For *Philosophy of Mathematics: 5 Questions*.

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The 5 Questions

1. Why were you initially drawn to the foundations of mathematics and/or the philosophy of mathematics?

2. What examples from your work (or the work of others) illustrate the use of mathematics for philosophy?

3. What is the proper role of philosophy of mathematics in relation to logic, foundations of mathematics, the traditional core areas of mathematics, and science?

4. What do you consider the most neglected topics and/or contributions in late 20th century philosophy of mathematics?

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

My Responses

1. I'm a philosopher by temperament but not by training, and a philosopher of logic and mathematics in part, as I shall relate, by accidents of study and career. Yet, it seems to me that if I was destined for anything it was to be a logician primarily motivated by philosophical concerns.

When I was a teenager growing up in Los Angeles in the early 1940s, my dream was to become a mathematical physicist: I was fascinated by the ideas of relativity theory and quantum mechanics, and I read popular expositions which, in those days, besides Einstein's *The Meaning of Relativity*, was limited to books by the likes of Arthur S. Eddington and James Jeans. I breezed through the high-school mathematics courses (calculus was not then on offer, and my teachers barely understood it), but did less well in physics, which I should have taken as a reality check. On the philosophical side I read a mixed bag of Bertrand Russell, John Dewey and Alfred Korzybski (the missionary for "General Semantics" in *Science and Sanity*, a mish-mash of the theory of types, non-

Aristotelian logic and colloidal chemistry, among other things). Also, I was fascinated by, and bashed my head against, Rudolf Carnap's *Logische Aufbau der Welt*, but couldn't penetrate it. Still, I should have taken its attraction for me as another sign. One thing I did know for sure, and that was that I wanted to have an academic career and become a professor. What the source of that was is a bit of a mystery to me, since my parents and their friends were working class and I had no personal role models. But I suppose I learned in one way or another that that was the way to go if I were to devote myself to theoretical research.

In 1945, I applied to both UCLA and CalTech for mid-year entry to undergraduate studies. I leaned toward UCLA since it was practically free, it had a broader curriculum, it was co-ed, and many of my friends were going there. By contrast, CalTech was focussed on science and engineering, it had an all male student body in those days (and did not become co-ed until many years later), and none of my buddies applied there. It was also more prestigious. Perhaps I would have had a different career path if I had not passed the entrance exam for CalTech and still had physics so strongly on my mind. Tuition expense was a serious obstacle, but I was offered a part-scholarship and a job as a lunch-time waiter in the *Athenaeum*, CalTech's faculty dining hall, and my proud parents made ends meet somehow or other. (A high point one day as a student waiter was serving a boiled egg and salad to Robert Oppenheimer, whom I met again years later in his capacity as Director of the Institute for Advanced Study in Princeton, when I was a fellow there in 1959-60.)

In my courses during the first two years at Caltech, mathematics was as before a breeze and fun, while in my physics courses I found that I lacked even minimal physical intuition. Still, mathematical physics was my goal, and for that the book that was touted for students was Harry Bateman's *Partial Differential Equations of Mathematical Physics.* Looking through it made clear to me that that wasn't at all the kind of thing I was after. So, in my junior year, as a kind of fall-back position, I switched majors to mathematics. But there I found that I had to enter a new mind-set, that of pure mathematics and theorems to be proved rather than problems to be solved and techniques to be mastered. Of all the courses that I took from then on, only one appealed as a possible direction for further study, and that was an introduction to logic taught by Eric Temple Bell—known to mathematicians as a number-theorist and author of the romantic and historically flawed, *Men of Mathematics* and *The Queen of Sciences*, and to science-fiction aficionados of the day through a pseudonym, John Taine (*Green Fire, The Iron Star,* etc.). The course, which hardly got beyond propositional calculi of various kinds, was a hodge-podge because Bell did not really know anything substantive in logic; I learned years later that he had a fatal attraction to Lukasiewicz' three-valued logic (in his *The Search for Truth*). Despite its incoherent presentation, the material of that course resonated with me, but there was no follow-up to be had at CalTech.

For a career in academia, it was clear I would have to go on to graduate work in mathematics, and in 1948 I applied to UC Berkeley and the University of Chicago. I was accepted at both places, but only Berkeley came through with an offer of a teaching assistantship, so that pretty much clinched it. (Also I had a strong personal reason to prefer Berkeley—a romantic interest.) In my first year, I took some of the basic required courses in algebra and analysis, and I met Fred Thompson, who was working on a PhD with Alfred Tarski. Thompson idolized Tarski, raved about him no end, and urged me to take his course in metamathematics, which I did the following year. To quote myself from the biography that I co-authored with Anita Burdman Feferman, *Alfred Tarski. Life and Logic* (2004), "I knew immediately that this was to be my subject and Tarski would be my professor. He explained everything with such passion and, at the same time, with such amazing precision and clarity, spelling out the details with obvious pleasure and excitement as if they were as new to him as they were to us." In the following years I went on to take courses in model theory, set theory and universal algebra with Tarski and became a regular attendee at his seminars, which in those years concentrated on algebraic logic. My introduction to recursion theory and Gödel's incompleteness theorems came via Andrzej Mostowski's 1952 book, *Sentences Undecidable in Formalized Arithmetic*, through its use as the text of a course taught by Jan Kalicki (a promising young logician who, tragically, died in an automobile accident in the fall of 1953). There were no courses in proof theory.

I did indeed end up working toward a PhD with Tarski, but the excellent initial progress I had made in my studies with him did not presage the difficulties that I would have in arriving at a dissertation result to his satisfaction. (I was not the only one with this problem.) That story has been told in our biography of Tarski, and in more detail in my article "My route to arithmetization" (1997). Briefly, Tarski suggested two problems for me to solve, one on cylindric algebras, and the other on a decision procedure for the ordinals under addition, both of which I attacked dutifully but with only partial success. Fate intervened when I was drafted into the US Army in the fall of 1953 (fortunately not a time of active war for the US, since it was post-Korea and pre-Vietnam). After basic training, I was assigned to a unit in the Signal Corps at Fort Monmouth, New Jersey, doing research on kill probabilities of hypothetical missile attacks on major cities and target sites in the US. My thoughts about this alternated between bemusement with the essential unreality of our calculations and anxiety about the possible reality of the scenarios with which they dealt. In what leisure time I had during off-hours, I read and reread Kleene's *Introduction to Metamathematics*, and that added significantly to my understanding of recursion theory and Gödel's theorems, as well as oriented me toward Hilbert's finitist consistency program. To my surprise during that period, Alonzo Church, as the *Reviews* editor of *The Journal of Symbolic Logic*, asked me to take on an article by Hao Wang. That concerned an arithmetized version of Gödel's completeness theorem that extended an earlier version in vol. II of Hilbert and Bernays' *Grundlagen der Mathematik*. (I think Dana Scott, who was by then studying in Princeton, suggested my name to Church.) That led me to the question as to how, precisely, one should deal with formalized consistency statements in general, and thence directly into my work on the arithmetization of metamathematics. When I was released from active army duty and returned to Berkeley in 1955 I proposed that to be the new topic of my dissertation.

As it happened, Tarski was on sabbatical leave in Europe that year, and Leon Henkin agreed to help supervise my work in his absence. With his constant encouragement in the following months I obtained a number of good new results and I sought Tarski's approval to have them form the main part of my thesis. Because it was out of the mainstream of his interests, and perhaps because it dealt with problems arising from Gödel's work rather than his own, he was initially resistant to that. But he consulted Mostowski and then acceded, though still with some reservations, after he received the latter's quite positive report.

Crucial to me in that period, and, as it turned out, for many years following, was my contact with Georg Kreisel. To quote myself again, in "My route to arithmetization" I wrote: "I first met Kreisel during the period in early 1956 when I was well into the research for my hoped-for dissertation; Kreisel happened to be visiting Berkeley for a month or so at that time. Our initial personal contact was magical for me: I had hardly to begin explaining what I had done and what I was in the process of working on, to see that Kreisel understood immediately, and that it related to things he had thought about and to a whole body of literature in which he was completely at home. His positive reception of my ideas confirmed my views of the significance of what I was up to, and added to my determination to make this work my thesis, despite Tarski's reservations. … the boost provided by Kreisel's quick appreciation was psychologically crucial at that agonizing time. In addition, Kreisel opened up a new world to me through his interests in constructivity, predicativity and proof theory, interests which I was naturally attracted to and which would come to dominate my own subsequent work."

I wrote up the dissertation itself during the academic year 1956-57 at Stanford University, where I had been appointed to an instructorship in mathematics and philosophy, and its results were eventually published in 1960 under the title, "Arithmetization of metamathematics in a general settting".

The influence of Tarski and Kreisel was decisive for me, the former in how I carried out my work and the latter in what I worked on. In their own pursuits, both were highly conscious of aims and programmatic development; for Tarski that was largely mathematical while for Kreisel it was primarily philosophical, though Tarski's work in the 30s on conceptual analysis of semantical notions has also been of great philosophical significance. Tarski emphasized clarity and precision of presentation and careful, sequential organization of material; no detail was too small to be overlooked. By contrast, Kreisel emphasized informal rigour and not taking received views for granted; once one had the right ideas, details were supposed to look after themselves. Personally, my relations with Tarski were friendly and frequent throughout the years following my PhD to the time of his death in 1983, but my work was largely disjoint from his, and even where it wasn't he reserved comment. By contrast, Kreisel and I were the closest of colleagues for some fifteen years up to the time we had a rather abrupt and complete falling out in the early 1970s. In any case, stimulating as our contact had been over such a long period, it was time to move on.

2. Here are some of the philosophical problems with which I have been concerned off and on over a long period of time. *What is the true reason for incompleteness? How may it be overcome? What ought we to accept once we have accepted given notions and principles? Does mathematics need new axioms? What is the significance of foundational work for mathematical practice?* Inevitably, each has given rise to more specific questions of philosophical relevance that I will also indicate in the following.[1]

To go back to the beginning; the work in my dissertation was driven by the aim to carry out in precise and substantial generality the arithmetization of metamathematics as exemplified in particular by Gödel's second incompleteness (or unprovability of consistency) theorem on the one hand and a formalized version of his completeness theorem on the other. Both involved consistency statements, the latter in the form that a recursively axiomatized theory S is interpretable in Peano Arithmetic (PA) when the consistency of S, Con_S, is added as an axiom. But just what is meant by Con_S in general? That is explained in terms of the arithmetized provability predicate for S, $Prov_S(x)$, and that in turn is determined by an arithmetized definition $Ax_S(x)$ of the set of axioms of S, once we fix the logic to be that of the classical first-order predicate calculus. It turns out that for the formalized completeness theorem it is sufficient for $Ax_S(x)$ to binumerate the axioms of S in PA. But that is not sufficient for the unprovability of consistency theorem, since an example can be given of a binumerative definition of the axioms of PA for which the associated statement Con_{PA} is provable in PA—in contrast, of course, to the "canonical" definition. On the other hand, if $Ax_S(x)$ is provably recursively enumerable (r.e.) and S is a consistent extension of PA (and already of much weaker systems) then Con_S is *not* provable in S; *any* such definition serves to verify the Hilbert-Bernays derivability conditions for $Prov_S(x)$. I showed how one could trade on the difference between these general statements, for example to show that PA + ($\neg Con_{PA}$) is interpretable in PA.

A non-chronological aside: while provably r.e. definitions are sufficient for general formulations of the second incompleteness theorem and other results of the same character in the arithmetization of metamathematics, they are not necessarily *intensionally* correct, so what was still called for was an account of *canonical consistency statements*. As it happens, it was not until the early 1980s that I returned to give full consideration to that matter. My solution, in an improved form in the 1989 paper "Finitary inductively presented logics", was to treat formal systems as they are actually presented to us in practice through the finite inductive generation of various syntactic categories of objects, operations on them, and relations between them; consistency statements are then canonically associated with those. Besides addressing the conceptual issue of finding the "right" framework for general developments, this work was conceived of as having potential pedagogical and practical value, the latter via the pursuit of computer implementation of a wide variety of logical systems.

[1] Because of limitations of space, I cannot go here into other parts of my work that I consider to be of philosophical significance, including that on systems of constructive analysis and explicit mathematics, type-free theories of truth, foundations of category theory, relativized Hilbert program, and the limits of logic.

Moving beyond these particular technical and conceptual questions, following my dissertation work I turned to the phenomenon of arithmetical incompleteness itself. What is the reason for it? Can it be overcome? Famously, Gödel in footnote 48a to his 1931 paper on undecidable propositions said that "the true reason for the incompleteness inherent in all formal systems of mathematics is that the formation of ever higher types can be continued into the transfinite … while in any formal system at most denumerably many of them are available. … An analogous situation prevails for the axiom system of set theory." To be sure, one can go beyond whatever axioms have already been accepted by adding axioms for the existence of sets that code a truth definition for a model of the previously accepted axioms, and thus prove their consistency. So Gödel's is *one* reason that can be given for incompleteness. But another one that can be given is that it is simply a matter of oversight: whatever has led one to accept a given system S of axioms ought to lead one to accept its consistency Con_S as a new axiom. More generally, one ought to accept an expression of the correctness of S in the form of the *local reflection scheme* $Prov_S(A) \rightarrow A$ for each A in the language of S. Moreover, such extensions are formulated without positing the existence of any sets whatever. Unlimited finite iteration of such schemes beginning with, say, PA, still leads to incomplete r.e. systems. So the natural question to ask is, to what extent can arithmetical incompleteness be overcome by the transfinite iteration of consistency statements and more generally of reflection schemata? The first attempt to answer such questions had been carried out by Alan Turing in 1939. He introduced the notion of an *ordinal logic*, which is a uniform means of associating an r.e. system S_a with each $a \in O$, the set of Church-Kleene recursive ordinal notations. Turing showed by an ingenious argument that if one forms the S_a by iterating consistency statements starting with $S_1 = PA$, every true statement A of the form $\forall xR(x)$ with R primitive recursive can be proved in S_a for some $a \in O$ that denotes $\omega+1$. Turing was particularly interested in obtaining a similar result for statements in the next higher quantificational form, $\forall x\exists yR(x,y)$, via iteration of the local reflection principle; this class includes many interesting open problems in number theory.

In my 1962 paper, "Transfinite recursive progressions of axiomatic theories" (my rechristening of Turing's ordinal logics), I showed that iteration of the local reflection principle is incomplete for $\forall\exists$ statements, but one does obtain completeness for them by iterating instead the *uniform reflection scheme*, $\forall xProv_S(A(num(x)) \rightarrow \forall xA(x)$, i.e. the formalized version of the ω-rule. In fact one obtains much more: for that progression, *every* true arithmetical statement A is provable in S_a for some $a \in O$ at level $\omega^2+\omega+1$. Moreover, there are paths P through O and recursive in O, such that every true arithmetical statement A is provable in S_a for some $a \in P$. However, these results, like Turing's, suffer from non-uniqueness: different notations a, b for the same ordinal may have quite different S_a and S_b. Indeed, Spector and I showed in the follow-up paper, "Incompleteness along paths in progressions of theories" (1962), that one has incompleteness with respect to $\forall$-form statements along *any* path P through O that is of the same logical form as O (i.e., one universal set quantifier), and moreover there are many such paths—thus putting us back to square one.

A side conceptual question raised by this work is: what is a *natural system of notations for ordinal numbers*? The paradigm example for that is Cantor's system of notations for the ordinals up to ε_0, the least solution of $\omega^\alpha = \alpha$. The cited completeness results for progressions depend crucially on the construction of non-natural notations that somehow encode the truths to be proved. Ever since Gentzen's proof in 1936 of the consistency of PA by transfinite induction up to ε_0 with respect to a primitive recursive predicate, the question of natural well-orderings has also been of prime significance for proof-theorists in pursuit of Hilbert's consistency program for systems much stronger than PA. In practice, that work applies transfinite induction up to ordinals α much larger than ε_0 for which one has a natural system of notations up to α. Moreover, every such system has a recursive ordering on it and thus is embeddable in an initial segment of O. But simple examples serve to show that recursiveness is far from sufficient as a condition for naturality. This is a problem that has yet to receive a satisfactory answer; some efforts to provide one are surveyed in my lecture text, "Three conceptual problems that bug me" (1996a).

In any case, the main problem with the work on ordinal logics/recursive progressions of theories was the lack of a conceptually motivated restriction on which ordinal notations ought to be accepted. Following Kreisel's own earlier work "Ordinal logics and the characterization of informal concepts of proof" (1960) on a progression related to finitism, he suggested that the choice of notations should be controlled by an *autonomy condition*. That is, one may proceed to an S_a only if it has been proved in some previously accepted S_b with $b < a$ that the ordering of notations up to a is well-ordered, so that a recognizably denotes an ordinal α.[2] On the other hand, the restriction to the language of arithmetic that had been taken before may be considered to be arbitrary. For, once one has accepted a system S as correct, one ought to accept the truth predicate for sentences of the language of S as a new basic notion with its usual closure conditions as new axioms; doing so automatically yields the uniform reflection principle for S. Thus one is led to consider a progression of theories starting with PA obtained by iterating autonomously the adjunction of truth predicates. It turns out that this is equivalent to an *autonomous progression of ramified second-order theories* RA_α, or ramified analysis. Just as Russell ramified the theory of types in order to meet the Vicious Circle Principle and thus satisfy Poincaré's injunction against impredicative definitions, so this could be considered to provide a characterization of the notion of *predicative provability given the natural numbers*. The first question to ask, assuming that, was, what is *the ordinal of predicativity*, i.e. what is the least ordinal not obtained in the autonomous progression of RA_α's? The answer to that was provided in my paper, "Predicative systems of analysis" (1964) and, independently, around the same time by Kurt Schütte. Denoted Γ_0, it is the least ordinal γ not obtained by transfinitely iterating the fixed point process applied to continuous increasing ordinal functions beginning with the exponential function. By its nature, this proposed characterization of predicativity is impredicative, since it requires the impredicative concept of ordinal or well-ordering. It is supposed to be complete as

[2] On the face of it, this requires a second-order quantifier over all subsets X of the ordering up to a, but restriction to a first-order language can be maintained by taking X to be a predicate parameter.

looked at from the inside, in the sense that everything the "ideal predicativist" ought to accept—and nothing more—is eventually accepted, but it is certainly incomplete looked at from the outside, since the consistency of the limit system RA_γ with $\gamma = \Gamma_0$ is not provable in that system.

My subsequent work on predicativity branched along two paths, each carried out over an extended period. The first was to reformulate the characterization of predicative provability without any overt appeal to the impredicative notions of ordinal or well-ordering, so that it would describe more directly the expansion of reasoning that could be admitted by an ideal predicativist. The second was to see what part of mathematical practice may be accounted for in predicative terms.[3]

Along the first path, my rethinking of the formulation of systems for predicativity went through several stages, and was eventually conceived in the mid 90s as part of a much wider project, namely the determination of what I call *the unfolding of open-ended schematic systems.* The initial spark for that was provided by Kreisel's 1970 article, "Principles of proof and ordinals implicit in given concepts". He posed the general question: "What principles of proof do we recognize as valid once we have understood (or, as one sometimes says, 'accepted') certain given concepts?" As he elaborated it, "[t]he process of recognizing the validity of such principles (including the principles for defining new concepts, that is, formally, of extending a given language) is here conceived as a process of reflection... Granted that we have to do with an area [C] which lends itself to the kind of analysis indicated, it is evident that ordinals play a basic role. They index the stages in the reflection process." The two principal basic concepts considered by Kreisel were, in his terminology: 1. the concepts of ω-sequence and ω-iteration, and 2. the concepts of set of natural numbers and numerical quantification, the first being related to his earlier work (1960) on an autonomous progressions for finitist mathematics and the latter to mine (1964) on an autonomous progression for predicative mathematics. However, I decided instead that the formal systems considered for a given C should not be taken to involve the notions of ordinal or well-ordering in any way that is not already contained in the basic concepts of C. Moreover, I thought that extensions of set theory by certain axioms for "large cardinals" should serve as another possible example, in accordance with Gödel's view in his famous 1947 article on Cantor's continuum problem that the familiar systems such as ZFC "can be supplemented without arbitrariness by new axioms which are only the natural continuation of those set up so far."

The general notion of unfolding that I arrived at was first explained in my article, "Gödel's program for new axioms: why, where, how, and what?" (1996). It is applicable to formal systems in which schematic axioms and rules of inference are expressed using free predicate variables in an *expandable language* for which each expansion leads to new accepted substitution instances. The general questions raised for such open-ended schematic systems S are: *which operations and predicates—and which principles*

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[3] See my survey article "Predicativity" (2005) for a much fuller description of my work on that subject, together with its background in the ideas and work of Poincaré, Russell, Weyl, Kleene and Kreisel.

concerning them—ought to be accepted if one has accepted S? The answer for operations is straightforward: any operation from and to individuals is accepted which is determined explicitly or implicitly (e.g., recursively) from the basic operations of S. Moreover, the principles which are added concerning such operations are just those which are derived from the way that they are introduced. The question concerning predicates in the unfolding of S is treated in operational terms as well: which operations on and to predicates–and which principles concerning them—ought to be accepted if one has accepted S? For this, it is necessary to tell at the outset which *logical operations* on predicates are taken for granted in S. For example, in the case of *non-finitist (classical) arithmetic* NFA, these would be (say) the operations $\neg$, $\wedge$ and $\forall$, while in the case of *finitist arithmetic* FA we would be limited to positive propositional connectives and (in one formulation) the existential operator. Both of these have been investigated in collaboration with Thomas Strahm, to begin with in "The unfolding of non-finitist arithmetic" (2000), with the following results. We take the initial axioms for NFA to be the usual ones for 0, successor and predecessor (as the only constants and operations on individuals) together with the induction scheme $P(0) \wedge (\forall x)[P(x) \rightarrow P(sc(x))] \rightarrow (\forall x)P(x)$. Further operations on individuals and predicates, and more elaborate axiom schemes are successively recognized via proofs of existence using the substitution rule. We showed that the operational unfolding of NFA is equivalent to PA, while the full (operational and predicate) unfolding is equivalent to predicative analysis, i.e. the union of the RA_α for $\alpha < \Gamma_0$.

In an unpublished MS in progress, "The unfolding of finitist arithmetic", Strahm and I have shown that both the operational and full unfolding of a system FA for finitist arithmetic are equivalent to the system PRA of Primitive Recursive Arithmetic. This supports Tait's argument in his paper "Finitism" (1981) that PRA represents the limit of finitist definitions and proofs, while it differs from Kreisel's claim (in his 1960 paper cited above, and elsewhere) that a system equivalent in strength to PA is its limit. I conjecture that a system of strength PA can be shown to be the unfolding of NF augmented by a suitable quantifier-free form of rules for definition and proof by induction on well-founded orderings. Finally, while there are no definitive results yet for the unfolding of set theory, a framework for that has been provided in my paper, "Operational set theory and small large cardinals" (2006a). There are other obvious candidates of open-ended schematic systems for which the unfolding notion ought to be investigated.

I can be somewhat briefer concerning the mathematical path in the work on predicativity. In Hermann Weyl's work *Das Kontinuum* (1918) of his predicativist period, he explained how all of 19th century analysis of piece-wise continuous functions could be accounted for in predicative terms. Examination of Weyl's system showed that it could be formalized within a theory of finite types conservative over PA. By modifying this to a more flexible system W of variable finite types also conservative over PA, I was able to verify that much of 20th century functional analysis of Lebesgue measurable functions can be formalized in W. I was then led to conjecture that *all of scientifically applicable mathematics can be formalized in W*, and hence rests on a completely predicative basis; see "Why a little bit goes a long way. Logical foundations of scientifically applicable mathematics" (1993). That conjecture has been verified to a considerable extent for the main fundamental results in functional analysis. This is relevant to the Quine-Putnam *indispensability thesis* that led them to accept substantial portions of impredicative set theory as seemingly inextricably necessary for science. As I have written op. cit., "By the fact of the proof-theoretical reduction of W to PA, the only ontology it commits one to is that which justifies acceptance of PA." Moreover, as is well-known, the latter is reducible to the intuitionistic system HA of Heyting Arithmetic, which does not require any platonistic ontology whatever. Thus, in my view, "if one accepts the indispensability arguments, practically nothing philosophically definitive can be said of the entities which are then supposed to have the same status—ontologically and epistemologically—as the entities of natural science." My conclusion was that the indispensability arguments are thus completely vitiated.

3. How can one's choice of philosophy of mathematics dictate what it is right to do and say in mathematics, i.e. in its foundations? Consider the candidates on offer: formalism, finitism, constructivism, predicativism, logicism, nominalism, fictionalism, instrumentalism, platonic realism, structuralism, modal structuralism, scientific naturalism, mathematical naturalism, and quasi-empiricism, among others, including some in competing subvarieties. For those thinkers who have arrived at what they take to be the one true philosophy, the answer goes without saying. Moreover, among the pioneers to our subject such as Cantor, Frege, Brouwer and Hilbert, that stance was very efficacious in leading to substantial research programs. However, as those programs were developed, along with great strides they were marked by serious difficulties. Comparable programs nowadays that are being vigorously pursued by a number of adherents are Martin-Löf's constructive type theory, the Bishop school of constructivity, the large cardinal program in set theory, and categorical foundations. For these, the difficulties are of a different nature. Like predicativity, the first two require radical restrictions of what is admitted to mathematics, while the large cardinal program makes use of a radical extension; finally, the categorical program claims to usurp logical foundations. Most logicians have not committed to such definite philosophical views, since active debate between the various positions makes a choice between them difficult, and radical solutions are discomfiting. Among mathematicians, there is a widespread view that ongoing current mathematics on the whole is more reliable than any of the philosophically motivated programs that have been proposed to replace it, and that the only foundations that need be considered (if any at all) is organizational.

My own view lies between these extremes. First of all, the historical development of mathematics shows that *not anything goes*, that a number of notions, assumptions and supposed results have been found to be seriously problematic at one stage or another, e.g. infinitesimals, imaginary numbers, points at infinity, trigonometric series expansions of arbitrary functions, probabilistic arguments, etc., etc. In the past, mathematicians dealt with these on a case by case basis. In my article, "Working foundations" (1993a), I have argued that outside of the grand foundational schemes, what logic has had to offer in these days is work that is "a direct continuation by more conscious, systematic means of foundational moves which have been carried on within mathematics itself from the very beginning." While not driven by any particular philosophical view, these *foundational ways* are often usefully informed by philosophical distinctions.

The second thing I have been concerned with at a philosophical level is a critical examination of several foundational schemes, including categorical foundations, Lakatosian quasi-empiricism, and the large cardinal program, in the articles (among others) "Categorical foundations and foundations of category theory" (1977), "The logic of mathematical discovery versus the logical structure of mathematics" (1981), and "Why the program for new axioms needs to be questioned" (2000), respectively. I have already mentioned another critique of this character above, in connection with the Quine-Putnam indispensability thesis. In all of these, while not espousing a fixed *positive* philosophical position, I have brought to bear some fairly strong *negative* views. Limitation of space prevents me from going into any detail here about the content of these critiques.

But let me enlarge on the positive vs. the negative aspects. Because of my substantial involvement over the years in studying the concept of predicative definability and provability, some have assumed that my philosophical position is that of predicativism; this is definitely not the case. As I have written in the preface to my collection of essays, *In the Light of Logic* (1998), rather, "I am a convinced antiplatonist in mathematics. …according to the platonist philosophy, the objects of mathematics such as numbers, sets, functions and spaces are supposed to exist independently of human thoughts and constructions, and statements concerning these abstract entities are supposed to have a truth value independent of our ability to determine them. Though this accords with the mental practice of the working mathematician, I find the viewpoint philosophically preposterous…" (or, as I have written elsewhere, set-theoretical platonism is the "medieval metaphysics of mathematics"). To go on, "[i]t should not be concluded from this, or from the fact that I have spent many years working on different aspects of predicativity, that I consider it the be-all and end-all in non-platonistic foundations. Rather, it should be looked upon as the philosophy of how we get off the ground and sustain flight mathematically without assuming more than the basic structure of the natural numbers to begin with. There are less clear-cut conceptions which can lead us higher into the mathematical stratosphere …That such conceptions are less clear-cut than the natural number system is no reason not to use them, but one should look to see *where* it is necessary to use them and what we can say about what it is we know when we *do* use them."

4, 5. I shall neglect question 4 about "the most neglected topics and/or contributions in late 20th century philosophy of mathematics" and go on directly to question 5, "What are the most important open problems in the philosophy of mathematics and what are the prospects for progress?" Here are some rather general questions:

(i) Is the Continuum Hypothesis a definite mathematical problem?

(ii) What is a natural system and what is the interpretability order of natural systems?

(iii) What makes mathematics such a distinctive body of thought? What determines what counts as mathematics and what doesn't?

(iv) Is the structure of mathematics essentially logical in nature? If not, what is it?

(v) Is the use of formal systems an adequate model of mathematical practice?

(vi) How is it that mathematics is so successfully applicable to natural science? Does that depend on what part of mathematics is being applied?

Concerning (i), I came to the conclusion some years ago that CH is an *inherently vague* problem (see, e.g., the article (2000) cited above). This was based partly on the results from the metatheory of set theory showing that CH is independent of all remotely plausible axioms extending ZFC, including all large cardinal axioms that have been proposed so far. In fact it is consistent with all such axioms (if they are consistent at all) that the cardinal number of the continuum can be "anything it ought to be", i.e. anything which is not excluded by König's theorem. The other basis for my view is philosophical: I believe there is no independent platonic reality that gives determinate meaning to the language of set theory in general, and to the supposed totality of arbitrary subsets of the natural numbers in particular, and hence not to its cardinal number. Incidentally, the mathematical community seems implicitly to have come to the same conclusion: it is not among the seven Millennium Prize Problems established in the year 2000 by the Clay Mathematics Institute, for which the awards are $1,000,000 each; and this despite the fact that it was the lead challenge in the famous list of unsolved mathematical problems proposed by Hilbert in the year 1900, and one of the few that still remains open.

I have been asked to explain what I mean by the statement of a problem being inherently vague. The idea is that, not only is it vague, but there is no reasonable way to sharpen the notion or notions which are essential to its formulation without violating what the notion is supposed to be about. For example, the notion of *feasibly computable number* is inherently vague in that sense. And, for the statement of CH, the notion of arbitrary subset of N can't be sharpened to arbitrary constructible subset of N, or any specific relativization thereof, without violating the idea of *arbitrary subset of a set*, independent of any means of definition. I think progress can be made on elaborating the idea of inherently vague notions; whether that can be used to strengthen the case that CH is an inherently vague problem remains to be seen.

Concerning (ii), much has been made by workers in the metamathematics of set theory of the observed fact that all natural systems extending ZFC that have been considered are comparable by the relation of relative interpretability in Tarski's sense. Moreover, in many cases systems based on quite different principles turn out to have the same interpretability strength. It happens that large cardinal axioms have in all such cases figured as an essential link in establishing the interpretation (see, for example, Steel 2000, p. 227). The central role of large cardinal axioms in these phenomena has been taken to suggest that that they in some way reflect a pre-established harmony. Be that as it may, the phenomenon of linear ordering under interpretability has been observed to hold much more generally for *all* natural systems that have been considered extending certain very weak subsystems of arithmetic (cf. Friedman 2007, Section 7), and large cardinal axioms don't have anything to do with that more general phenomenon. So, really, the underlying question here is: *what is a natural system*? And then, if there is a reasonable answer to that question, what is the interpretability order and related orders (translatability, consistency, etc.) between such systems? Friedman points out (loc. cit.) that it is not linear among algebraic systems, e.g. the theories of discrete linear order without end-points and dense linear order without end-points are incomparable. Nor is it linear among finite extensions of arithmetic (hence f.i.p. systems), as shown in Lindström 1997, Ch. 7. However, the latter systems are not natural because they are "cooked-up" by means of arithmetization techniques. It would be quite remarkable, and might be considered some sign of the inner harmony of mathematics, if all natural systems extending arithmetic turned out to be linearly ordered by interpretability. If one believes that this should be the case, a search for counterexamples among candidate explications could be a first step toward narrowing down the informal concept of natural system.

Problems (iii)-(v) are interrelated, and may be connected with problem (vi). Let me conclude with some ideas about these. Discovery in mathematics is one of the highest exercises of creative intelligence. But confirmation of mathematical discoveries requires rigorous calculation and demonstration, and in this respect mathematics is logical at its core. Moreover, mathematics is progressive, it builds on what came before. Thus, since there can be no infinite regress, from the point of view of logic mathematics must rest ultimately on some sort of axiomatic foundations. While mathematicians may accept this in principle, there is a sharp dichotomy between the logicians' conception of mathematics and that of the practicing mathematician. The latter pays little or no attention to logical or foundational axioms, even if he or she subscribes to some overall foundational viewpoint such as that of axiomatic set theory. And in fact, the logical picture of mathematics bears little relation to the logical structure of mathematics as it works out in practice. The use of certain basic structures like the natural numbers and the real numbers (and of structures built directly from them like the integers, rationals and complex numbers) is ubiquitous, and there is constant appeal to such principles as proof by induction and definition by recursion on the naturals and of the lub principle for the real numbers. But these are not viewed from an axiomatic point of view, e.g. from that of the Peano Axioms for the naturals. The essential difference is that the language of PA is limited to a fixed vocabulary, whereas induction and recursion can be applied in *any* subject in which natural numbers play some sort of role. For example, the operation x^n is defined in any (multiplicative) semi-group for every element x and natural number n, and its properties are proved by induction on n. So even where the practicing mathematician invokes the basic axioms of the natural numbers, that is done without restriction to a fixed vocabulary. According to the current set-theoretical point of view, all such concepts that the mathematician might want to use in addition to those expressed in PA are defined in the language of ZFC, so we need only look no further in order to give full logical scope to what underlies daily mathematics. It seems however, that if we accept the language of set theory we ought to accept notions *not* defined in that language, such as the notion of truth in the set-theoretical universe. Moreover there are informal outlying notions that have mathematical coherence, but are not (as given) defined within set theory. One such, for example, is the notion of free choice sequence used by the Brouwerian intuitionists; this is separate from the fact that a formal system for f.c.s.'s can be modeled in set-theoretical terms. Another example of a mathematical notion that is not set-theoretically defined is the informal concept of randomness applied in various contexts, though again axiomatics of randomness has been modeled set-theoretically. Finally, when mathematics is applied to natural science, it makes direct use of physical concepts like force, mass, charge, etc., etc., that are evidently not expressed in set-theoretical terms at all. In my 2005 ASL lecture, "Open-ended schematic axiom systems" (2006), I have proposed an informal framework to account for mathematical practice and its actual and future possible applications in a more direct way than through the use of the various formal systems currently dominating logical work. This is work in progress, as an extension of my earlier work on unfolding of open-ended schematic systems. An essential new feature is the introduction of a quite general underlying "proto-mathematical" framework for operations and properties; that allows for the interaction of basic schematic systems like those for the natural numbers, real numbers, and subsets of any domain. I believe the emphasis on conceptual open-endedness will also provide a new perspective on the phenomenon of incompleteness which was the preoccupation above.

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