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Philosophy of Mathematics: 5 Questions

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1 Why I am a logician

In 1977, when I was nine years old, Doubleday released *Asimov on Numbers,* a collection of essays that had first appeared in Isaac Asimov's *Science Fiction and Fantasy* column. My mother, recognizing my penchant for science fiction and mathematics, bought me a copy as soon as it hit the bookstores. The essays covered topics such as number systems, combinatorial curiosities, imaginary numbers, and π. I was especially taken, however, by an essay titled "Varieties of the infinite," which included a photograph and short biographical sketch of Georg Cantor, and a brief introduction to his theory of the infinite.

Not long after, for my tenth birthday, my parents gave me a calculator. I had requested Texas Instruments' model TI-30, but my father surprised me with an SR-40, which had essentially the same functionality but came with a rechargeable battery. I did not know what all the buttons did, but I soon learned, from the manual, how to convert degrees to radians, and how to calculate the height of a flagpole from the length of its shadow and angle of inclination at the shadow's tip. This calculator instantly became one of my favorite toys.

What struck me about mathematics, at this early age, was this strange juxtaposition of fantasy and hard-nosed reality. It wasn't just that wild notions of the infinite and no-nonsense calculation could coexist within the same discipline; it was that they could be seen as complementary aspects of one and the same thing. When I was in elementary school and high school, "no-nonsense calculation" meant, for the most part, being able to write

*My answers to the five questions are distributed across the four sections of this essay.

computer games with fancy graphics. But, at the same time, I was generally aware of the importance of mathematics to pursuits like economic forecasting and aeronautical engineering. As an undergraduate at Harvard, I majored in mathematics, and took the requisite core of courses in analysis, algebra, and geometry. But I also took a number of courses in computer science, including courses in data structures and algorithms, computational complexity, and graphics. Working part-time during the year and full-time over the summers, I wrote data analysis and control software for physicists, which served to bolster my faith in the links between abstraction and application.

I applied to graduate school in mathematics without a clear sense of what I wanted to do, beyond a vague desire to understand mathematics in computational terms and vice-versa. In my first semester at the University of California at Berkeley, I took Jack Silver's introductory logic course, and I was impressed by the promising blend of syntax and semantics. At the end of the year, Silver presented a model-theoretic proof of the Paris-Harrington theorem, which shows that a certain combinatorial statement is independent of of first-order Peano arithmetic. Over the summer, I read a paper by Wilfried Buchholz and Stan Wainer bounding the rates of growth of the computable functions that can be shown to total in Peano arithmetic, and Robert Solovay and Jussi Ketonen's proof that witnesses to the Paris-Harrington statement cannot be so bounded. This was what I has been looking for: the place where lofty assumptions about the nature of the infinite come to bear on calculation. I decided to become a logician, and have never regretted that choice.

My use of the term "logician" here is deliberate. My interests lie in mathematics, philosophy, and computer science, and I have found institutional pressures to pigeonhole research under departmental labels to have had baleful effects on these subjects. Below, I will use the terms "mathematical logic" and "philosophy of mathematics" to describe sustained reflection on the goals and methods of mathematics, regardless of its disciplinary origin.

As will become clear, I identify myself most distinctly as a proof theorist, working in the tradition of Hilbert's program. The perspective that emerges from that program involves, first, modeling mathematical methods of argumentation in syntactic, axiomatic terms, and, second, using explicit, finitary methods to study the resulting notions of provability. Below, I will focus on the ways that such a modeling contributes to our philosophical understanding.

2 What we have learned

Popular presentations of the history of modern logic make much of the discovery of set-theoretic paradoxes at the turn of the century, Brouwer's intuitionistic challenge, the ensuing crisis of foundations, Hilbert's attempt to settle the question of foundations once and for all, and the devastating effects of Gödel's incompleteness theorems. This dramatic arc makes for a good story, but one that ultimately lacks depth, and renders the episode little more than light entertainment for contemporary mathematicians who do not lose sleep over whether their methods are consistent.

The real drama emerges in a broader historical context. Late nineteenth century developments brought radical changes to the very conception of what it means to do mathematics, and we are still living with the aftermath. It is a remarkable fact that work of Euclid, Descartes, Newton, Leibniz, Euler, and Gauss can still be read, profitably and with great enjoyment, today. But although we recognize their work as mathematics *par excellence*, it is mathematics of a certain character, with a more explicit computational flavor than is now common. It was not until the turn of the twentieth century that mathematicians began to view their subject as a general theory of structures, often characterized in set-theoretic terms, rather than as a theory of calculation, involving the symbolic expressions that are now taken to denote elements of the abstract structures. The set-theoretic paradoxes were only one pointed manifestation of a general worry that mathematics was taking a wrong turn; the broader concern was whether the new methods, even if consistent, were meaningful, and appropriate to the subject. Today, mathematicians strive for conceptual understanding and powerful methods of calculation, and these two aims sometimes exert distinct methodological pressures. My goal, as a logician and philosopher of mathematics, is to understand how mathematics manages to balance the two.

Too many philosophers today have the annoying habit of commending themselves for having the audacity to take on questions that are so hard as to be, perhaps, ultimately unanswerable. I can't imagine a poorer excuse for failure to make progress with one's research, and I worry that when the public at large begins to tire of such talk, the field will suffer a severe backlash. The contrapositive to the claim that philosophy deals with unanswerable questions is that in those happy situations where we find ourselves with satisfying answers to fundamental conceptual questions, neither the questions nor the answers can be counted as properly philosophical. That attitude, too, is unfortunate, because it prevents the field from taking credit for some important advances. In this section, I'd like to enumerate, briefly, some of the ways that mathematical logic and the philosophy of mathematics have contributed to our understanding of the methodological tension I have just described.

The syntactic, axiomatic standpoint has enabled us to fashion formal representations of various foundational stances, and we now have informative descriptions of the types of reasoning that are justified on finitist, predicative, constructive, intuitionistic, structuralist, and classical grounds. The positions can be cast in different ontological terms, for example, in the language of set theory, first- or higher-order arithmetic, or type theory. By now, we understand these frameworks quite well. We have come to learn what sorts of things can and can't be done in the various foundational systems, and we have discovered a number of interesting relationships between them.

One of the things we have learned over the course of the twentieth century is that much of mathematics does not need strong set-theoretic axioms. Of course, a lot hinges on the interpretation of the word "need" in this assertion. Set-theoretic language is now pervasive, and few mathematicians have any qualms about quantifying over uncountably infinite domains. But although the general principles that are invoked are, indeed, logically strong, this strength can typically shown to be avoidable when one focuses on the particular ways in which the principles are used. In other words, foundational research has shown that one can generally describe workable formalizations of common mathematical developments in comparatively weak axiomatic theories. This sort of analysis tells us two things: first, that ordinary mathematical methods often have more constructive content than is immediately apparent, and, second, that logical strength alone does not account for all the benefits of modern methods, whatever these may be.

Throughout my career, I have been particularly interested in the ways that ordinary mathematical arguments can be understood in computational terms. Axiomatic theories can be used to model not just foundational stances, but also general patterns of reasoning, such as induction, compactness, and set existence principles like comprehension and choice. In studying such theories, I have made use of all the tools of the trade, including cut elimination, double-negation interpretations, realizability interpretations, and model-theoretic techniques. These provide ways of "reducing" classical theories to constructive ones, and "extracting" the computational and combinatorial content from nonconstructive arguments.

This work stands firmly in the proof-theoretic tradition. I do not have enough space here to provide an overview of the field, but I am pleased to see that many of those whose work I most admire have been asked to write for this collection. Some of the things I have done that I am especially proud of involve understanding how semantic arguments in logic can be understood in syntactic terms, and vice-versa; using forcing methods in various ways in service of proof-theoretic goals; and finding ways of representing infinitary mathematical methods in surprisingly weak theories. More generally, I have tried as best I can to sustain the core values of the proof-theoretic tradition, while at the same time addressing topics of contemporary concern.

3 What we are learning

The interdisciplinary and fundamentally reflective nature of mathematical logic is its greatest asset, but also its greatest burden. The field is often viewed as too mathematical to be philosophy, too philosophical to be mathematics, and too rarefied in both senses to find a home in computer science. Logicians, forced to justify their activities to deans and funding agencies, generally resort to one of two strategies: either we emphasize the intrinsic interest and importance of our work, or we emphasize the applications to other branches of mathematics and computer science.

Both strategies are reasonable, but also dangerous. Locating the importance of a logical development in its applications to a branch of mathematics, X, only passes the buck. One has to further explain why X, in turn, is so important, and then, given that it *is* so important, why one should not just forget about logic and focus on X instead. On the other hand, appeals to "intrinsic interest" fall flat when the interest is recognized by only a dozen or so logicians working in an isolated technical corner of the field, and otherwise opaque to the mathematicians whose basic methods and concepts are supposedly being illuminated.

The best work in mathematical logic arises when the two standards of success are met simultaneously, that is, when we are left with the impression that we have learned something fundamentally important and interesting about the nature of mathematics, and when that understanding leaves us better equipped to engage in our mathematical practices. Many proof-theoretic developments of the twentieth century have that character: syntactic modeling of mathematical methods and concepts not only helps us understand them better, but also supports algorithmic and computational efforts that rely on such models.

But the axiomatic studies of the twentieth century are only a start. An ordinary mathematician may prefer an elementary argument over a more conceptual one, for example, since the elementary argument provides explicit computational information. Telling that mathematician that the conceptual argument can be formalized in a weak subsystem of second-order arithmetic, and that our metatheorems tell us that such explicit information can always be extracted in principle, won't get you very far. Nobody cares much about what can be done "in principle"; logic is only interesting insofar as it tells us something about the mathematics that we actually practice. We therefore need to continue to refine our metamathematical modeling, remaining mindful of the foundational, conceptual, and methodological questions that make that modeling worthwhile.

I once heard Natarjan Shankar, a computer scientist, declare that we are in the "golden age of metamathematics." I like that phrase, and find it apt. It took a number of decades for the basic conceptual apparatus and terminology of mathematical logic to settle, and now, a century later, we have a powerful set of analytic tools are our disposal.

There are a number of ways that these tools can be put to good use. For one thing, proof-theoretic methods can be used to find useful information hidden in ordinary mathematical arguments. Georg Kreisel described the process of extracting such information as "unwinding proofs," and Ulrich Kohlenbach has more recently adopted the term "proof mining." The idea is simple. The proof-theoretic reductions described in the last section show that classical methods can often be interpreted in constructive, finitary, or otherwise explicit terms. One therefore need only apply the methods to particular proofs, and then harvest the epistemological gains. In reality, things are more complicated. Ordinary mathematical proofs are not presented in formal systems, so there are choices to be made in the formal modeling. In addition, the general metamathematical tools have to be tailored and adjusted to yield the information that is sought. But Kohlenbach and his students have had a number of striking successes in fields like numerical analysis and fixed-point theory, and researchers like Thierry Coquand have obtained interesting results by applying general insights from constructive mathematics to particular problems in algebra. I have been working with Philipp Gerhardy and Henry Towsner to apply similar methods in the realm of ergodic theory and combinatorics. The field is young, however, and it is only beginning to attract the attention it deserves from the logical and mathematical communities.

Another place where proof-theoretic methods and insights can be put to use is in the field of automated reasoning and formal verification. Since the early twentieth century, it has been understood that ordinary mathematical arguments can be represented in formal axiomatic theories, at least in principle. The complexity involved in even the most basic mathematical arguments, however, rendered most formalization infeasible in practice. The advent of computational proof assistants has begin to change this, making it possible to formalize increasingly complex mathematical proofs. Such methods are now being used to verify that descriptions of hardware and software components meet their specifications, something that is especially important when lives depend on their proper functioning. But the methods can also be used for the more traditional task of verifying ordinary mathematical proofs, and are especially pertinent to cases where proofs rely on computation that is too extensive to check by hand.

In 2004, I earned a measure of recognition from the formal verification community by verifying a proof of the prime number theorem, with the help of some students at Carnegie Mellon. Soon after, Georges Gonthier announced a verification of the four color theorem, and Thomas Hales announced a verification of the Jordan curve theorem. Hales has moreover launched an ambitious project to verify his proof of the Kepler conjecture, and Gonthier is currently overseeing a project to verify the Feit-Thompson theorem. Success in these endeavors requires, among other things, having the computer be able to verify straightforward mathematical inferences, without requiring the user to spell out the details. Getting computers to do so, in turn, requires a logical modeling and classification of the relevant inferences, and the development of procedures that can fill in the details automatically.

These are just some of the ways in which a proof-theoretic understanding can support computational developments in mathematics. Fields like computational algebra, computational number theory, computational ergodic theory, and so on, are persistent attempts to retrofit computational significance to mathematical developments that have developed free from computational concerns. A proof-theoretic perspective should be especially helpful when the computations themselves involve propositional data, in the tradition of Leibniz' "calculemus!" For example, in computer algebra systems, proof-theoretic methods can be used to manage constraints that can be used to simplify complex expressions. Syntactic methods of quantifier elimination play a key role in real algebraic geometry, where one wishes to manipulate sets and functions that can be described with prescribed linguistic resources. Quantifier elimination has also played an important role in the study of valued fields, and is a key component in theories of motivic integration. Within logic, the study of definability is often relegated to model theory and descriptive set theory, but computational concerns encourage a proof-theoretic perspective as well.

Yet another domain where a syntactic, foundational perspective is important is in the search for natural combinatorial independences, that is, interesting combinatorial principles that are independent of conventional mathematical methods. Harvey Friedman, in particular, has long sought to find exotic combinatorial behavior in familiar mathematical settings. Such work gives us glimpes into what goes on just beyond ordinary patterns of mathematical reasoning, and yields interesting mathematics as well.

4 What we have yet to learn

I have tried to show that mathematical logic has a lot to contribute to mathematics. But is that philosophy? Writing down axioms doesn't provide a sense in which these axioms provide *bona fide* knowledge, or tell us what the basic terms of the discourse refer to. Isn't that what philosophy is supposed to do?

Let's not downplay our philosophical gains. The axiomatic method clarifies the ontological commitments that are presupposed in mathematical developments, and tells us what can and cannot be done on the basis of those commitments. Proof-theoretic analysis also yields satisfying philosophical explanations as to how abstract, infinitary assumptions have bearing on computational concerns, and provides senses in which infinitary methods can be seen to have finitary content. Even if what you really want is a theory of mathematical knowledge and justification, it is hard to see how one can justify any sort of methods without a meaningful articulation of what these methods are supposed to accomplish, and an analysis of how the methods are suited to the goals. We do not need fairy tales about numbers and triangles prancing about in the realm of abstracta. What we need is serious thought about what it means to do mathematics, and why we do it.

In *The Problems of Philosophy*, Bertrand Russell noted that whenever philosophical inquiry reaches a stage where the questions can be posed with sufficient clarity to admit precise answers, then the inquiry is commonly viewed as scientific rather than philosophical. There may be some merit to this distinction: some of the most interesting philosophical developments arise in situations where we find that the conceptual resources available do not give us a sufficient grip on questions that force themselves upon us. In this section, I will focus on such situations.

To start with, we need more robust and informative models of mathematical proof. Coming on the heels of my praise of the axiomatic method, this claim may seem surprising. But one need not deny the successes we have had with the methods of contemporary logic in order to recognize that there are questions that these methods were not designed to address. One particularly salient one is the problem of multiple proofs: on the traditional logical story, the purpose of a proof is simply to warrant the truth of the resulting theorem, leaving it utterly mysterious why it is often the case that we are pleased to find a new proof of the same theorem. Of course, any mathematician will tell you that we learn different things from different proofs. So the question is by no means "unanswerable." On the contrary, we have lots of good ideas about how the answer should go. My point is simply that our logical theories currently have little to say about the matter, and that it will take honest philosophical work to fashion our intuitions into something precise.

Much of the difficulty stems from the fact that there is a wide gap between ordinary mathematical proofs and the logician's formal derivations. I have argued, in an essay titled "Mathematical method and proof," that although formal axiomatic systems provide good normative and descriptive accounts of the standards by which we judge proofs to be correct, the models are not rich enough to support other types of common evaluations. For example, we often talk about different styles of proof. Some proofs are algebraic, while others are geometric; some are abstract, while others are more concrete; some are explicitly computational, while others are more structural, or "conceptual"; and so on. It is difficult to explicate these characteristics with the formal axiomatic model, on which every proof can be viewed as a derivation in axiomatic set theory. It is not just a matter of taking definitions seriously, and noticing whether a proof uses the terms "complex number" or "algebraic variety." What really matters is the theoretical scaffolding, the conceptual resources at play, the ways that problems and goals are posed, and the general methods of analysis. It is these fuzzier aspects of proof that are ripe for logical and philosophical analysis.

The questions we ask need not always be cast in terms of proof. Mathematicians also solve problems, build theories, pose problems, formulate conjectures, introduce concepts, and model empirical data. Understanding mathematical proof will require understanding these types of activities as well. For example, making sense of the ways that we value certain mathematical proofs requires a better sense of why we value the resulting theorems, and making sense of the roles that concepts play in a mathematical proof requires a better sense of the roles that concepts play more generally.

Such reflection is germane to the "scientific" research I described in the last section. In proof mining, one has has to manipulate ordinary mathematical proofs, with sensitivity to the types of information that is commonly sought. In formal verification, ordinary mathematical proofs are, again, the focus of attention. The goal of research in combinatorial independences is to find logically strong principles that are "natural" and "interesting." Such research can therefore be supported by a better understanding of ordinary mathematical practice, and, conversely, more faithful modeling of that practice can provide a better understanding of mathematical epistemology.

Every time I write about these topics, I feel compelled to conclude with a rallying cry and a plea for more troops. Mathematics is a fascinating subject, and there is a wealth of insight to be gained from disciplined reflection on its methods. But the fundamental questions are too broad, and the subject too deep, for a handful of heterodox logicians to carry the day. This is a vast undertaking, and there is great progress to be made. Please, join the cause.