sets of natural numbers: this seems to be a simple mistake. Allowing the definition of functions on the natural numbers by primitive recursion suffices for Weyl's purposes. See Feferman, 'Weyl vindicated' for technical details. <sup>20</sup> I.§ 8, p. 48

<sup>21</sup> Zermelo, 'Untersuchungen' p. 201. I use '*definit*' to avoid any potential confusion with notions around indefinite extensibility.

it stand much philosophical scrutiny. The problem was, then, to give a satisfactory explanation of what the requirement that a property be *definit* amounted to.

Weyl published a paper suggesting an answer to this in 1910.<sup>22</sup> He suggested that a property is *definit* if it can be constructed from the basic relations of identity and membership by finitely many applications of basic logical operations; the account is much the same as that of properties in *Das Kontinuum* as sketched above. Weyl's discussion of this story<sup>23</sup> suggests that what he found unsatisfactory about his 1910 proposal was its dependence on the notion of finiteness, which would make the analysis of finiteness (and hence arithmetic) in terms of set theory circular. This led to Weyl's insight that the natural numbers are fundamental to mathematics, and cannot be reductively analysed by set theory.

The solution which eventually won acceptance in standard modern axiomatic set theory (ZF and its cousins) was Skolem's: a property is *definit* if it can be expressed by a sentence of the language of set theory (i.e. first-order predicate calculus with equality, with the single non-logical relation-symbol  $\in$ ). This is in effect equivalent to Weyl's 1910 proposal, as is the alternative approach of treating properties axiomatically (as predicative 'classes') in the set-class theory NBG.<sup>24</sup>

But the fact that Weyl explicates the concept of *definit* property mathematically, rather than settling it behind the scenes in the logic as Skolem does, is significant. Most important for our purposes is that it is the axiom schema of Separation that is the location of the formal impredicativity of standard axiomatic set theory. It is Separation that allows us to show, for any hypothesized enumeration  $f$  of  $\mathcal{P}(\omega)$ , the existence of the diagonal set  $D = \{n \in \omega \mid n \notin f(n)\}$  of natural numbers which is not in the enumeration. But if we take seriously the idea that the property in question (the property which separates  $D$  from  $\omega$ ) has been *built up* from other properties and relations, including  $f$ , then we might well wonder whether it is that we have specified a set of natural numbers in terms of  $f$ , which until now we had not been able to specify; or if instead, by reflecting on  $f$ , we have been led to a broader concept 'set of natural numbers' than we had before. The first is the impredicative

<sup>22</sup> Weyl, 'Über die Definitionen'. <sup>23</sup> I. § 8, p. 48 <sup>24</sup> NBG is predicative only in respect of its classes: it is a conservative extension of (impredicative) ZF. Von Neumann is the originator of this approach to *definit*, and his name provides the initial 'N'. As Gödel (the 'G') put it, 'Classes are what appear in Zermelo's formulation as 'definite Eigenschaften' [*definit* properties]. However, in the system  $\Sigma$  [i.e. NBG] (unlike Zermelo's) it is stated explicitly by a special group of axioms ... how *definite Eigenschaften* are to be constructed.' Gödel, *The Consistency of the Axiom of Choice* p. 2.

conception, according to which all of the subsets of the natural numbers, including  $D$ , exist independently of their defining properties, on the same logical level. The second, the predicative conception, is that because  $D$  is defined in terms of  $f$ , it must come later than  $f$  (and so later than those subsets of  $\omega$  which are the values of  $f$ ) in the process of building up sets. That is to say, there is no point in the process at which we have built what the impredicative set theorist would call the full powerset of the natural numbers.

My suggestion is that taking seriously the idea that *definit* properties are built up in a recursive process is what led Weyl from his 1910 position to the predicativism of *Das Kontinuum*, according to which there are the sets of natural numbers of level 0, specified by arithmetic properties; and then sets of naturals of level 1, specified by properties which may involve arithmetic operations and quantification over the sets of level 0; and so on; but on which there is no assumption of an overarching domain of the sets of natural numbers of any level whatsoever. (In fact, in *Das Kontinuum* Weyl recommends restricting our attention to the sets of level 0, so as to avoid the inconvenience of different types of real numbers.)

All three analyses of *definit* property — Skolem's, von Neumann's, and Weyl's 1910 proposal — are impredicative. Weyl's 1910 suggestion was in terms of applications of logical operations to primitive relations; but this cannot be thought of as 'building up' these properties if we also think of the sets as being formed in parallel with the properties, because among the operations is unbounded quantification over the sets. In order to 'construct' the sets of impredicative set theory we would need already to understand quantification over all of the sets.

## 6.4 Sets as dependent objects

Weyl introduces one- and multi-dimensional sets as extensional object-correlates of properties and relations. (An  $n$ -dimensional set is of course a set of ordered  $n$ -tuples. The now-standard set-theoretic reduction of ordered  $n$ -tuples to sets, developed by Kuratowski and Wiener, would be most unnatural in a typed system such as Weyl's.)

Functions are not simply appropriate multi-dimensional sets — to allow functions of sets, Weyl is slightly more permissive, allowing  $\in$  to appear in the definition of a functional relation. Of course, quantification over sets, and also identity claims between sets are ruled out in function definitions: so the sets taken as values are

always a definable set in the old sense.<sup>25</sup>

Weyl describes the sets as ‘a new derived system of ideal objects over and above the given primitive domain of objects. [...] Obviously, these new objects, the sets, are altogether different from the primitive objects; they belong to an entirely separate sphere of existence.’<sup>26</sup> The significance of placing sets in a ‘separate sphere’ is that they are not among the potential values of the variables which range over the primitive domain of objects.

Sets are formed by abstraction on properties:

[...] the transition from the ‘property’ to the ‘set’ (of those things which have the property) signifies merely that one brings to bear the *objective* rather than the purely logical point of view, i.e., one regards *objective* correspondence (that is, ‘relation in extension’ as logicians say) established entirely on the basis of acquaintance with the relevant objects as decisive rather than logical equivalence.<sup>27</sup>

The ‘acquaintance with the relevant objects’ is here of course merely the verification of whether or not each object has the property under consideration; and by logical equivalence, Weyl means the validity of the biconditional, where a ‘valid’ proposition is one which requires no such verification; i.e. a proposition true under every interpretation of the names and relation-symbols involved.<sup>28</sup>

As Feferman says,

Weyl describes this as the logical [predicative] conception of sets as opposed to the objective [platonistic] conception. Furthermore, under his conception there is no notion of set independent of given basic domains and relations.<sup>29</sup>

To summarize: for Weyl, sets (including multidimensional sets, among them functions) are ideal objects, derived from their members in an essentially pleonastic way: there is nothing more to their identity than the coextensionality of the relations which define them; and they are certainly not items of the same category as their members, the *relata*. Like the modern set-theorist’s classes, Weyl’s sets are really just an extensional way of looking at properties.

<sup>25</sup> For a discussion of the details of Weyl’s formal system, see Feferman, ‘Weyl vindicated’. <sup>26</sup> I.§ 4, p. 22 <sup>27</sup> I.§4, p. 23 <sup>28</sup> See Pollard, Introduction to Weyl, *The Continuum* p. xviii for support for this reading. <sup>29</sup> Feferman, ‘Weyl vindicated’. (Material in brackets Feferman’s.)

The question of ontological commitment therefore raises its head. It seems that on this conception of sets as dependent abstract objects, we do not need to take talk of sets as any more ontologically committing than we do talk of whereabouts, for example. To account for our talk of whereabouts, we do not need to know more about them, or to suppose that there is anything more to them than this abstraction principle: your whereabouts are the same as my whereabouts iff we are in the same place. For sets, the equivalence relation in the abstraction principle is coextensionality of the defining properties. This is really just Frege's infamous Basic Law V: but with the crucial difference that here it is understood predicatively, so that the sets which are introduced to us by abstraction on properties are items of a different logical type from those which have or lack those properties; so type restrictions rule out any question of sets being members of themselves.

More recently, interest in impredicative abstraction principles has been revived by the neo-logicist programme of Hale and Wright.<sup>30</sup> The motivating idea is that we should take Hume's Principle (the abstraction principle for cardinal equivalence) as serving to introduce us to new objects, the natural numbers. Numbers are supposed to be objects in the full, old sense, and so Hume's Principle is supposed to be taken impredicatively, that is, with the numbers we are introduced to on the left-hand side of the principle falling under the first-order quantifiers on the right-hand side. This is essential for the proof that Hume's Principle entails the existence of all of the natural numbers. The legitimacy of impredicative abstraction has been much disputed, but that of predicative set abstraction is not in doubt; as Hacking argues, it has a good claim to be counted as logic.<sup>31</sup>

Parsons<sup>32</sup> has argued that a substitutional interpretation of the quantifiers suffices to give the intuitively correct truth conditions for sentences involving quantification over predicative sets. The substitution class is of course open sentences with one free object variable (and no bound class variables). Parsons suggests that this can be seen as an analysis of the thinner sense of existence in which such sets may be said to exist; thinner, that is, than the full-blooded sense of existence which is conveyed by the standard objectual quantifiers, and which is the only sense of existence recognized by Quine (following Russell). But reductions always cut both ways: rather than taking the substitutional analysis as showing that we may harmlessly suppose sets to exist,

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<sup>30</sup> See e.g. the papers collected in Hale and Wright, *The Reason's Proper Study*. <sup>31</sup> Hacking, 'What is logic?'. <sup>32</sup> Parsons, 'Substitutional quantification'.

we may just as well be inclined (perhaps by Quinean considerations of economy and the univocality of existence) to say that it shows that sets do not really exist.

An alternative for the predicativist which is in the spirit of Parsons' plea (and which both preserves an extensional logic, and is more in keeping with attractive formalisms for predicativist mathematics such as  $\text{ACA}_0$ ) is to accept sets and instead to eliminate properties, by means of Skolem's open-sentence analysis of *definit* property. In  $\text{ACA}_0$ , sets are items of a different type from individuals (which in the intended interpretation are natural numbers); and they are given to us by means of a comprehension axiom:

$$\exists X \forall x (x \in X \leftrightarrow \phi(x)),$$

where  $\phi(x)$  is an open sentence containing no bound set (upper-case) variables, and not containing  $X$  free. (It is the prohibition on bound set variables which makes this predicatively acceptable, of course.) Just as with the classes of NBG, this can be presented so as to give it a more constructive appearance, by giving, instead of a comprehension scheme, a finite number of axioms for basic sets (the empty set, singletons of numbers) and permissible operations on them (complementation, pairwise union and intersection, projection, etc). This was Gödel's method in his presentation of NBG; but it is just a different route to the same destination, as the operations on sets correspond closely to the way logically complex sentences are built up by means of the logical constants.

There is, in short, a certain amount of ontological slack in the pair of ideas *property* and *set*, and the question of which to pull taut is in part a pragmatic, or even just a presentational issue. What is important — indeed, what is central to predicativism — is the recognition that both sets and properties are open-ended. As such, any (predicatively acceptable) collection of means for defining sets or properties is always incomplete. And expansion of the first-order part of the language (or, in the more constructive presentation, expansion of the permitted set-building operations) will give rise to new sets. As we will see below, it is for this reason that Weyl denies that sentences which quantify over sets express genuine propositions.

## 6.5 Quantification over open domains

As mentioned above, Weyl takes the natural numbers for granted as a basic domain of objects, which serve as the foundation for mathematics. What that ‘taking for granted’ comes to is that (classical) quantification over the natural numbers is always intelligible. Weyl does not think that this will be the case for every domain (‘category’) of objects: it is not the case, in particular, for the sets of natural numbers. What, then is required of a given domain to make quantification over it intelligible? Weyl writes that:

*existential judgments* play an essential role in mathematics. The concept of existence is overburdened by metaphysical enigmas. Luckily, however, for our current purposes we need only assume the following. If, say  $P(x)$ ,  $P'(x)$ , [...] are among the judgment schemes ( $R$ ) which apply to objects of our chosen category [...] then propositions such as ‘*There is an object (of our category) of which both  $P(x)$  and  $P'(x)$  are true [...]*’ [...] are said to be meaningful — that is, they affirm definite (existential) states of affairs concerning which the question of whether they obtain or not can now be raised. ([fn:] Naturally, whether we are actually able to answer this question is beside the point.) It is in this sense that we understand the hypothesis that *the characteristic features of the categorical essence under consideration* are supposed to determine a *complete system of definite self-existent objects* [namely, the *extension of that category*].<sup>33</sup>

A later footnote refers back to this passage: ‘[...] closed judgments are, intrinsically, just *propositions*. That they all have one meaning, i.e. express a judgment, is a precise formulation of the hypothesis mentioned at the end of §1 regarding the “complete system of self-existent objects”.’

It must be said that this is not as clear as might be wished. The footnote dismissing concerns about ‘whether we are actually able to answer this question’ makes it clear that Weyl is, in this period, not an anti-realist in the sense of Dummett’s meaning-theoretic arguments: our inability to verify a (quantified) mathematical proposition, even in principle, does not jeopardize its objective truth-value.

<sup>33</sup> I. § 1, p. 8. (The material in square brackets is the gloss of the translators of the English edition, Stephen Pollard and Thomas Bole.)

What does jeopardize the objective truth-value of an existentially quantified proposition is the possibility of more potential counterexamples. The assumption that we have a ‘complete system of self-existent objects’ is required for quantification to be fully meaningful, and it amounts to the assumption that the domain is definite. According to *Das Kontinuum*, we can make this assumption for the natural numbers, but not for the sets of natural numbers.

The reason why quantification over the sets of natural numbers is not in general meaningful is simply that we do not have a definite understanding of the domain of sets of natural numbers. We can characterize some of them by certain means of construction or definition: for example, the arithmetical sets, which are the intended model of Weyl’s principles of construction. But such a characterization is only ever partial, as it leads, by diagonalization, to further sets which fall outside the original characterization. It is this indefinite extensibility that makes non-schematic universal quantification problematic: the meaning of the claim that *all* *F*s are *G* is unclear if it is unclear just what *F*s there are. The exception is schematic quantification: in certain cases, it is unclear what *F*s there are, but clear that *any* *F* that there is must be *G*, for some general reason. But the absence of such a general reason is no reason to believe that the generalization is false.

On the other hand, an existential quantification can be seen to be stably true on the basis of a partial characterization of the domain: if we have found one *F* that is *G*, that is enough for the existential to be true, regardless of any haziness about the extent of the *F*s. Of course, a negated existential quantifier is a universal quantifier; so while we can establish true sentences of the form  $\exists X...$ , they do not express full propositions, in that they are not amenable to the normal logical operations. Limiting the applicability of classical logic in this way is of course undesirable, and in Ch. 5.3.3 above, I outlined an approach to justifying the application of classical logic to the indefinitely extensible continuum.

In later life, after having abandoned the predicativist project and joined Brouwer’s intuitionist ‘revolution,’ Weyl wrote that:

The leap into the beyond occurs when the sequence of numbers that is never complete but remains open toward the infinite is made into a closed aggregate of objects existing in themselves. Giving the numbers the status of ideal objects becomes dangerous only when this is done.<sup>34</sup>

<sup>34</sup> Weyl, *Philosophy of mathematics* p. 38.

While Brouwerian intuitionism was a substantial development, it does not represent a complete break in Weyl's thinking: Weyl reveals how he viewed his earlier work by going on to say that a consistency proof of axiomatic arithmetic 'would vindicate the standpoint taken by the author in *Das Kontinuum*, that one may safely treat the sequence of natural numbers as a closed sequence of objects,'<sup>35</sup> that is, that sentences quantifying over the numbers may be assumed always to have a determinate truth-value. Feferman suggests that this remark shows Weyl to have been unaware of Gödel's 1933 work on the negative translation, which established the intertranslatability of classical and intuitionistic logic;<sup>36</sup> or at least that Weyl did not grasp the import of this work for axiomatic number theory. Feferman suggests that 'In consequence of [Gödel's] reduction, even the natural numbers can be conceived of as a "potentially infinite" totality.'<sup>37</sup> But reductions cut both ways: for the predicativist, it is more to the point to see the negative translation as showing that the natural numbers, infinite though they are, can legitimately be conceived of as a closed or completed totality: in my terms, a definite domain.

Weyl endorses the traditional view that the only form of humanly-graspable infinity is the potential infinite.<sup>38</sup> What is distinctive about Weyl's position in *Das Kontinuum* is that he nonetheless makes a case for countably infinitary mathematics with classical logic. Weyl makes this case by first treating the infinity of the natural numbers as a definite domain, and then developing a theory of analysis based on an enumerated sequence of sets of numbers which can also be so regarded. The daring first step is what would be justified by a finitary consistency proof of arithmetic, were such a thing possible; and it *is* justified, insofar as it can be, by the proof of the intertranslatability of intuitionistic and classical logic. Feferman's point is to be understood in the tradition of Hilbertian proof-theoretic instrumentalism: the mathematician who accepts only intuitionistic arithmetic, and nothing else, should be convinced by Gödel's negative translation that the classical theorems vindicated by Weyl's predicative analysis, while they may perhaps not all be meaningful in themselves, are at least guaranteed not to lead her into contradiction. As such, they may be considered conservative extensions of her mathematics, parts of an 'ideal'

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<sup>35</sup> Weyl, *Philosophy of mathematics* p. 60. <sup>36</sup> See Ch. 5.2.3 above. <sup>37</sup> Feferman, 'Weyl vindicated'; Gödel, 'Intuitionistischen Arithmetik [1933e]'. <sup>38</sup> For example: 'The notion that an infinite set is a "gathering" brought together by infinitely many individual arbitrary acts of selection, assembled and then surveyed as a whole by consciousnesses is nonsensical; "inexhaustibility" is essential to the infinite.' I.§ 4, p. 23

calculus which may permit shorter derivations of contentual results.

## 6.6 The dependence of sets on their defining properties

Weyl's hypothesis in *Das Kontinuum* that the natural numbers are a closed system, permitting meaningful quantification over them, gives some insight into what Weyl thinks goes wrong with impredicative mathematics: the power-set of the naturals, for example, is not something which can legitimately view as a 'complete system of definite self-existent objects' — the collection is not complete in that it is indefinitely extensible; and the objects in it are not 'definite' and 'self-existent' in that some of them depend upon others.

For example, consider the property  $K$  which holds of just those natural numbers of which all of a certain specified collection  $\mathbf{k}$  of properties of naturals are true (i.e.  $\lambda n.(\forall P \in \mathbf{k})Pn$ ). The extension of  $K$  may very well be identical with the extension of one of the  $\mathbf{k}$ -properties; this is obviously the case if, for example, the empty property  $\lambda n.n \neq n$  is in  $\mathbf{k}$ . But if we take seriously the idea that  $K$ , the intersection of the  $\mathbf{k}$ -properties of  $\mathbf{N}$ , is a *property*, then it is clear that  $K$  cannot itself be one of the  $\mathbf{k}$ -properties. The sense of ' $K$ ', the rule for determining whether or not  $Kn$ , cannot require us already to know whether or not  $Kn$ .

On the standard, extensionalist view of sets, the relation of a set to its corresponding property is an external one: the property merely lets us pick the set out. But if we view sets not as pre-existent objects to be picked out in this way, but instead as derived from those properties, in that they are abstracted from them, then we must recognize that sets of natural numbers defined by means of quantification over other sets of naturals are of a higher logical type. In short, we are forced to ramify.

Weyl's insistence that objects come in categories imposes a rigid separation between the ideal objects formed by abstraction, and the 'basic' objects on which the abstraction is done. Quantification is always quantification over the objects of a single category, and objects defined by means of quantification over a category therefore belong to a different ('higher' or 'derived') category. In this, Weyl's system is very much like that of the Ramified Theory of Types.

This hierarchical, genetic organization of properties (and therefore also of sets) bears some comparison with the iterative hierarchy of sets. The iterative story is meant to motivate the axioms of Cantorian set theory, and to explain how such

theories avoid the paradoxical collections of naive set theory. On the story, sets are ‘formed’ in stages, and can therefore have as members only those sets which were formed at an earlier stage.

The basic difference from the ramified predicative hierarchy of properties is that set theory is thoroughly extensional, and so treats the sets which it describes as a ‘complete system of definite self-existent objects’. Hallett brings out this connection when he writes:

What is axiomatized [in (Cantorian) set theory] in effect is the notion of completed and fixed range or extension. The fundamental primitive is *set* [...] and these sets [...] are treated extensionally as objects existing *completed*, and independently of and separated from, any intension which may help us to define or recognize them.<sup>39</sup>

Hallett goes on to show the awkwardness faced by those who lean too heavily on the intuitive ideas of iteration as a motivation for Cantorian set theory. The intuitive idea is of a constructive process which takes place in time. The difficulty comes, of course, when we try to justify the full Separation axiom in this way. For example, Boolos,<sup>40</sup> one of the most careful exponents of the iterative justification, adopts an axiom scheme of Collection, which tells us which sets are formed at a certain stage,  $s$ :

$$\exists y \forall x (x \in y \leftrightarrow (\phi(x) \& \exists t (tEs \& xFt))),$$

(where  $tEs$  is to be read as ‘stage  $t$  is earlier than stage  $s$ ’, and  $xFt$  as ‘the set  $x$  is formed at stage  $t$ ’; as usual,  $\phi$  may not contain free occurrences of  $y$ ). As  $\phi$  here is entirely arbitrary, it can mention, and quantify over, sets formed at any stage, including those later than  $s$  — that is to say, sets which, according to the intuitive story, have not yet been formed. ‘Formation’ seems here to be a misleading metaphor: as Hallett says, ‘“formation” here can only really refer to the structural hierarchical organization of a *given* universe of objects.’<sup>41</sup>

The case of the ramified predicative hierarchy of sets is rather different. Here the metaphor of building up from the natural numbers can be successfully sustained — indeed, predicativism just *is* the mathematics which can be built constructively from

<sup>39</sup> Hallett, *Cantorian set theory*, pp. 195–6 (his emphasis).    <sup>40</sup> Boolos, ‘The iterative conception’.

<sup>41</sup> Hallett, *Cantorian set theory* p. 222; Hallett’s emphasis.

the natural numbers; and so constructivism about these sets (or indeed psychologism, of some appropriately liberal form) seems to be a coherent position.

What the iterative conception of set was supposed to explain was why we should believe what modern Cantorian set theory says about the sets: what sets there are and what sets there are not. Paradoxical sets, such as the Russell set of non-self-membered sets, are ruled out by the iterative story because there is no stage at which they could be formed. The guiding idea is that the story shows how sets are in some sense dependent on their members; they cannot come into existence until their members are there to be collected. But the temporal metaphor is disclaimed as metaphorical and potentially misleading. (It is also inadequate: a sequence of moments of time which is well-ordered by the normal temporal ordering cannot be more than countably infinite.) Neither can the dependence be explained modally: it is true that if the members of a set did not exist, the set would not exist either; but it seems that the reverse is also true, and in any case, the platonist would presumably insist that the members of the pure hierarchy of sets are all necessarily existent. The defender of the iterative story seems to be driven to say that the ontological dependence is a *sui generis* ontological primitive.<sup>42</sup>

The Cantorian's notion of the ontological dependence of a set on its members must be extended by the predicativist. The intersection considered above, of the **k**-properties of **N**, will not be formed until the stage after that at which all of the **k**-properties are formed (except in those cases where it so happens that the set is also defined by a different property, which does not quantify over the **k**-properties; in any case, the property will not be formed until that stage). So on the predicativist version of the iterative story, the ontological dependence of a set is both on its members, and on its defining property. Of course, as sets are extensional, the same set will have many defining properties; but this is established after the fact. What comes first is the hierarchy of properties, with its dependencies of sense.

But how, given this ramified view of sets, should we view the continuum? Weyl opted for an unramified mathematical system, because he felt that to deal with real numbers of different levels would be '*künstlich und unbrauchbar*'.<sup>43</sup> That is to say, in *Das Kontinuum*, all of the sets of natural numbers (and so all of the real numbers) which are dealt with are of the first level. Higher-level sets are simply ignored. It

<sup>42</sup> For discussion of the iterativist's difficulties here, see Potter, *Set Theory and its Philosophy*.

<sup>43</sup> 'Artificial and impractical', I. § 7, p. 32.

might seem at first that this move by Weyl is under-motivated, or that he is trying to sweep the complexity of the predicative continuum under the carpet. But in fact the stratification of sets (of any one type) into levels depends on the particular forms of set definition that we have chosen to allow for the first level of sets: the meaning of quantification over the first-level sets will of course be different if we allow more sets to be definable at the first-level. And so to draw a hard distinction between first- and higher-level sets of natural numbers would indeed be artificial. We will return to this issue in Ch. 7.1.5 below.

## 6.7 Conclusion

The account which predicativism gives of the grounds of mathematical truth and the nature of mathematical objects is one which keeps them within the grasp of our cognitive abilities, at least when those abilities are appropriately idealized so as to ignore the particular finite limits to which human thought is subject. Weyl's predicativism abandons the project of deducing arithmetic from something more fundamental, and simply takes the natural numbers and the relation of immediate succession as basic, and as given to us by means of intuition. What exactly that intuition amounts to is not a matter which can be examined here; I will merely note here the attraction of the Kantian answer that it is connected to our understanding the passage of time.

The properties and relations which Weyl considers are built up from that relation of succession; and the sets are ideal objects abstracted from those properties. The 'building up' of the properties is a wholly constructive process, and the individual steps are applications of elementary logical operations. The only doubt that can be raised against the cogency of this conception is whether we can be sure that the result of applying such classical logic over the infinite domain of the natural numbers is always meaningful. Quantification and negation are the troubling operations, and LEM the problematic principle, and it is these that were discussed and justified in Chapter 5. What remains is to see how much mathematics can be developed in accord with this conception.



## Chapter 7

# Predicative mathematics and its scope

The basic question with which this chapter is concerned is: How much of classical mathematics can be developed in a manner acceptable to predicativists? Or to put it the other way around: What parts of classical mathematics *cannot* be predicativistically justified, i.e., what parts are essentially impredicative?

It is clear that there are some such essentially impredicative areas of modern mathematics: areas which are essentially dependent on uncountable sets: examples are higher set theory (dealing with large cardinals and so on) and also other heavily set-theoretic branches of mathematics such as general topology. It will surprise no-one that these will not be justified from a predicativist standpoint. Conversely, primary school arithmetic certainly will be predicativistically acceptable.

But it is important to give a more precise answer than this: we need to do so in order to find out just how revisionary a position predicativism really is, and that is a necessary part of any full assessment of predicativism as a general position within the philosophy of mathematics.

There are two particular fears in connection with this which have to be overcome if we are to vindicate predicativism as an acceptable position. The first and most serious fear is that predicativistically acceptable mathematics is too weak to justify parts of classical mathematics which are directly used in the natural sciences. The second worry is that predicativistic restrictions might rule out parts which are essential to (modern) mathematics as a systematic whole, leaving an unnatural

assortment of fragments; such a rag-bag might contain enough tools for the engineers, but it would lack coherence and integrity as a subject in its own right.

These indispensability arguments — that impredicative methods are necessary, either to the scientific enterprise, or to a satisfying and coherent mathematical enterprise — will be discussed in the second section of this chapter.

First, however, we need to find out just how revisionary predicativist mathematics has to be.

## 7.1 What predicativists can get

One way to try to answer our main question is simply to have a go: to set up a formal system which is predicativistically acceptable, and to see how much of classical mathematics can be derived in that system. This was more or less Weyl's approach in *Das Kontinuum*.<sup>1</sup>

The trouble with such an approach is that it cannot deliver conclusive negative answers: our failure to prove a certain theorem in a given system might simply mean that we haven't been clever or lucky enough to hit on the proof; and even if we can show that there is no such proof in the system, that would not rule out the possibility that some other predicativistically acceptable formal system could deliver a proof of the desired theorem.

What we want is some sort of an analysis of what is needed for what: and the so-called 'reverse mathematics' programme holds out the promise of just such an analysis.

### 7.1.1 Reverse mathematics

The basic question which the reverse mathematics programme seeks to answer is: What set existence axioms are needed to prove various parts of classical mathematics?<sup>2</sup> The forward part is fairly unremarkable: a certain known theorem is proved from one of a range of axiom systems. The axioms systems used form a nested sequence of increasing strength; each system is presented as an extension of the previous system by means of a stronger set existence principle. The distinctive feature

<sup>1</sup> For a clear modern presentation of Weyl's project and its results, see Feferman, 'Weyl vindicated'.

<sup>2</sup> The best introduction to the topic is the first chapter of the encyclopedic Simpson, *Subsystems of Second Order Arithmetic*.

of the programme is that the analysis of the strength of a theorem can be shown to be sharp, by means of the proof of a ‘reverse’. A reverse is a proof, in a base theory, that the theorem entails the set existence axiom of the system used in the forward proof. A reverse establishes the equivalence (modulo the base) of the theorem and the axiom system, and shows that the system in question is the weakest which permits the proof of the theorem. The axiom systems studied by reverse mathematics are those which turn out to permit proofs of such reverses; the notable systems are the weak base  $\text{RCA}_0$ , and then (in order of increasing strength),  $\text{WKL}_0$ ,  $\text{ACA}_0$ ,  $\text{ATR}_0$  and  $\Pi^1_1\text{-CA}_0$ .

The reverse mathematics programme can be seen as a natural continuation and extension of Hilbert’s programme;<sup>3</sup> and some of the formal systems used ( $\text{RCA}_0$  and  $\text{ACA}_0$ ) have some claim to epistemological naturalness. But the programme is a mathematical one, and the axioms and systems are in large part the result of the research programme itself: the axioms studied are the ones which turn out to support the reversals. The fact that they do — and in a wide range of branches of mathematics — suggests that the axiom systems are ‘mathematical natural kinds’; but further work is required to examine the extent to which they might also be epistemological natural kinds.

The reverse mathematics programme has also concentrated primarily on systems of second-order arithmetic. This is not a serious limitation, as hereditarily countable sets can be coded up by classes of natural numbers; for example, the weak set theory  $\text{ATR}_0^{\text{set}}$  is a definitional extension of  $\text{ATR}_0$ .<sup>4</sup> However, such reliance on coding is undeniably rather unnatural, and we will examine whether this can be avoided.

In the context of our project here, investigating predicativism, the obvious question is which of the systems in the taxonomy established by the reverse mathematics programme count as legitimately predicative.

### 7.1.2 Justifying $\text{ACA}_0$

Predicativism, as an outlook on the philosophy of mathematics, endorses the natural numbers as unproblematically given. As we argued in Chapter 5, this means endorsing the meaningfulness of unrestricted quantification over them, and so accepting the Law of the Excluded Middle as valid for such (arithmetical) sentences.

<sup>3</sup> For argument for this point see Simpson, ‘Partial realizations’. <sup>4</sup> Simpson, *Subsystems of Second Order Arithmetic* §VII.3.

Every instance of the induction schema for the arithmetic sentences is also predicatively justified — it is, after all, of the essence of the natural numbers that they satisfy induction for fully meaningful predicates. So standard first-order Peano Arithmetic, PA, which consists of platitudes about zero and the successor function, recursive definitions of addition and multiplication, and the induction schema for open sentences of the language of arithmetic, is entirely acceptable to the predicativist.

Given this, the natural first theory to consider is PA extended by the addition of predicative sets of natural numbers: this is the theory known as  $\text{ACA}_0$ , and it has a central role in the Reverse Mathematics programme.

$\text{ACA}_0$  can be axiomatized by: (i) the usual first-order Peano axioms for zero and the successor function, and the recursive definitions of the addition and multiplication functions and the less-than relation; (ii) the induction axiom

$$\forall X([0 \in X \ \& \ \forall x(x \in X \rightarrow sx \in X)] \rightarrow \forall x(x \in X));$$

and (iii) the arithmetical comprehension axiom:

$$\exists X \forall x(x \in X \leftrightarrow \phi(x))$$

(where  $\phi(x)$  is an arithmetical sentence, i.e. one not containing any bound second-order variables, though free variables of both types may appear as parameters). The Arithmetical Comprehension Axiom gives the letters in ' $\text{ACA}_0$ ': the subscript zero refers to the fact that induction is expressed by an *axiom* rather than by a scheme; the significance of this is that it does not apply to arbitrary open sentences of the second-order language, but only to those predicates which have been recognized as forming sets. In the context of the Arithmetical Comprehension Axiom, this means that induction will not be provable for predicates which contain bound second-order variables, in accordance with the predicativist idea that quantification over the full continuum is not guaranteed to lead to determinately meaningful results.

### 7.1.3 Believing in $\text{ACA}_0$ ; the metamathematics of $\text{ACA}_0$

The intended interpretation of  $\text{ACA}_0$  is the obvious one: the first-order variables range over the natural numbers; and the second-order variables range over ARITH, the family of arithmetically definable classes of natural numbers. ARITH occupies a central place in recursion theory; it is also known as  $\Delta_\infty^0$  or  $\Delta_0^1$ , and can be

characterized as

$$\bigcup_{n \in \mathbf{N}} \text{TJ}^n(\emptyset),$$

where TJ is the Turing Jump operation, which takes a set  $X$  to a complete recursively enumerable set relative to  $X$ .

Of course, as  $\text{ACA}_0$  is a (two-sorted) first-order theory, the Löwenheim–Skolem theorem applies, and there are non-standard models. However, the intended model is the minimum (i.e. the unique smallest) model, in the sense that the range  $|M|$  of the first-order variables, in any model  $M = \langle |M|, \mathcal{S}_M, \circ_M, s_M, +_M, \cdot_M, <_M \rangle$ , has a  $<_M$ -initial subset isomorphic to the natural numbers; and that the range  $\mathcal{S}$  of the second-order variables for every  $\omega$ -model of  $\text{ACA}_0$  (i.e. for every model  $M$  with  $|M| = \mathbf{N}$ ) contains ARITH as a subset.

A simple argument shows that  $\text{ACA}_0$  is equiconsistent with PA (indeed, that any extension of a theory by the addition of predicative classes is equiconsistent with the original theory): any model of PA can be extended to a model of  $\text{ACA}_0$  simply by allowing the class variables to range over the definable subsets of the domain.<sup>5</sup>

While  $\text{ACA}_0$ , and its minimum  $\omega$ -model ARITH are simple and appealing, there is a sense in which they are *obviously* incomplete: it is straightforward to produce examples of classes of natural numbers which are not arithmetic, simply by diagonalizing. If we enumerate all of the arithmetic formulae with one free number variable (and hence the classes of ARITH which those formulae define) as  $\phi_0(x), \phi_1(x), \dots$ , then the class  $\{x \in \mathbf{N} \mid \sim \phi_x(x)\}$  is obviously not in ARITH, and so cannot be proved by  $\text{ACA}_0$  to exist.

How we should respond to this incompleteness is an issue to which I will shortly return.

#### 7.1.4 Predicative mathematics: $\text{ACA}_0$ and *Das Kontinuum*

The system  $\text{ACA}_0$  is the natural choice for a rational reconstruction of the mathematics Weyl puts forward in *Das Kontinuum*.<sup>6</sup> The results of classical analysis which Weyl proved in *Das Kontinuum*, and which can be formalized in  $\text{ACA}_0$ , include the

<sup>5</sup> As was mentioned above, Shoenfield, ‘Relative consistency proof’ gives a proof-theoretic argument for the equiconsistency that is effectively in PRA. <sup>6</sup> Feferman, ‘Weyl vindicated’ substantiates this in some detail. One wrinkle is that Weyl’s original treatment of set-definition by recursion turns out to be more generous than it should be, allowing the definition of non-arithmetical relations.

following: a continuous function on a closed interval attains its maximum, its minimum, and its mean-value, and is uniformly continuous; the Riemann integral exists for a piece-wise continuous function; and the Fundamental Theorem of Calculus. All of the familiar functions of analysis can be dealt with in  $ACA_0$ : the trigonometric and exponential functions, and in fact all functions which can be given by power series.

The enumerability of ‘all possible sets of natural numbers’, together with Cantor’s diagonal proof of their uncountability, is what Weyl calls ‘Richard’s antinomy’ in his discussion of the matter in *Das Kontinuum*.<sup>7</sup> But as Feferman remarks, Weyl’s discussion is thoroughly unsatisfactory.<sup>8</sup>

Weyl takes Cantor’s theorem as a piece of mathematics to be formalized. It can be proved even in systems of second-order arithmetic much weaker than  $ACA_0$  (quantifier-free class comprehension is sufficient for the result) that any class coding a sequence  $A_0, A_1, \dots$  of classes of naturals is incomplete, in that there is a class of naturals which is not  $A_i$  for any  $i \in \mathbb{N}$ . Such a sequence of classes is implemented in second-order arithmetic as a class  $B$  of pairs  $\{\langle m, n \rangle \mid m \in A_n\}$ . The proof of incompleteness is the usual diagonal construction: the class  $\{n \mid n \notin A_n\}$  can be proved to exist, but obviously cannot be in the enumeration.

What Weyl does not discuss is the possibility of seeing Cantor’s method of diagonalization as something which can be applied to the system from outside, as it were, to go from an enumeration of the classes definable in a formal system to a class which is not so definable. Weyl seems not to recognize that that class ought to exist; and that it has indeed been adequately defined, though of course not in terms of the principles of definition which the system permits. It is precisely the way that the argument leads us to a broader concept of *definition* that is the interest of Richard’s paradox, and that (for the predicativist) shows the indefinitely extensible nature of the classes of natural numbers.

The predicativist need not follow Weyl’s flat-footedness here. If not, the predicativist may still endorse the axioms of  $ACA_0$ , and in particular the axiom of arithmetic class comprehension; but she will not do so under the belief that those are all of the classes that there are.  $ACA_0$  will be used by such a predicativist as a convenient formal system for mathematics, but she will not suppose it to be an

<sup>7</sup> Ch. 1, §5, pp. 26–28. See also the discussion of Richard’s Paradox in Ch. 3.3.2 above. <sup>8</sup> Feferman, ‘Weyl vindicated’ p. 262.

exhaustive description of the continuum.

### 7.1.5 The Law of Excluded Middle

One aspect of  $\text{ACA}_0$  may seem to make it problematic for the predicativist: it validates the Law of Excluded Middle for the language as a whole.<sup>9</sup>

As we have seen, the predicativist regards the classes of natural numbers as an open domain: that is, one which we can only ever hope to *partially* characterize. For this reason, classical quantification over the classes of naturals is not, in general, meaningful. Of course, some sentences involving the second-order quantifiers will be validated: the instances of the arithmetical class comprehension scheme are  $\Sigma_1^1$  truths; and various trivialities, such as the  $\Pi_1^1$  sentence  $\forall X(o \in X \vee o \notin X)$ , can also be proved. We might not know just what classes of natural numbers there are; but we certainly do know that every class either contains or does not contain zero.

The issue hinges on how the predicativist is to understand the second-order quantifiers of a theory such as  $\text{ACA}_0$ . It seems that they could either be taken as ranging over the indefinitely extensible domain of all subsets of the natural numbers; or as ranging only over the arithmetical sets. In the first case, there is no reason to think that every second-order quantified sentence expresses a determinate meaning; and so no reason to think that such a sentence is either true or false, unless we can give a proof of it or its negation. Second-order quantification introduces a sort of vagueness; in the case of some sentences, this vagueness is inessential, in that any way of removing the vagueness will give the same truth-value to the sentence: these are the second-order quantified sentences that can be proved or disproved. But there is evidently no reason to suppose that that is always the case.

To understand the quantifiers in the second way is to conclude from the indefinite extensibility of the continuum that we should not try to quantify over it, and that we should instead see our quantification as restricted to some fully definite ersatz, such as the arithmetical sets of naturals.

Another way to put the contrast is to ask whether or not the minimal model of  $\text{ACA}_0$  is the unique intended model. The point is that the second-order sentences which are undecidable in  $\text{ACA}_0$  are those which have different truth values in different  $\omega$ -models.

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<sup>9</sup> See Ch. 6.5, Ch. 5, and Ch. 3.2.1 for discussion of the relationship between quantification over open domains and the failure of LEM.

It is this issue which is behind the contrast drawn in *Das Kontinuum* between delimited and non-delimited judgements. In his explanation of how logically complex judgements are built up, Weyl specifies a ‘narrower procedure’, which gives rise to ‘delimited’ judgements, and a broader procedure. The narrower procedure restricts the use of quantification to the basic category, that is, the natural numbers, whereas the broader procedure allows quantification also over the derived categories (sets and functions). Only delimited properties are permitted in class definitions.

Weyl notes that this restriction could be lifted in accordance with predicativist principles, if we were willing to distinguish first-level sets of natural numbers (defined by delimited properties) from second- and higher-level sets of natural numbers — if, that is, we were willing to ramify.

Ramification would force us to view second-order quantification as being always over some level of the ramified hierarchy. Weyl argues against ramification as unnatural and awkward in practice, and seems inclined to view quantification over sets of naturals as being over the full, indefinitely extensible continuum. He writes:

The primary significance of the narrower procedure is most clearly conveyed by the following observation: The objects of the basic categories remain uninterruptedly the genuine objects of our investigation only when we comply with the narrower procedure; otherwise, the profusion of derived properties and relations becomes just as much an object of our thought as the realm of those primitive objects. In order to reach a decision about ‘delimited’ judgements, i.e., those which are formed under the restrictions of the narrower procedure, we need only survey these basic objects; ‘non-delimited’ judgements require that one also survey all derived properties and relations.<sup>10</sup>

Later, in the development of analysis in his system, Weyl points to an important consequence of this:

the continuity of a function is *not* a delimited property; i.e., in order to decide whether a function defined with the help of our principles is continuous or not we have to inspect not just the totality of natural numbers, but also the totality of *sets* [...] which arise from an arbitrarily complex joint application of those principles. If we regard the principles of definition as an ‘open’ system, i.e., if we reserve the right to

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<sup>10</sup> Note 24 to p. 30.

extend them when necessary by making additions, then in general the question of whether a given function is continuous must also remain *open* (though we may attempt to resolve any *delimited* question). For a function which, within our current system, is continuous can lose this property if our principles of definition are expanded and, accordingly, the real numbers ‘presently’ available are joined by others in whose construction the newly added principles of definition play a role.<sup>11</sup>

Weyl immediately adds in a note:

Of course, in the case of every function one encounters in analysis, this question does *not* remain open, since the negative judgment which asserts their continuity is a logical consequence of the ‘axioms’ into which the principles of definition change when formulated as positive existential judgments concerning sets. But this is just a special characteristic of these ‘absolutely’ continuous functions.<sup>12</sup>

The arithmetical continuum is only a limited part of the continuum, as any part which is amenable to predicative mathematics must be; and as we are primarily interested in the continuum, rather than the ersatz, it makes sense to view our quantifiers as ranging widely, and so as making systematically ambiguous claims, rather than ranging only over the delimited sets of naturals.

Most concretely, it would not be sensible to extend  $\text{ACA}_0$  by adding a restrictive axiom which explicitly claimed that every set is an arithmetical set. Such an axiom could be framed by coding up the syntax of the language and constructing a numerical predicate  $\phi$  true just of codes of arithmetical sentences with one free number variable, and then expressed as

$$\forall X \exists a (\phi(a) \ \& \ \forall x (x \in X \leftrightarrow \text{Sat}(x, a))),$$

where  $\text{Sat}$  is a satisfaction predicate. An axiom like this would aim to rule out all but the minimal  $\omega$ -model of  $\text{ACA}_0$ .

But what is the upshot of this discussion for the Law of the Excluded Middle? It seems at first as if the predicativist should refuse to endorse consequences of LEM such as  $\forall X \Phi(X) \vee \exists X \sim \Phi(X)$  where  $\Phi$  contains class quantifiers. ( $\text{ACA}_0$  validates this.) But this should not trouble us too much: there is very little that one can actually

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<sup>11</sup> p. 87.    <sup>12</sup> Note 7 to p. 87.

do with it. And it can be justified by reflecting that, on any way of removing the vagueness from set quantification, it will be validated. The formal analogue is, again, that every  $\omega$ -model of  $\text{ACA}_0$  will make one of the disjuncts true.

### 7.1.6 Delimiting the scope of predicativism

So far, I have argued that  $\text{ACA}_0$  can be justified on predicative grounds; and we have seen that there are grounds within informal predicative mathematics for seeing  $\text{ACA}_0$  as incomplete. The obvious question, then, is how much more mathematics the predicativist can get. Drawing a limit to predicative mathematics is clearly something that cannot be done within predicative mathematics itself.

In ‘The philosophical significance of Gödel’s theorem’, Dummett suggests that the moral to be drawn from the Incompleteness Theorems is that the means we should find acceptable for proving arithmetical results are indefinitely extensible. Dummett’s view is in accord with Gentzen’s ‘shading-off’ interpretation of the proof-theoretic ordinals from  $\omega^\omega$  up to and (problematically) including  $\epsilon_0$ .<sup>13</sup> (It seems clear that Gentzen would have been willing to extend this analysis beyond  $\epsilon_0$ .)

This view seems to be in tension with the idea that there are principled intermediate stopping places on the road which leads from finitist mathematics, at one extreme, to higher set theory at the other. (For example, Isaacson has famously argued that (first-order) Peano Arithmetic is complete for a certain sort of arithmetical insight.)<sup>14</sup> The philosophical significance of the Reverse Mathematics programme is surely the claim that the subsystems studied are such principled stopping places.

This tension could just be apparent, however. I suggest that we should take the proof-theoretic ordinal of a formal system as a measure of its strength, that is, as a formal analogue of Dummett’s ‘means of proof’. These ordinals are transfinite; and so there are among them increasing  $\omega$ -sequences which are bounded above. There is therefore the possibility of what seems ‘from below’ to be an indefinitely extensible succession of justified formal systems, which, when looked at ‘from above’, are bounded above by a system which is their union.

I suggest that we can get a handle on the significant stopping places on the road by thinking harder about the proof-theoretic ordinals, and in particular the epistemic

<sup>13</sup> ‘I fail to see, however, at what “point” that which is constructively indisputable is supposed to end, and where a further extension of transfinite induction is therefore thought to become disreputable.’ (Gentzen, ‘Neue Fassung’ p. 286.) <sup>14</sup> Isaacson, ‘Some considerations on arithmetical truth and the omega-rule’.

means by which we come to grasp them (or rather, to grasp them as the order-types of *well*-orderings). Before addressing our central question in the next subsection, of how much mathematics is predicatively justified, I will first discuss a simpler case, which will illustrate the general idea.

PRA is conventionally taken as the best analysis of finitary arithmetic; it takes induction to be legitimate when applied to quantifier-free arithmetical formulae, and to  $\Pi_1^0$  formulae.<sup>15</sup> (These are argued to be finitistically justified because they can be expressed as free-variable, or schematic generalizations; they therefore do not require the acceptance of the natural numbers as a definite domain.) The proof-theoretic ordinal of PRA is  $\omega^\omega$ . If we are prepared to go beyond PRA to stronger systems, the natural way to go is to increase the strength of the induction scheme by allowing in greater quantifier complexity. If we add to PRA the  $\Pi_2^0$ -induction scheme, we get a system with the proof-theoretic ordinal  $\omega^{\omega^\omega}$ ; and indeed for all  $n \in \mathbb{N}$ , the proof-theoretic ordinal of PRA together with the  $\Pi_n^0$  induction scheme is  $\omega \uparrow\uparrow n$ . It seems clear that, after PRA, none of these systems is a natural stopping place.

The limit of the sequence is the first fixed-point of the operation  $\alpha \mapsto \omega^\alpha$ , which is known as  $\epsilon_0$ . This is the proof-theoretic ordinal of PA. Unlike the previous steps, taking this limit does, I suggest, require a new idea. If this is correct, then there is an epistemological position on which the theorems of PA are all acceptable, because they are obtainable in one of the sequence of formal systems (PRA together with  $\Pi_n^0$  induction for  $n \in \mathbb{N}$ ); but on which PA *as a whole* is not acceptable. What can this unacceptability mean, given that all of the theorems of PA are endorsed? I suggest that it means that, on this epistemological position, the consistency of PA is not acceptable or obtainable.

This line of thought is closely related to Dummett's argument that the 'simple argument' for  $\text{Con}(\text{PA})$  fails.<sup>16</sup> The simple argument is that acceptance of PA means acceptance of the truth of the axioms, and therefore a commitment to their consistency. In opposition to this, Dummett points out that to accept any proof in PA requires confidence only in the axioms used in that proof, and in particular in only finitely many instances of the induction schema. There is therefore a gap between a belief in the overarching consistency of the axioms, and a simple working acceptance

<sup>15</sup> For argument, see especially Tait, 'Constructive reasoning'; Tait, 'Finitism'; Tait, 'Remarks on finitism'. <sup>16</sup> Dummett, 'Gödel's theorem'.

of PA. An agent who has merely this working acceptance of the theory is disposed to accept any individual axiom when it is presented to her for evaluation; and she is prepared to accept as true the conclusion of any proof from the axioms of the theory; but in the absence of further reflection on her commitments, she has no route to justifying belief in the consistency of the system as a whole. PA therefore stands as the limit of the mathematics obtainable on this position. In a similar way, Simpson has claimed that  $\text{ATR}_0$  stands as the limit of predicativity.

### 7.1.7 Going beyond $\text{ACA}_0$

$\text{ACA}_0$  is an unramified theory. We can get stronger theories, which prove the existence of more collections of natural numbers, by ramifying. The system  $\text{RA}_\alpha$  of Ramified Analysis has second-order variables of different degrees,  $X^\beta, Y^\beta, Z^\beta, \dots$ , for all  $\beta \leq \alpha$ , and comprehension principles for each such degree ensuring the existence of a collection of natural numbers  $X^\beta$  defined by any open sentence with bound second-order variables of degree less than  $\beta$ , and free second-order variables (as parameters) of degree less than or equal to  $\beta$ . We can take these  $\alpha, \beta$  to be natural numbers coding ordinals, for example those from Kleene's set  $O$  of recursive ordinal notations; and so the progression of  $\text{RA}_\alpha$  will continue into the transfinite. How far can we go? The limiting factor is that the system  $\text{RA}_\alpha$  is only predicatively legitimate if  $\alpha$  can be proved, in a predicatively acceptable manner, to be a well-ordering.

This raises a problem: the claim that a recursive linear ordering  $<$  of natural numbers is a well-ordering is a  $\Pi_1^1$  sentence and so on the face of it, is not predicatively meaningful. This problem can be avoided, however, by using quantification over an arbitrary level of the ramified hierarchy. For such a  $<$ , abbreviate the claim that any non-empty subset  $X^\beta$  of its field has a  $<$ -minimal element as  $\text{WO}^\beta(<)$ . Then if  $\text{WO}^0(<)$  is provable, this proof will 'lift' to establish  $\text{WO}^\beta(<)$  for any  $\beta$  we come to accept as a well-ordering.

The answer to how far we can go reduces to the question of which ordinals can be reached 'autonomously'. If  $\beta$  is the order-type of the linear order  $<$ , then to show that  $\beta$  is a predicatively provable ordinal, we need to prove  $\text{WO}^0(<)$  in  $\text{RA}_\alpha$  for some ordinal  $\alpha$  which we have already established to be predicatively provable.

An independent characterization of the limit of this progression can be given impredicatively ('from outside'), in terms of the Veblen hierarchy. The least non-

predicatively provable ordinal is known as  $\Gamma_0$ .<sup>17</sup>

We can therefore identify what is predicatively provable with what is provable in  $RA_\alpha$  for some  $\alpha < \Gamma_0$ . However, ramified systems are unwieldy and unnatural (as Weyl emphasized); there has therefore been considerable interest in identifying unramified systems which can be predicatively proved (in the sense just given) to be justified. The system  $ATR_0$ , studied by the Reverse Mathematics programme, can be given a *local* proof-theoretic justification: that is to say, the proof-theoretic ordinal of  $ATR_0$  is  $\Gamma_0$ , and so the justification of  $ATR_0$  requires the union of the whole progression. However, every individual theorem of  $ATR_0$  can be justified using less — that is, using predicatively acceptable means.

### 7.1.8 Alternatives?

The choice of second-order arithmetic is largely one of convenience. Systems of second-order arithmetic are very well studied by proof-theorists, and are an adequate framework for all of the obvious areas of mathematics which are not clearly concerned with uncountability. (The reals ‘partake of countability’ through the rationals: continuous functions on the reals are wholly determined by their behaviour on the rationals, for instance. Real analysis is concerned with  $\mathbf{R}$  as a separable metric space, the completion of  $\mathbf{Q}$ ; not with  $\mathbf{R}$  simply as an uncountable set that we might consider under the discrete topology, for instance.)<sup>18</sup>

However, the choice of the framework of second-order arithmetic requires a certain amount of unnatural coding. In a sense, of course, this is also true of set theory as a framework for mathematics. But set theory is untyped, and therefore allows a uniform development of certain areas of mathematics, such as functional analysis, which in the framework of second-order arithmetic would require a great deal of ad hoc coding.

Feferman, in particular, has been concerned to develop systems of predicative mathematics less removed from mathematical practice. His most developed system is  $W$ ,<sup>19</sup> which is a conservative over standard first-order Peano Arithmetic.  $W$  features ‘flexible types.’ Formally, the language is two sorted: one sort of variables range over individuals — numbers, sets, and functions, and so on; and the other sort of

<sup>17</sup> See Feferman, ‘Predicativity’ for an overview of this investigation; the original proof is in Feferman, ‘Systems of predicative analysis [I]’. <sup>18</sup> Cf. the remark of Simpson, quoted p. 102 above. <sup>19</sup> Discussed in Feferman, ‘Weyl vindicated’

variable ranges over *types*: for example, the type of natural numbers, the type of sets of natural numbers, and the type of functions on the naturals. There are operators on individuals such as those which give ordered pairs and projections, definition by cases and recursion, and function application; and there are operators on types, for example those which give the type of ordered pairs of two given types, the type of functions from one type to another, and subtypes (those individuals in a given type which satisfies a predicative formula). This rich framework allows a direct and natural development of a great deal of modern functional analysis.

Such work is extremely valuable, and is indeed absolutely necessary if predicative mathematics is to have any sort of independent life of its own, rather than remaining merely as parasitic upon mathematics as carried out by ‘ordinary mathematicians’, without regard for the varying degrees of infinitary commitment required.

However, I believe that the results of Simpson’s Reverse Mathematics programme are a sound analysis of the infinitary requirements behind at least those (very substantial) parts of mathematics which can be directly stated in a second-order arithmetical manner. Work on alternative frameworks may fill in more details, but seems unlikely to change the general picture Simpson’s work gives us of the extent of predicatively justifiable mathematics.

### 7.1.9 The predicative continuum

We are now in a position to explain a little further the picture of the predicativist continuum sketched in Ch. 5.3. The most important part of that picture is that the continuum is indefinitely extensible. However many real numbers we have recognized, we can always come to recognize more. This creates problems for the completeness properties that characterize the continuum on the classical view.

The predicative continuum is sequentially complete: that is, every sequence of reals which is bounded above has a least upper bound. (This is provable in  $\text{ACA}_0$ .) However, the situation with arbitrary *sets* of reals is not so clear. In the framework of second-order arithmetic, a ‘set’ of reals can be given by means of an open sentence with one free set-variable,  $\Phi(X)$ , satisfying the appropriate conditions: the intended interpretation is of those real numbers which satisfy the formula. It is *not* evident on the predicative view that such sets obey the least upper bound principle.

Thinking about the ramified continuum, where the reals are introduced in stages, makes this clearer: for any given stage  $m$ ,  $\Phi^m(X^m)$  (where the (free and bound)

second-order variables in  $\Phi$  are restricted to range only over items of level  $m$ ) will be satisfied by reals of level  $m$ , and so will have a definable least upper bound at level  $m + 1$ ; but it may well not have one at level  $m$ . And this situation is repeated at level  $m + 1$ : there is no reason to think, in the general case, that all of the reals satisfying  $\Phi$  will have been ‘formed’ before some particular level, and so no reason to think that there will be a least upper bound to all of the  $\Phi$ .

Classically speaking, the general completeness principle is equivalent to its special case, sequential completeness, by the following argument. Given an arbitrary set  $S$  of reals which is non-empty and bounded above, we can set  $x_0$  to be some member of  $S$  and  $y_0$  to be some upper bound for  $S$ . Now consider the midpoint of  $x_0$  and  $y_0$ : if this is an upper bound for  $S$ , set  $y_1$  to be the midpoint, and set  $x_1$  to  $x_0$ ; otherwise, set  $x_1$  to the midpoint, and  $y_1$  to  $y_0$ . Continuing in this way, halving the interval each time, we generate two convergent sequences which must converge to the same limit; and it is not hard to see that this limit is the least upper bound for  $S$ .

The problem with this argument, from the predicativist’s point of view, is that in general we just don’t know what a formula such as  $\Phi$  means, because it may contain embedded second-order quantifiers. While in some special cases, as we have seen, we can show that any way of precisifying these quantifiers will give rise to the same set of objects satisfying the formula, in general, we cannot. And so we can’t tell whether or not a given real number — such as the midpoints in the construction above — satisfies the criterion defining the ‘set’; and therefore we cannot actually construct the sequences.

## 7.2 What we all need — indispensability arguments

The previous section examined the question of how much mathematics can be justified on a predicativist view. This section will look at the question of how much mathematics we need. If predicativism is to be tenable, we need to dispel the worry that it gives us less than we need. The last section suggested that predicativism gives us more than we might have thought; this section suggests that we need less than we might have thought.

The question of what we need will be discussed in two parts: what we need for science; and what we need for mathematics itself.

### 7.2.1 Science — Quine–Putnam

The indispensability arguments of Quine and Putnam argue for mathematical realism from the premise of scientific realism. The idea is that if we are naturalists, we ought to take seriously the claims of our best scientific theories; our best scientific theories are highly mathematical, and so make purely mathematical claims as well as scientific claims; so we ought to take mathematics seriously. This ‘seriousness’ is both ontological — we should believe in the mathematical entities which feature in our scientific theories — and semantic — we should take the mathematical claims which follow from our theories, and the mathematics used in our theories, to be *true*.<sup>20</sup>

The position is the opposite of instrumentalism, which is precisely a refusal to take seriously claims of a certain sort. In recent philosophy of mathematics, instrumentalism about the role of mathematics in science has gone under the banner of fictionalism.

Field’s eliminativist nominalism can be seen as an alternative response to the Quine–Putnam argument. Field’s programme is to show that mathematics does not feature essentially in scientific theories; that is, that the mathematical formulations can be dispensed with by reformulating the theory in purely physical terms.

The ontological form of the Quine–Putnam argument goes something like this:

- (P1) we ought to believe in all of the entities which feature in our current best (scientific) theories of the world;
- (P2) mathematical entities feature in our current best (scientific) theories of the world; so,
- (C) we ought to believe in mathematical entities.

As presented here, the argument is valid, and (P2) looks very difficult to deny. (P1) is supported by two major themes of Quine’s philosophy, naturalism and holism.

Naturalism in fact motivates a stronger version of (P1): that we ought to believe in all of *and only* the entities which appear in our current best (scientific) theories of the world; but the additional strength is not wanted in this argument. Naturalism is

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<sup>20</sup> There is an obvious tension between mathematical naturalism and the Dummettian ‘logicism’ endorsed in Ch. 5.3.1. Presumably the hard-nosed Quinean line would be to deny that there is any such thing as ‘mathematics’ in this privileged sense.

the endorsement of scientific, and the rejection of non-scientific modes of inquiry, as means of discovering what there is in the world.

Holism motivates or strengthens the ‘all’ of (P1): it rejects the idea that we could make a distinction among our theories or the entities they invoke between the parts which naturalism counsels us to take seriously, and parts which we are free to take as purely instrumental. Our theories are confirmed *en masse*, and as such we cannot pick and choose.

The Quine–Putnam indispensability arguments have received a great deal of philosophical attention. Despite having received a great deal of criticism, the argument has also been defended by many. It is perhaps the most commonly accepted reason for mathematical realism among analytically inclined philosophers working today.

I will not be concerned here so much to assess the soundness of the argument: there is an enormous literature doing that. I will therefore take it that the argument is at least *prima facie* compelling; and I want instead to see exactly what the conclusion amounts to. That is to say, what will concerns us here is the question of how much mathematics is justified by the indispensability arguments.

The first point to note is that Quine endorses set-theoretic reductionism as a conceptual economy; so the ‘how much?’ question becomes ‘how much set theory?’ The next point is that Quine draws a distinction between the parts of set theory which receive empirical support, and those parts which do not, such as higher set theory:

I recognize indenumerable infinities only because they are forced on me by the simplest known systematizations of more welcome [*sc*: scientific] matters. Magnitudes in excess of such demands, e.g.,  $\aleph_\omega$  or inaccessible numbers, I look upon only as mathematical recreation and without ontological rights.<sup>21</sup>

In view of this, it should be stressed that Quine is not fully naturalistic with respect to mathematical practice. Quine takes the indispensability argument to support only a certain amount of currently existing mathematics.<sup>22</sup>

In discussing the indispensability argument, Feferman has claimed<sup>23</sup> that all

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<sup>21</sup> Quine, ‘Reply to Parsons’ p. 400.    <sup>22</sup> This is in contrast to Maddy, *Naturalism in Mathematics*, who urges that we should not look outside mathematical practice for the justification of parts of mathematics.    <sup>23</sup> Feferman, ‘Infinity in mathematics’.

scientifically applied mathematics is predicatively acceptable, and can be formalized in something like his formal system W:

*the necessary use of higher set theory in the mathematics of the finite has yet to be established. Furthermore, a case can be made that higher set theory is dispensable in scientifically applicable mathematics, that is, in that part of everyday mathematics which finds its applications in the other sciences. Put in other terms: the actual infinite is not required for the mathematics of the physical world.*<sup>24</sup>

What this hinges on is the fact that more or less all of the positive results of functional analysis are provable in systems such as W: it is these parts of mathematics which are heavily used in physics. The negative results — the counterexamples, the pathological cases — are for the most part not provable. For example, the existence of non-measurable sets cannot be proved by predicatively acceptable means. (In the framework of second-order arithmetic, indeed, there seems to be no natural way even to define discontinuous functions on the reals, never mind to prove results about them.)

While it is true that some highly theoretical physics uses mathematics far in excess of what is predicatively obtainable, this work seems to be very speculative, and certainly a long way from what is needed to explain the phenomena. A distinction should be drawn between what physicists say as physicists, which naturalism requires us to accept as a basis for an indispensability argument, and what physicists say when they are engaged in something more akin to pure mathematics. This may indeed be inspired by physics; but that is not in itself enough to make it worthy of the respect that we give to the working parts of the machinery.

In the postscript to ‘Weyl vindicated: *Das Kontinuum* seventy years later’, Feferman considers two proposed locations for potential counterexamples that all empirically applicable mathematics can be formulated in W.<sup>25</sup> One is the use of non-separable Hilbert spaces in quantum mechanics and in statistical mechanics; the other is the use of non-measurable sets in a proposed hidden-variable interpretation of quantum mechanics. Both are highly speculative on the theoretical level, and it is currently unclear how these theoretical models could be applied if they were to be accepted.

<sup>24</sup> Feferman, ‘Infinity in mathematics’ p. 30. Emphasis in original. <sup>25</sup> Pp. 281–283; added in 1996.

### 7.2.2 Mathematics — Gödel–Friedman

While the scientific indispensability argument just discussed is extremely well-known, there is also a form of indispensability argument which draws primarily on mathematical resources. The *locus classicus* of such a form of argument is the analogy drawn by Gödel between mathematics and physics:

It seems to me that the assumption of such objects [i.e. classes and concepts, realistically construed and so not subject to the VCP] is quite as legitimate as the assumption of physical bodies and there is quite as much reason to believe in their existence. They are in the same sense necessary to obtain a satisfactory system of mathematics as physical bodies are necessary for a satisfactory system of our sense perceptions....<sup>26</sup>

This line of argument against predicativism has been explicitly pressed by Hellman.<sup>27</sup>

Since Gödel's Incompleteness Theorem, it has been clear that no single formal system of mathematics will be complete, even with respect to  $\Pi_1^0$  sentences of arithmetic. Nonetheless, some incompletenesses are more troubling, or seem more unnatural, than others. And since the discovery of Gödelian incompleteness, there have been continued efforts to find examples of undecidable sentences which are genuinely 'mathematical' in character, rather than metamathematical, as Gödel sentences are. The Paris–Harrington sentence and Goodstein's theorem are examples of this sort, which are formulable but not provable in PA. Somewhat more recently, Harvey Friedman has been engaged on a programme to find examples which require higher set theory (such as the Axiom of Replacement, or even large cardinal hypotheses) for their solution. This programme was indeed first suggested by Gödel, which explains the title of this subsection.

Most relevant to the predicativist programme is the example (also due to Friedman) of a finite form of Kruskal's theorem that can be shown to imply the 1-consistency of  $\text{ATR}_0$ .<sup>28</sup> As such, it is predicatively unprovable.

On reflection, the supposed contrast between these (supposedly mathematical) examples and the (supposedly metamathematical) Gödelian statements becomes murkier. As is well known, for theories of the usual sort, Gödel sentences and consistency sentences can be found in  $\Pi_1$  form. It is certainly true that these will

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<sup>26</sup> Gödel, 'Russell's mathematical logic [1944]' p. 137.    <sup>27</sup> Hellman, 'Predicativism as a philosophical position'.    <sup>28</sup> For excellent discussion, see Smoryński, 'Varieties of arboreal experience'.

typically be very lengthy sentences which are only considered by mathematicians because of their metamathematical meaning: but that is no fault of the sentences; it is just a fact about how we happen to have come by them. In themselves, these sentences are simply universal generalizations about the natural numbers. What is supposedly more mathematical about examples such as the Paris–Harrington sentence is that they are more closely related to sentences which mathematicians have considered for reasons internal to the development of mathematics (in this case, Ramsey theory; in the case of Kruskal’s Theorem, graph theory).

The key question is whether any such examples can form the grounds of a compelling indispensability argument for impredicative mathematics. How is it to be established that these results, which are predicatively meaningful but beyond the scope of predicative proof, are indispensable to mathematics?

A necessary preliminary is to establish that the results claimed indispensable are in fact true. The evidence that can be presented for such claims will be either a formal proof, or some sort of intuitive argument. Formal proof seems to be of no use to Hellman here: a proof serves only to move us from belief in the axioms to believe in the conclusion, and the issue here is precisely that the predicativist sees no reason to take the classicist’s axioms to be true.

The intuitive arguments which can be given in support of the claims will of course vary with the claims. Most of the claims studied so far have been combinatorial in character. The extent to which direct intuitive arguments for such principles are found compelling is a somewhat subjective matter. But it seems to be perfectly possible for the predicativist simply to dig in her heels in the face of such claims: she need not grant them any status higher than conjectures.

## Chapter 8

# Conclusion

The picture of mathematics as ‘abstract physics’ (to use Coffa’s vivid phrase) is now so familiar as to be a philosophical commonplace. While the most famous defence of the view is due to Gödel, it is more or less explicit in the work of Zermelo and Russell since about 1905, with their appeal to the regressive method of justifying the axioms. The familiarity of the picture should not blind us to its unhappiness and its unnaturalness, at least if we take a longer view of the history of mathematics. What, after all, is special about mathematics? Where, to take Gordan’s contrast, should we draw the line between mathematics and theology? Or between mathematics and physics?

A vague formulation of the traditional answer is that mathematics has some sort of compulsory character for us as rational beings (which, most concede nowadays, theology does not have); and that unlike physics, this compulsory character is not based on empirical evidence. Mathematics is not simply a matter of drawing consequences from bald existential assumptions.

My main object in this thesis has been to show that, if we take the natural numbers as given — that is to say, if we set aside the sceptical doubts of the finitist — then predicative mathematics has this compulsory character, whereas the classical mathematics of the continuum does not.

I have not been able to discuss in any great depth the question of why we are justified in taking the naturals as given; indeed, I am unsure if there is much which can helpfully be said. I would suggest that our knowledge of finitary mathematics is ultimately derived from our symbolic intuition of stroke strings; this is a faculty

which underlies our capacity to understand language, and is perhaps related to our intuition of the passage of time. But I would also suggest that we have the ability to reflect on this finitary mathematics, and so to see the natural numbers as, in principle, completable. It is on this conception that classical arithmetic — and so also predicativism — are based.

Further, I have argued that this apparently modest basis suffices for the development of an enormous amount of mathematics; following Feferman, I suggest that it accounts for all of the mathematics which is scientifically applicable.

The predicativist view that I present can be located in philosophical space somewhere between the positions of Isaacson and Dummett: it shares Isaacson's view that first-order Peano arithmetic (with classical logic) is primitively compelling, and Dummett's criticisms of the classical mathematics of the continuum and beyond, based on the idea of indefinite extensibility.

It is unseemly and inappropriate for philosophers to try to tell mathematicians what to do, and unrealistic to imagine that the mathematicians would listen if they did not like what they were being told. (One might perhaps, in a Lakatosian spirit, think of trying to influence the bodies which fund research in pure mathematics.) The proper role for philosophy of mathematics is not to tell mathematicians what to do, but to tell them what they are doing. In this spirit, I will put my conclusion like this. The distinction between the part of mathematics that is wholly predicative and the part which makes essential use of impredicative methods is a philosophical watershed; it is a distinction with deep epistemological significance. Mathematicians who work on the far side of the divide are doing something else, and something which has a much weaker claim to be taken seriously, and indeed which has a much weaker claim to be taken as mathematics, in the traditional sense.

There is of course much work left to do. In Chapter 4 I gave a criticism of classicism; in the rest of the thesis I have been advocating predicativism as an alternative: I have argued that it avoids the problems identified with classicism, and that it is compelling as the unfolding of a few simple and intuitively attractive ideas. However, an area that deserves more attention is to investigate whether any of the potential positions intermediate between predicativism and full classicism can be given similar justifications. Notably, Feferman has written that while he is sympathetic to predicativism, his own mathematical beliefs are based on a somewhat broader base: he finds certain principles based on iterated inductive definitions to

be compelling. While the mathematics of such principles have been studied,<sup>1</sup> the philosophical work has barely begun.

More relevant to the predicativist project, though, is further philosophical work on the intuitive meaning and acceptability of various mathematical principles or formal systems which are widely taken to be predicatively acceptable. The sort of work I have in mind is of the same sort as that which I attempted in Ch. 5.3. Such work could tell us, for instance, whether the predicativist can straightforwardly endorse the legitimacy of the full second-order induction scheme in the context of an otherwise predicative formal system (such as ACA rather than  $ACA_0$ ). Of particular interest would be a thorough account of Feferman's system W. An assessment of the predicative acceptability of various forms of Choice principle would also be of interest; again, there is much technical work done here, but very little philosophical analysis.

Even in the absence of such further work, though, I hope that what I have done here is enough to show that predicativism is worthy of serious consideration as a philosophy of mathematics. It vindicates the traditional conception of mathematics as a body of truths which are rationally compelling, independently of empirical evidence; but it also justifies the mathematics that is used in the empirical sciences. It neither cripples mathematics, nor betrays its nature.

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<sup>1</sup> Buchholz et al., *Iterated Inductive Definitions*



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