CHAPTER 22

LOGIC AND THE FOUNDATIONS OF MATHEMATICS

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Catalyzed by the failure of Frege's logicist program, logic and the foundations of mathematics first became a philosophical topic in Europe in the early years of the twentieth century. Frege had aimed to show that logic constitutes the foundation for mathematics in the sense of providing both the primitive concepts in terms of which mathematical concepts were to be defined and the primitive truths on the basis of which mathematical truths were to be proved.[1] Russell's paradox showed that the project could not be completed, at least as envisaged by Frege. It nevertheless seemed clear to many that mathematics must be founded on *something*, and over the first few decades of the twentieth century four proposals emerged: two species of logicism—namely, ramified type theory as developed in Russell and Whitehead's *Principia* and Zermelo–Frankel set theory—and Hilbert's finitist program (a species of formalism), and, finally, Brouwerian

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[1] As is well known, Frege himself took this logicist thesis to apply only to arithmetic, not also to geometry; more recent conceptions of geometry suggest that the logicist thesis, if it is applicable to mathematics at all, is applicable to all of mathematics.

FN:2 intuitionism.[2] Across the Atlantic, already by the time Russell had discovered his famous paradox, the great American pragmatist Charles Sanders Peirce was developing a radically new *non-foundationalist* picture of mathematics, one that, through the later influence of Quine, Putnam, and Benacerraf, would profoundly shape the course of the philosophy of mathematics in the United States.

Three trends in the philosophy of mathematics as it is currently practiced in the United States will concern us. The first is the acknowledged mainstream, comprising the New World's non-foundationalist correlates to the Old World's 'big three' (logicism, formalism, and intuitionism): namely, FN:3 structuralism, nominalism, and post-Quinean naturalism.[3] One of our central tasks will be to show that the difficulties that have given rise to these three schools can ultimately be traced to issues generated by the rise of a fundamentally new mathematical practice in the nineteenth century, the practice of deriving new results by reasoning from concepts alone: it was manifest that this new practice of reasoning from concepts yielded new, important, and contentful mathematical knowledge, and yet, for reasons that will become clear, it seemed impossible to understand how it *could*.

The second trend began to take off in the Seventies and is self-styled as FN:4 a 'maverick' tradition.[4] It is one that is for the most part more radically naturalistic than even post-Quinean naturalism; and it has provided useful FN:5 correctives to some of the excesses of the mainstream.[5] It has also become increasingly mainstreamed. With almost everyone in disagreement with almost everyone else, and an apparently endless stream of publications to explain why,

[2] The reasons for thinking that mathematics is founded on something may have varied considerably. Both Russell and Hilbert are on record as self-consciously searching for an indubitable ground for our knowledge of mathematics. Brouwer presumably had a more Kantian conception of foundations in terms of constructions, and Zermelo may have been after nothing more than a consistent set of axioms from which to derive all of arithmetic. Good introductions to these various positions are given in George and Velleman (2002) and Giaquinto (2002). Jacquette (2002) includes a selection of papers on intuitionism, and Shapiro (2005) contains recent appraisals of all three schools. Important original works are collected in Benacerraf and Putnam (1983), as well as in Ewald (1996) and van Heijenoort (1967).

[3] This is not to say that there is not important work being done by Americans on the more traditional big three. Certainly there is: e.g. Feferman (1998). But such work tends to be more mathematical: it aims to show how far one can get by pursuing a particular train of thought rather than what, precisely, mathematics is. So, e.g., Burgess (2005) takes up the neo-logicism of Crispin Wright not to assess whether or not it is right but to assess how much mathematics it can support.

[4] The term was first used by Aspray and Kitcher in their 'opinionated' introduction to Aspray and Kitcher (1988).

[5] It has also had some excesses of its own, among them Lakoff and Núñez (2000).

the distinction between mainstream and maverick has become a distinction without a difference.

The final trend we will consider is the burgeoning tradition of Frege studies in the United States, or, more exactly, some strands in that burgeoning tradition. Recent reappraisals of Frege's work are uncovering fundamental Peircean pragmatist themes at the heart of Frege's thought; and these themes, together with some Kantian ideas from Peirce, will help us to begin to understand how a radically new sort of resolution of the difficulties that have arisen in the wake of contemporary mathematical practice might be achieved, one that would not merely add yet another voice to the cacophony, but instead diagnose the root cause of the debates that are currently taking place throughout the philosophy of mathematics, both in the United States and around the world. We begin with Peirce.

At the heart of Peirce's pragmatism is a conception of sentential meaning focused not on what is the case if the sentence is true—that is, on truth conditions—but instead on what follows if the sentence is true—that is, on a sentence's inferential consequences. The basic idea can be found already in Kant's *Critique* (1781/7), in the idea that 'if the grounds from which a certain cognition should be derived are too manifold or lie too deeply hidden, then one tries whether they may not be reached through their consequences . . . one uses this kind of inference, though to be sure with some degree of care, if it is merely a matter of proving something as an hypothesis' (A790/B818). The difference between Kant and Peirce is that, according to Peirce, but not Kant, this kind of inference is the only way to establish truth in the sciences. Because such a method of proving something—by testing its consequences rather than by deriving it from its proper grounds—obviously cannot yield certainty, but only 'experimental' grounds for judgment (because it is impossible to be confident that one has exhausted the consequences of any given claim), it follows directly from this pragmatist conception of meaning in terms of consequences that there is no certain or indubitable truth. Anything we think we know, however self-evident it may seem, can turn out to have been mistaken. Nothing is or can be (as Sellars would say) Given—that is, absolutely unquestionable. On the other hand, there will always be a great deal that is not (currently) in question in our practice, a great deal that we currently do not have any reason to doubt. That is where our inquiries must start, from where we are, while at the same time recognizing that in our scientific inquiries we do not stand 'upon the bedrock of fact' but are instead 'walking upon a bog, and can only say, this ground seems to hold for the present' (*RLT* 176–7).

Peirce's pragmatism begins with a consequentialist conception of meaning, and hence a fallibilist conception of knowing. Peirce furthermore holds that this lack of any certain foundation is as true in mathematics as it is in the empirical or natural sciences. Indeed, for Peirce, mathematics is the *paradigm* of such an experimental science. As Ketner and Putnam explain in their introduction to Peirce's Cambridge lectures of 1898:

> Epistemologically at any rate, mathematics was [for Peirce] an observational, experimental, hypothesis-confirming, inductive science that worked only with pure hypotheses without regard for their application in 'real' life. Because it explored the consequences of pure hypotheses by experimenting upon representative diagrams, mathematics was the inspirational source of the pragmatic maxim, the jewel of the methodological part of semeiotic, and the distinctive feature of Peirce's thought. As he often stated, the pragmatic maxim is little more than a summarizing statement of the procedure of experimental design in a laboratory—deduce the observable consequences of the hypothesis. And for Peirce the simplest and most basic laboratory was the kind of experimenting upon diagrams one finds in mathematics. (*RLT* 2)

But although Peirce rejected the foundationalist picture according to which mathematics is founded on certain and unshakable truths, he did not thereby embrace, as James and Dewey (and later, Rorty) did, a conception of truth according to which it depends ultimately on our practices and interests. Although the trail of the human serpent is over all (just as James said), we can, Peirce thinks, nonetheless get things objectively right, know things as they really are. Like Sellars, Peirce saw the lack of foundations not as a *barrier* to a robust notion of objective truth but instead as *enabling* of it—however exactly this was to work.

By the early decades of the twentieth century, James's version of pragmatism had become well known in the United States. Nevertheless, it is the logicism of the logical positivists—that is, of the scientifically trained European philosophers making up the Vienna Circle, many of whom were to flee to the United States to escape the Nazis—that sets the stage for the philosophy of mathematics as it would unfold over the course of the twentieth century in the United States. Hempel explains, with characteristic clarity, the positivist's view of mathematics:

> Mathematics is a branch of logic. It can be derived from logic in the following sense:
>
> a. all the concepts of mathematics, i.e. of arithmetic, algebra, and analysis, can be defined in terms of four concepts of pure logic.
> b. All the theorems of mathematics can be deduced from those definitions by means of the principles of logic (including the axioms of infinity and choice).

> In this sense it can be said that the propositions of the system of mathematics as here delimited are true by virtue of the definitions of the mathematical concepts involved, or that they make explicit certain characteristics with which we have endowed our mathematical concepts by definition. The propositions of mathematics have, therefore, the same unquestionable certainty which is typical of such propositions as 'All bachelors are unmarried,' but they also share the complete lack of empirical content which is associated with that certainty: The propositions of mathematics are devoid of all factual content; they convey no information whatever on any empirical subject matter. (Hempel 1945: 389–90)

Notice the implicit suggestion. The propositions of mathematics are devoid of factual content—but not of content *über haupt*. They convey no information on any empirical matter—but are nevertheless informative regarding some other, presumably appropriately mathematical, subject matter. They enjoy 'unquestionable certainty', but are nonetheless contentful pieces of knowledge; they are ampliative, extensions of our knowledge, but also a priori and analytic. It was a fine view. But it could not long stand up to the pragmatism that greeted it on its arrival in the New World. To understand why it could not, we need to start our story quite a bit further back, with some ideas about mathematical practice that were current before Kant appeared and changed everything.

Throughout its long history, mathematical practice has involved the use of written marks according to rules. To count, for example, is at its most basic to inscribe marks—say, notches in a bone, one for each thing counted. Similarly, in a demonstration in Euclid, one begins by drawing a diagram according to certain specifications. To learn basic arithmetic or elementary algebra is to learn to manipulate signs according to rules. And so on. But although all of mathematics uses signs, inscribed marks according to rules, not all of mathematics uses *symbols*. Whereas a collection of four strokes can be taken to be not merely a symbol but instead an actual instance of a number (conceived as a collection of units), the corresponding arabic numeral '4' can seem instead to be *only* a symbol, a sign that stands in for or is representative of the number, and is enormously useful in calculations, but nonetheless is not an instance of the thing itself. Even more obviously, a drawn circle can be seen as an instance, however imperfect, of a geometrical circle, as an instance of that which is only represented by the equation $x^2 + y^2 = r^2$ of elementary algebra.

There is a significant difference between our two pairs of examples, however. Whereas the arabic numeration system can be (and for a long time was) treated merely as a convenient shorthand by means of which to solve arithmetical problems, the rules of elementary algebra permit transformations that are, from the ancient and medieval perspective, sheer nonsense. If numbers are collections of units, and if the signs of arabic numeration are merely representatives of numbers otherwise given, then it will seem manifest that many apparent problems in arithmetic can have no solution, and so are not really arithmetical problems at all: although $3 + x = 5$ is a perfectly good arithmetical problem, $5 + x = 3$ is not, because there is nothing that one might add to five (things) to yield three (things). From the perspective of the rules of elementary algebra, in particular, for this case, the rule that if $a + x = b$, then $x = b - a$, the two cases are exactly the same: the solution in the first case is $x = 5 - 3$, and in the second, $x = 3 - 5$. The language of algebra introduces in this way something essentially new; whereas basic arithmetic is, originally, bounded by our intuitions regarding what makes sense, the rules of algebra seem to enable us somehow to transcend those bounds, to find a new kind of meaning in the rule-governed manipulation of signs.[6] Because it does, the introduction of the language of algebra inevitably generates a tension between two quite different conceptions of rigor and proof.

Algebra uses a system of signs governed by algorithms to solve problems; Euclidean geometry instead uses diagrams that involve (so it seems) instances of geometrical figures to demonstrate the truth of theorems and the solutions of problems. Arabic numeration, like algebra, uses a system of signs governed by algorithms to solve problems; pebble arithmetic involves not signs for, but instead actual instances of, numbers, conceived as collections of units. Standard arithmetic and algebra, on the one hand, and pebble arithmetic and geometry, on the other, are thus two very different sorts of systems; and they give rise in turn to two quite different conceptions of rigor and of proof. A Euclidean demonstration would seem to be a course of pictorial thinking, one that reveals connections to an attentive audience. It does not *require* assent, but to one attentive to what is being claimed and to what is depicted in the diagram, the chain of pictorial thinking shows the truth of the conclusion. The rigor of the demonstration thus crucially depends on intuition and meaning. From the perspective of this case, the often merely mechanical manipulation of signs according to rules in algebra can seem anything but

[6] In Macbeth (2004) I explore in detail just what it is that is new with the introduction of the symbolic language of algebra, and also argue that Descartes, not Viète, ought to be seen as the first truly modern mathematician.

rigorous, leading as it apparently does to the 'obscurity and paradox' of, for instance, numbers that are less than nothing, or worse the roots of numbers that are less than nothing.[7] But in a different sense it can seem that *only* a calculation in arithmetic or algebra is properly rigorous, because only such a calculation *requires* assent by anyone who knows the rules governing the use of the signs involved. From this perspective, rigor crucially depends on ignoring intuition and meaning because they are, or at least can be, a source of error and prejudice. Kant's contemporary Lambert championed just this conception of rigor and proof, arguing that a proof should 'never appeal to the thing itself . . . but be conducted entirely symbolically', that it should treat its premises 'like so many algebraic equations that one has ready before him and from which one extracts x, y, z, etc. without looking back to the object itself'.[8]

A Euclidean demonstration enables one to see why a conclusion holds by literally showing the connections that are the ground of its truth, but apparently does so at the expense of rigor in our second sense. An argument that proceeds by means of the often merely mechanical manipulation of symbols according to strict algorithms, by contrast, definitively establishes a truth, but seems to do so at the expense of understanding. It does not show, with the intuitive clarity of a Euclidean demonstration, why the conclusion should be as it is, and sometimes yields results that run directly counter to intuition. How extraordinary, then, that Kant should claim in the first *Critique* that *all* of mathematics functions in the same way: namely, through constructions in pure intuition, constructions that enable one literally to see, and so to understand, the necessity of the conclusion drawn. How *could* this be, given that in geometry one focuses on the things themselves, whereas in arithmetic and algebra one focuses not on the things but instead on arbitrary (rule-governed) signs?

Before Kant, we have seen, there were two incompatible ways to think about mathematical practice. The first, and more intuitive, more classical way was bottom-up: beginning with concrete sensible objects such as drawn geometrical figures or 'actual' numbers—that is, collections of things—the mathematician learns to abstract from these concrete cases and to reason about mathematical entities that are only imperfectly depicted in a Euclidean

7 The description is John Playfair's in 'On the Arithmetic of Impossible Equations' (1778); quoted in Nagel (1935: 173).

8 'Theorie der Parallellinien' (1786); quoted in Detlefson (2005: 250).

demonstration or by a collection of strokes on a page. From this perspective, the manipulation of symbols according to algorithms in arithmetic and algebra was to be understood instrumentally; such systems of signs are useful, but not meaningful in their own right. The second way was top-down, beginning with the symbolic language of algebra and, by extension, that of arithmetic. On this view the meanings of the signs used in calculations are exhausted by the rules governing their use; they have no meaning or content beyond that constituted by the rules governing their use, and because they do not, our inability to imagine the results of certain computations—for example, that of taking a larger from a smaller number—is irrelevant to meaning. Euclidean geometry, dependent as it is on drawn figures rather than on rule-governed signs, is to be replaced by 'analytic geometry': that is to say, algebra, because only as algebra is geometry properly rigorous.

Kant overcomes this either/or. According to him, all of mathematics—whether a Euclidean demonstration, a calculation in arithmetic, or a computation in algebra—functions in the same way, through the construction of concepts in pure intuition. The truths of mathematics are known, in every case, by reason 'guided throughout by intuition'—that is, through the construction of concepts in pure intuition. Because the intuition is pure, grounded in the forms of sensibility, space, and time, it follows immediately that mathematics is applicable in the natural sciences; although not itself empirical knowledge, mathematical knowledge, on Kant's view, is nonetheless knowledge about empirical objects. But if that is right, then both earlier accounts must have been wrong. If Kant is right, Euclidean geometry cannot function by picturing, however imperfectly, geometrical objects, as the first, bottom-up account would have it (as if arithmetic were some kind of empirical science), and the symbolic languages of arithmetic and algebra similarly cannot be *merely* symbolic, their meaning exhausted by the rules governing the use of their signs, as the second, top-down account would have it (as if mathematics were merely logic). Instead, if Kant is right, both sorts of systems of signs function to *encode information* (contained in the relevant concepts) in a way that enables rigorous reasoning *in* the system of signs, reasoning that is revelatory of new and substantive mathematical truths. We will later see in more detail how this is to work.

Kant provided an illuminating and intellectually satisfying account of the practice of early modern mathematics, one that revealed it to be rigorous *both* in the sense of compelling one's assent *and* in the sense of bringing one to see just why the results hold. It was an extraordinary philosophical achievement. Unfortunately, even as Kant was working out his philosophy of mathematics, mathematicians were coming more and more to eschew the sorts of constructive problem-solving techniques that Kant focuses on, in favor of a more conceptual approach. A new mathematical practice was emerging, and it did so, over the course of the nineteenth century, in roughly three, not strictly chronological, stages. First, mathematicians began to introduce new mathematical objects that could not, by any stretch of the imagination, be constructed in an intuition—for example, the projective geometer's points at infinity where parallel lines meet. Second, the focus was shifting more generally from algebraic representations of objects to descriptions of them using concepts. In the case of limit operations, for example, instead of trying to compute—that is, to construct—the limit as Leibniz had done, Cauchy, Bolzano, and Weierstrass aimed instead to describe what must be true of it. Riemann similarly did not require, as Euler had, that a function be given algebraic expression; it was enough to describe its behavior. And finally, a new sort of algebra was emerging, one that concerned not any particular mathematical objects, functions, or relations, but instead the kinds of structures they can be seen to exhibit—that is, groups, rings, fields, and so on. In each case, the focus was on reasoning from concepts; the construction of concepts in intuition was not needed, and intuition itself was coming to be seen as a 'foreign' element to be expelled from mathematics.[9]

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Developments in the practice of mathematics over the course of the nineteenth century suggested that merely by reasoning from concepts one could extend one's knowledge. But if so, then either Kant was wrong to have thought that analytic judgments are only explicative, that all ampliative judgments are instead synthetic, or he was wrong to have thought that by reason alone only analytic judgments are possible—that is, that synthetic judgments inevitably involve intuitions as contrasted with concepts, whether pure or empirical. For Kant himself, of course, the analytic/synthetic distinction exactly lines up both with the explicative/ampliative distinction and with the by-logic-alone/involving-intuition distinction: analytic judgments are known

[9] Very helpful and illuminating discussions of these transformations can be found in Stein (1988), Nagel (1939), Wilson (1992), Ferreiros and Gray (2006), Gray (1992), Avigad (2006), and Tappenden (2006). One caveat: Stein describes this transformation of mathematics in the nineteenth century as 'so profound that it is not too much to call it a second birth of the subject—its first birth having occurred among the ancient Greeks' (1988: 238). This is misleading insofar as it ignores Descartes's transformation of mathematics in the seventeenth century, the transformation without which modern science would have been impossible. See Macbeth (2004).

by reason alone and are merely explicative, and synthetic judgments are ampliative, because an intuition serves in such judgments to connect the predicate to the concept of the subject. What the new mathematical practice of reasoning from concepts showed was that this picture of the analytic/synthetic distinction had to be wrong. What it did not settle was how it was wrong. The logical positivists, focusing on the knowledge produced, drew our first conclusion; they took it that what had been shown was that some analytic judgments, known by logic and reasoning alone, are in fact ampliative, not merely explicative. Peirce, focusing instead on the activity of mathematical inquiry, drew the second conclusion; he thought that the lesson of modern mathematical practice was that even reasoning from concepts by logic alone can involve constructions and so be synthetic, hence ampliative. The two responses will be considered in turn.

According to the positivists, developments in mathematics in the nineteenth century showed that there can be judgments—that is, judged (or judgeable) contents—that are true in virtue of the meanings stipulated in one's axioms, and so analytic a priori, but which are far from trivial, which can be proved only from a whole collection of axioms in what is perhaps a quite complex series of steps.[10] Although concepts do often contain contents that can be made explicit in trivial analytic judgments (the concept *human*, for example, contains a content that is made explicit in the analytic judgment that all humans are rational), it was coming to seem that concepts can also acquire meaning through their relations one to another, relations that can be made explicit in an axiomatization. Theorems that can be derived from a set of such axioms, solely in virtue of the stipulated relations, thus follow 'by meaning alone', and so are analytic in Kant's sense despite being ampliative—that is, significant extensions of our knowledge. Coffa explains the point this way:

> If Kant was right, concepts without intuitions are empty, and no geometric derivation is possible that does not appeal to intuition, But by the end of the nineteenth century, Bolzano, Helmholtz, Frege, Dedekind, and many others had helped determine that Kant was not right, that concepts without intuitions are not empty at all. The formalist

[10] One will perhaps think of Frege's familiar remark in 1980 [1884]: §88: 'the conclusions . . . extend our knowledge, and ought therefore, on Kant's view, to be regarded as synthetic; and yet they can be proved by purely logical means, and are thus analytic. The truth is that they are contained in the definitions, but as plants are contained in their seeds, not as beams are contained in a house'. In fact, we will see, Frege's own views were much closer to Peirce's than they were to those of the later logical positivists.

project in geometry was therefore designed not to expel meaning from science but to realize Bolzano's old dream: the formulation of non-empirical scientific knowledge on a purely conceptual basis. (Coffa 1991: 140)

For the positivists, mathematical knowledge can be ampliative despite being analytic, because meaning can be determined for a set of expressions, assumed to be otherwise meaningless, through an axiomatization involving those expressions.

Peirce's response to these same developments in nineteenth-century mathematics was very different. His thought was not that what those developments show is that constructions are not needed in mathematics, but instead that even logic, even reasoning from concepts alone, can involve constructions. He explains in 'The Logic of Mathematics in Relation to Education' (1898):

Kant is entirely right in saying that, in drawing those consequences, the mathematician uses what, in geometry, is called a 'construction', or in general a diagram, or visual array of characters or lines. Such a construction is formed according to a precept furnished by the hypothesis. Being formed, the construction is submitted to the scrutiny of observation, and new relations are discovered among its parts, not stated in the precept by which it was formed, and are found, by a little mental experimentation, to be such that they will always be present in such a construction. Thus the necessary reasoning of mathematics is performed by means of observation and experiment, and its necessary character is due simply to the circumstance that the subject of this observation and experiment is a diagram of our own creation, the condition of whose being we know all about.

But Kant, owing to the slight development which formal logic had received in his time, and especially owing to his total ignorance of the logic of relatives, which throws a brilliant light upon the whole of logic, fell into error in supposing that mathematical and philosophical necessary reasoning are distinguished by the circumstance that the former uses constructions. This is not true. All necessary reasoning whatsoever proceeds by constructions; and the difference between mathematical and philosophical necessary deductions is that the latter are so excessively simple that the construction attracts no attention and is overlooked. (*CP* iii. 350)

On Peirce's view, the lesson of the developments in mathematical practice in the nineteenth century is not that mathematics, which (as developments in the nineteenth century had shown) can involve reasoning from concepts alone, is for that reason analytic, rather than synthetic as Kant thought, but instead that reasoning from concepts alone is, like the rest of mathematical practice, synthetic—that is, ampliative—albeit a priori and necessary, because even reasoning from concepts involves constructions. We will come back to this.

For now, we need to return to the positivist view, and to Quine's utterly devastating critique of it.[11]

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We have seen that, according to the positivists, the truths of mathematics are analytic despite being ampliative; they are contentful, non-trivial truths that, like any analytic truths, are known by appeal to meanings alone. Quine's essential, and fundamentally pragmatist, objection to the view is that if the judgments of mathematics really are analytic, founded on meaning alone, hence incorrigible and unrevisable, then they are not and cannot be *true*; alternatively, if they are true (or false), then they are not founded on meaning alone, because in that case they can be revised as needed. Quite simply, if it really is impossible to get it wrong (save by merely making a mistake in one's formal reasoning, in one's manipulation of signs according to rules), then there is no content to the claim at all. And this is true furthermore even of simple analytic judgments such as that all humans are rational. If that judgment is true by virtue of meaning, because being human is by definition being (say) a rational animal, then the judgment that all humans are rational is not a piece of knowledge of any kind. It is nothing more than a substitution instance of the logical schema 'all (A&B) is A', and this substitution instance absolutely must not be confused with a truth, however trivial, about humans; for if it were really a *truth* about humans, then it would be possible that it be mistaken. Insofar as it is not possible that one is mistaken (except in the trivial sense of having misapplied some rule governing one's use of signs), it is also not possible that what one has is an item of knowledge. What is unquestionable in principle is not knowledge but merely blind prejudice, merely (as Peirce would say) what one is inclined to think.[12]

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This point can be hard to see, and it can be hard to see at least in part because there is a perfectly good distinction to be drawn between claims that are *believed* on the basis of meanings and claims that are believed on the basis of fact. I might, for example, believe that humans are rational on the grounds that in the language that I speak part of what it means to be human is to be rational. Quine's point is that either sort of belief, either belief on the basis of meanings or belief on the basis of fact, can turn out to have been mistaken and require revision. Even Kant himself took the contents of at least our empirical concepts to be subject to empirical inquiry, and hence subject to

[11] Quine's criticisms of the logical positivists' conception of mathematics first appeared in print in Quine (1937), but the most famous and influential formulation is given in Quine (1951), repr. in *From a Logical Point of View* (1953).

[12] See Lecture Four, 'The First Rule of Logic', of *RLT*, and also 'The Fixation of Belief' in *EP* i.

revision. In particular, if *human* (say) is an empirical concept, then there are, on Kant's view, no analytic truths about humans, but only synthetic ones that are discovered as we come to know more and more about the sorts of beings we are. Only an a priori, or non-empirical, concept could possibly ground an analytic truth on Kant's account. Given that the positivist rejects the notion of an a priori concept, there is simply no place left for the positivist to stand between infallible but empty logical forms, on the one hand, and corrigible, empirical contents, on the other. The judgments of mathematics, if they have any content at all, are contentful in virtue of facing the tribunal of experience in just the way any other claim does. The only tribunal is experience, and the only science is empirical—that is, a posteriori, science.

The situation, then, seems to be this. Mathematics starts with some definitions, either explicit or implicit (that is, given by a collection of axioms), and derives theorems using familiar rules of logic. The process can be understood merely mechanically—that is, as a process of formal, syntactic derivation. Such a process furthermore might seem to explain how we know that the theorems are true, indeed necessarily so given the axioms. Unfortunately, as Benacerraf (1973) argues, it is in that case utterly mysterious why we should say that those derived theorems are *true*; for to understand truth, in mathematics as in the rest of the language, we need to provide an interpretation, a semantics for the language, and as Tarski has taught us, this means providing a domain of objects for our names to refer to and for our quantifiers to range over. And here there would seem to be only two options: either those objects are everyday empirical objects, or they are abstract objects. Now, mathematics does not seem to be about empirical objects, because that would be incompatible with the characteristic necessity of its results; its timeless necessity suggests instead that its objects are likewise timeless and unchanging—that is to say, Platonic. But, mathematics aside, Platonism is incompatible with everything we have come to understand about the world: although Plato in his time may have had good reason to posit such objects, we in ours do not. We cannot in good conscience be Platonists about mathematical objects. But if that is so, then the content of mathematics, if it has any content at all, is empirical—just as Quine argues.

Mathematics seems to involve more than mere empty formalisms, the mere manipulation of signs; it seems, that is, to have content, and indeed content that is not empirical. On the other hand, it appears to be impossible to fit timeless, abstract objects into any robust and intellectually respectable picture of empirical reality. So how are we to account for the fact that mathematics has content? Quine's solution to the difficulty is to start with naturalism, and so with the rejection of abstract objects (hence with a kind of formalism in mathematics), but then to suggest that as we proceed with our natural, empirical scientific investigations, we discover that we cannot do without the sorts of things mathematicians talk about—for instance, sets. So, much as we posit unobservables such as electrons to make sense of our experience of the world, we can similarly posit sets, thereby providing distinctively mathematical content without falling into Platonism.[13]

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In 'Mathematics without Foundations', Putnam (1967) offers a different way out of the difficulty. He suggests that there are available in mathematics two very different perspectives: that of modal logic and that of set theory. From the modal logic perspective, the propositions of mathematics appear to be logically necessary, albeit empty, and from the set theory perspective, they appear contentful, because about sets, though now it is hard to see why they are necessary. It is a bit, Putnam suggests, like the physicist thinking now in terms of particles and now in terms of waves; you can have it one way for some purposes and the other for other purposes, but never both together. The problem for such a suggestion, as well as for the Quinean attempt to have it both ways rehearsed above, is, as Benacerraf (1973) makes clear, that one does not really have any understanding either of necessity or of content in mathematics without at the same time having an understanding of both. And that, as work both within and outside the mainstream had made amply clear, is just what we do not know how to achieve.

We have seen that Quine aims to treat the ontological commitments of mathematics as continuous with those of science. His starting point is a naturalism of natural, empirical science, which requires the rejection of mathematical Platonism; but he thinks that he can then reintroduce a kind of relaxed Platonism on the basis of the needs of natural science. But, as Parsons (1980) argues, this seems to get the practice of mathematics wrong, and so to sin against naturalism for the case of the science of mathematics. It is for just this reason that post-Quinean naturalists such as Maddy and Burgess eschew legislating for mathematics from the outside (as Quine does) to pursue instead a naturalized mathematics. Unfortunately, they seem as a result to have no means of counteracting the unbridled Platonism of the practicing mathematician.[14]

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[13] This is Quine's so-called indispensability argument, which appeared first in Quine (1948). Putnam also rehearses this argument (with an explicit nod to Quine) in Putnam (1971).

[14] For an introduction to the views of post-Quinean naturalists such as Maddy and Burgess, as well as further references, see Maddy (2005).

Nominalism is, then, a natural alternative, one that is pursued, for instance, by Field—though, as the discussion in Burgess (2004) shows, it is a natural alternative only if one is a post-Quinean rather than a Quinean naturalist.[15] Unfortunately, nominalism is just another way of being a formalist. What we wanted was to be able to dismount the Platonism/formalism seesaw, not to settle, however temporarily, on one end of it.

The post-Quinean naturalist seems to settle on the Platonist horn of our dilemma, and the nominalist on the formalist horn. What then of structuralism, the last of the big three in the mainstream of the American logic and foundations tradition? Structuralism seems originally to have been motivated by Benacerraf's well-known argument (in Benacerraf 1965) to show that numbers cannot be identified with sets—despite the fact that structuralism provides an obvious, albeit internal, answer to the question of what at least some contemporary mathematics is about.[16] And structuralism can seem to be the one position in the philosophy of mathematics that can go between the horns of Platonism and formalism. After all, one might think, surely there are structures, and not in any objectionable Platonist sense. In fact, just the same debates arise again, only now they are between formalists and Platonists within structuralism; and they are unresolvable for precisely the same reasons as before.[17] In the nature of the case, neither the formalist (focused on syntax alone) nor the Platonist (positing abstract structures for mathematics to be about) can provide what is wanted, a compelling and intellectually respectable account of the capacity of the mathematician to achieve interesting, new, and true results by reasoning alone.

We have seen that the mainstream has its roots in Quine's rejection of the positivist idea that mathematical judgments are truths that can be known to be necessary, a priori, and infallible because they are known by meanings alone. The maverick tradition, similarly, takes its starting point from the fact

[15] Chihara (2005) provides a lively introduction to nominalism, as well as further references. Field (1980) is perhaps the most famous nominalist essay.

[16] Bourbaki, e.g., explicitly suggests that mathematics concerns various sorts of structures. (See Corry (1992) for discussion of Bourbaki's notion structure). And many mathematicians are quite happy to describe mathematics as a science of structures, or of patterns. Nevertheless, as Parsons (1990) points out, the characterization nonetheless cannot serve, as the structuralist claims it does, for the whole of mathematics.

[17] Resnick (1997), Shapiro (1997), and Hellman (1989), e.g., are all structuralists, though they differ markedly in what they take the ontological commitments of structuralism to be. See Hellman (2001, 2005).

FN:18 that mathematics is not at all infallible in its judgments.[18] Unfortunately, this tradition sometimes understands the fallibility of our mathematical knowledge, not in terms of the deep reason that, as Peirce and Quine saw, the possibility of error is built into the very fabric of inquiry, but instead in terms of the trivial reason that we can make mistakes in our calculations, mistakes that though they may go undetected, even for years, are nonetheless invariably detectable. The following passage, which otherwise makes a very good point, falls into just this error. Azzouni is responding to mavericks who suggest that mathematical practice is, like any other practice, essentially social; Azzouni's point is that mathematical practice is quite unlike any other social practice insofar as its standards are, as he puts it, robust.

> Mistakes *can* persevere; but mostly they're eliminated, even if *repeatedly* made. More important, mathematical practice is *so* robust that even if a mistake eludes detection for years, and even if many later results presuppose that mistake, this won't provide enough social inertia—once the error *is* unearthed—to prevent changing the practice back to what it was originally: In mathematics, even after lots of time, the subsequent mathematics built on the 'falsehood' is repudiated. (Azzouni 2006: 128)

Azzouni assumes that errors in mathematics can, with sufficient care and attention, be avoided by the practicing mathematician, that mathematicians are fallible but not essentially so. But think again of Peirce's picture according to which an error can lie hidden because the consequences that would at last reveal it have not been drawn—indeed, perhaps cannot (yet) be drawn because the additional premises that would be required have not yet been so much as formulated. Is it at all credible to think, for example, that Aristotle could have corrected his conception of number, could have come to have realized that zero is a perfectly good number, merely by more careful reflection? No. What Aristotle needed was not further and more careful attention to his mathematics, but instead a different conception of number, one that would become possible only in the light of the development of the symbolic language of algebra nearly 2,000 years after Aristotle lived. More generally put, the problem, in the interesting cases, is not that someone has made some error in reasoning, an error that could have been recognized and corrected had only more care been taken; it is that one's conceptions of things can be flawed in ways that can require hundreds, even thousands, of years of ongoing intellectual inquiry to reveal.

[18] An important early manifesto is Lakatos (1978). See also Putnam (1975), Kitcher (1983), and essays in Grosholz and Breger (2000).

What the Azzouni passage is right about, of course, is that there is only one right answer in mathematics: that truth, at least in mathematics, is not at all relative—to anything. It is the same for all cultures, all times, and all people, even (although Azzouni himself would not go this far) all rational beings. (Suppose that mathematical truth is not the same for all rational beings, but only for us. To explain that fact would require positing some form of cognitive 'hardwiring' in us that could, for all we know, be different in other rational beings. But that is just the Myth of the Given again: if we were hardwired to do mathematics as we do, then at a certain point there would no longer be any possibility of revision, and hence no longer any notion of truth. The fact of the matter is that we can make mistakes, and that we can correct our mistakes: truth is the same for all rational beings, just as Frege said. Anything less would not be truth.) Azzouni is enough of a maverick to avoid the word 'objective', but that is just what he seems to be pointing to, the fact that there are objective standards in mathematics, standards that all mathematicians are answerable to. Mathematics is clearly not just another social practice. What one would like, then, is an account of mathematical practice that explains that objectivity while avoiding Platonism, that explains what Wilson (1992: 111) has called mathematics' 'hidden essentialism'.

A further development within the maverick tradition is the emphasis it has put on the actual practice of mathematics, both at a given time and as it has evolved over its long history, and hence on informal as contrasted with formal proof, and on explanation and understanding as contrasted with proof and the establishment of truths.[19] Work in the mainstream as well has begun to look more carefully at what mathematicians actually do, and also at the history of mathematics.[20] Indeed, analytic philosophy more generally, which (historically) has been notoriously a- and sometimes even anti-historical, is coming more and more to realize that understanding must be historically

[19] See, e.g., Rota (1997: chs. 9–11). Coming from a quite different perspective, Manders (1987) calls for an account not just of the reliability of mathematical practice but of the fact that it yields understanding. Also relevant are Tappenden (1995*a* and *b*).

[20] There are two quite different ways to do this, however. The first, more superficial way is merely to "check in" with mathematicians both past and present to see whether what they are doing is consistent with what one is saying about what they do. The second, deeper way is to recognize that the practice of mathematics, and thereby its historical unfolding, is the key to understanding mathematics itself; that it is the practice, not the product, that needs to be the focus of philosophical inquiry. (It is worth recalling in this context our discussion above of the differences between Peirce's and the positivists' response to the idea of reasoning from concepts alone that is nonetheless ampliative.)

informed. We turn finally to some fruits of that realization, the third strand in the tradition—which is to say, recent work on Frege insofar as it bears on the issues of concern here.

Benacerraf once again provides a critical point of orientation. As he argues in 'Frege: The Last Logicist' (1981), the tradition has profoundly misunderstood Frege's logicism; it has failed to distinguish Frege's logicism from that of the later logical positivist tradition. Also significant is Burge's recent collection of papers (2005), especially his 'Frege on Knowing the Foundation', which emphasizes some of the deeply pragmatist themes in Frege's thought—in particular, his fallibilism and his essentially historicist understanding of intellectual inquiry. What I want to focus on, however, is the novel reading of Frege's strange two-dimensional notation that is developed in Macbeth (2005), because it is on the basis of this reading that we will be able to see most clearly both the connections to Peirce and the outlines of a way out of the impasse generated by the idea that through reasoning from concepts alone mathematicians can advance our knowledge and understanding. Frege's two-dimensional logical language, it will be suggested, is just what is needed to understand Peirce's idea that reasoning is a matter of construction and so in its way synthetic a priori despite by concepts alone. And that in its turn will help us to understand the nature of mathematical reasoning in a way that avoids both formalism and Platonism. First, however, we need to understand better the role of constructions in Kant's account of the practice of mathematics.

Mathematics seems essentially to involve written marks. As one recent author has put it, 'one doesn't speak mathematics but writes it. Equally important, one doesn't write it as one writes or notates speech; rather, one "writes" in some other, more originating and constitutive sense' (Rotman 2000: p. ix). Already in 1764, in his 'Inquiry Concerning the Distinctness of the Principles of Natural Theology and Morality', Kant offers the beginnings of a theory about how such marks function in the practice of mathematics. He suggests, first, that the signs and marks employed in mathematics 'show in their composition the constituent concepts of which the whole idea . . . consists'. The arabic numeral '278', for example, shows (on this reading) that the number designated consists of two hundreds, seven tens, and eight units. A drawn triangle similarly is manifestly a three-sided closed plane figure; like the numeral '278', it is a whole of simple parts. These complexes are then further combined, according to Kant, to show 'in their combinations the relations of

FN:21 the . . . thoughts to each other'.[21] In mathematics, one combines the wholes that are formed out of simples into larger wholes that exhibit relations among them. The systems of signs thus have three levels of articulation: first, the primitive signs; then the wholes formed out of those primitives, wholes that constitute the subject matter of the relevant part of mathematics (the numbers of arithmetic, say, or the figures of Euclidean geometry); and finally, the largest wholes—for example, a Euclidean diagram or a calculation in arabic numeration, that are wholes of the (intermediate) wholes of the primitive parts. In the *Critique*, by which time Kant had discovered the logical and metaphysical distinction between intuitions and concepts, Kant further indicates that it is precisely because it is possible to reconceptualize at the second level, to see a collection of marks now this way and now that—possible, that is, to synthesize the given manifold of marks under different concepts—that one can come in the course of one's reasoning to see that the predicate of the judgment in question belongs necessarily to the concept of the subject despite not being contained in it.

Consider a demonstration in Euclid. Such a demonstration consists of a diagram constructed out of the primitives of the system (points, lines, angles, and areas) together with a commentary that, among other things, instructs one how to conceive various aspects of the diagram—a given line, for instance, now as a radius of a circle and now as a side of a triangle. What Kant saw is that such reconceptualizations are critical to the cogency of the demonstrations. The very first proposition in Euclid's *Elements* illustrates the essential point. The problem is to construct an equilateral triangle on a given finite straight line. To demonstrate the solution, one first constructs a circle with one endpoint of the given line as center and the line itself as radius, and then another circle with the other endpoint as center and the line as radius. Then, from one of the two points of intersection of the two circles, one draws two lines, one to each of the endpoints of the original line. Now one reasons on the basis of the drawn diagram: two of the three lines are radii of one circle and so must be equal in length, and one of those radii along with the third line are radii of the other circle, so must be equal in length. But if the two lines in each of the two pairs are equal in length, and there is one line that is in both pairs, then all three lines must be equal in

[21] All quotations are from Kant (1764: 251). In this passage Kant is in fact describing what the words of natural language that are used in philosophy cannot do. It is clear that he means indirectly to say what the signs and marks in mathematics can do.

length. Those very same lines, however, can also be conceived as the sides of a triangle. Because they can, we know that the triangle so constructed is equilateral. QED.

As this little example illustrates, a demonstration in Euclidean geometry works because the diagram has three levels of articulation that enable one to reconfigure at the second level, to see the primitive signs now as parts of this figure and now as parts of that. The diagram does not merely picture objects; it encodes information such as that two line lengths are equal (because they are radii of one and the same circle). And because it encodes information in a system of signs that involves not only complexes of primitives but (inscribed) relations among those complexes, it is possible to 'gestalt' the parts of a Euclidean diagram in a variety of ways, and thereby to identify new complexes in the diagram and new relations. What one takes out of the diagram, then, was already in it only, as Frege would say, as a plant is in the seed, not as beams are in a house. The demonstration is ampliative, an extension of knowledge, for just this reason.

One can similarly demonstrate a simple fact of arithmetic through the successive reconceptualizing of the units of a number. One begins, for instance, with a collection of seven strokes and a collection of five strokes. Again, there are three levels of articulation: the primitives (the individual strokes), the two collections of those primitives, and the whole array. Because the two collections are given in the array, it is possible to reconceive a unit of one collection as instead a unit of the other and in this way to 'add the units . . . previously taken together in order to constitute the number 5 one after another to the number 7, and thus see the number 12 arise' (B16).

Calculations in arabic numeration are more complex, but as Kant teaches us to see, the basic principle is the same. Suppose that the problem is to determine the product of twenty-seven and forty-four. One begins by writing the signs for the two numbers in a particular array: namely, one directly beneath the other. Obviously, here again the three levels can be discerned, and here again it is this that enables one to do one's mathematical work. Suppose that one has written '44' beneath '27'. The calculation begins with a reconfiguration at the second level of articulation: the rightmost '4' in '44' is considered instead with the '7' in '27'. Multiplying the two elements in this new whole yields twenty-eight, so an '8' goes under the rightmost column and a '2' above the left. Next one takes the same sign '4' and considers it together with the '2' in '27', and so on in a familiar series of steps to yield, finally, in the last (fifth) row, the product that is wanted. The fifth row is, of course, arrived at by the stepwise addition of the numbers given in the columns at the third and fourth row; it is by reading down that one understands why just those signs appear in the bottom row. But it is by reading across, by conceptualizing the signs in the last row as a numeral, that one knows the answer that is wanted.

Exactly the same point applies in algebra; even in algebra, there is 'a characteristic construction, in which one displays by signs in intuition the concepts, especially of relations of quantities' (Kant, A734/B762). In every case, through the reconfiguration of parts of wholes (made possible, on the one hand, by the fact that those wholes are themselves parts of wholes, and on the other, by the Kantian dichotomy of intuition and concept), one comes in the course of one's reasoning to see something new arise, and thereby to extend one's knowledge. Because the relevant intuitions are pure, rather than empirical, the results are necessary, that is, a priori; and because the whole process is made possible by space and time as the forms of sensibility, the results are obviously and immediately applicable in the natural sciences.

Earlier we distinguished between two very different conceptions of rigor and proof, on the one hand, the intuitive rigor of a picture proof, and on the other, the formal rigor of a calculation or deduction achieved through the essentially mechanical manipulation of signs. Kant effectively combines these two quite different conceptions of rigor and proof in the idea that mathematical practice involves constructions, whether ostensive or symbolic, in pure intuition, and so is able to explain how the judgments of mathematics can be at once necessary, or a priori, and ampliative, substantive extensions of our knowledge. And this works in virtue of the three levels of articulation discernible in each of the systems of signs that Kant considers combined with the fact, made possible by Kant's recognition of two logically different sorts of representations, concepts and intuitions, that collections of those signs can be variously conceived. In such cases one neither merely pictures things nor merely manipulates signs; one's work is neither merely intuitive nor merely formal. Instead, one reasons *in* the system of signs, actualizing, through the course of one's reasoning, the conclusion that is latent in one's starting point.

Unfortunately, this works only because one starts with something that can be exhibited in an intuition—a number, say, or a finite line length. It is not surprising, then, that we find that once intuition had been banished from the practice of mathematics over the course of the nineteenth century, the two notions of rigor, intuitive and formal, came apart again—only this time there could be no question which of the two notions was needed in mathematics. As Einstein famously put it in 1921, 'insofar as mathematical theorems refer to reality, they are not certain, and insofar as they are certain, they do not refer to reality . . . The progress entailed by axiomatics consists in the sharp separation of the logical form and the realistic and intuitive contents.'[22] One can have deductive rigor by focusing on the signs and the rules governing their use—that is, syntax—or one can have meaning and truth by focusing on that for which the signs stand, their semantic values. What one cannot have is both at once, not now that it is concepts rather than intuitions that are at issue.

As Kant helps us to see, objects given in intuitions are thought through concepts, and so can be variously conceived in ways that combine intuitive clarity and deductive rigor. Our problem is that the same obviously cannot be said of concepts themselves. Although it is possible to take a bottom-up view of concepts by appeal to objects that exemplify them, and also possible to take a top-down view of concepts by appeal to inferential relations among them as stipulated in an axiomatization, it is impossible to do both at once. Deductive rigor which requires fixing in advance the logical relations among one's primitive concepts in axioms and definitions, thus seems to require a kind of formalism, and thereby the loss of the sort of rigor that, if Kant is right, is displayed both in a Euclidean demonstration and in calculations in arithmetic and algebra. This (exclusive) either/or, either (top-down, syntactic) deductive rigor or (bottom-up, semantic) content and meaning, emerged first in geometry, in Pasch and in Hilbert, but it eventually, and inevitably, surfaced in logic as well, and is now the standard, model-theoretic view.[23] It is also the source of the oscillations between formalism and Platonism that have bedeviled the philosophy of mathematics for the last century.

What is needed is an account of mathematical practice that combines deductive rigor with a clear understanding of why and how the conclusion follows from the premises. And for that, so it would seem, following Kant, we need a conception of the written language of mathematics that enables reasoning from concepts that is deductively rigorous but also ampliative, a language that does for concepts and reasoning what, on Kant's reading, the language of arithmetic and algebra does for numbers, functions, and computations. But how is such a conception possible? Have we not shown

[22] From his lecture 'Geometrie und Erfahrung'; quoted in Freudenthal (1962: 619).

[23] For some of the history, see Nagel (1939), Goldfarb (1979), and Demopoulos (1994).

that, with the shift over the course of the nineteenth century from (as Kant would put it) reason in its intuitive employment to reason in its discursive employment, the two conceptions of rigor came again to be utterly incompatible? Have we not seen that if mathematics as it has come to be practiced is to be deductively rigorous (hence top-down), then it can have *no* truck with 'meaningful mathematical problems', 'mathematical ideas', or 'clarity' (which seem one and all to be bottom-up)? Fortunately, in FN:24 philosophy as in mathematics, there is no *ignorabimus*.[24] What is needed is a new, post-Kantian conception of concepts; and Frege shows us how to FN:25 develop one.[25]

Although not always recognized as such, Kant's distinction between intu- FN:26 itions and concepts is (among other things) a properly logical advance.[26] Ancient logic is a term logic in which no logical distinction is drawn between terms that can be applied only to one thing and terms that can be applied to many. Any individual, Socrates, say, can be called many things (that is, by many names): Socrates, pale, man, snub-nosed, wise, mortal, and so on. In effect, the terms in a classical term logic combine both a 'referential' and a 'predicative' aspect—which is why it is valid in such a logic to infer from the fact that all S is P that some S is P. (If there is nothing to call an S, then there is nothing to be said about the Ss.) Later rationalists and empiricists would emphasize, respectively, the predicative or the referential aspects of terms, or of their cognitive correlates, but it is only with Kant that a clean break is made. Intuitions are referential; they give objects. Concepts are predicative; they are ways the objects given in intuitions can be thought (correctly or FN:27 incorrectly).[27]

[24] The claim, for the case of mathematics, is of course Hilbert's: 'The conviction of the solvability of every mathematical problem is a powerful incentive to the worker. We hear within us the perpetual call: There is the problem. Seek its solution. You can find it by pure reason, for in mathematics there is no *ignorabimus*' (1902); quoted in Detlefson (2005: 278).

[25] It must be emphasized that the conception of language to be outlined here, following Frege, is deeply different from standard conceptions. Here I can only gesture at the reading that is developed in Macbeth (2005).

[26] Thompson (1972–3) makes the point that Kant has a monadic predicate calculus, i.e. the calculus that Russell once described as 'the first serious advance in real logic since the time of the Greeks'—which Russell himself thought was discovered first by Peano, and independently, by Frege (Russell 1914: 50). As Russell saw, the extension of the monadic predicate calculus to the full logic of relations is a merely technical advance.

[27] This new conception of concepts in Kant is also indicated by the fact that Kant is the first to hold that not concepts, but judgments, are the smallest unit of cognitive significance, that concepts are significant only as predicates of possible judgments. (On the Peircean conception, as in Frege, the smallest unit of significance is the inference, because without inference there is no truth.)

From Kant's perspective, ancient term logic rests on a conflation of two logically different sorts of representations. What Frege realized, by the early 1890s, is that this Kantian distinction of two logically different sorts of representations *similarly involves a conflation*, this time of two logically different logical distinctions, that of *Sinn* and *Bedeutung* with that of concept and object. As Frege puts the point, 'it is easy to become unclear . . . by confounding the division into concepts and objects with the distinction between sense and meaning so that we run together sense and concept on the one hand and meaning and object on the other' (1892–5: 118). Frege himself at first made just this mistake; he confused cognitive significance or *Sinn*, sense, with the notion of a concept, and objective or semantic significance with the notion of an object. As a result, he at first thought, with Kant, that all content and all truth lie in relation to an object. Our quantificational logics, as Tarski has made explicit, are founded on precisely this Kantian thought—which is why we are faced with the dilemma of formalism or Platonism in the philosophy of mathematics. The dilemma is forced on us because we have not yet learned, following Frege, to distinguish between objects and objectivity, on the one hand, and between concepts and 'conceptual' (better: cognitive) significance, on the other. Once having made this distinction between these two different distinctions, we can begin, at least, to understand the nature of a properly logical language *within which* to reason discursively—that is, from concepts alone—and thereby how again to combine intuitive with deductive rigor in the practice of mathematics.[28]

Concepts, on Frege's mature view, are laws of correlation, objects to truth-values in the case of first-level concepts, and lower-level concepts to truth-values in the case of higher-level concepts. A concept is thus something in its own right, something objective that can serve as an argument for a function in a judgment. One can, for example, judge of two concepts that one is subordinate to the other; that is, that the second-level relation of subordination is correctly applied to them. That second-level relation of subordination, similarly, is something objective about which to judge, correctly or incorrectly. It is, for instance, a transitive relation; *subordination* has the property of being transitive. There is, then, for Frege a natural division of 'levels' of knowledge. First, there are facts about the everyday objects of which we have sensory experience. Such facts are one and all a posteriori and

[28] By 'intuitive' here I do not mean involving Kantian intuition, but instead that one can see or understand clearly what is proved and why it must be true given one's starting points.

contingent (setting aside instances of laws of logic or of a special science); and in *Begriffsschrift*, Frege's formula language of pure thought, these facts are expressed using object names and first-level concept words.

One level up are facts about first-level concepts: for example, the fact that *cat* is subordinate to *mammal*—that is, that being a cat entails being a mammal (from which it follows that any particular cat is necessarily a mammal). Such a law can be expressed in *Begriffsschrift* either using the conditional stroke and Latin italic letters lending generality of content, or using the conditional stroke together with the concavity and German letter.[29] In the latter case, we have (at least on one function/argument analysis of the sentence) a sign for the second-level relation of subordination, one that is formed from the conditional stroke together with the concavity. To move up a level again, to consideration of second-level concepts such as subordination, is to move from the domain of the special sciences to the domain of the science of logic. The subject matter of logic on Frege's (mature) view is the second-level properties and relations that hold of the first-level concepts that constitute in turn the subject matter of the special sciences—including mathematics if logicism is wrong.

In *Begriffsschrift*, Frege's formula language of pure thought, concept words are given *only* relative to a function/argument analysis of a whole *Begriffsschrift* sentence, and inevitably more than one analysis is possible. Independent of any analysis, a sentence of his logical language expresses only a thought, which is a function of the senses of the primitive signs involved, and designates a truth-value. To determine what such a sentence is about, what the argument is, and for what function, one must provide an analysis, carve the sentence into function and argument, and there will be many ways to do this, as Frege's two-dimensional notation makes especially perspicuous.

Concept words, which are given only relative to an analysis, designate concepts—or at least, they purport to. They also express senses. Indeed, a *Begriffsschrift* expression, which is formed by combining the primitive signs of the language in appropriate ways, most immediately maps or traces a sense; it shows in its composition the sense through which something objective is (or purports to be) grasped. Consider, for example, the concept of continuity (of a function at a point). This concept takes mathematical functions and

[29] True accidental generalities can also be expressed these ways in Frege's logic. (Because inferences can be drawn only from acknowledged truths in Frege's logic, this does not introduce any difficulties into the logic.) The basic case, however, is that of a relationship among concepts that is not grounded in contingent facts about objects, but is immediately about concepts. See Macbeth (2005).

points as arguments to yield truth-values as values. But our grasp of that concept is mediated by a sense, one that we can have more or less clearly in mind. By the time Frege was writing, the content of this concept—that is, the sense through which it is grasped—had been clarified, and in 'Boole's Logical Calculus and the Concept Script' (1880/1) Frege shows just how that sense is expressed in a complex two-dimensional array in *Begriffsschrift*. Such an expression, formed from the primitives of *Begriffsschrift* together with some signs from arithmetic, *designates* the concept of continuity; it is a *name* for that concept. But that complex of primitive signs also expresses a sense: that is, the inferentially articulated content that is grasped by anyone who clearly understands what it means for a function to be continuous at a point. Concepts, as Frege understands them, are thus internally inferentially articulated. Such inferential articulation fundamentally contrasts with the sort of external articulation that is provided by an axiomatization on Hilbert's understanding of it—that is, an axiomatization fixing inferential relations between concepts—rather than, as is the case on Frege's conception, within concepts.[30]

FN:30

In a *Begriffsschrift* sentence, then, at least as it is conceived here, three levels of articulation are discernible. First, there are the primitive signs out of which everything is composed; then there are the concept words, the function and argument, that are given relative to an analysis of the sentence; and finally there is the whole sentence, which expresses a thought and designates, or ought to designate, a truth-value, either the True or the False. As should be evident, the primitives of the language so conceived cannot be taken to designate independent of a context of use. Only in the context of a sentence, and relative to an analysis into function and argument, can we speak of the designation of a sub-sentential expression. What one grasps when one grasps the meaning of a primitive of the language, then, is the sense expressed, and thereby the contribution which that primitive makes to the thought expressed by a sentence containing it, and, relative to some assumed analysis in which the primitive occurs as a designating expression, the designation. But that primitive expression can equally well appear as a component of a complex expression that designates something quite different. Frege's concavity, for example, which, taken alone, functions rather like a quantifier, also occurs in a wide range of expressions for other higher-level concepts such as the

[30] See Macbeth, forthcoming,' for a more extended discussion of these different conceptions of inferential articulation.

second-level relation of subordination, or that of the continuity of a function at a point. Again, it is only relative to a function/argument analysis that subsentential expressions of *Begriffsschrift*, whether simple or complex, can be said to designate. (Needless to say, such a conception of language is essentially late; only someone already able to read and write could devise or learn such a language.)

As it is understood here, a *Begriffsschrift* sentence is rather like a Euclidean diagram, in that it can be regarded in various ways, given now one function/argument analysis and now another. A sentence containing Frege's (complex) sign for the concept of continuity can, for instance, be analyzed so as to yield that concept word; but it can also be analyzed in other ways, in ways that effectively cut across the boundaries of this concept word—and it may need to be so analyzed for the purposes of proof. It is precisely because concept words in *Begriffsschrift* are at once wholes made up of primitive parts and themselves parts of larger wholes—namely, sentences—that a *Begriffsschrift* proof can be fruitful, an extension of our knowledge. Given what the concepts involved mean, the senses through which they are given, one comes to see in the course of the proof how aspects of those senses can be figured and refigured to yield something new.[31]

Kant already taught us to see the course of a demonstration in Euclid as realizing the figure that is wanted, and the course of a calculation in arabic numeration as realizing the product (say) that is wanted. Frege, I am suggesting, similarly can teach us to see the course of a proof as realizing the conclusion that is wanted—teach us, that is, to see a proof as a *construction*, just as Peirce suggests we should. So conceived, a proof does not merely establish the truth of the conclusion (assuming the truth of the premises); it shows *how* the conclusion follows from the premises, how the conclusion is contained in the premises 'as plants are contained in their seeds, not as beams are contained in a house' (Frege 1884: §88). Much as one calculates *in* arabic numeration, so one reasons *in Begriffsschrift* in a way that is at once deductively rigorous and intuitively rigorous, in a way that one can follow and understand. And one can do this because *Begriffsschrift* puts a thought before one's eyes. Frege's logical language is in this way an utterly different sort of logical language from any with which we are familiar. As the point might be put, it is not a *mathematical* logic at all; it is not an algebra of thought that reveals valid patterns or structures. It is instead a *philosophical*

[31] See Macbeth (2007) for some examples of how this works in practice.

logic within which to express thoughts, and thereby to discover truths. As Kant taught us to read calculations in arithmetic and algebra not as merely mechanical manipulations of signs but as constructions revelatory of truth, so Frege teaches us to read a proof in symbolic logic not as a merely formal, deductively valid derivation but as a fully meaningful inference to a conclusion that is thereby revelatory of truth.

But not all contentful chains of reasoning are revelatory of their conclusions. As Lakatos (1976) shows, the practice of mathematics often proceeds by way of a process of proof and refutation—that is, dialectically in a way that involves both *modus ponens*, inference from acknowledged truths to other truths that are entailed by them, and also *modus tollens*, inference from the acknowledged falsity of some putative conclusion to the falsity of what had mistakenly been taken to be true. But if that is right, then an axiomatization of some domain of knowledge should be seen as providing not so much the foundation of that domain as a vehicle for the discovery of truths. Quite simply, by making one's conceptions explicit in axioms and definitions, one can then test their adequacy 'experimentally', by deriving theorems, just as Peirce suggests. If a manifest falsehood—for example, a contradiction—is derived, then one knows that one's conceptions are faulty, that one has not achieved adequate grasp of some concept, or perhaps is mistaken in thinking that there is any concept there to be grasped at all.

This is, of course, just what happened to Frege. His Basic Law V, which made explicit the notion of a course of values as Frege understood it, was shown by Russell to be flawed. But whereas for Russell (who, by his own admission, wanted certainty the way others want religious faith) that discovery was a disastrous blow to the very foundations of arithmetic, for Frege, a mathematician concerned with understanding, it was a crucial step forward, one that, as Frege (1980 [1902]: 192) writes to Russell, 'may perhaps lead to a great advance in logic, undesirable as it may seem at first'. For it is only through such a discovery that we can recognize the flaws in our understanding, and on that basis formulate better conceptions. In fact, Frege came to think, the notion of a course of values cannot be salvaged; his logicist thesis had been mistaken. But, as Frege was well aware, his logic and the language he devised for it remained intact. What has been suggested here is that it is just such a logic and such a language that are needed if we are to understand the practice of contemporary mathematics, reasoning from concepts alone.

Mathematics has its own proper subject matter, its own concepts and conceptions on the basis of which mathematicians not only reason but also judge, for instance, the 'naturalness' (that is, the inherently mathematical character) of various structures and lines of argument. But if the language that mathematicians use is understood as the tradition has taught, in terms of a fundamental dichotomy of logical form and (empirical) content, of syntax and semantics, then, as we have seen, there is, and can be, no properly mathematical subject matter. Frege long ago developed an alternative conception of language, one that combines deductive rigor with contentfulness, syntax with semantics, and thereby enables reasoning from the contents of concepts—that is, on the basis of their inferentially articulated senses. Such a conception of language is essentially Peircean, and because it is, it enables us to understand just how mathematics can be at once a priori, by reason alone, and also inherently fallible. Because concepts are grasped through senses that may be only imperfectly understood, we can make mistakes, just as Frege did with his Basic Law V; and so too we can correct our mistakes. It is for just this reason that mathematics is rational; it is rational, in Sellars's words (1956: §38), 'not because it has a *foundation* but because it is a self-correcting enterprise which can put *any* claim in jeopardy, though not *all* at once'.[32] Though it cannot be the whole story, this pragmatist conception of language, truth, and logic would thus seem to be an essential ingredient in any intellectually satisfying account of mathematical practice as it emerged in the nineteenth century and continues today.

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[32] It should be noted that Sellars himself is talking about empirical knowledge rather than mathematical knowledge, but as Frege and Peirce together help us to see, the point generalizes.

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Queries in Chapter 22

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