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The Five Questions

1. A Road to Philosophy of Mathematics

l became interested in philosophy and mathematics at more or less the same time, rather late in high school; and my interest in the former certainly influenced my attitude towards the latter, leading me to ask what mathematics is really about at a fairly early stage. I don 't really remember how it was that I got interested in either subject. A very good math teacher came to my school when I was in 9th grade and I got caught up in his course on solid geometry; but he soon left and math then lost its luster again in the hands of teachers who neither liked nor understood it. Calculus wasn't taught in high school in those days, or at least not in mine: besides geometry we learned some algebra (how to solve some equations) and trigonometry (with, of course, very little proved). I doubt that even the *word* "philosophy" passed the lips of any of my teachers. My mother, who worked for a publishing house, brought home for me copies of, among other works, the Jowett translations of Plato's *Dialogues*, Will Durant's *Story of Philosophy* and Courant and Robbins' *What Is Mathematics?*; but I can't remember why she did that: She wasn't at all intellectual and, as far as I recall, my interests at the time were mostly confined to sports and girls—in some order. Maybe she just thought it was time for me to develop new interests.

After high school, I went in 1948 to Lehigh University, then at least primarily an engineering school, on an athletic scholarship (which I was lucky to get: I wasn't that good an athlete and there was a glut of more talented GI's returning to school). There I had the good fortune in my first year to have an introduction to philosophy course with Lewis White Beck. He had just moved there from the University of Delaware and shortly thereafter moved on to the University of Rochester, where he became one of the leading lights of American Kant studies. My good luck was compounded when, in my second year, Adolph Grünbaum arrived at Lehigh, fresh from graduate school at Yale, and stayed at least long enough for me to graduate, before moving to the University of Pittsburgh as Andrew Mellon Professor of Philosophy of Science. Beyond his excellent course on the philosophy of science, in which I was exposed to the main currents of thought on the subject at that time, he was both an important source of encouragement and of enormous help to me in getting started as a scholar: He introduced me to Theodore Hailperin, who taught me some logic (I can't remember whether it was in a regular course or in a special reading course) and to another young member of the mathematics department, Samuel Goldberg, who met with me every week to study, eventually, Hardy's *Pure Mathematics*—a wonderful, although humbling, experience. I should also mention that Adolph, along with other members of the Philosophy department at Lehigh then, were extraordinarily supportive and helpful in enabling me to transfer from an athletic scholarship to an academic one: With an injury in the spring of my sophomore year and, probably more importantly, a radical change in my interests, going out every afternoon to get pounded had lost a lot of its appeal.

Following Adolph's example, if not his advice (I can't remember), I went to Yale as a graduate student in philosophy in 1952. In my first year, I took in sequence two semester-long courses in set theory. In the first of them, Fredrick Fitch began to formally develop the Gödel monograph *The Consistency of the Axiom of Choice and the Generalized Continuum Hypothesis with the Axioms of Set Theory* in his system of natural deduction. I can't remember how far we got; but given the fact that the details in that monograph already threaten to overwhelm the ideas, the addition of formal deductions did little to lend light. Following that, John Myhill gave a course in foundations of set theory in which he compared various axiomatizations—*ZF*, The predicative second-order systems of von Neumann and Bernays/Gödel, Quine's *New Foundations*, etc. The course slightly swamped me and when, after I had (more or less) finished a difficult final exam, Myhill asked me to lend him a quarter to buy a beer, I almost strangled him. (Incidentally, you got quite a big glass of beer for a quarter in those days.) John went on leave the next year and never returned to Yale—a large-size loss for me. I had a Fullbright fellowship in my third year to go to Amsterdam to study intuitionism with, I thought, Brouwer; but he had retired by the time I arrived and my study of intuitionism was confined to some lectures by Heyting and talks with his assistants and students. The most interesting activity in logic was a short series of lectures by Leon Henkin, also there on a Fullbright as I remember, on cylindrical algebras. Nevertheless, I had a profitable time in Amsterdam, beginning a serious study of mathematics, which I continued when I returned to Yale. Logic in New Haven at that time was represented by Fitch, whose main interest was in a variation of combinatorial logic, which he called *basic logic*, and Alan Ross Anderson, who had been a fellow student in Myhill's class that first year but who returned after a few years as an assistant professor. I very much admired Alan, but his interests were then primarily in modal logic and so remote, or so I believed, from foundational issues. (In spite of suggestions about introducing modality into mathematics, I still believe that.) I was working my way through Kleene's *Introduction to Metamathematics*, but entirely in isolation: I remember hours of confusion because I failed to recognize that, e.g., of two upper case "A" 's involved in the same argument, one was italicized and the other not. (It is not a very humane book.) I also remember thinking for most of a week that I had gone mad, because due to a misunderstanding of the statement of Gentzen's *Hauptsatz*, I thought I had a very elementary proof of the consistency of first-order arithmetic. The philosophy department itself was at that time, I felt, in serious decline.1 My discussions with Fitch about combinatorial logic/lambda calculus (we never talked about my work) probably served me well: it became a staple for me in thinking about various problems in proof theory. But on the whole, although I had many very bright fellow-students, I found that the interests represented by the other members of the philosophical community there were generally quite remote from mine.

I believe (but am not certain) that I remained in logic/philosophy and went on to obtain a PhD in that field only because of the Summer School in Logic at Cornell in 1957, the first I believe of its kind. Although philosophy remained (and remains) my main interest, my experience in graduate school did not lead me to high expectations for life in a philosophy department, and I had been drifting away from the subject. I stayed at Cornell for the first five of the six-week program and left with my head spinning. At that time, journals in logic were years behind the frontiers of the subject and, after the isolation of Yale, I had had no idea of the riches I began to glimpse. I spent quite a bit of time, as I remember, speaking with Anil Nerode, who wasn't that much older than I, but light-years ahead of me in matters logical. He was very encouraging and it was he who convinced me that some things I had worked on, computable second-order functions and restricted forms of Turing reducibility for them, might actually be of interest. (I never tested the conviction, however: It all went into my dissertation, which I wrote in the summer of 1958 and, after defending it the following autumn at Yale, never looked at again.) I also remember evenings listening to Paul Halmos and Alfred Tarski, to both of whom I have remained grateful for the time that they spent with students at Cornell that summer. It was there, too, that I was exposed, primarily through Georg Kreisel's lecture on Gödel's *Dialectica* interpretation, to the possibility that there still remained after Gödel's incompleteness theorems a program of constructive interpretation of classical mathematics—the possibility that my taste for logic could be comfortably united with my feeling that philosophy is, after all, the serious matter.

It was pure coincidence that, a year later, I connected up with Kreisel at Stanford. My interest in logic had been rekindled and back at Yale I was working my way through volume 2 of Hilbert and Bernay's *Grundlagen der Mathematik*. The job offer from Stanford, probably

1 For some people, the decline began somewhat later, in the 1960's: It probably depends upon what their interests in philosophy were. There is, however, general agreement that there was a serious and long-lasting period of decline beginning at least in the 1960's, but also that the process reversed and that the present department is quite strong.

engineered by Alan Ross Anderson, was the second one I had in the academic year 1957-8 and, as I did the first, I was inclined to turn it down.[2] I had never heard of Stanford (and so of course had no idea that Kreisel was about to begin a part-time appointment there) and, although graduate students of today will find this hard to believe, life as a graduate student in those days was very pleasant: There was almost no tuition and, with a little bit of teaching, one could live quite comfortably, studying the things one wanted to study, without the hassle of a real job. But ARA began to get seriously angry with me—as did quite possibly my wife as well—and so off we went. I have certainly never regretted it: Besides Kreisel, Sol Feferman, whom I had met at Cornell, had arrived at Stanford the previous year; and through our logic seminar and what was then a very close connection with the logic group at Berkeley, we had through the years I was there (up to the summer of 1965) a rich assortment of logicians hanging about at any time.

But the program of constructive foundations of classical mathematics did not in the end fare so well. Spector's extension of the *Dialectica* interpretation to second-order number theory using bar recursion of higher types (1961) and Takeuti's consistency proof for Π^1_1 analysis (appearing in unpublished form around 1964) were the highpoints. But one could find no grounds for accepting higher order bar recursion as constructive and Takeuti's proof—essentially a proof that cuts can be eliminated from deductions in $\Pi^1_1 - CA$ with the ω-rule—proceeds by showing that a certain quite unintuitive system of ordinal notations is well-founded, the proof of which can in no reasonable sense be termed constructive. My own program of attempting to constructively interpret second-order number theory using the epsilon-substitution method bit the dust in the winter of 1962-3. (In my defense, I wasn't the only one naive enough to think that such a result was obtainable: There wasn't then the same clear sense we have now of the limits of constructive methods as we then understood them.) I was at IAS in Princeton at the time and one fallout of my discussions with Gödel about my failure was his suggestion to me that one should consider what instances of second-order comprehension could be satisfied in a theory of inductively defined sets. I don't know whether this was the source of the initiation of studies of iterated inductive definitions at Stanford around that time; but it was for me. For it was immediately clear that the classical theory of finite iterations of inductive definitions of sets of numbers was sufficient to satisfy $\Pi^1_1 - CA$ and almost as immediately clear that a partial cut-elimination result for that theory with the ω-rule—the elimination of cuts in deductions of purely arithmetic formulas—was provable in the constructive version of that theory.[3] In other words, it was possible to end-run around Takeuti's argument: I doubt that I was the only one to sigh in relief that one didn't have to learn that awful argument. But alas, as Harvey Friedman pointed out to me at the Buffalo Conference on Intuitionism and Proof Theory in 1968, $\Pi^1_2 - CA$ is a barrier for iterated inductive definitions. The least ordinal α for which the second-order version of L_α satisfies $\Pi^1_2 - CA$ is non-projectible.

For me, after thrashing around for a year or so, that was (until recent times) the end of proof theory: It seemed impossible that constructivity as we understood it had the resources for interpreting classical second-order number theory. In the light of work on Martin-Löf's type theory and intuitionistic set theory, that judgment might have been premature. It is also the case that proof theory survived as a purely mathematical theory. For example, the techniques of proof theory may be used to extract information implicit in classical proofs—an application of proof theory, called *proof-mining*, that was initiated by Kreisel in the early 1950's and has been pursued in recent times by Kohlenbach and others. It is also the case

2 In those days, there wasn't much formality—or evenhandedness— about hiring: If a department wanted to recruit, someone simply called up his favorite department and asked who they had available. I had never previously met any of my new colleagues at Stanford in philosophy nor had they, I believe, previously ever read a word that I had written.

3 My notes on this are in the form of copies of letters to Kreisel, dated in 1966. In fact, I had noted that, further iterating inductive definitions, one could embed the *rule* of Δ^1_2 comprehension. I lectured on these things at Rockefeller University in February 1967 and discussed it fairly broadly through the summer of 1967.

that Rathjen has recently extended Takeuti's result by obtaining the proof-theoretic ordinal of $\Pi^1_2 - CA$. But it still remains to be shown that proof theory has any remaining and redeeming philosophical virtue.

Of course, constructive mathematics itself flourished: Perhaps the most important development in that field was the publication of Errett Bishop's *Foundations of Constructive Analysis* in 1967, although good work has continued to be done in Brouwer-style analysis. One reaction of people who had worked in the program of constructive foundations for classical mathematics to its failure was to feel "So much for classical mathematics!" and to thenceforth restrict their attention to something less than the latter. I did not share that reaction: I appreciate the attraction of constructive mathematics, but the game was (is) to understand classical mathematics.

Abandoning proof theory was defining myself as primarily a philosopher. Even if one felt driven by philosophical motives, Hilbert's program, even in its extended form, gave one something to do without philosophical reflection: Reduce mathematics based on the axiomatic conception to mathematics based, if not on the conception of Kant, Kronecker and Hilbert/Bernays, nevertheless on a reasonable extension of those ideas (allowing non-algorithmic properties, but, if one proved the existence of an object with such a property, one could extract an algorith for it). Its failure left for me an itch, but I didn't know where to scratch. I wanted to try to understand why we felt that something needed to be said or done about the foundations of mathematics, and what it really was that needed to be done. So I began, in the mid-1970's, a study in philosophy of mathematics and, equally importantly, in the history of the development of the central concepts of mathematics: number, function, and set.

2. The Role of Mathematics in Philosophy

Aside from the negative business of letting the fly out of the fly-bottle, I think of philosophy as being primarily foundational: it has no subject matter of its own, but rather refers to a characteristic way of approaching the sciences—physical, biological, social, cognitive, and mathematical—like Plato's dialectician, seeking clarity and the first principles of each science. Where works in philosophy appear to be advancing theories about language, the mind, or whatever, I tend to see nascent science at best and bootleg science at worst.4

However, I don't want to entirely downplay the freeing of flies, nor do I think that it is entirely divorced from philosophy in its foundational role. For example, in the foundations of mathematics itself, historical resistance to the actual infinite was based upon supposed paradoxes, including in recent times the so-called 'paradoxes of set theory', which have all been seen to be based upon confusion. Yet the resistance persists on other grounds. Thus, the pursuit of the actual infinite leads us to speak of the existence of objects which are not simply in themselves infinite, but also cannot even be effectively approximated by finite things. There are those who not simply choose to pursue constructive mathematics, which avoids such objects, but argue that speaking of them is meaningless or wrong. There are others, in this case, perhaps, more often philosophers quite removed from mathematics itself, who advocate some form of nominalism, on the grounds that, no matter how internally coherent mathematics might be, it speaks of 'abstract objects', and these simply don't exist. In both cases, I believe, there are captive flies buzzing around. Most of my own non-technical publications have been of the freeing-of-the-flies variety, perhaps the most notable example being "Proof and truth: the 'Platonism' of mathematics". But what I want to point out here is that this enterprise is not totally unconnected to the foundational enterprise. Many of those who have been involved in the development of set theory, itself, for example, have been afflicted with 'philosophical' doubts about the existence of sets—Tarski's well-known 'finitism' being a case in point. Freeing

4 Of course I am excluding here the many instances of philosophers who publish work that they quite frankly see as having scientific content in the usual sense, subject to the usual critical standard.

the *internal* problems of foundations from these external and groundless concerns is surely part of the foundational role of philosophy.

I do not see that mathematics itself plays any role in this more therapeutic kind of philosophy. So the question of the use of mathematics in philosophy can only be, for me, the question of its use in foundations of science. The idea of 'mathematizing' a science, of creating mathematical theories which idealize the phenomena in question—that is, in terms of which we can understand and reason about these phenomena and control them, goes back at least as far as Plato. In near contemporary times its great exponent was Hilbert, and the ubiquity of titles in the last century and earlier of the form "The mathematical foundations of ..." attests to the success of this approach to foundations of science.

Because of a primarily foundationalist conception of philosophy, I include in the field much more (and much less) than the twentieth century division of the disciplines allows for. In particular, many works on foundations of mathematics, that are generally counted only as mathematics—*unless they are very old*—I think of as philosophy and hence as instances of the 'use of mathematics for philosophy'. A few examples: Riemann's "On the hypotheses which underlie geometry", Dedekind's "Continuity and irrational numbers" and "The nature and significance of numbers", Hilbert's *Foundations of Geometry*, Cantor's theory of transfinite numbers in *Foundations of a General Theory of Manifolds: A Mathmatico-Philosophical Investigation into the Theory of the Infinite*, Frege's analysis of quantification in his *Begriffsschrift*, the whole nineteenth century movement in the foundations of function theory and search for the proper definition of the integral, culminating in Lebegue's "On a generalization of the definite integral", the works of Zermelo, von Neumann and Gödel on foundations of set theory—as well as much of the contemporary work in this area. The analysis of computability in the works of Turing and others belongs in this list. And of course, if we move to the foundations of other sciences—physics, biology, economics, etc., a whole class of other examples come to mind.

Of my own work, perhaps the paper "Finitism" may be regarded as an application of mathematics to philosophy, in that it attempts to give an analysis of a particular conception of mathematics in terms of the formal system of primitive recursive arithmetic. Indeed, I begin to believe that, independently of whatever version of Kantianism Hilbert and Bernays were drawing on in their conception of finitism, primitive recursive arithmetic is the genuine heir of Kant's conception of mathematics—indeed, of a pre-nineteenth century constructivist conception to which Kant gave voice. (My view of Kant in this respect is heavily influenced by Michael Friedman's work on Kant's philosophy of mathematics.) My work in proof theory in the 1960's and early 1970's was in aid of a philosophic program; but, whatever intrinsic value that work has, the philosophic program failed. Moreover, the program presupposed a radical difference between constructive mathematics and classical mathematics, the former based on an idea of construction, the latter based upon an idealized domain which we access by axiomatically describing it. As a result of subsequent philosophical reflection (of the freeing-of-the-flies variety), I no longer believe that: I don't see constructive mathematics as based on a different conception of mathematics but as, basically, a subdomain of classical mathematics. I first discussed this in a (badly written) paper "Against intuitionism: Constructive mathematics is part of classical mathematics." A further discussion is in the introduction to a collection of my philosophical essays, *The Provenance of Pure Reason: Essays in the Philosophy of Mathematics and its History*.

3. The Proper Role of Philosophy of Mathematics in relation to Logic, Foundations of Mathematics, Mathematics, and Science

In its positive, foundationalist, guise, the proper role of philosophy and, specifically, philosophy of mathematics in foundations of mathematics, mathematics and science is obvious. Much of the work in philosophy of mathematics of the last century was concerned with foundations in this sense; and indeed, it continues to this day.

Some of it, in the logicist program and Hilbert's program (and its extension) in particular, was concerned to give a foundation for all of classical mathematics. The logicist program in the sense of Frege, of reducing mathematics to logic, was of course doomed to failure. In the sense of Dedekind, however, in which the aim was to eliminate intuition and replace it by logical analysis, it led to the modern conception of mathematics as based on the axiomatic method. This in fact set the stage for Hilbert's program: To prove the axioms consistent or, in the case of the extended program, to interpret the theorems of classical mathematics as theorems of constructive mathematics. The failure of this program was not so immediate. It should be mentioned, too, that its failure is not the failure of the axiomatic conception of mathematics—and that is fortunate, since it is the only viable conception we have. Rather, the significance of Gödel's second incompleteness theorem is that it is a fact of mathematical life that we are forever at risk of encountering a contradiction.

Incompleteness, on the other hand, is the engine driving contemporary foundations of classical mathematics, i.e. philosophy of mathematics in the positive sense, although the work done in this area is quite technical and is not philosophy as the term is usually used: There are many questions that the axioms of *ZFC* do not suffice to settle and so one is led to believe that the axioms do not sufficiently express the conception of a universe of sets obtained by iterating the powerset operation and, following the suggestion of Gödel, one would like to find axioms expressing even higher iterations of this operation that will lead to the solution of open problems in everyday mathematics. The discovery that certain of these large cardinal axioms yield the solution of problems in descriptive set theory, such as whether all projective sets are Lebesgue measurable and whether they have the property of Baire, has been one great success in this direction and leads to the hope that further axioms expressing even higher iterations of powerset will lead to solutions of, say, the Continuum Problem.

But there were and are, too, revisionary programs aimed at restricting the scope of mathematical reasoning. One example is the predicativism developed by Weyl and, later, Feferman. Another is the strict finitism of Kronecker, in which the objects of mathematics are restricted to those representable by whole numbers and whose concepts are restricted to those equipped with algorithms for determining which objects fall under them—the position that Hilbert adopted as the methodological stance upon which to prove the consistency of axiomatic mathematics, and the more liberal constructivism of Brouwer, of Weyl, and, later, of Bishop. Of course, both predicative and constructive mathematics can be pursued as interesting domains of investigation in their own right—subdomains of classical mathematics; but I am referring here to a stance according to which we *ought* to adopt a more restrictive kind of mathematics. The arguments for this have been various: For example, in the early part of the last century, such as in the writings of Weyl, they were often based on the so-called 'paradoxes of set theory'. Brouwer also referred to these 'paradoxes' in his polemic against classical mathematics; but his more positive argument (and one would suppose this to be so of Kronecker, too, if he had chosen to write more on the subject) appealed to an earlier tradition in which, at least if one sufficiently hid epsilon-delta arguments behind infinitesimals, one could believe in the picture of mathematics presented by Kant, that all of mathematics consists essentially of construction according to rules. In more recent times there has been Michael Dummett's argument for constructive mathematics based upon a theory of meaning. Also, in philosophy of mathematics itself, largely in isolation from the actual practice of mathematics, general and *a priori* views on ontology—about the existence of what some writers call 'abstract objects'—have led to the charge that mathematics or at least some parts of it are meaningless or false and/or to the view that at least a part of it can be understood only as a formalism.

My own non-technical papers in philosophy of mathematics, other than some of them of primarily historical content, are philosophical in the negative sense: Their primary aim has been to disarm the arguments behind these revisionary programs. In one direction, I have attempted to counter the idea that constructive mathematics is a different subject from classical mathematics and have argued that one can understand constructive mathematics as a subdomain of classical mathematics. In several papers and in my collection of essays *The Provenance of Pure Reason: Essays in the Philosophy of Mathematics and Its History*, I pointed out that the conception of meaning that Dummett believes to support intuitionistic mathematics is equally compatible with the classical conception and that the apparent constructive refutations of classical theorems often referred to are in fact simply a matter of changing the meanings of words—that with a certain disambiguation, the 'counterexamples' are classically valid, too. In another direction, I have attempted to show that the qualms about the existence of 'abstract objects' that have led some philosophers and mathematicians to reject or at least to question parts of mathematics are based upon an illusion that there is some univocal notion of existence on the grounds of which we can legitimately argue for or against the existence of mathematical (or physical or mental) objects. This is a lesson that I learned from Wittgenstein's ıPhilosophical Investigations, although, paradoxically, his own views about mathematics were so out of sync with the actual mathematics of his time that he failed to apply his own lesson. Finally, by arguing, again following a line of thought I believe to be in Wittgenstein's *Investigations*, that Hilbert's conception that the (categorical) axioms define the mathematical structure—that the objects of the structure are, so to speak, constituted in the axiom system—is not entirely different from the sense in which the objects of our daily life are constituted in the language in which we speak and think about them, I have attempted to disarm the charge of formalism that has been leveled against the axiomatic conception of mathematics.

4. Late 20th century philosophy of mathematics

It is disappointing to me, now in the twenty-first century, that so many of the ghosts that haunted philosophical discussions of mathematics at the beginning of the twentieth century are still with us. At the beginning of that century, the concepts of set and (in our sense) function and, generally, the explicit acceptance of the actual infinite (in the sense, not of there being infinitely many things—a potential infinity, but of there being infinite things) were still relatively new in mathematics: A new language had to be learned and old misconceptions and fallacious 'paradoxes' had to be exposed. True, the latter had already been done in Bolzano's ıParadoxes of the Infinite and in Cantor's *Foundations of a General Theory of Manifolds: A Mathmatico-Philosophical Investigation into the Theory of the Infinite*, but the latter of these was of relatively recent vintage (1883) and apparently not much read by philosophers and Bolzano's work, because of, ultimately, an inadequate notion of a set, failed to lay to rest the ancient 'paradox' concerning 'unequal infinities', i.e. sets of the same size as one of their proper subsets, and well as those problems that arose from failing to distinguish between what we would call structures and their underlying sets.

Of course, the appearance of the *new* 'paradoxes of set theory' contributed to the sense that the new language might turn out to be incoherent; but a conception of set theory having as its models a potential infinity of universes of sets (where each universe appears as a set in another one and there are no absolute 'proper classes') and which is not in the least subject to these paradoxes has been in existence since Zermelo's 1930 paper.

Resistance to accepting the new language has alas been reinforced by superstition concerning the issue of "what there is," where this is taken to be, not an issue *internal* to the language or theory in which the objects are purported to make their appearance, but an external question concerning the legitimacy of the theory itself. In United States and England, at least, the hegemony of W.V. Quine among philosophers on the subject of mathematics through much of the last half of the century had a lot to do with this resistance. I'm referring here not only to his unwarranted "common sense is bankrupt" point of view concerning set theory, but also his views about ontology. His slogan,"To be is to be the value of a bound variable," turned out to be a *criterion* for ontological commitment of theory, one which mathematics might fail to satisfy, rather than a banishment of the issue of ontological commitment to mathematical objects (sets, functions, numbers, etc.) from consideration entirely, as it might and should have been. The misfortune was compounded by Quine's view of the role of mathematics in natural science. As opposed to the view that Euclidean geometry, arithmetic, and set theory concern their own ideal domains, Euclidean space, the system of natural numbers, and suitable universes of sets, respectively, *independently of any possible applications that they might have in our theories about the natural world*, Quine took the position that mathematics has no autonomous status and that its validity rests holistically with its role in natural science. This view, too, framed many of the topics of discussion in the last part of the century, which were, therefore, far from any involvement with real issues concerning mathematics.

The same lack of involvement may be ascribed to the contemporary neo-logicism. When it is considered, not as a possibly interesting—though surely quite limited—investigation in its own right, but as an alternative to mathematics as it is being practiced, one is moved to ask: Why? The motivation for it as a better alternative for doing arithmetic and analysis seems based upon the same monochromatic conception of existence as Quine's. (In fact, it goes back through Frege to Kant and ultimately to Aristotle, and is opposed to the tradition, going back through Leibniz to Plato and forward through Dedekind and Cantor to Hilbert, according to which mathematics concerns ideal domains.) Frege, realizing that the demands of mathematics in his time required that whole numbers be regarded as objects, needed to make a correction in Kant's philosophy, one that would admit numbers into the *same* universe that Kant had wanted to restrict to things representable in sensible intuition. The neo-logicists seem committed to the same view: whole numbers and real numbers, say, are part of the same universe as physical objects, arising out of equivalence classes of concepts that are meaningful for all objects—so that, for them too, it makes sense to ask whether Julius Caesar is a number! The difference being that, instead of Frege's inconsistent assumption that arbitrary extensions of concepts belong to the universe, they make the more modest assumption that this is so (essentially) of suitable equivalence classes. As a philosophical stance, it seems sterile; as for the development of the theory itself, it lacks the kind of connection with actual mathematics that constructive mathematics has, for example, as a style of proof in which existence proofs yield algorithms.

In speaking about philosophy of mathematics in the late twentieth century, one certainly needs to mention the influence of Gödel. Aside from his technical work, his 1948 paper "What is Cantor's continuum problem" along with the supplement of 1964 have been quite influential both in foundational work in set theory and, alas, in muddying the waters over the issue of 'what there is' with his subscriptions to 'Platonism'. In the former respect, I have already mentioned that his view that the pursuit of large cardinal axioms might lead to the solution of mathematical problems has served as motivation for research in set theory and, indeed, has born fruit—although not with respect to the problem at issue in that paper. The publication of his collected works has led to fairly intense discussion of his philosophical views in recent times; and having contributed rather more substantially to that discussion than I ever intended, perhaps I can beg off discussing it further here. One matter though that I would like to mention is his interest in Husserl's phenomenology, which he began to study, it seems, in the late 1950's. I don't know how much more there is to be found out about it in the Gödel archives; but it has attracted considerable attention among phenomenologists and it will be interesting to see what might develop from it.

5. The most important open problems in the philosophy of mathematics and the prospects for progress?

For me, the most important open problem in philosophy of mathematics is in foundations of mathematics, and that is the search for new axioms of set theory—which means, too, the search for grounds for accepting them. There are many interesting directions of development in logic, but in philosophy of mathematics, I believe that this is the overwhelmingly most important problem. But it is a problem now largely in the hands of set theorists. Maybe one important open problem for those of us who are primarily philosophers is that of gaining access to that problem.