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

Like many philosophers of mathematics, I suspect, I started out in mathematics and drifted into philosophy. My undergraduate work was mostly in pure mathematics. I loved (and still love) algebra, functional analysis, topology, and complex analysis. While immersed in the many elegant proofs in these areas, I began wondering about the nature of proof and mathematical truth. I thus became interested in, and began studying, logic and philosophy as complements to my mathematics major. I completed a Bachelor of Science with honours in pure mathematics, with a thesis on the Dirichlet problem in potential theory. By the time I finished honours, I was fascinated by the many philosophical issues arising in mathematics. Some of those that attracted my attention were: (1) the existence or non-existence of mathematical objects, (2) the applications of mathematics in empirical science and (3) the question of proof and the status of axioms such as the axiom of choice. Around this time I read Hartry Field's *Science without Numbers* (1980) and Penelope Maddy's paper 'Indispensability and Practice' (1992). After reading these two pieces, there was no turning back. I switched from mathematics to philosophy. I started a PhD in the philosophy of mathematics at the Australian National University, where I was very fortunate to have Jack Smart as my supervisor. Jack's genuine, on-going fascination with science and mathematics, and his passion for learning has been an inspiration to me. Indeed, not long after starting my graduate work, Jack expressed interest in supervising me because he had always wanted to work on the philosophy of mathematics and then in his retirement, he finally had the time to come up to speed in this area. We thus worked though the contemporary literature—Maddy (1990), Field (1980, 1989) etc.—together. It was a wonderful experience and I am very pleased to say that Jack too has written up some of his thoughts on the philosophy of mathematics (Smart manuscript).

During my time as a graduate student I was fortunate enough to meet, and spend time with, Hartry Field, Penelope Maddy, and Mike Resnik. These three, in particular, have been enormously influential on my intellectual development. They set very high standards in their own work, providing shining examples of how good philosophy should be done and how such philosophy can genuinely advance debates in the philosophy of mathematics (and in mathematics itself). They were also very generous with their time and ideas. Mike Resnik, for instance, effectively steered me towards my thesis topic: a defence of the Quine-Putnam indispensability argument. (This is an argument that we ought to take mathematical objects to exist because of the indispensable applications of mathematics in empirical science (Quine 1981, Putnam 1979, Colyvan 2001).) Initially I thought Mike was crazy. A whole thesis on *one* argument! Surely, I thought, I'd need to do more than that. But Mike's advice was right on the money. Not only did he direct me to a *manageable* thesis topic, he directed me to a topic of considerable contemporary signigicance. Although my research interests have since broadened, I continue to work on the indispensability argument and other topics in the philosophy of mathematics. This is in part a result of the ongoing intellectual engagement with the likes of Hartry Field, Penelope Maddy, Mike Resnik, Jack Smart, and many others (indeed, many of the philosophers interviewed for this volume).

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

Where do I begin? Mathematical methods are useful almost everywhere in philosophy. There are many obvious examples in the more technical areas of philosophy, such as philosophy of science, philosophy of mathematics, philosophical logic, decision theory, and formal epistemology. But there are also many less-technical areas where mathematics has been usefully employed to shed light on philosophical problems. Let me give an example of the latter from a paper I wrote with Jay Garfield and Graham Priest (Colyvan et al. 2005) on fine tuning arguments for the existence of an intelligent designer. (Also see some of Elliott Sober's terrific work on more general design arguments, for example, Sober 2002. On a related philosophy of religion theme, see Alan Hájek's (2003) fascinating discussion of Pascal's Wager—especially the discussion of the non-standard analysis angle.)

Design arguments draw attention to some feature of the world and suggest that the feature in question would be unlikely without the intervention of an intelligent designer. Various features of the world have been put forward as the focus of design arguments: the mechanical clock-like universe and complex biological structures. Biology was an especially popular focus for design arguments before Darwin's time (although sadly almost 150 years after the publication of *The Origin of Species* such arguments are still with us). Recently a physics-based version of the design argument has reared its head. This latter argument is usually known as *the fine-tuning argument*. Here the special feature of the universe that allegedly is in need of explanation is the existence of carbon-based life. It is noted that the universes seems to be fine tuned for the emergence of such life: had things been just slightly different, carbon-based life could not have evolved. For example, (for reasons that need not concern us here) had the fine-structure constant been even a few percent from its actual value, there would be no carbon molecules and hence there would be no carbon-based life. So, the argument continues, the universe as we find it (i.e. *with* carbon-based life) is improbable and this improbable state of affairs requires an explanation. The final move in the argument is to invoke an intelligent designer to provide the required explanation. This designer is presumably predisposed to value carbon-based life and is usually taken to be something like the Judeo-Christian god. Putting aside the obvious dubiousness of the last move, consider the argument up to the conclusion that an intelligent designer exists. Almost every premise and inference of even this part of the argument can be challenged, but one rather interesting mathematical challenge is right at the start, at the move from the universe being fine tuned to the universe being improbable.

Let's look at this move a little more closely. Presumably the idea is that the fine-structure constant, say, could have taken a value from a large range of real numbers and yet only a small subinterval in this range will permit carbon-based life. But before we can move to assigning even qualitative probabilities, we need to know something about the probability distribution in question. Of course, if the proponents of this argument are supposing that the fine-structure constant could have taken any real number as its value and all such values are equally likely, then the argument is fatally flawed. There simply can be no uniform distribution over an unbounded set like the reals (or even the positive reals). So how do we get to the improbability claim? The claim must be that the distribution in question is not uniform or that it has compact support. But that's not enough. We also require that the shape of the distribution is such that it delivers a low probability to the carbon-based-life-permitting interval. Typically, none of this is argued for by proponents of the fine-tuning argument. Indeed, some of the discussions in this area are mathematically very naïve, bogging down in extended and misguided treatments of how to give frequency interpretations of the probabilities in question, rather than adopting the standard measure-theoretic approach.

These mathematical reflections on the problem don't provide a knock-down argument against all fine tuning arguments, but they do block many of the less careful presentations. These reflections also make it clear what proponents of these arguments need to demonstrate. I can't help stressing how difficult their task is though. They need to argue that the distribution in question has exactly the right shape to ensure that the carbon-based-life-permitting interval has low (but non-zero) probability, and that this probability is higher under the assumption of an intelligent designer. Moreover, they need to show that the intelligent designer is the best explanation of the presence of carbon-based life in the universe. The latter, of course, is highly non-trivial. To take one often-overlooked hypothesis that strikes me as very plausible: the distribution in question is such that it has most of its density over the carbon-based-life-permitting interval. Given that we know next to nothing about this distribution (hence the aforementioned assumption of the distribution being uniform in many naïve presentations of the fine-tuning argument), we can use the evidence that there is carbon-based life to support my suggested alternative hypothesis about the shape of the distribution. A little bit of mathematics and these design arguments start to crumble.

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

My view about the relationship between science and philosophy is a naturalistic one, where I take the role of philosophy as that of helping to understand science from within the scientific enterprise. This does not, for example, mean that philosophy is merely a powerless public servant, rubber stamping all and only the pronouncements of our current best science. Philosophy has an important role here, providing details of a plausible epistemology (for example, by providing an account of scientific confirmation), shedding light on metaphysical issues (for example, about the nature of causation), and critiquing science and subjecting it to scrutiny (for example, by examining the philosophical underpinnings of the statistical methods used in various branches of science). There is nothing new in any of this. This view of philosophy's relationship to science goes back a long way (at least to Russell) and was made famous and elegantly articulated by W.V.O. Quine in a number of places (for example, in Quine 1981).

How this naturalistic attitude plays out in relation to mathematics and logic is a little more complicated, but the basic idea is the same. The philosophical enterprise in relation to logic and mathematics is to provide a satisfying epistemology and, amongst other things, to make sense of current debates about the appropriate logic and new candidate axioms for set theory. Penelope Maddy (1997), in particular, has done some important work on the 20^{th} Century debates over the axioms of ZFC set theory and the contemporary debate over new axioms. She takes a naturalistic approach and, in passing, shows how such an approach can genuinely advance both philosophy and mathematics.

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

Let me start with a topic that was neglected but has since become one of the main foci of contemporary philosophy of mathematics: the applications of mathematics. The original motivation for much of the work on the applications of mathematics was the Quine-Putnam Indispensability Argument. Hartry Field, Penelope Maddy, and other critics of this argument were led to examine, in detail, specific applications of mathematics in science. This worked has helped focus the debate about the indispensability argument, but it has also been interesting in its own right. Irrespective of your metaphysical leanings, an important fact about mathematics is that it finds widespread and diverse applications in science: from group theory in fundamental particle physics, to differential equations in population ecology. The recent work on applications, begun by Field (1980, 1989) and Maddy (1997), and, in a different context, by Steiner (1998) and Wigner (1960), has resulted in what might even be considered a new area—the philosophy of applied mathematics. This area has been very fruitful and has helped shift, or at least broaden, the focus of the philosophy of mathematics from the more traditional, foundational questions about pure mathematics. These traditional questions are, of course, still interesting and deserving of attention, but focusing on pure mathematics and its foundations, in isolation, I think, is to miss an important part of the story. Pure mathematics finds applications elsewhere in mathematics and in science. And as I have argued elsewhere (Colyvan 2001a), philosophers of both realist and anti-realist stripes need an account of applied mathematics, so this no place for passing the buck on the required philosophical work. Happily, this work on applications is now well and truly under way and forms a significant thread in contemporary philosophy of mathematics. I should add that this work on applications also has interesting connections with related work in the philosophy of science on idealizations in scientific models.

Now to one of the neglected topics in the philosophy of mathematics. The old debate about the correct logic for mathematics never moved past intuitionism versus classical logic, but in the context of inconsistent mathematical theories there is the issue of paraconsistent logic versus the rest. A paraconsistent logic is one in which *ex contradictione quodlibet* (or "explosion") fails. That is, unlike classical logic, a paraconsistent logic does not allow the derivation of an arbitrary sentence from an arbitrary contradiction. Such logics have important applications, such as in modeling inconsistent agents and as insurance in large, possibly inconsistent, data sets (so that the contradictions don't spread and trivialise the data set). But there are also important applications in the philosophy of mathematics. Let me briefly mention a couple.

There have been times when we've been forced to work with inconsistent mathematical theories (for example, naïve set theory and, arguably, the early calculus). What is more, we were forced to work with these theories at times when they were known to be inconsistent. The first point to note is that using classical logic and taking an explicit statement of the contradiction as an assumption, a proof of any mathematical sentence can be derived in a few lines. But, perhaps unsurprisingly, such cheap proofs were not taken to be legitimate. What does this suggest about the logic of mathematics? Is it paraconsistent? Or is the logic classical but with further pragmatic constraints imposed to avoid the cheap proofs just mentioned? It is also interesting to note that the two contradictory theories I just mentioned (the early calculus and naïve set theory) were major mathematical theories with wide-ranging applications. This gives rise to another question: how is it that an inconsistent theory can be successful in its applications?

Another application of paraconsistent logic in the philosophy of mathematics is to model unashamedly inconsistent theories, such as those discussed by Bob Meyer and Chris Mortensen (1984), Chris Mortensen (1995), and Graham Priest (1997, 2000). Such theories have a number of virtues: they are non-trivial (in the sense that despite their inconsistency, not every sentence is provable), and they do not need to be incomplete. (After all, Gödel's incompleteness theorems, show that we must choose between consistency and completeness.) Even if we prefer our mathematics to be consistent (and we can find reasons to suppose that the theories in question are in fact consistent), inconsistent mathematics theories still have considerable interest. For example, the mere fact that inconsistent mathematical theories can find wide-spread applications in empirical science suggests that any philosophical account of the applications of mathematics in science had better not lean too heavily on consistency to do the work. It would seem that consistency is neither necessary nor sufficient for applicability. Thus far, only a few people (mostly paraconsistent logicians) have worked on these issues and they are yet to find their way into mainstream philosophy of mathematics. But I'm hoping that will change!

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

There are many important questions in the philosophy of mathematics and almost all of them are open: how does an (apparently) a priori discipline like mathematics find applications in empirical science?; what is the appropriate logic for mathematics?; what status should we give to non-trivial inconsistent mathematical theories?; what is the appropriate attitude to have towards the posits of mathematics?; is mathematics a science of structures (see Resnik, 1997 and Shapiro, 1997), what should count as an appropriate standard of rigour, and can picture proofs meet such standards (see Brown 1999), and many others. Here I'll say a little about the issue of explanation in mathematics and its relationship to theories of explanation elsewhere in science. This issue has received relatively little attention from philosophers of mathematics (although see Baker 2005, Colyvan 2007, Mancosu 2001, Resnik and Kushner 1987, and Steiner 1978).

It is generally thought that some proofs of theorems are explanatory while others are not. Indeed, two different proofs of the one theorem may differ in their explanatory power. The idea is that anyone who properly understands an explanatory proof of a theorem, understands *why* the theorem in question is true. While a full understanding of an non-explanatory proof merely convinces one that the theorem is true, but does not give any insight into why it is true. It is sometimes thought that this is just another way of marking the constructive–non-constructive distinction, but that's not right. Some non-constructive proofs are explanatory and some constructive proofs fail to be explanatory. The problem for the philosophy of mathematics is to give an account of the notion of explanation at work here. It clearly can't be any causal account of explanation, such as those dominating discussions of explanation in the philosophy of science literature. According to the causal account of explanation, providing an explanation of an event is (roughly) to trace the event's causal history. No matter what your theory of causation is, a mathematical truth cannot be explained in terms of causal histories. There is no causal chain that has as its end point Green's Theorem. Proofs, whatever they are, are not narratives about causal histories.

Does this mean that mathematical explanation is different in kind from explanation elsewhere in science? I don't think so. Apart from a prima facie case (driven by simplicity considerations) that there ought to be just one account of explanation, irrespective of the domain in which the explanations arise, there is a more substantial reason for insisting on a unified account of explanation. Often the explanation of a mathematical result, such as Green's Theorem, can "spill over" into empirical science. There are many physical systems (for example fluids flowing through a region of space) that can be modelled by differential equations and the explanation of certain features of this system will be provided by the relevant mathematical results. (For example, the proof of Green's theorem will provide the explanation of the relationship between the flow in the interior of the region and on the boundary of the region.) So if we are to treat mathematical explanation as different in kind from explanation in physics, we will also, it seems, need to countenance two kinds of explanation in physics. That, of course, is not to say that such a piecemeal account of explanation is not right or can't be made to work, but it does suggest that the possibility of a unified account is worth exploring.

One initially promising candidate for a satisfying account of explanation in mathematics is the unification account of explanation (Kitcher 1981, Friedman 1974). According to this account, an explanation is just a unification of the phenomenon in question. Think, for example, of how one of the jewels of complex analysis, the Residue Theorem, generalizes Cauchy's Integral Theorem and provides a means for calculating the integrals of many real-valued functions. Or to take another example, consider how, Euler's formula, $e^{ix} = \cos x + i\sin x$ (where $i = \sqrt{-1}$ and x is any real number), unifies analysis and trigonometry. Indeed, our understanding of both analysis and trigonometry is significantly enhanced by such results in complex analysis. According to the unificatory account of explanation, these unifications are genuinely explanatory. Although there are various objections to the unification account of explanation, it seems the one account with any chance of success in mathematics and hence the only account with any chance of yielding a unified account of explanation across both mathematical and other scientific domains. Or so it seems to me, at least. In any case, I think that there is a great deal of interesting work yet to be done on mathematical explanation and its relationship to explanation in the broader scientific context.

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