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An Opinionated Introduction

Each of the essays that follow could be sent forth as an orphan, with only the most perfunctory comment, to make its own solitary way in the world, and some of them would undoubtedly do well. However, we believe that there are important reasons for setting them side by side, that each gains from the company of the others. In our judgment, the present volume offers a representation of the current state of history and philosophy of mathematics. Hence we have felt encouraged—perhaps foolhardily—to offer perspectives on the past, the present, and the future. This introduction attempts to sketch the history of the philosophy of mathematics in the last century and the recent history of the history of mathematics. We then endeavor to relate the individual studies not only to one another, but also to the traditions of work in the history of mathematics and in the philosophy of mathematics. We conclude with some speculations about the present trends and possible futures. Quite plainly, any effort of this kind must embody the authors' ideas, perspectives, and, maybe, prejudices about what kinds of researches are important. So we have rejected the plain title "Introduction" and announced our essay for what it is—an opinionated introduction.

1. Philosophy of Mathematics: A Brief and Biased History

Philosophers of mathematics may argue about many things, but, until recently, there has been remarkable agreement concerning when their discipline began. Prevailing orthodoxy takes the history of the philosophy of mathematics to start with Frege. It was Frege who posed the problems with which philosophers of mathematics have struggled ever since, Frege who developed modern logic and used it to undertake rigorous exploration of the foundations of mathematics, Frege who charted the main philosophical options that his successors were to explore. To be sure, there were earlier thinkers who considered mathematics from a philosophical point of view, scholars of the stature of Kant, Mill, and Dedekind. But they belong to prehistory.

The orthodox account embodies something of Frege's own view of what he achieved, as well as reflecting his harsh judgments about some of his predecessors and contemporaries (see Frege 1884). Within the last decades there have been criticisms of the conventional wisdom, in part because philosophers have discovered merit in the work of some of Frege's predecessors (Parsons 1964, 1969 and Friedman 1985 on Kant; Kessler 1980 and Kitcher 1980 on Mill; Kitcher 1986 on Dedekind) and in part because there have been systematic and thorough studies of Frege's place in the history of philosophy (Sluga 1980; Resnik 1980). Nevertheless, because Frege has so often been seen as the *fons et origo* of real philosophy of mathematics, it is well to begin with him and his accomplishments.

Frege was originally trained as a mathematician, and it is clear from his early writings that he became interested in integrating late-nineteenth-century mathematics into an epistemological picture that he drew (at least in large measure) from Kant. During the 1870s he seems to have become convinced that Kant's ideas about arithmetic were incorrect and that the deficiency could only be made up by a thorough revision of logic, a project that he began in his brilliant monograph (1879). By 1884, he was prepared to publish his most articulate account of the philosophy of mathematics. At the outset, he adopted the view that the central problem for the philosophy of mathematics was to identify the foundations of mathematics (1884, 1-2). Holding that the task of setting arithmetic on a firm foundation was a natural extension of the enterprise of the foundations of analysis (pursued by Dedekind, Weierstrass, Heine, and others), Frege undertook to review the possibilities.

Since the goal of the foundationalist is to display the proper justification for the mathematical statements under study, Frege suggested that there were as many possible approaches as his background epistemology allowed—to wit, three. The basic division is between justificatory procedures that are a priori and those that are a posteriori. Within the category of the a priori, there are two options. Arithmetic is either ultimately derivable from logic plus definitions of the special arithmetical vocabulary, or it is founded (at least in part) on some a priori intuition (as Frege took Kant to claim). Having argue that arithmetical statements do not admit empirical justification and that no a priori intuition is available in the case of arithmetic, Frege concluded that arithmetic was simply a development of logic.

In fact, he knew of and argued against a fourth possibility. From the perspective of the *Grundlagen* (Frege 1884), arithmetic is taken to be a science with a definite content. Elsewhere, Frege responded to the proposal that, strictly speaking, arithmetical statements are meaningless and arithmetic is simply a game that mathematicians (and others) play with signs (see the essays collected in Frege/Kluge 1971). We believe that, until the end of his life, Frege operated with the view that there were only a very small number of possibilities for finding the proper justification of arithmetical statements and that the strategy of elimination of alternatives, adopted in his early writings, had a profound effect on the development of his views (see Kitcher 1979).

Frege's eliminative argument for logicism was only the first step in the project of *Grundlagen*. Having argued that arithmetic had to be a development of logic, Frege took it upon himself to exhibit the development. One important part of the task was to delineate clearly the principles of logic—an enterprise that Frege had already begun (1879) and was to continue (1893-1903). Another was to show how the concepts of arithmetic could be defined in logical terms. Most of *Grundlagen* was devoted to the latter project, and Frege argued first that attributions of number are assertions about concepts and subsequently that the natural numbers should be identified with the extension of particular concepts.

If Frege was the "onlie begetter" of the philosophy of mathematics, then it is easy to identify his offspring. First, and of prime importance in the later history of the field, is his conception of the central problems. Philosophers (or philosophically minded mathematicians) should disclose the foundations of parts of mathematics by identifying the proper justifications for the statements belonging to those parts. Second is his taxonomy of options: logicism, formalism, appeals to some kind of intuition (in the manner of Kant), and (definitely bringing up the rear) empiricism. Third is the collection of arguments that are designed to reveal logicism as the only possible position. Fourth is the analysis of arithmetical discourse and, in particular, the claim that arithmetical statements record the properties of (logical) objects. Last, but definitely not least, is the elaboration of a logical system, which, to our mind, constitutes the greatest single achievement in the history of logic—bar none.

Of course, Frege's ideas suffered a temporary setback. In 1902 Russell discovered that the logical system, so painstakingly constructed and so little appreciated, was inconsistent. Ironically, if the discovery of Russell's paradox cast doubt on Frege's substantive ideas about the nature of mathematics, it served only to underscore the importance of the task in which he had been engaged. In the last decades of the nineteenth century, professional mathematicians rarely saw Frege as tackling a genuine problem that was continuous with the difficulties that Weierstrass and his school had attempted to address. Although the disentangling of concepts of continuity and convergence had an obvious mathematical payoff, there seemed little point to the minute excavation of the foundations of arithmetic. The paradoxes of set theory, first noted by Cantor and Burali-Forti in the 1890s and sharply presented in Russell's observation about Frege's system, changed that confident assessment. They showed that the intuitive reasoning about sets (or systems, collections, or multitudes) on which mathematicians had relied, increasingly and with ever greater explicitness, during the latter part of the nineteenth century, were surprisingly hard to represent in a precise fashion. Nobody had doubted that there was a "proper justification" for the principles of arithmetic. But the first serious attempt to identify it had exposed difficulties in widely adopted modes of reasoning that were disturbingly reminiscent of those that had been recently resolved in the foundations of real analysis.

For the first three decades of the twentieth century, the philosophy of mathematics was dominated by the rival claims of three main foundational programs, each of which pursued the Fregean task of exposing the proper justification for arithmetic (and analysis), and each of which embodied a response to the paradoxes. This is not to deny that there were critics, most prominently Poincaré, who offered alternative visions of what philosophy of mathematics ought to be (see Goldfarb, this volume). But Frege's identification of philosophy of mathematics with foundations of mathematics won the day, and the principal differences were internal to the general Fregean framework. Closest to Frege's original approach was the program undertaken by Russell and Whitehead in *Principia Mathematica*, although it was apparent, almost from the beginning, that some of the principles that Russell and Whitehead took as their starting assumptions were not uncontroversially *logical*. The theory of types was able to circumvent the paradoxes, but the accommodation of arithmetic it offered was purchased at the cost of axioms, most notoriously the axioms of infinity and reducibility, that sat uneasily with Frege's characterization of laws of logic—laws without which no thinking is possible.

A second proposal, deeply at odds with Frege's doctrine that mathematics is a doctrine all of whose statements have an identifiable content, was Hilbert's suggestion that a foundation for mathematics should be provided by constructing finitary consistency proofs for mathematical systems. As Hilbert's program was articulated in its mature form (Hilbert 1926), there is a body of contentful mathematical statements whose credentials are above suspicion. This body of contentful statements is to be used in proving the consistency of formal systems in which contentful mathematics is itself embedded and which also include ideal statements. The role of these ideal statements is to facilitate transitions among the contentful statements, and the set-theoretic paradoxes show us that, if we are careless in adjoining such ideal auxiliary statements to our mathematical systems, we generate contradictions. Consistency proofs will guard us against such possibilities—so long, of course, as they are given within a mathematical framework (contentful mathematics) that cannot itself be questioned. Hence, for Hilbert, the important task is to give finitary consistency proofs for formal systems that formalize classical mathematics. Gödel's theorems seemed to show that the task cannot be completed, even when the part of classical mathematics in question is first-order arithmetic. (For illuminating discussions of Hilbert's program and of the significance of Gödel's incompleteness theorems, see Kreisel 1958; Resnik 1974; Detlefsen 1979; Tait 1981; Simpson forthcoming.)

The third program, developed by Brouwer, sought a foundation for mathematics by returning to Kant. Brouwer claimed that we construct the objects of mathematics and that our knowledge of their fundamental properties is based on an a priori intuition. Through this intuition, we are able to recognize a potential infinity of mathematical entities (the natural numbers), and these can form the basis for further constructions. But classical mathematics goes astray both in its positing of objects that are beyond the reach of human constructive powers and it its use of nonconstructive existence proofs, achieved through the employment of the law of excluded middle. Brouwer, and later Heyting (1956), proposed to substitute for classical mathematics an alternative that would be free of these mistakes and that would be true to the constructive/intuitive foundation of mathematics. To the response that the result was a mutilation of mathematics, in which long, ugly proofs of intuitionistic analogs replaced the simple and elegant classical proofs and theorems, they responded that such alterations were needed to avoid the profligate and incoherent metaphysics of the classical mathematicians. (For seminal works in classical intuitionism, see Brouwer 1949, Heyting 1956; a recent development of constructivist mathematics is Bishop 1967; Dummett 1977 gives a profound interpretation of the intuitionist program; for an interesting alternative view, see Shapiro 1985).

The three programs we have mentioned correspond to three of the four possibilities that occur in Frege's eliminative argument for logicism. Russell and Whitehead carry on with the logicist strategy, Hilbert develops a sophisticated version of the idea that (at least part of) mathematics is a formal game, and Brouwer suggests that the foundation of mathematical knowledge lies in nonlogical, a priori, intuition. The one of Frege's options that does not issue in a developed program is the suggestion of which he was most scornful: there is no empiricist tradition in the philosophy of mathematics in the early twentieth century (although Curry's version of formalism has some connections with empiricism; see his 1951).

In noting the relationship between the dominant themes in philosophy of mathematics from 1900 to 1930 and Frege's basic epistemological categories, we should not overlook the fact that each of the programs also has roots in nineteenth-century mathematics (for different amplification of the theme, see Resnik 1980 and Stein, this volume). However, insofar as they continue the Fregean identification of philosophy of mathematics with the foundations of mathematics and insofar as they see the task of laying the foundations as finding a priori justifications for parts of mathematics, they are naturally seen as internal modifications of a very general research program that Frege set for his successors. We shall now suggest that the next phase of developments in the philosophy of mathematics remained deeply Fregean in spirit.

During the 1920s and early 1930, logicism underwent a fundamental change, a change that would affect the credibility of each of the alternative views of the foundations of mathematics. Inspired by their reading of Wittgenstein's *Tractatus*, the logical positivists suggested that the idea of tracing arithmetic back to logic-plus-definitions gave the false appearance that two different types of principles were fundamental to mathematics. Instead, they recommended, we should think of logic itself as tacitly embodying the definitions that we have fixed for the logical vocabulary, the connectives and quantifiers. Thus the entire corpus of logic and mathematics can be conceived as elaborating the conventions that underlie our language; the statements that make up this corpus are true by virtue of meaning (see Carnap 1939; Ayer 1936; Hempel 1945). From this perspective it does not matter, in the end, whether the axioms of various bits of mathematics are (or are derivable from) laws of logic. What counts is that they should be (or be derivable from) principles that embody the conventions that govern the primitive vocabulary.

The significance of this change for the credibility of the other programs is that it provides a way to accommodate certain ideas of formalism and to remove the sting of the intuitionist critique of classical mathematics. One of the chief points of dispute between Hilbert and Frege had surrounded Hilbert's early proposal that the primitive vocabulary of an axiomatic system is implicitly defined by the axioms that are set down. On Frege's conception of logic (which held no place for the notion of an uninterpreted statement), this claim of Hilbert was absurd (see Resnik 1980 for a lucid discussion of the controversy.) In the wake of the development of the semantics of logic (see Goldfarb 1979 and Moore, this volume), Frege's conception of logic was abandoned and logicists came to adopt Hilbert's proposal. Thus, at a time when the mature Hilbert program was in difficulties, it appeared possible to salvage part of Hilbert's early motivations. Furthermore, instead of conceiving intuitionism to be a rival mathematics, the emphasis on the conventionality of logic made it possible to see intuitionistic logic and mathematics as embodying a different set of conventions. The system based on those conventions could be tolerated as an interesting curiosity, something that could be placed alongside classical mathematics but that could not pose any threat of displacement (see Carnap 1937).

The new logicism represented both a return to Frege and a departure. Because of Frege's opposition to the founding of mathematics on intuition, his scorn for empiricism, and his emphasis that mathematical statements are meaningful, the logical positivists could legitimately cite him as a predecessor. However, their ideas about logic and semantics were importantly different from those that Frege had espoused (see Dummett 1973; Sluga 1980; Ricketts 1985). Hence there was a genuine transition between one version of logicism, on which the principles of logic were seen as fundamental to all thought, to another, on which logic was true by convention (Friedman, this volume, provides an illuminating account of part of the transition).

The new form of logicism avoided awkward questions about how we know the principles that prescribe to all thought, but it quickly became vulnerable to a different challenge. In 1936, Quine published a seminal article containing two distinct arguments against the idea that logic and mathematics are true by convention. The first of these arguments contends that any part of our knowledge can be replaced by a system whose principles are true by convention. Thus it appears (although Quine does not hammer home the point) that the thesis that logic and mathematics are true by convention cannot explain the epistemological status of these disciplines, unless their epistemological status is the same as that of other branches of inquiry. The second argument, adapted from an idea of Lewis Carroll, is that logic could not be true by convention for the simple reason that logic is required to extract the consequences of any conventions we may lay down.

Quine's second argument seems to have been more immediately influential than his first, for it prompted a reformulation of the canonical doctrines of the new logicism. Although logicists continued to describe logic and mathematics as conventional, they preferred to speak of the truth of these disciplines as resting on semantical rules—rules that were explicitly formulated in the construction of formal systems aimed at explicating prior mathematical usage and that were taken to underlie that prior usage—rather than thinking of truth as resulting from an explicit convention (see Carnap 1939; Hempel 1945). Quine's first argument unfolded into a critique of the notion of semantical rule (and the related notion of truth by virtue of meaning) designed to circumvent these reformulations. Thus in (1953), in (1962), and in a host of later writings, Quine argued at length that there are no statements true by virtue of meaning. One consequence of these arguments was a muted (and underdeveloped) empiricism.

As logicism suffered under Quine's attack on its fundamental conception of logical truth, a different Fregean theme underwent a revival. Since he did not believe the truths of logic to be true in virtue of meaning *and therefore as devoid of factual content* (as some of the positivists maintained), Frege saw no tension between claiming that arithmetic is disguised logic and declaring that numbers are objects. Logical positivism downplayed the latter declaration, and the positivists sometimes maintained that, as statements that are true in virtue of meaning, truths of logic and mathematics are devoid of any reference. Quine's scrutiny of the concept of truth by virtue of meaning led him to pose the ontological questions: what, if anything, is mathematics about? Accepting Frege's analysis of the logical forms of arithmetical statements and adopting the semantics for first-order languages developed by Tarski, Quine came to Frege's own Platonistic conclusion: mathematics is about abstract objects, numbers and sets. (Earlier, Goodman and Quine [1947] had explored the possibility of managing without any abstract objects at all, but they concluded that nominalism was inadequate to allow for the truth of those mathematical statements that are required in empirical science.) Quine's later writings have attempted to show that the only abstract objects needed by mathematics and science are sets and to integrate this Platonistic position with his epistemology.

Quine was not alone in fostering a renaissance of Frege's doctrine that numbers are objects. In investigating one of the great outstanding problems in set theory in the 1940s, the truth of Cantor's continuum hypothesis (which states that the power of the continuum is $\aleph_1$), Gödel pondered the possibility that the continuum hypothesis is independent of the axioms of set theory (that is, the axioms of a standard set theory such as ZF or NBG). A possible response to the discovery of independence (which Gödel envisaged and which was established in part by his own work and in part by subsequent work of Paul Cohen) would be to claim that the continuum hypothesis has no truth value until we specify the system in which it is to be embedded. Thus, just as there are Euclidean and non-Euclidean geometries, and just as the parallel postulate holds in the former but not the latter, so, perhaps, we shall have to talk about Cantorian and non-Cantorian set theories. Gödel took this response to be profoundly misguided. Even if the continuum hypothesis were found to be independent of the rest of set theory (as in fact it was), we should continue to ask whether it is true and to seek principles that give an adequate and complete characterization of the universe of sets. Pursuing the point, Gödel contended that there is a definite world of mathematical entities, that these entities are independent of human thought, and that human beings have a way of apprehending the fundamental properties of these entities. In a phrase that was to resonate in subsequent philosophy of mathematics, he wrote that the truth of the axioms of set theory "forces itself upon us" (Gödel 1964).

Like Quine, Gödel departed from the positivist conception that, in mathematics and logics, truth is dependent not on reference to objects but on meaning (or, in the more primitive versions, on conventions). Frege's old conception of mathematics as a body of statements about abstract objects came to occupy center stage in the philosophy of mathematics, appearing as the only serious possible view about mathematical ontology and mathematical truth. But some of the connections within Fregean epistemology had now been broken. Frege's own wedding of the ontological thesis to the claim that mathematics is logic seemed no longer available, for, according to the orthodox view of logic, logic has no special content, not even objects so ethereal as those apparently needed for mathematics. Quine's attacks on the notion of truth by virtue by meaning had apparently led him to a version of empiricism about mathematics that was not vulnerable in the ways that Mill's empiricism had been, but it was far from clear what positive claims about mathematical knowledge were embodied in Quine's writings. Finally, Gödel's ontological Platonism was accompanied by a clear and straightforward epistemological thesis that fractured Frege's old (Kantian) link between intuition and the construction of mathematical objects. According to Gödel, we have the ability to intuit the properties of *mind-independent* objects, and this accounts for our knowledge of the basic principles of set theory (and, in the future, it may account for the principles that we add to complete our knowledge of the universe of sets). Yet, although it may be relatively easy to understand, Gödel's view is not so easy to believe, for the notion of intuition on which it relies appears somewhat nebulous. Frege's old warning is apt: "We are all too ready to invoke inner intuition, whenever we cannot produce any other ground of knowledge" (1884, 19).

During the 1950s and 1960s, the philosophy of mathematics centered around efforts to displace the versions of logicism, formalism, and intuitionism that had been current in previous decades and to articulate a *neo-Fregean* view of mathematics whose distinctive claim was that set theory (specifically some extension of ZF) provided the foundations for mathematics. To a large extent, epistemological issues were neglected in favor either of technical explorations (concerning the import of the Gödel-Cohen discoveries or the significance of the Löwenheim-Skolem theorem, for example) or of attempts to show the superiority of neo-Fregeanism to one or the other of the (now unfashionable) earlier foundational programs. However, in the 1960s and early 1970s, a number of important criticisms of the Platonist orthodoxy began to emerge, and these criticisms have played a major role in shaping current work in the philosophy of mathematics.

On the neo-Fregean conception, all mathematics is embedded in set theory and all the entities of mathematics are sets. In particular, numbers are sets. But, it appears, if numbers are sets then they must be particular sets, and the philosopher should be able to identify *which* ones they are. However, mathematicians have long recognized that arithmetic can be reduced to set theory in many different ways: the natural numbers can be identified with the von Neumann numbers, the Zermelo numbers, or with any of an infinite number of other sequences of sets. Quine (1960, 1970) used the point to argue for his own ideas about reference and reduction, but the most forceful posing of the problem occurred in an article by Benacerraf (1965). In effect, Benacerraf's discussion delineated three options for the philosopher of mathematics: we can either try to find some principle for choosing one of the set-theoretic identifications of the natural numbers as privileged (and Benacerraf carefully explored some main possibilities, with negative results), we can try to maintain the thesis of ontological Platonism without identifying numbers with any particular sets (as Quine had argued, and as subsequent writers—White 1974; Field 1974; Resnik 1980, 1981; Maddy 1981—were to do), or we can abandon ontological Platonism. In the 1960s and 1970s, few philosophers were prepared to consider the last option.

However, there were occasional expressions of dissent. In 1967, Putnam contrasted the "mathematics of set theory" picture (our neo-Fregeanism) with an alternative that he called "mathematics as modal logic" (Putnam 1967). His suggestion was that mathematical statements can be formulated as necessary conditionals whose antecedents are the conjunctions of axioms of standard mathematical theories. In this way, we may capture parts of the idea that mathematics is concerned with the properties of structures, while avoiding the conventionalist notions that accompanied that idea during the heyday of positivism. Putnam also proposed that neither the neo-Fregean picture nor the modal logic picture should be viewed as offering the fundamental correct view of mathematics. Both should be recognized as "equivalent descriptions."

A more direct attack on ontological Platonism was offered in 1973 by Chihara, who developed a detailed account of a nominalist version of mathematics. Chihara proposed to reevaluate the Quinean argument for Platonism by showing how the mathematics needed for science could be articulated without commitment to abstract entities. Chihara's work may fairly be taken as the beginning of a revival of interest in nominalism, which has been pursued in his own subsequent work and (in different ways) in Gottlieb (1980) and Field (1980, 1982, 1984, 1985).

However, the most influential study of the new orthodoxy in philosophy of mathematics was another article by Benacerraf (1973). Benacerraf constructed a dilemma, designed to show that our best views about mathematical truth do not fit with our ideas about mathematical knowledge. Start from the relatively unproblematic theses that there are some mathematical statements that are true and that some of these are known by some people. Then a task of philosophy is to provide an account of the truth of those statements that are true and an account of how we know the mathematics that we know. On our best accounts of truth in general and of the form of mathematical statements (accounts that derive from Frege and Tarski), true statements of mathematics owe their truth to the properties of and relations among mathematical objects (paradigmatically sets). It appears that such objects are outside space and time—for there seem to be too few spatiotemporal objects to go around, and, in any case, the truths of mathematics seem independent of the fates of particular spatiotemporal entities. According to Benacerraf, on our best account of human knowledge, knowledge requires a causal connection between the knower and the objects about which the knower knows. Since mathematical objects are outside space and time, there can be no such connection between them and human beings. Hence, on the best accounts of truth and knowledge, mathematical knowledge turns out to be impossible after all.

Since the early 1970s, much of the research in the philosophy of mathematics has been devoted to evaluating the merits of neo-Fregeanism in the light of the studies that I have briefly reviewed. Particularly important has been the task of finding an appropriate response to Benacerraf's dilemma. Some writers (notably Steiner 1975; Maddy 1980; Kim 1982) have argued that neo-Fregeanism is unthreatened by the dilemma: they hold that a proper understanding of the conditions on human knowledge will allow for us to have knowledge of objects that are outside space and time. Others (Lear 1977; Jubien 1977) have deepened the dilemma and have proposed revisions of neo-Fregeanism to accommodate it. In addition, some writers have proposed approaches to the ontology of mathematics that question the thesis that mathematical objects are sets. The less radical of these is *structuralism*, the doctrine that mathematics describes the properties of mathematical structures. More radical is the nominalism of Chihara, Gottlieb, and Field.

Structuralism is a natural reaction to Benacerraf's other problem, the question of how to identify the *genuine* natural numbers among all the possibilities that set theory supplies. It is tempting to reply that all the set theoretical identifications share a common structure and that it is this structure that is the subject matter of arithmetic, not any particular ω-sequence of sets. One can remain completely within the preferred ontological framework of neo-Fregeanism by supposing that arithmetic describes the properties of a structure that is multiply realized in the universe of sets (White 1974; Field 1974; see Kitcher 1978 for criticism and Maddy 1981 for an attempt at revival). Alternatively, the structuralist may treat mathematical structures as entities in their own right, contending that set theory, like other mathematical disciplines, is concerned with a particular kind of structure (Resnik 1975, 1981, 1982; Shapiro 1983a). On either of these approaches, mathematical entities seem to be abstract objects, so that the structuralist ontology will have to be supplemented with a response to Benacerraf's dilemma. But structuralism may also be articulated with an eye to coping with the epistemological difficulty raised by Benacerraf, perhaps by using notions from modal logic (as in Putnam 1967 or in Jubien 1981a, 1981b) or by working out a constructivist version of structuralism (Kitcher 1978, 1980, 1983). We believe that it is worth noting that structuralism has some obvious kinship with the ways in which some contemporary mathematicians (especially algebraists) present their discipline—and this seems particularly true of the version of structuralism favored by Resnik and Shapiro.

Contemporary nominalism has been elaborated in three rather different ways. Chihara's version (1973, 1984) retains the traditional idea that statements appearing to bear commitment to the existence of abstract objects can be reconstrued as assertions about linguistic entities. Gottlieb (1980) proposes to reinterpret the quantifiers, making use of substitional quantification to avoid commitment to abstract entities (see Parsons 1971 for a lucid account of substitutional quantification and Parsons 1982 for an illuminating appraisal of Gottlieb's program). Field (1981, 1982, 1984, 1985) has made the most decisive break with the tradition, arguing that physical theories can be reformulated so that they bear no commitment to numbers or to other mathematical entities. The conclusion he draws is that mathematical knowledge is simply logical knowledge, and that mathematical vocabulary functions as a device for obtaining consequences that would have been more difficult to reach without that vocabulary. It seems to us that there is an obvious similarity here to Hilbert's notion of "ideal statements," and indeed Field's program might seem closer to Hilbert's formalism rather than to traditional nominalism. (For penetrating evaluations of Field's work, see Malament 1982 and Shapiro 1983b.)

Besides these two lines of reaction to problems (actual or perceived) with neo-Fregeanism, there have been a number of other important developments in recent philosophy of mathematics. Some philosophers have been primarily concerned to explore the special issues that arise within various parts of logic and set theory, the status of conceptions of set, the credentials of second-order logic, the role of reflection principles, and so forth. Thus Parsons has produced an important series of essays (collected in his 1984) that examines questions of ontology as they arise locally in various versions of set theory and in other parts of mathematics. Boolos (1975, 1984, 1985) has offered a fresh approach to second-order logic, which resists the orthodox account (originally defended by Quine 1963 and 1970) that second-order logic is a cloudy way of doing set theory. Both these investigations have provided glimpses of fresh approaches to the more general problems discussed above, approaches that preserve insights from traditions that had previously seemed outmoded.

Because the influence of Frege's ideas on contemporary philosophy of mathematics is so evident, it should hardly be surprising that some current work in the philosophy of mathematics is centered around reappraisals of Frege. Thus, there have been detailed explorations of parts of Frege's philosophy of mathematics (Resnik 1980; Wright 1983), attempts to articulate Fregean themes (Hodes 1984; Boolos 1985), and an important series of investigations of the conceptions of logic and knowledge in which Frege's technical work was set (Goldfarb, this volume; Ricketts 1985). Although philosophical exploration of the origins of the main categories of analytic philosophy may initially appear rather remote from questions about mathematics, it is surely appropriate to remind ourselves that the problems whose history we have been tracing are continuous with the central problems of epistemology and metaphysics in the twentieth century. Understanding how the latter problems emerged will, perhaps, enable us to avoid errors and confusions in formulating them, mistakes that may lead us to favor nonsolutions or may prevent us from finding any solutions at all.

We have attempted to give a whirlwind tour of present work in *mainstream* philosophy of mathematics and to show where that work comes from. We now want to emphasize a point that has, we hope, been obvious to the reader who is following the story for the first time. Although our tale begins with the researches of mathematicians, originating with questions that seem to arise from the mathematics of the late nineteenth century, and although, with the discovery of the paradoxes, the task of providing foundations for mathematics seems to assume some mathematical importance, the distance between the philosophical mainstream and the practice of mathematics seems to grow throughout the twentieth century. Philosophy of mathematics appears to become a microcosm for the most general and central issues in philosophy—issues in epistemology, metaphysics, and philosophy of language—and the study of those parts of mathematics to which philosophers most often attend (logic, set theory, arithmetic) seems designed to test the merits of large philosophical views about the existence of abstract entities or the tenability of a certain picture of human knowledge. There is surely nothing wrong with the pursuit of such investigations, irrelevant though they may be to the concerns of mathematicians and historians of mathematics. Yet it is pertinent to ask whether there are not also other tasks for the philosophy of mathematics, tasks that arise either from the current practice of mathematics or from the history of the subject.

A small number of philosophers (including one of us) believe that the answer is yes. Despite large disagreements among the members of this group, proponents of the minority tradition share the view that philosophy of mathematics ought to concern itself with the kinds of issues that occupy those who study other branches of human knowledge (most obviously the natural sciences). Philosophers should pose such questions as: How does mathematical knowledge grow? What is mathematical progress? What makes some mathematical ideas (or theories) better than others? What is mathematical explanation? Ideally, such questions should be addressed from the perspective of many areas of mathematics, past and present. But, because the tradition is so recent, it now consists of a small number of scattered studies, studies that may not address the problems that are of most concern to mathematicians and historians or explore episodes or areas within mathematics that most require illumination.

If the mainstream began with Frege, then the origin of the maverick tradition is a series of four papers by Lakatos, published in 1963-64 and later collected into a book (1976). Echoing Popper, Lakatos chose the title *Proofs and Refutations*, and the choice is, in part, apt. One obvious theme of the essays is the discussion of a segment of the history of mathematics in terms of categories that are adapted from Popper's methodology for the natural sciences. We believe that the significance of the papers is best appreciated by looking past the suggested Popperian solutions to the problems that Lakatos was seeking to address. Selecting the sequence of attempts to formulate precisely and to prove the Euler-Cauchy conjecture about the relation among the faces, edges, and vertices of polyhedra as his historical example, Lakatos posed such questions as: How do mathematical definitions get revised? How are methods of proof modified? Are there methodological rules that mathematicians follow in pursuing these changes? If so, what are they? Now we take it that questions like these are important both for historians of mathematics and, perhaps, for practising mathematicians. Any serious investigation of the history of mathematics must embody some ideas about how the subject proceeds when it is done properly, and Lakatos's questions promise to move historiography beyond a set of tacit ideas, distilled from a (possibly incompatible) collection of sources, toward the explicit formulation of methodological principles. If we had such a canon, then historians could use it to investigate the match between actual history and the ideal, perhaps finding intriguing cases in which some extrinsic factor prompted a departure from the recommendations of methodology. Moreover, mathematicians might find it illuminating both to see how their chosen field of investigation had emerged from the mathematics of the past and how certain kinds of methodological considerations were paramount in fashioning its central concepts. It is not even out of the question that the answers to Lakatos's questions might help illuminate disputes among mathematicians about the legitimacy of various approaches or the significance of certain ideas.

Although Lakatos's discussion was the first extended philosophical treatment of methodological issues in mathematics, there were similar themes in the earlier work of Wilder, who subsequently developed his approach further (1975). Steiner (1975) and Putnam (1975) argued for the importance of nondeductive arguments in mathematical *justification* (as opposed to mathematical discovery, which had been explored with great thoroughness and elegance by Polya [1954]). In subsequent work, Steiner has discussed the character of mathematical explanation (1978) and has posed again Wigner's famous question about the "unreasonable effectiveness" of mathematics in the scientific investigation of the world.

Grosholz (1980, 1985) has investigated the manner in which new mathematical fields emerge from the synthesis of prior disciplines. The general task of articulating an account of mathematical methodology, begun by Lakatos, has been tackled by writers from several different perspectives (see, for example, Hallett 1979; Kitcher 1983).

The work that we have mentioned focuses on a few episodes in the history of mathematics and on a scattering of fairly elementary examples from contemporary mathematics. Many mathematicians are inclined to take this as symptomatic of the inevitable irrelevance of the general approach, rather than as a side effect of the fact that responsible studies begin with examples that are best suited for developing the central ideas of the new field. We hope that the enterprise of articulating the methodology of mathematics will attract scholars who are able to investigate examples from all phases of the development of mathematics, including the most exciting current contributions, and that, as this is done, the skepticism of parts of the mathematical community will be overcome.

The current state of the philosophy of mathematics thus reveals two general programs, one that is continuous with Frege's pioneering endeavors, and which conceives of the philosophy of mathematics as centered on the problem of the foundations of mathematics, and another that is new and less well developed, and which takes the central problem to be that of articulating the methodology of mathematics. It is natural to wonder whether the two programs are compatible. Both Lakatos and Kitcher proceed to the enterprise of exploring the methodology of mathematics after arguing that there is no a priori foundation for mathematics. Parsons (1986) has questioned the idea that there is a genuine inconsistency between the search for a priori foundations and the contention that there is a serious problem of the growth of mathematical knowledge. Moreover, the work of some writers who have pondered methodological questions (Steiner 1975, 1983; Maddy 1982) seems to presuppose that the two programs are ultimately compatible.

The present volume brings together contributors to the Fregean tradition and enthusiasts for new departures. We shall now take a look at the recent history of the history of mathematics, before offering our views about how exactly the spectrum of historical and philosophical approaches is distributed among the essays.

2. History of Modern Mathematics: A Brief and Biased History

We have seen that contemporary philosophy of mathematics has grown out of two traditions: one studying the foundations of mathematics in the analytic style prevalent in twentieth-century philosophy, and the other a recent approach drawing on both history and philosophy to investigate the methodology of mathematics. Contemporary history of mathematics also has two flourishing traditions. One is an older tradition, affiliated with professional mathematics and mathematics education, that studies the history of great men and ideas from their published papers. The other tradition, affiliated most closely with postwar professional history of science, focuses on conceptual and social issues and employs a wide range of source materials.

One of our objectives is to identify trends in the history and philosophy of modern mathematics since the last assessment of the field in 1974 at the American Academy of Arts and Sciences Conference on the Evolution of Modern Mathematics (see the November 1975 issue of *Historia Mathematica* for details). The "great ideas" tradition has flourished for hundreds of years (see Jayawardene 1983) with no recent signs of change. We only hastily recount its history before turning our attention to newer trends that have been stimulated in great part by the historical community. This focus on recent trends may seem to disparage the continuing contributions made in the old style of mathematics history, although it is not our intention to do so. Two other caveats should be kept in mind. First, our attention is restricted to the history of modern mathematics; thus, we do not consider the ample fine scholarship of mathematics before the nineteenth century. Second, the reader may detect a bias toward English language literature and to research conducted by mathematicians and historians of the United States. This shortcoming is a simple reflection of our knowledge of the state of the field.

Although historians of mathematics have traced their discipline back to antiquity, our interest is in more recent historical work. The first important modern turn came with the Göttingen encyclopedia historian Kastner, who introduced a higher standard of scholarship at the end of the eighteenth century. In the nineteenth century a number of editions of manuscripts, bibliographies, and general histories of mathematics were published in Europe, especially in Germany (e.g., Cantor 1880-1908; Todhunter 1861).

In the first two decades of the twentieth century, science educators gained an appreciation for the value of the history of science in teaching science because of the human dimension it gives to science as well as its demonstration of the importance of science in western civilization. This recognition stimulated the preparation of elementary histories of the sciences and shorter historical articles useful to undergraduate teachers of science. Smith (1906) was among the first to produce these materials for mathematics. It is evident from the number and duration of these publications that history has proved to be an effective tool in introducing students to the culture of mathematics. General histories by Cajori (1968), Smith (1923-25), Archibald (1932), Bell (1940), Boyer (1968), and Eves (1976), as well as the journal *Scripta Mathematica*, founded by Archibald, Smith, and others in 1932 for the philosophical, historical, and expository study of mathematics, all had this educational purpose in mind.

This pedagogic movement stimulated more scholarly research. The results are seen in the historical dissertations written in the 1920s and 1930s at Columbia under Smith and at Michigan under Louis Karpinski, the increased production of historical articles, and the new bibliographies (e.g., Loria 1946) and source books (e.g., Smith 1929) that appeared.

Meanwhile, the mathematical community came to appreciate the value of technical historical surveys of mathematics to their research—both to reappropriate methods and facts lost over time and to understand the origins of research areas. The work that established history's research value beyond any doubt was the *Encyklopädie der mathematischen Wissenschaften* (1898-1935), in which Felix Klein and other distinguished mathematicians attempted a comprehensive review of mathematics as it existed at the turn of the century. Many other historical studies (e.g., Dickson 1919-23) and volumes of collected papers were prepared by and for mathematicians between the world wars. After 1945 these internalist histories appeared at an accelerated rate.

The seeds of a new tradition in history of mathematics were sewn in the 1930s by George Sarton through his efforts to professionalize history of science in the United States. Sarton's own contributions to the history of mathematics (Sarton 1936) are best viewed as a continuation and reinforcement of the older tradition. Indeed, the orientation of historians in the 1930s and 1940s was decidedly internalist. Their work focused on the content of science, and they were skeptical of history of science or mathematics conducted by those without advanced scientific training. This led to a natural alliance between the fledgling history of science and the scientific disciplines and to a unified approach that "explored the filiation and intellectual contexts of successful ideas" (Thackray 1983, 17).

In the 1950s the first professional historians of science entered the universities, bringing with them an increase in the number and sophistication of historical studies of science. Most of these studies were internalist, but a separate "externalist" tradition developed, inspired by the work of Robert Merton. The externalist studies traced the social structure of the scientific community and the relation of this community to the external world. Internalist and externalist approaches were seldom employed in the same study.

In this period the rise of the history of science profession had little impact on the history of modern mathematics, except perhaps to publicize the importance and legitimacy of historical study and to stimulate the mathematical community to produce greater numbers of collected works, monographs, and smaller studies. Modern mathematics received almost no attention from historians of science in this period for at least two reasons. First, the number of practitioners was small and they tended to be scientific generalists, or at least to focus on a wide range of scientific activities in a single chronological period. Mathematics, especially after the mid-eighteenth century, was considered to have a content and method distinct from the other sciences, and even today this belief creates a gulf between historians of modern mathematics and historians of other modern sciences. Second, following Sarton and Merton, early historians of science were attracted to the great revolutions in science—particularly to the Scientific Revolution. Thus, the little energy of early historians of science for mathematics was consumed in the study of topics from the sixteenth through the early eighteenth centuries, particularly the rise of algebra, the beginnings of analytic geometry, and, most of all, the calculus.

In the 1960s and early 1970s the history of science profession grew at a rapid pace, stimulated in part by the substantial government support to science and its cultural study. As the number of practitioners increased, subspecialties in histories of the individual sciences emerged. History of science programs—Harvard and Wisconsin in particular—produced their first historians of modern mathematics: Michael Crowe, Joseph Dauben, Judith Grabiner, Thomas Hawkins, Uta Merzbach, and Helena Pycior. Since then, a small but continuous stream of young historians with training in both history of science and mathematics has entered the field.

The line drawn between internal and external history of science was too tenuous to resist fading. In the 1970s and 1980s, historians of science have balanced their internalist studies with social studies and have advanced a more sophisticated historiography incorporating both internal and external factors. These studies increasingly consider social factors when examining not only the institutions of science, but also the form and content of scientific ideas.

Social histories of modern mathematics are relatively uncommon, probably because in comparison with other sciences mathematics is regarded as least affected by factors beyond its intellectual content. Yet mathematicians have long recognized the importance of communities such as those in Göttingen, Paris, Berlin, and Cambridge in sponsoring particular styles of research and producing certain kinds of research mathematicians; a number of studies of institutions, their educational programs (Biermann 1973), and the individuals who shaped them (Reid 1970, 1976) have appeared.

For Sarton, the study of national science made no sense because history of science was the study of scientific ideas, which knew no national boundaries. But mathematicians have long understood that there are national differences, both in the subjects studied and the way in which they are approached. One famous example is the geometric approach to calculus in vogue in eighteenth- and early-nineteenth-century Britain in contrast to the analytic approach of Leibniz favored on the Continent. But contrasts can be drawn in nineteenth-century mathematics, as well, for example, between German and British approaches to applied mathematics, between the Italian and French work in projective geometry and the German work in transformational geometry, and in the German dominance of the arithmetization of analysis. The appreciation of national styles of mathematics has resulted in a few studies, for example of the introduction of Continental methods into British analysis (Enros 1979). American mathematics has come under close scrutiny, partly through the interests of American mathematical societies and further encouraged by the nation's bicentennial celebration in 1976 (Tarwater 1977). In the 1980s, American mathematics has become an active area of research.

Although mathematicians and historians have come to understand the value of studying professional societies, journals, prizes, institutions, funding agencies, and curricula, they have considerably less appreciation for the study of the social roots of the form and content of mathematics. This is evidence of the firmly seated belief that mathematicians but not their ideas may be affected by external factors. This attitude is slowly beginning to change, as Daston (this volume) and others are able to demonstrate the interplay between social factors and mathematical ideas. Another group assaulting this belief are the feminist historians (J. LePage, E. Fee, E. F. Keller), who variously are trying to establish cognitive differences between male and female mathematicians and to explain why mathematics has long been considered a male vocation.

Historical studies conducted before the 1960s sometimes projected contemporary standards of proof, rigor, problem definition, and discipline boundary onto their mathematical subject. The effect was to "explain" the subject in anachronistic concepts and terminology, to select topics for study only insofar as they had a connection to more recent developments, and to praise or damn these efforts on the basis of whether they anticipated (took a step toward) the current state of mathematical knowledge. Thus, the focus was on the great "successes" and sometimes the "blunders" of the greatest mathematicians. To accomplish this they studied the great men (there were almost no women mathematicians) without considering the lesser, but able practitioners that comprised the wider mathematical community. These studies of the great ideas were accompanied by anecdotal biographies of the great men, in the worst cases amounting to no more than hagiographic tributes. These authors relied principally upon their mathematical acumen and personal experiences to evaluate the published corpus of the great mathematicians, instead of examining a wider range of published and unpublished documents.

One historian of science has noticed a similar trend in historical writing about the sciences (Thackray 1983, 33):

> Discipline history by scientists has usually been based on an individualistic epistemology, in keeping with the image of the scientist as one voyaging through strange seas of thought, alone. There are also individualistic property relations in science, giving importance to the adjudication of rival claims. One result has been an historical interest in questions of priority—of who first exposed "error" and established "right" answers, or who developed successful instruments and techniques.

With the advent of a professional history of science, a new and more sophisticated historiography has arisen and is being put into practice in the history of mathematics. This historiography measures events of the past against the standards of their time, not against the mathematical practices of today. The focus is on understanding the thought of the period, independent of whether it is right or wrong by today's account. The historiography is more philosophically sensitive in its understanding of the nature of mathematical truth and rigor, and it recognizes that these concepts have not remained invariant over time. This new historiography requires an investigation of a richer body of published and unpublished sources. It does not focus so exclusively on the great mathematicians of an era, but considers the work produced by the journeymen of mathematics and related scientific disciplines. It also investigates the social roots of mathematics: the research programs of institutions and nations; the impact of mathematical patronage; professionalization through societies, journals, education, and employment; and how these and other social factors shape the form and content of mathematical ideas.

The new historiography has not been universally adopted. Historical works of the older style continue to be written by mathematicians and some historians. Perhaps because of the perceived differences in method and content between mathematics and the other sciences and because of the slow rate at which the history of science profession has produced scholars interested in modern mathematics, historiographic change has been relatively slow in coming. However, one area of considerable activity is the preservation and use of archival materials. The mathematics community has a long tradition of publishing collected papers of eminent mathematicians. This tradition continues today, but attention is also being given to the publication of collections of unpublished manuscripts and correspondence such as those of Wiener, Gödel, and Russell that are now in production. Many fine European archival collections relating to nineteenth-century mathematics (e.g., at Institute Mittag-Leffler, the Berlin Akademie der Wissenschaften der DDR, the West Berlin Staatsbibliothek Preussischen Kulturbesitz, and others in Cambridge, Freiburg, and Göttingen) have been in existence for many years, but they have received little attention over the years. Recently, historians have used these materials to great effect in preparing new interpretations and more accurate accounts of classic events—for example, Dauben (1979) on Cantor, Moore (1982) on the set-theoretic paradoxes, and Hawkins (1984) on the *Erlanger Programm*. Others have used these sources to pioneer new areas—for example, the work of Cooke (1984), Koblitz (1983), and Kochina (1981) on Kovalevskaya.

Institutions have been founded in the last decade with a major objective of collecting and preserving important archival materials on mathematics. These include the Archives of American Mathematics at the Humanities Research Center of the University of Texas, the Charles Babbage Institute for the History of Information Processing Archives at the University of Minnesota, the Bertrand Russell Archives at McMaster University, and the Contemporary Scientific Archives Center at Oxford University. For more information on archival resources in repositories in the United States, see Merzbach (1985).

The new professionalism in history of mathematics is reflected in the formation and growth of specialist societies and journals in the 1970s and 1980s. These include the Canadian and British societies for the history of mathematics and the journals *Historia Mathematica*, *Archive for History of Exact Science*, *History and Philosophy of Logic*, *Annals of the History of Computing*, *Mathematical Intelligencer*, and *Bolletino di storia della scienze matematiche*. Joseph Dauben, Ivor Grattan-Guinness, and Kenneth May have made noteworthy contributions to professionalization.

The professionalization of the history of mathematics has stimulated the production of a rich set of publications. The remainder of this section presents a brief survey of the literature of the 1970s and 1980s. For reasons of space and manageability, the survey is restricted to full-length studies. This does not do justice to the contributions of some scholars, like Thomas Hawkins or Helena Pycior, who contribute primarily through journal articles. For a fuller discussion of both book and journal literature see Grattan-Guinness (1977), Jayawardene (1983), and Dauben (1985).

Historians of mathematics have long appreciated the value of bibliographies. Dauben (1985, p. xxii) lists nine bibliographies produced in the nineteenth century, and his list does not include important ones appearing in the journal literature, notably the bibliographies produced between 1877 and 1900 by Moritz Cantor in *Zeitschrift für Mathematik und Physik*. In the twentieth century there have been four major bibliographies of the history of mathematics: Sarton (1936), Loria (1946), May (1973), and Dauben (1985). The two of greatest research value today are the last two. May's bibliography is intended to be comprehensive, and therefore lists everything of which he knew, whereas Dauben's is selective, critical, and annotated. Sarton and May in particular saw their bibliographic work as important to discipline building, as a way to define the field and to guide further research.

Mathematicians have also long been interested in general histories of their field. May (1973) lists approximately 150 general histories written between 1742 and 1968, and Dauben (1985) lists 29 such works of current value. Some (e.g., Struik 1967) provide a synoptic overview suitable for introducing students to the field, whereas others (e.g., Cantor, 4 vols., 1880-1908) with more comprehensive coverage serve as reference works to the mathematically educated. These two needs have been met in recent years by Boyer (1968) and Kline (1972), respectively. Kline's work is particularly impressive, generally accurate and far in advance of its predecessors in interpretation. There is growing opportunity, however, for a new general history of mathematics able to synthesize the more detailed, focused studies being produced today. Others (Bourbaki 1974, Dieudonné 1978) have written general histories of mathematics intended to advance their position on the "correct" approach to mathematics.

Sourcebooks of mathematics have been published throughout the twentieth century. Their function is to unite in one volume primary source material on a single topic, so as to make these materials more widely available and easier to compare. Sourcebooks have found frequent use in the classroom, and it is no surprise to learn that the first ones appeared (Smith 1929) at the time when it was first recognized that history can be an effective tool in introducing students to the culture of science and mathematics. Nor is it surprising that the number of sourcebooks to appear has increased in proportion to the professionalization of the history of mathematics. Some of these recent sourcebooks (Calinger 1982) are intended primarily for educational use, whereas others (van Heijenoort 1967; Birkhoff 1973) fulfill an additional need of the practicing mathematician for access to classic papers.

Since the late nineteenth century, mathematicians have regularly paid tribute to the most eminent members of their profession by publishing their collected works or selected editions of their papers. These volumes are not merely honorific, for they serve a useful research function. Dauben (1985) has identified about 300 mathematicians active before World War I whose collected works have been published since 1880. A survey of a subset of Dauben's list consisting of mathematicians from the nineteenth or twentieth century indicates steady publication of their collected works over the last ten decades, with little activity in the 1940s (because of the war, presumably) and a slight increase in the 1960s and 1970s. Volumes published in recent years seem to be more sensitive to historical concerns, for example in being less likely to introduce anachronistic modernization in notation. In fact, many of the recent collections have simply photo-reproduced the articles as they originally appeared in print. In the last ten years a number of the older of these collected works have been reprinted, demonstrating their enduring value. Although collected works are primarily published to meet mathematical research needs—at least in the case of modern mathematicians, whose collected works are published in far greater (absolute) numbers than those of "mathematicians" of the sixteenth through eighteenth centuries—they also serve the historical community. Thus the number published and kept in print is likely to increase.

Dauben (1985) also identifies about 75 mathematicians (active before World War I) whose correspondence has been published. Of these a disproportionately high percentage (in comparison to the number of mathematical practitioners at different periods of time) flourished prior to the nineteenth century, perhaps indicating the unavailability of more recent correspondence. It may also result from a lessening in the number and quality of letters over the last century with the improved opportunities for mathematical communication through journals and professional meetings, easier travel, and more recently the widespread use of the telephone and the computer. Or perhaps an explanation is provided by the fact that early mathematicians were often involved in a range of scientific activities, that history of science (especially of the Scientific Revolution) matured more rapidly than history of modern mathematics, and that historians place greater value on correspondence than scientists do. If the history of modern mathematics follows the trend in the history of science, the number of volumes of collected correspondence will grow and will have increasingly sophisticated annotation and analysis.

Biographies of eminent mathematicians have been popular among mathematicians and mathematics educators since early in the century, when history was first perceived as an effective introduction to the culture of mathematics. Before that time, mathematical biographies had appeared only occasionally, often written by younger contemporaries of the biographical subject, such as Koenigsberger (1904) on Jacobi. Although full-length biographies were written in the early twentieth century, most biographical writings were semipopular sketches emphasizing the personal and the anecdotal. Some of these were very popular, serving pedagogic function and also offering "intellectual challenge, inspiration, and recreation to mature practitioners" (Thackray 1983, 39). But as the field has matured, these works have outlived their historical utility. One immensely popular example (Bell 1937) is castigated by historians today for its gross historical inaccuracies.

In recent years many full-length biographies have appeared. Biographical subjects since 1970 include Babbage (Hyman 1982), Cantor (Dauben 1979), Lazare Carnot (Gillispie 1979), Courant (Reid 1976), Dedekind (Dugac 1976), Fisher (Box 1978), Fourier (Grattan-Guinness and Ravetz 1972), Gauss (Hall 1970; Wussing 1974), Hamilton (Hankins 1980), Hilbert (Reid 1970), Kovalevskaya (Cooke 1984; Koblitz 1983; Kochina 1981), Neyman (Reid 1982), Turing (Hodges 1983), and Ulam (Ulam 1976). Some of these (Reid 1970, 1976, 1982; Ulam 1976) are semipopular and continue the older biographical tradition. Others (Dauben 1979, Gillispie 1979, Hankins 1980) have employed a rich set of source materials to examine in detail the mathematical contributions of their subject in his or her social and intellectual context. There is considerable variability in the extent to which these works emphasize the mathematics (Dugac 1976; Cooke 1984) or the personalities and social context (Reid 1970; Koblitz 1983). Even the semipopular biographies are historiographically mature in comparison to biographies written earlier in the century. Constance Reid, for example, is attuned to institutional context in her studies of Courant, Hilbert, and Neyman. And as one historian (Thackray 1983, 40) has noted, biographies result in "a fresh awareness of the subtle, elusive quality of the [scientists'] ideas and of the persistence and intricacy of their patterns of thought. That awareness has done much to challenge stereotypes of science as impersonal, value-free inquiry." Popular and scholarly biographies are likely to appear in increasing numbers in the coming decades.

In the last fifteen years, mathematicians and historians have begun to publish monographs on the historical development of subdisciplines within mathematics. These studies either survey the history of a subdiscipline from its modern roots in the nineteenth century, or they examine in detail some particular problem or episode and demonstrate how it contributed to subdiscipline formation. Three studies are models of this kind of research because of their mathematical and historical acumen: Moore (1982) on Zermelo and the origins of axiomatic set theory, Hawkins (1970) on measure and integration theory, and Wussing (1969) on the abstract group concept. Others include Mehrtens (1979) and Novy (1973) on algebra;

Dugac (1980), Grattan-Guinness (1970), Goldstine (1980), Grabiner (1981), Medvedev (1976), and Monna (1975) on analysis; Bashe (1985), Goldstine (1972, 1977), and Williams (1985) on computing; Dieudonné (1974), Gray (1979), Pont (1974), and Scholz (1980) on geometry and topology; Biggs, Lloyd, and Wilson (1976) on graph theory; Edwards (1977) and Weil (1975, 1984) on number theory; and Maistrov (1974) on probability. As this list indicates, analysis has drawn by far the greatest attention. Applied mathematics, non-Euclidean and projective geometry, operations research, probability, and statistics have received little attention. Undoubtedly, the number of historical studies of subdisciplines will multiply in the coming years. Authors will come increasingly to appreciate the importance of examining correspondence, unpublished manuscripts, records of professional societies, and the work of lesser mathematicians in understanding the contributions of the leading mathematicians. These writers will also be more historically sensitive than their predecessors to projection of modern standards of notation, rigor, problem definition, and disciplinary boundaries onto the mathematics of the past.

At least mathematicians who participated in the founding of academic programs, professional organizations, or journals have long appreciated the impact of institutions on mathematics. A good example is R. C. Archibald, not only a leading figure in the rise of mathematics in the United States between the two wars, but also a devoted student of its history. His 1938 semicentennial history of the American Mathematical Society provides useful information on the society's financial affairs, programmatic activities, and key personnel. In the last fifteen years, interest in mathematical institutions has increased among both mathematicians and historians. Biermann (1973) has produced a fine study of the Berlin school of mathematics. Enros (1979) has traced the formation and effect of the Cambridge Analytic Society. Kuratowski (1980) has provided us with reminiscences of Polish mathematical figures and institutions between the two wars.

Stimulated by the interest of such senior American mathematicians as Birkhoff, Bochner, Browder, Halmos, MacLane, Stone, and Tucker and by the United States bicentennial celebration in 1976, there has been heightened interest during the last decade in the history of American mathematics (Tarwater 1977; May 1972). Historians and mathematicians including Aspray, Cooke, Grabiner, Merzbach, Reingold, Rickey, and Rider are continuing investigation of this history, and many additional studies should appear in the coming decade.

Outside of histories of specific institutions, little has been written on the social context of mathematics. Fang and Takayama (1975) present a survey of methods and theory in the social history of mathematics. Mehrtens, Bos, and Schneider (1981) have edited a useful volume of conference proceedings on the social history of nineteenth-century mathematics, focusing mainly on professionalization and education. Interest in women in mathematics has resulted in several volumes of biographical sketches of women mathematicians (Osen 1974; Perl 1978), as well as a recent industry producing full-length biographies of famous women in mathematics, including Ada Lovelace (Stein 1985), Germain (Bucciarelli and Dworsky 1980), Kovalevskaya (Cooke 1984; Kochina 1981; Koblitz 1983), and Noether (Brewer and Smith 1981; Dick 1981). A number of other, more sociologically oriented studies of women in mathematics are now underway.

Some of the most effective recent work in the history of science has brought to bear both internal and social factors in a unified historical analysis. MacKenzie's study (1981) of British statistics and Daston's article in this volume point the way to this kind of integrated historical study of the history of modern mathematics.

3. The Essays

We have divided the essays in this volume into four sections with the aim of bringing together papers that address similar topics or share common themes or approaches. The first section consists of three studies that tackle the traditional area of concern to philosophers of mathematics—logic and the foundations of mathematics. The second contains essays that articulate the historian's enterprise in different ways, either by displaying the structure of some particular episode of the history of modern mathematics or by using a historical example to comment on how that enterprise should be conducted. In the third section, historians and philosophers of mathematics explore the two fields in attempts to find illumination of one by the other. Finally, the fourth section comprises two studies of the interactions between mathematics and the broader social context.

In "Poincaré Against the Logicists," Warren Goldfarb considers Poincaré's criticisms of the logicist program. On Goldfarb's account, Poincaré was not primarily guilty of missing the point of the work of Frege, Russell, Couturat, and others. His objections were founded in a quite different conception of the philosophy of mathematics. The root of the difference is Poincaré's refusal to emancipate logic from psychology in the way for which Frege had campaigned.

Poincaré criticized the logicist definitions of the numerals on the grounds that they were ultimately circular, and he contended that the proper resolution of the set-theoretic paradoxes should proceed by honoring the vicious circle principle. Goldfarb argues that the former criticism is not an "elementary logical blunder," but the product of Poincaré's insistence that legitimate definitions must trace the obscure to the clear, where the notions of clarity and obscurity are understood psychologically. Poincaré rejected the framework elaborated by Frege, within which what we actually think of in connection with a given mathematical notion becomes irrelevant and all that is pertinent is the logical issue of what concepts are rationally presupposed. Similarly, Goldfarb contends that the full force of Poincaré's vicious circle principle and the notion of predicativity to which it gives rise can only be appreciated by recognizing Poincaré's concerns about the mutability of mathematical definitions. Here again, he was refusing to adopt a central principle of the logicist view, that "logic applies to a realm of fixed content."

Ultimately, then, the difference between Poincaré and his opponents comes down to a deep divergence in agendas for the philosophy of mathematics. Where Frege and later logicists saw the task of finding foundations as one of showing how mathematics results from the most general conditions on rational thought, Poincaré saw mathematics as the product of natural objects—human beings—so that the task of finding foundations is intimately linked to bringing clarity (judged by the standards appropriate for such beings) to areas that are currently obscure (again, judged by the standards appropriate for such beings). As Goldfarb hints, this contrast between Poincaré and the defenders of the logicist program is not only useful for throwing into relief the central tenets of logicism, but it also enables us to see interesting parallels between the early criticisms of logicism and contemporary naturalistic approaches to the philosophy of mathematics.

Michael Friedman also explores an episode from the history of logicism. "Logical Truth and Analyticity in Carnap's *Logical Syntax of Language*" illuminates the differences among different phases of the logicist program by seeing Carnap's endeavors in *Syntax* as an ingenious—but unsuccessful—attempt to find an intermediate position between the early logicism of Frege and the later conventionalism that would come under fire from

Quine. On Friedman's interpretation, the Fregean influence on Carnap runs very deep. Although it may appear that Carnap preserved little of any significance from Frege, and though the claim to reconcile Frege and Hilbert may seem spurious, Friedman argues that Carnap's own view of his connections to the tradition was correct.

Like Goldfarb, Friedman sees the Fregean enterprise as an attempt to show how mathematics is "built in to the most general conditions of thought itself." Carnap endorsed Wittgenstein's interpretation of Frege's view of logic: logic sets forth the conditions on rational thinking by elaborating those features that make any system of representation possible. But he transformed the Wittgensteinian insight, suggesting that logic can be formulated exactly as "the syntax either of some particular language or of languages in general." The disjunction prepares us for Carnap's new version of logicism. Recognizing that the principles that emerge from the syntax of "languages in general" will not suffice to generate classical mathematics, Carnap proposed to relativize logic. Thus, he offered the Principle of Tolerance, designed to permit freedom of choice with respect to languages and syntax—and thus freedom of choice with respect to logic. The task of foundations of mathematics remains in its Fregean form, for we are to show how mathematics is "built in" to the structure of thought and language, but the new relativization is supposed to accommodate the insights and achievements of Hilbert and Gödel.

Friedman goes on to argue that the new program was unstable. Carnap's sensitivity to the Gödel results forced him to complicate his account of analyticity. Indeed, Friedman claims, the full import of the Gödel theorems, appreciated only belatedly by Carnap, prevented him from characterizing analyticity along the lines favored by Frege and the early Wittgenstein, so that Carnap was pushed toward the *non*-Fregean construal of analyticity as truth-in-virtue-of-meaning that makes him vulnerable to Quinean criticisms. Hence, through its focus on an important transitional episode, Friedman's essay brings before us an interpretation of the entire history of logicism. The gap between Frege's original attempt to show how mathematics is built in to the general conditions of rational thought and the conventionalist idea that mathematical propositions are true in virtue of the concepts they contain is bridged by Carnap's heroic effort to honor Frege's view of foundations and yet discover a foundation for classical mathematics.

Gregory Moore considers the history of logic in the late nineteenth and early twentieth centuries from the different perspective of the historian. Moore's project is to investigate the way in which first-order logic became the canonical framework for the logical formulation of mathematics. His main thesis presents a striking irony: the canonization of first-order logic was achieved through the work of Skolem, specifically work that was designed to show the limitations on first-order formalism and to argue on this basis for the relativity of important mathematical concepts.

Moore reviews the history of modern formal logic from the mid-nineteenth century, beginning with Boole's algebra of logic. The connection with Boole is important for his argument, for part of the novelty of his story is the influence of the Boole-Peirce-Schröder tradition on Löwenheim (and, indirectly, on Skolem). Moore proceeds to examine the contributions of Peirce and Frege, emphasizing how different the Fregean picture of logic is from that which became accepted in the 1920s. As Moore notes, Frege did not separate the first-order part of his system from the rest, so that, in Frege's conception of the subject, there would have been no thought of responding to the difficulties with the notorious Basic Law V (the source of the Russell paradox) by banishing the second-order machinery from the province of logic. Instead, Moore proposes, the influence of Peirce and Schröder was essential for this separation to occur.

To see how this influence worked on Löwenheim and Skolem, it is necessary to achieve a clearer view of Schröder's accomplishments than has been available. Because we have seen Schröder through the lenses of Frege's criticisms, we have not understood either how he advanced the approach to logic favored by Boole and Peirce or how he influenced Hilbert. Moore seeks to correct the distortion. He also explains how Russell and Whitehead perpetuated the Fregean conception of logic, in which there is no place for any study of logic as a system or for any conception of a metalanguage. Thus Moore offers a picture of the state of logic in the first decades of the century: because of the influence of *Principia Mathematica*, Frege's conception of logic was dominant; nonetheless, there were indirect influences from the Peirce-Schröder tradition, through the work of Peano and Hilbert, and the work of Peirce and Schröder continued to be studied in its own right. (For lucid presentation of ideas that have some kinship with Moore's, see Goldfarb 1979.)

In the final sections of his essay, Moore argues in some detail that the important result for which Löwenheim is famous developed directly out of his research on Schröder's system of logic. He goes on to present

Skolem's further elaboration of Löwenheim's result, the explicit formulation of it as a theorem about first-order logic, and Skolem's connection of the resultant theorem with the relativity of notions of cardinality. A final, surprising, twist to the story is that even such talented mathematical logicians as von Neumann and Gödel were apparently unaware that the Skolem relativity thesis does not hold in second-order systems. Thus, in Moore's historical reconstruction, the naive idea that the canonization of first-order logic was inevitable once it was recognized that Frege's original system leads to paradox gives way to a complex and subtle account, in which now unfashionable historical traditions exert an influence and in which substantive philosophical doctrines have an important role to play.

Like Moore, Harold Edwards is concerned to alter our common vision of a historical episode. There is a familiar tale that reports the baneful influence of Kronecker on late-nineteenth-century mathematics. In this tale, Kronecker plays the part of the wicked and powerful gnome, whose evil schemes almost prevent the heroic prince (Cantor) from unlocking the gate of paradise (the transfinite). Fortunately, despite nervous breakdowns induced by Kronecker's machinations, despite his enforced banishment to Halle, the hero succeeds, and, in the light of the set-theoretic universe, the dark incantations of the villain are almost forgotten.

On Edwards's account, the usual historical assessment of Kronecker is almost as far from reality as most fairy stories. Edwards concedes that Kronecker's constructivist views have been a minority tradition in the history of the philosophy of mathematics. However, he finds no evidence for the allegations that Kronecker was personally hostile and aggressive, or for the charge that he was more guilty than other German professors of manipulating the social context of late-nineteenth-century mathematics to advance his own philosophical ideas. Edwards suggests that we should free ourselves from the stereotypes and consider Kronecker's work—both the actual mathematics that he produced and the philosophical viewpoints that motivated it—on its own merits. For the latter, we can find little that is explicit in Kronecker, except for a few scattered statements, and Edwards briefly suggests that we think of contemporary constructivists, such as Bishop, as pursuing Kronecker's main themes. On the other hand, Kronecker's mathematical papers are a treasure trove for historical investigation.

Edwards concedes that identifying the treasures is not easy. Kronecker's mathematical style is difficult. Edwards attempts to convey the ideas that he finds valuable by drawing a contrast between the work of Dedekind and Kronecker on divisibility in algebraic number theory. For Kronecker, it was important that the divisors be defined in such a way that the statements about them continue to hold under extensions or contractions of the field. Even more significantly, Kronecker's way of treating division was directed toward *computing* with divisors. Edwards claims that not only do these features mark a *difference* between Kronecker's approach and the familiar Dedekindian treatment in terms of ideals, but they constitute *advantages* in Kronecker's point of view. He concludes by speculating that a detailed exposition of the contributions of Kronecker may enable us to achieve a more sympathetic appreciation of constructivist ideas about mathematics.

Garrett Birkhoff and M. K. Bennett reevaluate the historical assessments of the influence of Felix Klein and his *Erlanger Progamm*. They view their work as a companion piece to a 1984 article by Thomas Hawkins that used a rich collection of published and unpublished sources to call into question the reception of Klein's work in the decades following its publication, as well as the authorship of the fundamental ideas. Relying mainly on published writings and letters by leading mathematicians (Lie, Engel, E. Cartan, and Weyl), and on their own familiarity with the role of continuous groups in geometry, Birkhoff and Bennett emphasize Klein's contributions in developing and disseminating group-theoretic concepts in geometry and geometric function theory.

Joseph Dauben shares with Edwards the concern that contemporary ideas and fashions may all too easily limit our appreciation of the content and development of past mathematics. Dauben chooses for his study an episode that has received great attention from historians and philosophers of mathematics. In Cauchy's *Cours d'Analyse*, there is a celebrated "theorem" to the effect that the sum of a convergent series of continuous functions is continuous. We know that the "theorem" is false, and historians have shown how the efforts to refine the theorem in the light of counterexamples gave rise, at the hands of Seidel and Weierstrass, to the modern distinction of convergence and uniform convergence.

Cauchy had an argument for his claim, a "proof" of the "theorem," and any satisfactory account of the development of concepts of convergence should explain how Cauchy's argument works, showing how a mathematician of his stature could have been led to advance the statements he did. Some recent developments in model theory and analysis allow us to consider a radical possibility. Perhaps Cauchy's "theorem" is actually *correct*, and it is we who are mistaken in interpreting it. Robinson's nonstandard analysis permits us to consider the possibility of nonstandard continua—continua in which Cauchy's result would hold and in which his argument would work. In a provocative essay, Lakatos proposed that this is indeed the case, and suggested that we view the transition from Cauchy to Weierstrass not in terms of the disambiguation of concepts of convergence but as the replacement of the nonstandard continuum with the standard one.

Dauben presents Lakatos's approach to the history of analysis, contrasting it with the more traditional attempts to diagnose the subtle fallacy in Cauchy's reasoning. Dauben argues that the Lakatosian reinterpretation cannot be sustained, and he uses this conclusion to explore the methodological constraints that a historian of mathematics should honor. The danger that he sees, one that has become the most commonplace for historians of science during the past thirty years, is that contemporary notions will be read back into the mathematics of the past, so that the history of mathematics will consist, not in a sequence of efforts to fathom the mathematics of our predecessors on its own terms, but in "discoveries" of "anticipations" of the latest interesting ideas. We believe that Dauben's resistance to this "Whig history" is salutary for the history of mathematics, and that it is of a piece with the attempts of Moore and Edwards to reclaim those figures of the past whose ideas seem at odds with contemporary fashions.

Dauben goes on to consider the influence of historical researches on Robinson's own presentation of his ideas. He points out that, despite the formal equivalence of nonstandard and standard analysis, Robinson was still able to contend that his approach enjoyed a special intuitive evidence and that he could base this claim on the history of analysis. Moreover, in opposition to those (like Bishop) who deny the meaningfulness of nonstandard analysis, Dauben describes the findings of studies that show how beginning students are able to solve calculus problems with greater skill if they are trained in the Robinsonian approach. Thus, like the great mathematicians of the eighteenth century, it seems that the neophytes of today can often make good use of infinitesimalist reasoning.

Richard Askey offers a perspective on the practice of history of mathematics that may at first appear to run counter to that taken by Dauben. Askey suggests that historians of mathematics ought to know a lot of mathematics—in fact, he hints, far more than they do. Unless they are trained beyond "the current undergraduate curriculum and first-year graduate courses," there is a danger that they will overlook those episodes in the history of mathematics that are really significant and concentrate on peripheral issues. However, Askey does not simply suggest that history of mathematics should be turned over to professional mathematicians, as a recreation in which they can indulge when they take themselves to be on the verge of their dotage. History has its own standards and methodological canons, and Askey, despite his keen interest in history, is quite modest about his knowledge of these. The heart of his paper is thus a plea for cooperation and development. Mathematicians can contribute "protohistory" (our term, not his) by drawing the attention of historians to problems and episodes that are mathematically significant. The mathematician, playing protohistorian, will not attempt any coherent treatment of these episodes. The goal will simply be to assemble "mathematical facts" whose normal form may be the attribution of a relation of kinship between the writings of a past mathematician and some (perhaps sophisticated) piece of contemporary mathematics. Once confronted with these suggestions of kinship, the historian must go to work, following the canons and standards of the discipline. However, part of Askey's message is that doing the work properly may involve a great deal of further study in mathematics.

Historians are likely to view protohistory as incomplete in two respects. The more obvious deficiency, touched on in the last paragraph, is that the mathematician's recognition of kinship needs to be scrutinized from the perspective of an understanding of the concepts and standards in force in the historical epoch under study. In addition, we should not assume that the most significant historical problems concern work that has any straightforward connection with (or offers any anticipation of) problems and methods of current interest. Askey seems to us tacitly to appreciate the point when he couches his discussion in terms of the writing of history that mathematicians will find interesting.

Askey illustrates his general proposal by recounting some of his own historical research on series identities. After contending that some of the historical attributions commonly made by professional mathematicians seem erroneous, Askey recounts the reactions of historians to his work. His tale seems clearly to be one of missed communication. The technical discussions of series identities and hypergeometric series are dismissed as not amounting to "real history." Nonetheless, we are sympathetic to Askey's contention that his research—and work like it—brings before historians things that they ought to know. Hence, we accept *half* of his general thesis: mathematicians can help historians of mathematics by assembling "mathematical facts," an activity that Askey explicitly recognizes as not the same as full history.

But this is only part of Askey's thesis, for he argues that historical investigations may be of value in the development of mathematics. Askey relates how ongoing work on the Bierberbach conjecture led to a problem about series identities. His own historical interests had led Askey to a perspective from which he could assist in the solution of this problem. Thus, the excavation of ideas from past mathematics may contribute to current research. Here we find a familiar theme, but one which Askey illustrates in a dramatic way. Perhaps there are hints of something similar in Edwards's suggestions for resurrecting the main ideas of Kronecker's number-theoretic work.

We believe that the juxtaposition of Dauben's essay with Askey's is especially happy, for both can be seen as warning against a particular sort of danger. To do adequate history of mathematics one must avoid both dangers, relinquishing both the chauvinism of the professional historian (as Dauben surely would) and the chauvinism of the professional mathematician (as Askey clearly does). If we may be allowed yet another variation on Kant's celebrated dictum: For the purposes of doing history of mathematics, knowledge of contemporary mathematics without historiographical sensitivity is empty, historiographical sensitivity without knowledge of contemporary mathematics is blind.

Lorraine Daston is also concerned to find ways in which the mathematics of the past can be treated accurately without introducing anachronistic distinctions. "Fitting Numbers to the World: The Case of Probability Theory" considers the contemporary distinction between pure and applied mathematics and its relation to the eighteenth-century idea of "mixed mathematics." Daston focuses her discussion by considering three branches of eighteenth- and early nineteenth-century probability theory, three fields of inquiry that we might naturally call applications of probability theory. She endeavors to show how differently the mathematicians who pursued these fields conceived of them when they placed them under the rubric of mixed mathematics.

Daston begins with a review of the historical roots of ideas about the relation of mathematics to the external world. She concludes that, for the mathematicians of the Enlightenment, "abstract" mathematics occupied one end "of a continuum along which mathematics was mixed with sensible properties in varying proportions." "Mixed" mathematics occupied much of the energy of eighteenth-century mathematicians, and yet, as Daston notes, there was a sense that, in introducing a greater "mixture" of sensible ideas, mathematicians ran the risk of error and "retrogression."

Daston's first illustration concerns the art of conjecture, and her discussion centers on one celebrated problem and its impact. The problem is the St. Petersburg paradox—a paradox in virtue of the fact that application of the art of conjecture to a contrived game offers a recommendation that is intuitively unacceptable. Daston argues that we can only see the St. Petersburg paradox as a paradox—that is, as the eighteenth-century discussants saw it—if we recognize the status of the art of conjecture as a piece of mixed mathematics. The ability of the problem to threaten the credentials of probability theory must strike us as absurd if we approach the situation from the perspective of our modern distinction between pure and applied mathematics. *We* can consign the puzzle to economics. Our eighteenth-century predecessors could not.

The second example concerns the use of probability in a legal context, the probability of judgments. Daston explains how the mathematical community set itself the task of deciding on the optimal design of a tribunal of judges, where the criterion for optimality consisted in minimizing the risk of error. The hope, bizarre as it now seems, was that, by treating judges as akin to dice, the practice of legal judgment could be "reduced to a calculus." Daston relates the fate of this discipline, showing how the understanding of legal reasoning became divorced from the calculus of probabilities, so that a branch of "mixed mathematics" came to be viewed as a faulty combination of an unassailable piece of pure mathematics (probability theory) and a misguided application.

Finally, she presents us with one of the success stories in the "application" of probability theory, the development of actuarial mathematics. Here Daston seeks to understand why the use of probability theory in insurance took so long to become established, why the early ventures in actuarial mathematics were undertaken with such extreme caution, why an antistatistical attitude was displaced so slowly. The kernel of her answer is that the phenomena to be discussed seemed insusceptible to proper mathematical analysis because it appeared that statistical treatment would blur subtle distinctions that experienced insurers would be able to use advantageously in their decisions.

Taken together, the three examples show how the eighteenth-century notion of mixed mathematics differed from our concept of applied mathematics, how the distinction of a pure discipline from its applications can be achieved, and how mathematical theories are sometimes dependent on the fates of their applications. It seems to us that Daston's essay raises interesting philosophical questions concerning the traditional topic of the relation of mathematics to reality and that her treatment of the examples she has chosen offers some fruitful suggestions for addressing those issues. Moreover, her study shows the effect of the social context on the development of a branch of mathematics. In this way, Daston touches on themes akin to those pursued by Grabiner and Aspray.

If Daston demonstrates how the detailed study of the history of mathematics can have significance for philosophical issues, then it seems to us that Howard Stein's essay shows how attention to philosophical questions can shed considerable light on episodes in the history of mathematics. Stein is explicitly concerned to trace the main foundational programs of the early twentieth century to mathematical roots in the nineteenth century. He begins from the thesis that mathematics underwent a "second birth" in the nineteenth century and that it is a primary task for philosophy to understand this transformation. After reviewing the early-nineteenth-century developments in algebra, analysis, and geometry, Stein identifies several "pivotal figures"—Dirichlet, Riemann and Dedekind—the latter two greatly influenced by Dirichlet's teaching. With the filiations to the early nineteenth century in place, he then begins a more detailed account of some late-nineteenth-century developments.

The account starts with a problem, a problem that occupies anyone who ponders Dedekind's dual status as a respected mathematician and as a figure in the "foundations of mathematics." Why did Dedekind write his monograph on the natural numbers (*Was sind und was sollen die Zahlen?*)? Stein's answer consists in a careful tracing of the connections between the project of this monograph and Dedekind's earlier work in number theory, work that shows the influence of both Gauss and Dirichlet. The account culminates in the contention that the famous supplement to Dirichlet's lectures in number theory not only served as a major source in the history of algebraic number theory, but was also the origin of some of Dedekind's deepest philosophical ideas. Stein uses this contention to provide an illuminating contrast between the project undertaken by Dedekind in his monograph and the famous enterprise that Frege began in *Begriffsschrift*.

After what he concedes may sound like a panegyric on Dedekind (and one of us agrees that panegyrics are not here misplaced), Stein turns to consider the work of Kronecker. After noting Kronecker's famous stringent requirements on mathematics, the insistence on constructivity, he points out that Kronecker excepted geometry from these requirements. Considering Kronecker's position in the light of Riemann's conception of geometry, Stein argues that the demands Kronecker made are ultimately unjustified. The concession granted to geometry ought to be allowed to other areas of mathematics as well.

Finally, Stein turns his attention to the work of Hilbert. Here he is concerned to recapture the insights of Hilbert's early foundational work—the work of the *Grundlagen der Geometrie*—and to view the later slogans about the meaninglessness of finitary mathematics as overstatements of a sensible attitude that was present in all of Hilbert's foundational thought. For Stein, Hilbert's fundamental point is that there is no formal requirement on mathematics beyond consistency, and the original, deep suggestion is that the consistency of systems is itself open to mathematical investigation. The results of Gödel and others thus constitute a "final irony" in the story of Hilbert's development.

Any brief summary inevitably misrepresents Stein's inquiry, for his major theme is the intricacy of the connections among early-twentieth-century foundational programs, long-standing philosophical issues, and achievements within nineteenth-century mathematics. The articulation of that theme offers a new view of the main traditions and problems in philosophy of mathematics. Michael Crowe's essay, "Ten Misconceptions about Mathematics and Its History," also proposes to revise commonly accepted views. But Crowe's strategy is more direct. He sees much historical writing as, tacitly or explicitly, adopting ideas about mathematics and its history that cannot be sustained on closer inspection. Instead of offering a detailed discussion of a single episode, Crowe argues by assembling counterexamples drawn from a variety of periods and subjects in the history of mathematics.

Traditional historiography is in the grip of a conception of mathematics as deductive, certain, and cumulative. Mathematics students, many mathematics teachers, and even some historians of mathematics think that mathematical statements are invariably correct, that the structure of mathematics accurately reflects its history, that mathematical proof is unproblematic, and that standards of rigor are unchanging. They also assume that the methodology of mathematics is radically different from the methodology of science, that mathematical claims admit of decisive falsification, and that the philosophical options are empiricism, formalism, intuitionism, and Platonism. Crowe's attack on this tenfold conception uses a number of recent philosophical sources, notably Lakatos, and ranges over examples from many branches of modern mathematics—algebra, geometry, analysis, vector algebra and analysis.

We believe that Crowe has issued a broad and clear challenge, and we think that there are three main lines of response. First, one may object to his treatment of the mathematical examples, holding that when the cases are elaborated more carefully, the theses that he is concerned to rebut can survive unscathed. Second, a critic might urge that the theses themselves cannot be so straightforwardly assessed and that considerable preliminary conceptual analysis is required before they can be confronted with historical analysis. (What does it mean to claim that mathematics is "certain" or that "standards of rigor" are immutable?) Finally, there is the rejoinder that Crowe has simply erected a straw man and that the actual practice of doing history of mathematics is free of the simplistic ideas that he castigates. Whatever the merits of any (or all) of these objections, we think that Crowe has raised interesting questions about the historiography of mathematics and that the theses that he has selected deserve to be analyzed and evaluated by philosophers and historians of mathematics.

As its title suggests, Felix Browder's "Mathematics and the Sciences" focuses on the relationship between mathematics and the natural sciences. Browder is concerned to illustrate a recent major trend in preconceptions about the most significant directions in mathematical research. He starts by reviewing a number of ways in which, in the last decade, developments in the natural sciences, most notably physics, have employed very sophisticated mathematical ideas. He continues by considering the relationship between mathematics and computer science, noting that those parts of mathematics that seem least applicable in physical modeling—to wit, algebraic number theory and mathematical logic—have been of enormous value in computer science. Browder suggests that the old distinction between "applicable" and "inapplicable" mathematics may be outmoded, and he opposes any attempt to separate natural science from the "artificial" sciences.

However, any picture of mathematics that ignores the autonomy of mathematical research appears to Browder to be misguided. Whether the solution of a mathematical problem is undertaken in the context of a physical theory or without any particular physical result in mind, those who tackle that problem are functioning as mathematicians. Thus Browder is led to consider historically influential attempts to specify the nature of mathematics and of mathematical problems. He goes on to note two different ways in which important ideas in recent mathematics have emerged from interactions between prior mathematics and the sciences. He concludes by relating these examples to the history of mathematics, arguing that the current dialogue between mathematics and the sciences is merely the latest exchange in a conversation that has been going on since the seventeenth century and that has been of value to all the disciplines that have been party to it.

Philip Kitcher's "Mathematical Naturalism" offers an agenda for the philosophy of mathematics that is distinct from the set of problems that have dominated the subject since Frege. Kitcher argues that the identification of philosophy of mathematics with the construction of a foundation for mathematics depends on a commitment to apriorism. The principles to which mathematics is to be reduced are supposed to be a priori, and, if they do not have this special epistemological status, then the reduction loses its point. Kitcher outlines some arguments for believing that an apriorist theory of mathematical knowledge will not succeed, and he recommends a naturalistic approach to mathematical knowledge, according to which the mathematics of one generation is built on the achievements of the previous generations. Within this framework, the problems that occupy center stage are those of understanding rational development of mathematics and of characterizing mathematical progress.

The later sections of Kitcher's essay are focused on these problems. Kitcher argues that there are various different notions of rationality that ought to be given a place in a history and philosophy of mathematics. He contrasts cases in which mathematical change is driven by factors external to the discipline and those in which the prior state of a branch of mathematics furnishes reasons for amending it in a particular way. On the basis of this distinction, he proposes that mathematical progress should be understood in terms of the advancement of ends that are ultimately external to mathematics. The connections between mathematics and practical concerns or the results of other sciences may, in some cases, be extremely remote, but, Kitcher contends, even the most "useless" parts of mathematics constitute objectively valuable accomplishments in virtue of the fact that they result from practical projects through a chain of rational transitions.

The position Kitcher sketches provides an alternative not only to apriorist epistemologies for mathematics but also to Platonist accounts of mathematical truth. Kitcher uses his treatment of the problems of mathematical rationality and mathematical progress to propose that truth in mathematics is what is achieved in the long run through the application of the principles that govern the rational development of mathematics. He points out that this proposal would have some radical consequences for the usual image of mathematics, and he concludes by suggesting some ways in which his proposals might be articulated in the history and philosophy of mathematics.

The last two essays are concerned with the interplay between mathematics and society. Judith Grabiner uses the recent controversies about the claims of artificial intelligence to illustrate a general pattern of change in the history of science. According to Grabiner, there are many episodes in the development of the sciences that go through the following phases: first, new ideas and methods are introduced; they prove successful in solving some outstanding set of problems and are taken up enthusiastically by their originators, who proclaim that vast insights are at hand; provoked by what they see as overambitious claims, detractors outside the discipline criticize the use of the new ideas and methods, finding fault even with the results originally accomplished; finally, there is a more thorough critique from within the scientific community.

Grabiner provides a number of examples of this general pattern. She discusses the methodological revolution in seventeenth-century science, the eighteenth-century "spirit of systems," the Industrial Revolution, the introduction of Darwinian ideas in mid-nineteenth-century biology. In each example she shows how the episode divides naturally into four phases. Thus, in the last case, the ideas of Darwin and Wallace achieved initial success in accounting for phenomena of biogeographical distribution, relationships among organisms past and present, adaptations, and so forth. In the hands of the social Darwinists and eugenicists, Darwinism was hailed as the key to solving all kinds of social problems. These enthusiastic extrapolations provoked continued criticism from outsiders, who objected even to the early achievements of Darwinian evolutionary theory. Finally, the more thorough internal critique from practicing biologists and social scientists trimmed away the excesses of social Darwinism while preserving the genuine biological successes.

Grabiner suggests that we are currently in the middle of the last stage in the debate about artificial intelligence. She outlines the early successes of computer science and relates the confidence with which its advocates—such as Simon, Newell, and McCarthy—predicted dramatic results both in constructing machines that would perform all kinds of intelligent tasks and in shedding light on the nature of human intelligence. Grabiner cites the philosophers Dreyfus and Searle as prominent representatives of the external critics, who announce the "triviality" of the entire venture. The role of internal critic is filled by Weizenbaum, who has attempted to chart the limits of artificial intelligence research instead of dismissing it entirely.

While Grabiner is concerned with the ways in which mathematical ideas (and scientific ideas) are elaborated, disseminated and criticized in the social context, William Aspray focuses on the impact of social structure on the development of mathematics. Aspray considers one of the most prominent success stories in the history of American mathematics, the development of mathematics at Princeton in the early decades of the twentieth century. He begins by giving a detailed account of the conditions under which professional mathematicians worked at the end of the nineteenth century. After showing how heavy teaching loads and few incentives to research were the order of the day, Aspray describes how Wilson's presidency at Princeton University initiated the building of a modern mathematics department.

A key figure in the building was Fine. Originally trained as a classicist, Fine had pursued his mathematical education in Germany and had become impressed with the high academic quality of the German universities. Returning to Princeton, first as a professor and later as dean, Fine devoted considerable energies to reorganizing the structure of appointments and the commitments to research. Veblen was also instrumental in the development of a research community in mathematics at the university, and Aspray traces the ways in which mathematical research was fostered by Fine and Veblen.

The implementation of the program would have been impossible without funding from a wealthy patron, and Fine was fortunate to secure support from a former classmate. One of the important consequences of the influx of money was the opportunity to construct a building for the mathematics department, Fine Hall, designed by Veblen to facilitate exchange of ideas among mathematicians. Aspray documents the significance of this physical facility in attracting promising young mathematicians either to come to Princeton or to return there. He also recounts the founding of the Institute for Advanced Study and shows how cooperation between the institute and the university further promoted Princeton as a center for mathematical research. The exodus of brilliant mathematicians from central Europe in the 1930s combined with the attractiveness of Princeton as a haven to create an extraordinary mathematical community.

Although Aspray does not exhibit in detail how the institutional factors he describes actually influenced the development of any particular mathematical field, his central message is abundantly clear. It is surely hard to believe that the ideas of the great mathematicians who were at Princeton in the 1930s were unaffected by their frequent exchanges with one another. Those exchanges were made possible by a number of historical contingencies: the drive of a university president, accidents in the education of a dean, the lucky business success of a classmate, an unwonted understanding of the importance of architecture, the emergence of an evil dictatorship. We do not ordinarily think of the course of mathematics as being affected by such chances. Aspray reminds us that they may easily leave their mark.

4. Common Themes and Possible Futures

We would like to conclude by pointing to some connections (and contrasts) among the essays and by offering some brief speculative comments about the possible future of history of mathematics, philosophy of mathematics, and history-and-philosophy of mathematics. Some common themes are already indicated in our division of the articles. Other connections crisscross the groupings we have imposed.

A. Reading the past through the categories of the present. Many of the writers are concerned to warn against the dangers of Whig history. This is especially obvious in the essays of Dauben and Daston, but it is implicit in the studies of Edwards, Moore, Crowe, and Kitcher as well. Askey's paper serves as an important reminder that sensitivity to the possibility that the conceptions of the past may not be those of the present needs to be tempered by an ability to pose questions about affinities between old and new ideas. We believe that Dauben, Daston, et al., have drawn an important lesson from the general history of science, a lesson that has often been neglected in the work of historians of mathematics, and that Askey's insights are complementary to their recommendations. Perhaps there may be some divergence of opinion or of emphasis implicit in the essay by Birkhoff and Bennett, which offers a more traditional exercise in the history of mathematics—to wit, the assembly of connections among the works of past mathematicians, conceived in the image of present mathematics. We believe that mathematicians will continue to find this kind of historical research interesting and illuminating.

B. Correcting common distortions of particular figures. Several essays are concerned to stress the fact that familiar ideas about some great figure or achievement of the past are quite inaccurate. Thus Edwards contends that we have a badly distorted picture of Kronecker, Moore argues that we do not understand Skolem's work, Goldfarb suggests that the debate between Poincaré and the logicists has been misunderstood, Friedman claims that Carnap's connections with Frege's logicism have been missed, and Stein proposes that we do not see how the foundational work of Frege, Hilbert, and Brouwer was connected to nineteenth-century mathematics. We see these essays as underscoring the need to be cautious in reading the past through the spectacles of the present.

C. Relating mathematics to the social context. Although only the essays by Grabiner and Aspray are explicitly devoted to studying the interactions between mathematics and social institutions, several other authors touch on this theme. Thus Daston considers the ways in which the enterprises of the eighteenth and nineteenth centuries shaped the development of probability theory, Browder examines the response of mathematics to extra-mathematical concerns, and Kitcher is concerned to emphasize the need to consider broader notions of rationality and to appreciate the effects of institutions on mathematical practice. None of the authors adopts the radical view that the evolution of mathematics is entirely driven by social forces, but those we have mentioned are sympathetic to the claim that the history of mathematics cannot be adequately written in a purely "internal" idiom. Thus historians of mathematics are beginning to turn to the appreciation of social factors, embracing the methodology that has affected general history of science in the past decade. Although we hold no brief for the extreme claims sometimes made on behalf of the sociology of knowledge, we believe that this is an extremely important trend that will illuminate many aspects of the historical development of mathematics.

D. Finding a foundation for mathematics. The question whether philosophy of mathematics should consist in identifying foundations for mathematics provokes one of the clearest divisions in the volume. Goldfarb and Friedman are both sympathetic to the Fregean project, and their subtle reconstructions of logicist ideas indicate how they believe that that project has been misunderstood. Crowe and Kitcher are, just as obviously, unsympathetic to the traditional view of philosophy as laying foundations. The character of the opposition is, in some ways, parallel to the debate between Poincaré and the logicists, and, as we have noted, Goldfarb hints at the relevance of his interpretation to current philosophical controversies.

E. The relation between history and philosophy of mathematics. Several of the contributors appear to envisage a fruitful relationship between historical and philosophical study of mathematics. However, we think it fair to say that there is no single view of that relationship. Moore employs the techniques of the historian to explore the genesis of a philosophically important thesis—that logic is first-order logic. Dauben uses ideas in contemporary philosophy of mathematics to illuminate the work of the past and, conversely, appeals to the history of mathematics to counter some current philosophical arguments. Daston argues at length that the historian cannot come to terms with significant features of past mathematics without philosophical analysis of major categories and distinctions. In complementary fashion, Stein contends that our understanding of contemporary programs in philosophy of mathematics will prove deficient if we do not see their roots in the history of mathematics. Crowe warns against a variety of historiographical errors that he takes to be the product of simplistic philosophical ideas about mathematics. Kitcher is also concerned to use philosophy to outline a positive historiography, and he adds the suggestion that historical research is needed to complete the philosophical project of understanding our mathematical knowledge.

If this is an adequate representation of where things stand in the present, what (if anything) can we predict about the future? In our judgment, it would be surprising if the historiographical sensitivities that many of the contributors want to stress (A) were to be forgotten in future research in history of mathematics. We suspect that there will continue to be revisions in our common understanding of those few mathematicians who have actually been studied by historians (B). We hope that the work of enhancing and deepening our appreciation of past mathematics will exemplify that cooperation between mathematicians and historians that Askey's essay identifies.

What of the interplay between mathematics and the broader social context? We think the essays of Grabiner and Aspray establish that it is sometimes necessary to locate factors in society that are efficacious in modifying the course of mathematics. Perhaps historians of mathematics will find, as general historians of science have already found, that the most illuminating studies integrate social factors with the forces recognized by those older historical traditions that focus on the "dynamics of ideas." In our judgment, Daston offers a vision of this unified approach. Kitcher's essay also offers a preliminary attempt to sketch a historiography appropriate to it.

The future course of philosophy of mathematics looks much less certain. We should note explicitly that the present collection represents only those trends in contemporary philosophy of mathematics that are concerned in some way with history. As we have emphasized in our biased history of the philosophy of mathematics, there are currently many different ways in which the post-Fregean tradition is being elaborated. Goldfarb and Friedman represent just one version of this tradition, a version that emphasizes the need for reanalyzing the philosophical roots of Anglo-American philosophy. Their contributions to the volume share the theme that the important Fregean enterprise is to show how mathematics is "built in" to the conditions of all rational thought.

Crowe, Browder, and, most explicitly, Kitcher argue for the maverick approach, originally pioneered by Lakatos, on which the project of securing foundations for mathematics is regarded as pointless. A major part of the argument is that a successful Fregean foundation for mathematics would trace mathematical theorems to axioms that have a special status (that are a priori in the sense that Kitcher ascribes to that notion). To settle the dispute between the two points of view, a number of issues need to be addressed: Is it impossible to find foundations that are a priori in this sense? Is this the sense of apriority that is crucial for the Fregean project? Would a demonstration that mathematics is reducible to axioms that are "built in" to the conditions of rationality be epistemologically significant? Is it reasonable to expect any such demonstration?

Stein's essay hints at a different image for the philosophy of mathematics, one that stresses the filiations both to the history of mathematics and to philosophical questions that have engaged thinkers since Plato. Thus, in our judgment, Stein offers a revision of the main twentieth-century tradition in philosophy of mathematics, but one that is less radical than that suggested by Lakatos, Crowe, Kitcher, et al. We think that it will be interesting to see which, if any, of the approaches we have mentioned prove influential in the philosophy of mathematics in the next decade.

Finally, what are the chances for an informative synthesis and for the development of history-and-philosophy of mathematics? We are encouraged by the number of contributors who have succeeded in bringing together historical and philosophical insights. Even if there is no uniform view of how history and philosophy should relate to one another (E), the essays that follow demonstrate that each field can help the other. To our minds, this is especially evident in the essays of Daston, Stein, and Moore, where, in very different ways, the authors have forged a genuine synthesis.

Although the communication between historians and philosophers, obvious both in the papers and in discussions at the conference, is exciting, we are less sanguine about the prospects for dialogue among historians, philosophers, and mathematicians. One disappointing feature of conference discussions was the tendency for some of the mathematicians present to dismiss the work of historians and philosophers as ignorant invasion of the mathematicians' professional turf. We believe that Askey's discussion in his essay captures what bothers these mathematicians, while formulating the point in a constructive way. Genuine progress is possible if all parties to the dialogue recognize that each of the others has professional expertise that the rest lack and that the point of the conversation is to remedy deficiencies, not to announce that one's own profession has all the answers.

It is evident that professional historians and philosophers of mathematics know less mathematics than professional mathematicians, and, even when the historian or philosopher has considerable "second-hand" knowledge, it is rare to find someone who has worked *creatively* in more than one of the disciplines. But typically the mathematician is far more ignorant of the practice and standards of history or philosophy than the historian or philosopher is ignorant of mathematics. Thus, while the mathematician may be bothered by the fact that the historian of number theory is not *au fait* with the advances of the 1960s, 1970s, and 1980s, historians and philosophers are just as irritated by crude precursor-hunting masquerading as history and by attempts at "philosophy" that consist in the mathematician's pet expositions of the parts of their subject that interest them most. We hope that it will help to make the point explicit so that more scholars will be led to pose questions of the kind that Askey takes for his title and to advance constructive suggestions of the sort presented in his essay.

In short, we counsel patience and the search for mutual understanding. The interdisciplinary study of mathematics has come a considerable distance since Birkhoff's pioneering attempt to bring historians and mathematicians together in 1974. We are optimistic that that study will continue to flourish and hope that the present collection of essays will advance that end.

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