REALISM IN MATHEMATICS

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REALISM

1. Pre-theoretic realism

Of the many odd and various things we believe, few are believed more confidently than the truths of simple mathematics. When asked for an example of a thoroughly dependable fact, many will turn from common sense—'after all, they used to think humans couldn't fly'—from science—'the sun has risen every day so far, but it might fail us tomorrow'—to the security of arithmetic—'but 2 plus 2 is surely 4'.

Yet if mathematical facts are facts, they must be facts about something; if mathematical truths are true, something must make them true. Thus arises the first important question: what is mathematics about? If 2 plus 2 is so definitely 4, what is it that makes this so?

The guileless answer is that $2 + 2 = 4$ is a fact about numbers, that there are things called '2' and '4', and an operation called 'plus', and that the result of applying that operation to 2 and itself is 4. '$2 + 2 = 4$' is true because the things it's about stand in the relation it claims they do. This sort of thinking extends easily to other parts of mathematics: geometry is the study of triangles and spheres; it is the properties of these things that make the statements of geometry true or false; and so on. A view of this sort is often called 'realism'.

Mathematicians, though privy to a wider range of mathematical truths than most of us, often incline to agree with unsullied common sense on the nature of those truths. They see themselves and their colleagues as investigators uncovering the properties of various fascinating districts of mathematical reality: number theorists study the integers, geometers study certain well-behaved spaces, group theorists study groups, set theorists sets, and so on. The very experience of doing mathematics is felt by many to support this position:

The main point in favor of the realistic approach to mathematics is the instinctive certainty of most everybody who has ever tried to solve a problem that he is thinking about 'real objects', whether they are sets, numbers, or whatever . . . (Moschovakis (1980), 605)

Realism, then (at first approximation), is the view that mathematics is the science of numbers, sets, functions, etc., just as physical science is the study of ordinary physical objects, astronomical bodies, subatomic particles, and so on. That is, mathematics is about these things, and the way these things are is what makes mathematical statements true or false. This seems a simple and straightforward view. Why should anyone think otherwise?

Alas, when further questions are posed, as they must be, embarrassments arise. What sort of things are numbers, sets, functions, triangles, groups, spaces? Where are they? The standard answer is that they are abstract objects, as opposed to the concrete objects of physical science, and as such, that they are without location in space and time. But this standard answer provokes further, more troubling questions. Our current psychological theory gives the beginnings of a convincing portrait of ourselves as knowers, but it contains no chapter on how we might come to know about things so irrevocably remote from our cognitive machinery. Our knowledge of the physical world, enshrined in the sciences to which realism compares mathematics, begins in simple sense perception. But mathematicians don't, indeed can't, observe their abstract objects in this sense. How, then, can we know any mathematics; how can we even succeed in discussing this remote mathematical realm?

Many mathematicians, faced with these awkward questions about what mathematical things are and how we can know about them, react by retreating from realism, denying that mathematical statements are about anything, even denying that they are true: 'we believe in the reality of mathematics, but of course when philosophers attack us with their paradoxes we rush to hide behind formalism and say "Mathematics is just a combination of meaningless symbols" . . .'.[1] This formalist position—that mathematics is just a game with symbols—faces formidable obstacles of its own, which I'll touch on below, but even without these, many mathematicians find it involving them in an uncomfortable form of

[1] Dieudonne, as quoted in Davis and Hersh (1981), 321.

double-think. The same writer continues: 'Finally we are left in peace to go back to our mathematics and do it as we have always done, with the feeling each mathematician has that he is working on something real' (Davis and Hersh (1981), 321). Two more mathematicians summarize:

> the typical working mathematician is a [realist] on weekdays and a formalist on Sundays. That is, when he is doing mathematics he is convinced that he is dealing with an objective reality whose properties he is attempting to determine. But then, when challenged to give a philosophical account of this reality, he finds it easiest to pretend that he does not believe in it after all. (Davis and Hersh (1981), 321)

Yet this occasional inauthenticity is perhaps less troubling to the practising mathematician than the daunting requirements of a legitimate realist philosophy:

> Nevertheless, most attempts to turn these strong [realist] feelings into a coherent foundation for mathematics invariably lead to vague discussions of 'existence of abstract notions' which are quite repugnant to a mathematician . . . Contrast with this the relative ease with which formalism can be explained in a precise, elegant and self-consistent manner and you will have the main reason why most mathematicians claim to be formalists (when pressed) while they spend their working hours behaving as if they were completely unabashed realists. (Moschovakis (1980), 605–6)

Mathematicians, after all, have their mathematics to do, and they do it splendidly. Dispositionally suited to a subject in which precisely stated theorems are conclusively proved, they might be expected to prefer a simple and elegant, if ultimately unsatisfying, philosophical position to one that demands the sort of metaphysical and epistemological rough-and-tumble a full-blown realism would require. And it makes no difference to their practice, as long as double-think is acceptable.

But to the philosopher, double-think is not acceptable. If the very experience of doing mathematics, and other factors, soon to be discussed, favour realism, the philosopher of mathematics must either produce a suitable philosophical version of that position, or explain away, convincingly, its attractions. My goal here will be to do the first, to develop and defend a version of the mathematician's pre-philosophical attitude.

Rather than attempt to treat all of mathematics, to bring the project down to more manageable size, I'll concentrate here on the mathematical theory of sets.[2] I've made this choice for several reasons, among them the fact that, in some sense, set theory forms a foundation for the rest of mathematics. Technically, this means that any object of mathematical study can be taken to be a set, and that the standard, classical theorems about it can then be proved from the axioms of set theory.[3]

Striking as this technical fact may be, the average algebraist or geometer loses little time over set theory. But this doesn't mean that set theory has no practical relevance to these subjects. When mathematicians from a field outside set theory are unusually frustrated by some recalcitrant open problem, the question arises whether its solution might require some strong assumption heretofore unfamiliar within that field. At this point, practitioners fall back on the idea that the objects of their study are ultimately sets and ask, within set theory, whether more esoteric axioms or principles might be relevant. Given that the customary axioms of set theory don't even settle all questions about sets,[4] it might even turn out that this particular open problem is unsolvable on the basis of these most basic mathematical assumptions, that entirely new set theoretic assumptions must be invoked.[5] In this sense, then, set theory is the ultimate court of appeal on questions of what mathematical things there are, that is to say, on what philosophers call the 'ontology' of mathematics.[6]

Philosophically, however, this ontological reduction of mathematics to set theory has sometimes been taken to have more dramatic consequences, for example that the entire philosophical foundation of any branch of mathematics is reducible to that of set theory. In this sense, comparable to implausibly strong versions of

[2] A set is a collection of objects. Among the many good introductions to the mathematical theory of these simple entities, I recommend Enderton (1977).

[3] See e.g. the reduction of arithmetic and real number theory to set theory in Enderton (1977), chs. 4 and 5. There are some exceptions to the rule that all mathematical objects can be thought of as sets—e.g. proper classes and large categories—but I will ignore these cases for the time being.

[4] Some details and philosophical consequences of this situation are the subject of ch. 4.

[5] Eklof and Mekler (forthcoming) give a survey of algebraic examples, and Moschovakis (1980) does the same for parts of analysis.

[6] In philosophical parlance, 'ontology', the study of what there is, is opposed to 'epistemology', the study of how we come to know what we do about the world. I will use the word 'metaphysics' more or less as a synonym for 'ontology'.

the thesis that physics is basic to the natural sciences,[7] I think the claim that set theory is foundational cannot be correct. Even if the objects of, say, algebra are ultimately sets, set theory itself does not call attention to their algebraic properties, nor are its methods suitable for approaching algebraic concerns. We shouldn't expect the methodology or epistemology of algebra to be identical to that of set theory any more than we expect the biologist's or the botanist's basic notions and techniques to be identical to those of the physicist. But again, this methodological independence of the branches of mathematics from set theory does not mean there must be mathematical entities other than sets any more than the methodological independence of psychology or chemistry from physics means there must be non-physical minds or chemistons.[8]

But little hangs on this assessment of the nature of set theory's foundational role. Even if set theory is no more than one among many branches of mathematics, it is deserving of philosophical scrutiny. Indeed, even as one branch among many, contemporary set theory is of special philosophical interest because it throws into clear relief a difficult and important philosophical problem that challenges many traditional attitudes toward mathematics in general. I will raise this problem in Chapter 4.

Finally, it is impossible to divorce set theory from its attendant disciplines of number theory and analysis. These two fields and their relationship to the theory of sets will form a recurring theme in what follows, especially in Chapters 3 and 4.

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~~2. Realism in philosophy~~

~~So far, I've been using the key term 'realism' loosely, without clear definition. This may do in pre philosophical discussion, but from~~

[7] This view is called 'physicalism'. I'll come back to it in ch. 5, sect. 1, below.

[8] There was a time when the peculiarities of biological science led practitioners to vitalism, the assumption that a living organism contains a non-physical component or aspect for whose behaviour no physical account can be given. Nowadays, this idea is discredited—simply because it proved scientifically sterile—and, as far as I know, no one ever urged the acceptance of 'chemistons'. Today, psychology is the special science that most often lays claim to a non-physical subject matter, but as suggested in the text, it seems to me that a purely physical ontology is compatible with the most extreme methodological independence. For discussion, see Fodor (1975), 9–26.

~~correspondence truth and reference, but here, again for the record, I want to emphasize that this way of stating things is convenient but not essential. Those realists who believe that a robust reference relation is not needed in science, either because the disquotationalist is right, or for some other reason, are invited simply to recast the discussions that follow in terms of 'reliable connection'.~~

~~One final remark on realism and truth. Some anti-realists, assuming the realist is wedded to correspondence truth, have argued that realism is unscientific because it requires a connection between scientific theory and the world that reaches beyond the bounds of science itself.~~[44] ~~Here the anti-realist attempts to saddle the realist with the now familiar unnaturalized standpoint, the point of view that stands above, outside, or prior to, our best theories of the world, and from which is posed the question: what connects our theories to the world?~~

~~We've seen that in epistemology, the contemporary realist has answered by rejecting the extra-scientific challenge itself, along with the radical scepticism it engenders. The same goes for semantics. There is no point of view prior to or superior to that of natural science. What we want is a theory of how our language works, a theory that will become a chapter of that very scientific world-view. In order to arrive at this new chapter, it would be madness to cast off the scientific knowledge collected so far. Rather, we stand within our current best theory—what better account do we have of the way the world is?—and ask for an account of how our beliefs and our language connect up with the world as that theory says it is. This may be the robust theory of reference required by correspondence truth. If the disquotationalist is right, it may be something less structured, an account of reliable connection. But neither way is it something extra-scientific.~~

4. Realism in mathematics

Let me turn at last to realism in the philosophy of mathematics proper. Most prominent in this context is a folkloric position called 'Platonism' by analogy with Plato's realism about universals. As is

[44] See e.g. Putnam (1977), 125. Or, from a different point of view, Burgess (forthcoming *a*).

common with such venerable terms, it is applied to views of very different sorts, most of them not particularly Platonic.[45] Here I will take it in a broad sense as simply synonymous with 'realism' as applied to the subject matter of mathematics: mathematics is the scientific study of objectively existing mathematical entities just as physics is the study of physical entities. The statements of mathematics are true or false depending on the properties of those entities, independent of our ability, or lack thereof, to determine which.

Traditionally, Platonism in the philosophy of mathematics has been taken to involve somewhat more than this. Following some of what Plato had to say about his Forms, many thinkers have characterized mathematical entities as abstract—outside of physical space, eternal and unchanging—and as existing necessarily—regardless of the details of the contingent make-up of the physical world. Knowledge of such entities is often thought to be a priori—sense experience can tell us how things are, not how they must be—and certain—as distinguished from fallible scientific knowledge. I will call this constellation of opinions 'traditional Platonism'.

Obviously, this uncompromising account of mathematical reality makes the question of how we humans come to know the requisite a priori certainties painfully acute. And the successful application of mathematics to the physical world produces another mystery: what do the inhabitants of the non-spatio-temporal mathematical realm have to do with the ordinary physical things of the world we live in? In his theory of Forms, Plato says that physical things 'participate' in the Forms, and he uses the fact of our knowledge of the latter, via a sort of non-sensory apprehension, to argue that the soul must pre-exist birth.[46] But our naturalized realist will hardly buy this package.

Given these difficulties with traditional Platonism, it's not surprising that various forms of mathematical anti-realism have been proposed. I'll pause to consider a sampling of these views before describing the two main schools of contemporary Platonism.

[45] For example, though the term 'Platonism' suggests a realism about universals, many Platonists regard mathematics as the science of peculiarly mathematical particulars: numbers, functions, sets, etc. An exception is the structuralist approach considered in ch. 5, sect. 3, below.

[46] See his *Phaedo* 72 D–77 A.

In the late 1600s, in response to a number of questions from physical science, Sir Isaac Newton and Gottfried Wilhelm von Leibniz simultaneously and independently invented the calculus. Though the scientist's problems were solved, the new mathematical methods were scandalously error-ridden and confused. Among the most vociferous and perceptive critics was the idealist Berkeley, an Anglican bishop who hoped to silence the atheists by showing their treasured scientific thinking to be even less clear than theology. The central point of contention was the notion of infinitesimals, infinitely small amounts still not equal to zero, which Berkeley ridiculed as 'the ghosts of departed quantities'.[47] Two centuries later, Bolzano, Cauchy, and Weierstrass had replaced these ghosts with the modern theory of limits.[48]

This account of limits required a foundation of its own, which Georg Cantor and Richard Dedekind provided in their theory of real numbers, but these in turn reintroduced the idea of the completed infinite into mathematics. No one had ever much liked the seemingly paradoxical idea that a proper part of an infinite thing could be in some sense as large as the whole—there are as many even natural numbers as there are even and odd, there are as many points on a one-inch line segment as on a two-inch line segment—but the infinite sets introduced by Cantor and others gave rise to outright contradictions, of which Bertrand Russell's is the most famous:[49] consider the set of all sets that are not members of themselves. It is self-membered if and only if it isn't. The opening decades of this century saw the development of three great schools of thought on the nature of mathematics, all of them designed to deal in one way or another with the problem of the infinite.

The first of these is intuitionism, which dealt with the infinite by rejecting it outright. The original version of this position, first proposed by L. E. J. Brouwer,[50] was analogous to Berkeleian

47 See Berkeley (1734), subtitled 'A Discourse Addressed to an Infidel Mathematician. Wherein It is Examined Whether the Object, Principles, and Inferences of the Modern Analysis are More Distinctly Conceived, or More Evidently Deduced, than Religious Mysteries and Points of Faith'. The quotation is from p. 89.

48 For a more detailed description of the developments sketched in this paragraph and the next, see Kline (1972), chs. 17, 40, 41, and 51, or Boyer (1949).

49 The paradox most directly associated with Cantor's work is Burali-Forti's (1897). See Cantor's discussion (1899). Russell's primary target was Frege, as will be noted below.

50 Brouwer (1913; 1949). Other, less opaque, expositions of this position are Heyting (1931; 1966) and Troelstra (1969).

idealism: it takes the objects of mathematics to be mental constructions rather than objective entities. The modern version, defended by Michael Dummett,[51] is a brand of verificationism: a mathematical statement is said to be true if and only if it has been constructively proved. Either way, a series of striking consequences follow: statements that haven't been proved or disproved are neither true nor false; completed infinite collections (like the set of natural numbers) are illegitimate; much of infinitary mathematics must either be rejected (higher set theory) or radically revised (real number theory and the calculus).

These forms of intuitionism face many difficulties—e.g. does each mathematician have a different mathematics depending on what she's mentally constructed? how can we verify even statements about large finite numbers? etc.—but its most serious drawback is that it would curtail mathematics itself. My own working assumption is that the philosopher's job is to give an account of mathematics as it is practised, not to recommend sweeping reform of the subject on philosophical grounds. The theory of the real numbers, for example, is a fundamental component of the calculus and higher analysis, and as such is far more firmly supported than any philosophical theory of mathematical existence or knowledge. To sacrifice the former to preserve the latter is just bad methodology.

A second anti-realist position is formalism, the popular school of double-think mentioned above. The earliest versions of the view that mathematics is a game with meaningless symbols played heavily on a simple analogy between mathematical symbols and chess pieces, between mathematics and chess, but even its advocates were uncomfortably aware of the stark disanalogies:[52]

> To be sure, there is an important difference between arithmetic and chess. The rules of chess are arbitrary, the system of rules for arithmetic is such that by means of simple axioms the numbers can be referred to perceptual manifolds and can thus make [an] important contribution to our knowledge of nature.

The Platonist Gottlob Frege launched a fierce assault on early formalism, from many directions simultaneously, but the most

[51] Dummett (1975; 1977).

[52] Frege cites this quotation from Thomae in his critique of formalism: Frege (1903), § 88.

penetrating arose from just this point. It isn't hard to see how various true statements of mathematics can help me determine how many bricks it will take to cover the back patio, but how can a meaningless string of symbols be any more relevant to the solution of real world problems than an arbitrary arrangement of chess pieces?

This is Frege's problem: what makes these meaningless strings of symbols useful in applications?[53] Suppose, for example, that a physicist tests a hypothesis by using mathematics to derive an observational prediction. If the mathematical premiss involved is just a meaningless string of symbols, what reason is there to take that observation to be a consequence of the hypothesis? And if it is not a consequence, it can hardly provide a fair test. In other words, if mathematics isn't true, we need an explanation of why it is all right to treat it as true when we use it in physical science.

The most famous version of formalism, the one expounded during the period under consideration here, was David Hilbert's programme.[54] Hilbert, like Brouwer, felt that only finitary mathematics was truly meaningful, but he considered Cantor's theory of sets 'one of the supreme achievements of purely intellectual human activity' and promised, in a famous remark, that

> No one shall drive us out of the paradise which Cantor has created for us. (Hilbert (1926), 188, 191)

Hilbert proposed to save infinitary mathematics by treating it instrumentally—meaningless statements about the infinite are a useful tool in deriving meaningful statements about the finite—but he, unlike the scientific instrumentalists, was sensitive to the question of how this practice could be justified. Hilbert's plan was to give a metamathematical proof that the use of the meaningless statements of infinitary mathematics to derive meaningful statements of finitary mathematics would never produce incorrect finitary results. The same line of thought might have applied to its use in natural science as well, thus solving Frege's problem. Hilbert's efforts to carry through on this project produced the rich

[53] See Frege (1903), § 91.
[54] See Hilbert (1926; 1928).

new field of metamathematics, but Kurt Gödel soon proved that its cherished goal could not be reached.[55]

For all the simplicity of game formalism and the fame of Hilbert's programme, many mathematicians, when they claim to be formalists, actually have another idea in mind: mathematics isn't a science with a peculiar subject matter; it is the logical study of what conclusions follow from which premisses. Philosophers call this position 'if-thenism'. Several prominent philosophers of mathematics have held this position at one time or another—Hilbert (before his programme), Russell (before his logicism), and Hilary Putnam (before his Platonism)[56]—but all ultimately rejected it. Let me briefly indicate why.

A number of annoying difficulties plague the if-thenist: which logical language is appropriate for the statement of premisses and conclusions? which premisses are to be presupposed in cases like number theory, where assumptions are usually left implicit? from among the vast range of arbitrary possibilities, why do mathematicians choose the particular axiom systems they do to study? what were historical mathematicians doing before their subjects were axiomatized? what are they doing when they propose new axioms? and so on. But the question that seems to have scotched if-thenism in the minds of Russell and Putnam was a version of Frege's problem: how can the fact that one mathematical statement follows from another be correctly used in our investigation of the physical world? The general thrust of the if-thenist's reply seems to be that the antecedent of a mathematical if-then statement is treated as an idealization of some physical statement. The scientist then draws as a conclusion the physical statement that is the unidealization of the consequent.[57]

Notice that on this picture, the physical statements must be entirely mathematics-free; the only mathematics involved is that used in moving between them. Unfortunately, many of the

[55] See Gödel (1931). Enderton (1972), ch. 3, gives a readable presentation. Detlefsen (1986) attempts to defend Hilbert's programme against the challenge of Gödel's theorem. Simpson (1988) and Feferman (1988) pursue partial or relativized versions within the limitations of Gödel's theorem.

[56] See Resnik (1980), ch. 3, for discussion. There if-thenism is called 'deductivism'. See also Putnam (1979), p. xiii. Russell's logicism and Putnam's Platonism will be considered below.

[57] See Korner (1960), ch. 8. Cf. Putnam (1967*b*), 33.

statements of physical science seem inextricably mathematical. To quote Putnam, after his conversion:

> one wants to say that the Law of Universal Gravitation makes an objective statement about bodies—not just about sense data or meter readings. What is the statement? It is just that bodies behave in such a way that the quotient of two numbers *associated* with the bodies is equal to a third number *associated* with the bodies. But how can such a statement have any objective content at all if numbers and 'associations' (i.e. functions) are alike mere fictions? It is like trying to maintain that God does not exist and angels do not exist while maintaining at the very same time that it is an objective fact that God has put an angel in charge of each star and the angels in charge of each of a pair of binary stars were always created at the same time! If talk of numbers and 'associations' between masses, etc. and numbers is 'theology' (in the pejorative sense), then the Law of Universal Gravitation is likewise theology. (Putnam (1975*b*), 74–5)

In other words, the if-thenist account of applied mathematics requires that natural science be wholly non-mathematical, but it seems unlikely that science can be so purified.[58]

The third and final anti-realist school of thought I want to consider here is logicism, or really, the version of logicism advanced by the logical positivists. Frege's original logicist programme aimed to show that arithmetic is reducible to pure logic, that is, that its objects—numbers—are logical objects and that its theorems can be proved by logic alone.[59] This version of logicism is outright Platonistic: arithmetic is the science of something objective (because logic is objective), that something objective consists of objects (numbers), and our logical knowledge is a priori. If this project had succeeded, the epistemological problems of Platonism would have been reduced to those of logic, presumably a gain. But Frege's project failed; his system was inconsistent.[60] Russell and Whitehead took up the banner in their *Principia Mathematica*, but were forced to adopt fundamental assumptions no one accepted as

[58] Hartry Field's ambitious attempt to do this will be considered in ch. 5, sect. 2, below. See Field (1980; 1989).

[59] See Frege (1884).

[60] The trouble was the original version of Russell's paradox. (See Russell's letter to Frege, Russell (1902).) Frege's numbers were extensions of concepts. (See ch. 3 below.) Some concepts, like 'red', don't apply to their extensions, others, like 'infinite', do. Russell considered the extension of the concept 'doesn't apply to its own extension'. If it applies to its own extension then it doesn't, and vice versa. This contradiction was provable from Frege's fundamental assumptions. There have been efforts to revive Frege's system; see e.g. Wright (1983) and Hodes (1984).

purely logical.[61] Eventually, Ernst Zermelo (aided by Mirimanoff, Fraenkel, Skolem, and von Neumann) produced an axiom system that showed how mathematics could be reduced to set theory,[62] but again, no one supposed that set theory enjoys the epistemological transparency of pure logic.

Still, the idea that mathematics is just logic was not dead; it was taken up by the positivists, especially Rudolf Carnap.[63] For these thinkers, however, there are no logical objects of any kind, and the laws of logic and mathematics are true only by arbitrary convention. Thus mathematics is not, as the Platonist insists, an objective science. The advantage of this counterintuitive view is that mathematical knowledge is easily explicable; it arises from human decisions. Question: Why are the axioms of Zermelo–Fraenkel true? Answer: Because they are part of the language we've adopted for using the word 'set'.

This conventionalist line of thought was subjected to a historic series of objections by Carnap's student, W. V. O. Quine.[64] The key difficulty is that both mathematical and physical assumptions are enshrined in Carnap's official language. How are we to separate the conventionally adopted mathematical part of the language from the factually true physical hypotheses? Quine argues that it isn't enough to say that the scientific claims, not the mathematical ones, are supported by empirical data:

> The semblance of a difference in this respect is largely due to overemphasis of departmental boundaries. For a self-contained theory which we can check with experience includes, in point of fact, not only its various theoretical hypotheses of so-called natural science but also such portions of logic and mathematics as it makes use of. (Quine (1954), 367)

Mathematics is part of the theory we test against experience, and a successful test supports the mathematics as much as the science.

Carnap makes several efforts to separate mathematics from natural science, culminating in his distinction between analytic and synthetic. Mathematical statements, he argues, are analytic, that is,

[61] See Russell and Whitehead (1913).

[62] Zermelo's first presentation is Zermelo (1908*b*). See also Mirimanoff (1917*a*, *b*), Fraenkel (1922), Skolem (1923), and von Neumann (1925). The standard axioms are now called 'Zermelo–Fraenkel set theory' or ZFC (ZF when the axiom of choice is omitted). See Enderton (1977), 271–2.

[63] See Carnap (1937; 1950).

[64] See Quine (1936; 1951; 1954).

true by virtue of the meanings of the words involved (the logical and mathematical vocabulary); scientific statements, on the other hand, are synthetic, true by virtue of the way the world is. Quine examines this distinction in great detail, investigating various attempts at clear formulation, and concludes:

> It is obvious that truth in general depends on both language and extralinguistic fact. The statement 'Brutus killed Caesar' would be false if the world had been different in certain ways, but it would also be false if the word 'killed' happened rather to have the sense of 'begat'. Thus one is tempted to suppose in general that the truth of a statement is somehow analyzable into a linguistic component and a factual component. Given this supposition, it next seems reasonable that in some statements the factual component should be null; and these are the analytic statements. But, for all its a priori reasonableness, a boundary between analytic and synthetic statements simply has not been drawn. That there is such a distinction to be drawn at all is an unempirical dogma of empiricists, a metaphysical article of faith. (Quine (1951), 36–7)

Without a clear distinction between analytic and synthetic, Carnap's anti-Platonist version of logicism fails.

I will leave the three great schools at this point. I don't claim to have refuted either formalism or conventionalism, though I hope the profound difficulties they face have been drawn clearly enough. Intuitionism I reject on the grounds given above; I assume that the job of the philosopher of mathematics is to describe and explain mathematics, not to reform it.

Let me return now to Platonism, the view that mathematics is an objective science. Platonism naturally conflicts with each of the particular forms of anti-realism touched on here—with intuitionism on the objectivity of mathematical entities, with formalism on the status of infinitary mathematics, with logicism on the need for mathematical existence assumptions going beyond those of logic—but the Platonist's traditional and purest opponent is the nominalist, who simply holds that there are no mathematical entities. (The term 'nominalism' has followed 'Platonism' in its migration from the debate over universals into the debate over mathematical entities.) Two forms of Platonism dominate contemporary debate. The first of these derives from the work of Quine and Putnam sketched above—their respective criticisms of conventionalism and if-thenism—and the second is described by Gödel as the philosophical underpinning for his famous theorems.[65] As Quine and Putnam's writings have just been discussed, let me begin with them.

Quine's defence of mathematical realism follows directly on the heels of the defences of common-sense and scientific realism sketched above. On the naturalized approach, we judge what entities there are by seeing what entities we need to produce the most effective theory of the world. So far, these include medium-sized physical objects and the theoretical entities of physical science, and so far, the nominalist might well agree. But if we pursue the question of mathematical ontology in the same spirit, the nominalist seems cornered:

> A platonistic ontology . . . is, from the point of view of a strictly physicalistic conceptual scheme, as much a myth as that physicalistic conceptual scheme itself is for phenomenalism. This higher myth is a good and useful one, in turn, in so far as it simplifies our account of physics. Since mathematics is an integral part of this higher myth, the utility of this myth for physical science is evident enough. (Quine (1948), 18)

If we countenance an ontology of physical objects and unobservables as part of our best theory of the world, how are we to avoid countenancing mathematical entities on the same grounds? Carnap suggested what Quine calls a 'double standard'[66] in ontology, according to which questions of mathematical existence are linguistic and conventional and questions of physical existence are scientific and real, but we've already seen that this effort fails.

We've also seen that Putnam takes the same thinking somewhat further, emphasizing not only that mathematics simplifies physics, but that physics can't even be formulated without mathematics:[67] 'mathematics and physics are integrated in such a way that it is not possible to be a realist with respect to physical theory and a nominalist with respect to mathematical theory' (Putnam (1975*b*), 74). He concludes that talk about[68]

> mathematical entities is indispensable for science . . . therefore we should

[65] See his letters to Wang, quoted in Wang (1974*b*), 8–11, and Feferman's discussion (1984*b*).

[66] Quine (1951), 45.

[67] See the long quotation from Putnam (1975*b*) above. A more complete account appears in Putnam (1971), esp.§§ 5 and 7.

[68] He really says 'quantification over', which derives from Quine's official criterion of ontological commitment (1948), but I don't want to get into the debate over that precise formulation.

accept such [talk]; but this commits us to accepting the existence of the mathematical entities in question. This type of argument stems, of course, from Quine, who has for years stressed both the indispensability of [talk about] mathematical entities and the intellectual dishonesty of denying the existence of what one daily presupposes. (Putnam (1971), 347)

We are committed to the existence of mathematical objects because they are indispensable to our best theory of the world and we accept that theory.

The particular brand of Platonism that arises from these Quine/Putnam indispensability arguments has some revolutionary features. Recall that traditional Platonism takes mathematical knowledge to be a priori, certain, and necessary. But, if our knowledge of mathematical entities is justified by the role it plays in our empirically supported scientific theory, that knowledge can hardly be classified as a priori.[69] Furthermore, if we prefer to alter our scientific hypotheses rather than our mathematical ones when our overall theory meets with disconfirmation, it is only because the former can usually be adjusted with less perturbation to the theory as a whole.[70] Indeed, Putnam[71] goes so far as to suggest that the best solution to difficulties in quantum mechanics may well be to alter our logical laws rather than any physical hypotheses. Thus the position of mathematics as part of our best theory of the world leaves it as liable to revision as any other part of that theory, at least in principle, so mathematical knowledge is not certain. Finally, the case of necessity is less clear, if only because Quine rejects such modal notions out of hand, but the fact that our mathematics is empirically confirmed in this world surely provides little support for the claim that it is likely to be true in some other possible circumstance. So Quine/Putnam Platonism stands at some considerable remove from the traditional variety.

But while disagreement with a venerable philosophical theory is no clear demerit, disagreement with the realities of mathematical practice is. First, notice that unapplied mathematics is completely without justification on the Quine/Putnam model; it plays no indispensable role in our best theory, so it need not be accepted:[72]

[69] See Putnam (1975*b*) for an explicit discussion of a posteriori methods in mathematics. Kitcher (1983) attacks the idea that mathematics is a priori from a different angle.

[70] See Quine (1951), 43–4.

[71] Putnam (1968).

[72] See also Putnam (1971), 346–7.

> So much of mathematics as is wanted for use in empirical science is for me on a par with the rest of science. Transfinite ramifications are on the same footing insofar as they come of a simplificatory rounding out, but anything further is on a par rather with uninterpreted systems. (Quine (1984), 788)

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

If this weren't enough to disqualify Quine/Putnamism as an account of mathematics as it is practised, consider one last point. In this picture of our scientific theorizing, mathematics enters only at fairly theoretical levels. The most basic evidence takes the form of non-mathematical observation sentences—e.g. 'this chunk of gold is malleable'—and the initial levels of theory consist of non-mathematical generalizations—'gold is a malleable metal'. Mathematics only enters the picture at the more theoretical levels—'gold has atomic number 79'—so it is on an epistemic par with this higher-level theory.[73] But isn't it odd to think of '2 + 2 = 4' or 'the union of the set of even numbers with the set of odd numbers is the set of all numbers' as highly theoretical principles? In Charles Parsons's phrase, Quine/Putnamism 'leaves unaccounted for precisely the *obviousness* of elementary mathematics'.[74]

By way of contrast, the Gödelian brand of Platonism takes its lead from the actual experience of doing mathematics, which he takes to support Platonism as suggested in section 1 above. For Gödel, the most elementary axioms of set theory *are* obvious; in his words, they 'force themselves upon us as being true'.[75] He accounts for this by positing a faculty of mathematical intuition that plays a role in mathematics analogous to that of sense perception in the physical sciences, so presumably the axioms force themselves upon us as explanations of the intuitive data much as the assumption of medium-sized physical objects forces itself upon us as an explanation of our sensory experiences. To push this analogy, recall that this style of argument for common-sense realism might have been

73 See Quine (1948), 18–19.
74 Parsons (1979/80), 151. See also Parsons (1983*b*).
75 Gödel (1947/64), 484.

undercut if phenomenalists had succeeded in giving non-realistic translations of our physical object statements. Similarly, Gödel notes that Russell's 'no-class' interpretation of *Principia* was an effort to do the work of set theory, that is, to systematize all of mathematics, without sets. Echoing the common-sense realist, Gödel takes the failure of Russell's project as support for his mathematical realism:

This whole scheme of the no-class theory is of great interest as one of the few examples, carried out in detail, of the tendency to eliminate assumptions about the existence of objects outside the 'data' and to replace them by constructions on the basis of these data.[76] The result has been in this case essentially negative . . . All this is only a verification of the view defended above that logic and mathematics (just as physics) are built up on axioms with a real content which cannot be 'explained away'. (Gödel (1944), 460–1)

He concludes that

the assumption of [sets] 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 theory of our sense perceptions . . . (Gödel (1944), 456–7)

But this analogy of intuition with perception, of mathematical realism with common-sense realism, is not the end of Gödel's elaboration of the mathematical realist's analogy between mathematics and natural science. Just as there are facts about physical objects that aren't perceivable, there are facts about mathematical objects that aren't intuitable. In both cases, our belief in such 'unobservable' facts is justified by their role in our theory, by their explanatory power, their predictive success, their fruitful interconnections with other well-confirmed theories, and so on. In Gödel's words:

even disregarding the [intuitiveness] of some new axiom, and even in case it has no [intuitiveness] at all, a probable decision about its truth is possible also in another way, namely, inductively by studying its 'success'. . . . There might exist axioms so abundant in their verifiable consequences,

[76] In this passage, 'data' means 'logic without the assumption of the existence of classes' (Gödel (1944), 460 n. 22). Earlier in this same paper, Gödel refers to arithmetic as 'the domain of the kind of elementary indisputable evidence that may be most fittingly compared with sense perception' (p. 449).

shedding so much light upon a whole field, and yielding such powerful methods for solving problems . . . that, no matter whether or not they are [intuitive], they would have to be accepted at least in the same sense as any well-established physical theory. (Gödel (1947/64), 477)

Quite a number of historical and contemporary justifications for set theoretic hypotheses take this form, as will come out in Chapter 4. Here the higher, less intuitive, levels are justified by their consequences at lower, more intuitive, levels, just as physical unobservables are justified by their ability to systematize our experience of observables. At its more theoretical reaches, then, Gödel's mathematical realism is analogous to scientific realism.

Thus Gödel's Platonistic epistemology is two-tiered: the simpler concepts and axioms are justified intrinsically by their intuitiveness; more theoretical hypotheses are justified extrinsically, by their consequences. This second tier leads to departures from traditional Platonism similar to Quine/Putnam's. Extrinsically justified hypotheses are not certain,[77] and, given that Gödel allows for justification by fruitfulness in physics as well as in mathematics,[78] they are not a priori either. But, in contrast with Quine/Putnam, Gödel gives full credit to purely mathematical forms of justification —intuitive self-evidence, proofs, and extrinsic justifications within mathematics—and the faculty of intuition does justice to the obviousness of elementary mathematics.

Among Gödel's staunchest critics is Charles Chihara.[79] Even if Gödel has succeeded in showing that the case for the existence of mathematical entities runs parallel to the case for the existence of physical ones, Chihara argues that he has by no means shown that the two cases are of the same strength, and thus, that he has not established that there is as much reason to believe in the one as to believe in the other.[80] Furthermore, Chihara argues, the existence of mathematical entities is not required to explain the experience of mathematical intuition and agreement:

I believe it is at least as promising to look for a naturalistic explanation based on the operations and structure of the internal systems of human beings. (Chihara (1982), 218)

[77] Gödel (1944), 449.
[78] Gödel (1947/64), 485.
[79] See Chihara (1973), ch. 2; (1982).
[80] Chihara (1982), 213–14.

. . . mathematicians, regarded as biological organisms, are basically quite similar. (Chihara (1973), 80)

And finally, he questions whether Gödel's intuition offers any explanation at all:[81]

the 'explanation' offered is so vague and imprecise as to be practically worthless: all we are told about how the 'external objects' explain the phenomena is that mathematicians are 'in some kind of contact' with these objects. What empirical scientist would be impressed by an explanation this flabby? (Chihara (1982), 217)

Now the Gödelian Platonist is not entirely defenceless in the face of this attack. For example, Mark Steiner[82] points out that Chihara's 'explanation' is likewise lacking in muscle tone: the similarity of human beings as organisms can hardly explain their agreement about mathematics when it is consistent with so much disagreement on other subjects. Still, most observers tend to agree that no appeal to purported human experiences of *x*s that underlie our theory of *x*s can justify a belief in the existence of *x*s unless we have some independent reason to think our theory of *x*s is true.[83] Thus the purported human dealings with witches that underlie our theory of witches don't justify a belief in witches unless we have some independent reason to think that our theory of witches is actually correct.

But notice: we have recently rehearsed just such an independent reason in the case of mathematics, namely, the indispensability arguments of Quine and Putnam. Unless endorsing these commits one to the view that there is no peculiarly mathematical form of evidence—and I don't see why it should[84]—there is room for an attractive compromise between Quine/Putnam and Gödelian Platonism. It goes like this: successful applications of mathematics give us reason to believe that mathematics is a science, that much of it at least approximates truth. Thus successful applications justify, in a general way, the practice of mathematics. But, as we've seen, this isn't enough to give an adequate account of mathematical practice,

[81] These remarks of Chihara's are actually addressed to a quotation from Kreisel, but it is clear from the context that he thinks the same objection applies to Gödel's intuition.

[82] Steiner (1975*b*), 190.

[83] See Steiner (1975*b*), 190. For a similar sentiment, see Putnam (1975*b*), 73–4.

[84] Nor does Parsons (1983*b*), 192–3.

of how and why it works. We still owe an account of the obviousness of elementary mathematics, which Gödel's intuition is designed to provide, and an account of other purely mathematical forms of evidence, like proof and various extrinsic methods. This means we need to explain what intuition is and how it works; we need to catalogue extrinsic methods and explain why they are rational methods in the pursuit of truth.

From Quine/Putnam, this compromise takes the centrality of the indispensability arguments; from Gödel, it takes the recognition of purely mathematical forms of evidence and the responsibility for explaining them. Thus it averts a major difficulty with Quine/Putnamism—its unfaithfulness to mathematical practice—and a major difficulty with Gödelism—its lack of a straightforward argument for the truth of mathematics. But whatever its merits, compromise Platonism does nothing to remedy the flabbiness of Gödel's account of intuition. And it is in this neighbourhood that many contemporary objections to Platonism are concentrated.[85]

I opened this chapter with the hope of reinstating the mathematician's pre-philosophical realism, of devising a defensible refinement of that attitude that remains true to the phenomenology of practice. Along the way, I've sided with common-sense realism, scientific realism, and philosophical naturalism, and seconded many of the advances of Quine/Putnam and Gödelian Platonism. It will come as no surprise, then, that the position to be defended here is a version of compromise Platonism. I'll call it 'set theoretic realism'.

Chapter 2 outlines a naturalistic epistemology for items located on the lower tier of Gödel's two-tiered epistemology, a replacement for Gödel's intuition. The ontological question of the relationship between sets and other mathematical entities, particularly natural and real numbers, is the subject of Chapter 3. Chapter 4 contains some preliminary spadework on the problem of theoretical justification, the second of Gödel's two tiers. I argue that this ill-understood problem is the most important open question of our day, not only for set theoretic realism, but for many other mathematical philosophies as well. Chapter 5 takes a final look at set theoretic realism from physicalist and structuralist perspectives.

[85] See ch. 2, sect. 1, below.