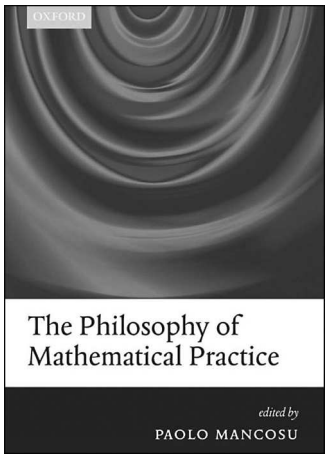
Book Review



The Philosophy of Mathematical Practice

Reviewed by Timothy Bays

The Philosophy of Mathematical Practice
Edited by Paolo Mancosu
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For mathematicians, modern philosophy of mathematics may seem somewhat puzzling. In the late nineteenth and early twentieth centuries, the borders between philosophy and mathematics were porous. Influential mathematicians like Poincaré, Brouwer, Ramsey, and Hilbert wrote extensively on philosophical topics, and philosophers like Russell, Wittgenstein, and Quine made serious philosophical use of contemporaneous mathematics. More importantly, the issues that concerned philosophers of mathematics were often continuous with developments in mathematics proper—e.g., the rigorization of analysis in the nineteenth century, the discovery of the set-theoretic paradoxes and Gödel's incompleteness theorems, or Bourbaki's project of systematizing mathematics as a whole.

In recent years, mathematics and the philosophy of mathematics have become somewhat more distant. The topics that most exercise philosophers of mathematics—realism and anti-realism, the metaphysics of structuralism, or arguments concerning the indispensability of mathematics to natural science—don't connect very well with the day-to-day concerns of practicing mathematicians. This is not to say that philosophy of mathematics has become insular and unmotivated. It's just that its connections tend to be to other areas of philosophy—to metaphysics, epistemology, and the philosophy of language in particular—and its motivating problems tend to be purely philosophical. Further, the mathematics one needs to address these problems is often quite rudimentary—in many cases, reflection on elementary arithmetic and geometry is enough to make philosophical progress. For philosophers, therefore, there's often little motivation to engage with the parts of mathematics which are of most interest to practicing mathematicians.[1]

Of course, some philosophers *have* worked on mainstream mathematical topics, and in some cases—Imre Lakatos and Philip Kitcher, in particular—their work has been well received by the larger philosophical community. But, in all too many cases, philosophers pursuing this kind of work have seemed more focused on discussing cutting-edge mathematical *topics* than on generating genuine philosophical results from those topics. This work has been less well received. Like mathematicians, philosophers can be pragmatists: it's not enough to *talk about* exciting mathematics; you have to show that there's a properly philosophical payoff to the discussion.

Timothy Bays is an associate professor of philosophy at the University of Notre Dame. His email address is `timothy.bays.5@nd.edu`.

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[1] *This point needs to be emphasized. I've occasionally heard mathematicians suggest that certain questions in the philosophy of mathematics are ill-motivated (or even pointless!) simply because they lack immediate relevance to the day-to-day concerns of working mathematicians. In my view, this is a mistake. It's the equivalent of dismissing work in the philosophy of language on the grounds that such work often lacks immediate relevance to working novelists. More pointedly, I think it's the equivalent of dismissing work in more abstract branches of mathematics on the grounds that this work lacks immediate applications to, say, aerospace engineering.*

This brings us to Paolo Mancosu's new collection, *The Philosophy of Mathematical Practice.* The authors of the papers in this collection share a vision of the philosophy of mathematics that is at once attentive to the history and practice of working mathematicians *and* to the necessity of making this history and practice philosophically relevant. Their papers have several distinctive features. First, they address a collection of philosophical issues that have been underdiscussed in the recent philosophical literature—e.g., the role of visualization in mathematical reasoning, the idea that some proofs are more "explanatory" than others, or the relevance of category theory to philosophical structuralism. Second, they treat a broader range of mathematical history and practice than has been customary in philosophical contexts: not just logic and arithmetic, but category theory, complex analysis, algebraic geometry, and number theory as well.[2] Finally, they insist on the relevance of this mathematics, not only to the new topics this volume aims to introduce, but also to classic topics in traditional philosophy of mathematics.

The collection begins with a brief introduction in which Mancosu situates the project against some other recent attempts at developing a more mathematically sophisticated philosophy of mathematics.[3] For mathematicians, this introduction will provide a useful guide to some of the most interesting work in recent philosophy of mathematics along with a picture of the larger philosophical milieu in which that work (and this volume) are embedded. Following the introduction, the book divides into eight sections, each of which contains a general introduction to a topic followed by a more focused research paper that develops one aspect of that topic in more depth. For reasons of space, I will group these sections under three general headings, and I will say more about the research papers than about the introductions.

[2] *These last two points should not be misconstrued. Neither the philosophical nor the mathematical topics discussed in this book are entirely original. Most of them have been discussed before (and often by these very authors!). Instead, the book is an attempt to highlight this kind of work by collecting some of its best practitioners together in one volume.*

[3] *Mancosu focuses, in particular, on recent work by Lakatos, Kitcher, Corfield, and Maddy. Most mathematicians will, I suspect, be familiar with Lakatos. Kitcher emphasizes the importance of historical studies of the growth of mathematical knowledge and of the changes in mathematical methodology and practice over time. Corfield argues that philosophers should pay more attention to "real mathematics"—to the mathematics that leading mathematicians are most interested in and to the ways that the mathematical community structures and conceptualizes its most central research programs. Maddy highlights the significance of informal aspects of mathematical justification—e.g., the sophisticated but nonrigorous arguments that set theorists give for accepting and/or rejecting new set-theoretic axioms.*

What We See

The first two sections concern the use of diagrams and visualization in mathematics. In practice, these are things we use all the time: we draw pictures on the blackboard for our students, we include diagrams in our papers, and we make sketches for ourselves when trying to think through a proof. Since the nineteenth century, however, both mathematicians and philosophers have questioned whether such practices should play any *formal* role in mathematics. After all, geometric diagrams can be both imperfect and atypical, and the use of such diagrams seems to have led traditional geometers to overlook important axioms—e.g., axioms of completeness. As even Roger Nelson, the author of *Proofs without Words,* has insisted: "of course, 'proofs without words' are not really proofs."

The first two papers, by Marcus Giaquinto (University College London), concern visual reasoning in mathematics. Giaquinto's introductory paper highlights some of the (many) ways we use visualization: to help us prove theorems, to discover and motivate new results, and to deepen our *understanding* of particular pieces of mathematics. In the end, he argues that visual reasoning can play a legitimate—and an important—role in grounding both proof and understanding. His research paper explores the more specific role that visualization plays in grasping *particular* mathematical structures. Using some recent work in cognitive science, Giaquinto claims that we can, in fact, use visualization to gain knowledge of small finite structures, and he argues that this process can be extended to get a grasp on the structure of the whole set of natural numbers. He then explores how far this kind of visual thinking can take us—probably to the ordinal ω^2, less probably to the ordinal ω^ω, but almost certainly not to the whole set of real numbers.

Ken Manders (Pittsburgh) focuses on classical Euclidean geometry. In his introductory paper, he notes that whatever worries we *now* have about diagrammatic reasoning, diagram-based geometry was an extraordinarily successful mathematical practice for over 2,000 years. The success of this practice—the fact that it didn't run aground on the problems we have now become aware of—is something that philosophers need to explain. Manders's research paper explores the kinds of "diagram discipline" that enabled traditional geometers to avoid fallacies and to *legitimately* draw inferences from their diagrams. So, for instance, Manders distinguishes between *exact* features of diagrams—features, like the straightness of a line

or the equality of two angles, that are unstable under even slight perturbations of the diagram—and *coexact features*—features, like the inclusion of one region in another or the intersection of two lines, that remain stable under appropriate (small) perturbations. He notes that, in practice, traditional geometers inferred only coexact attributes from a diagram and that the restriction to co-exact inferences explains the reliability of classical geometric reasoning. He also provides an illuminating discussion of indirect reasoning in geometry—reasoning in which the diagrams are *by design* inaccurate since they are supposed to represent geometric situations that cannot be instantiated. This paper is a modern classic that has circulated informally since the mid-1990s and that is published here for the first time; it is rich and deep and will repay careful, repeated reading.

What We Care About

Many philosophers have a simple picture of mathematics that focuses almost entirely on the issues of truth and proof: we start with simple, self-evident axioms, and we then ask whether various theorems can be proved from those axioms. For this picture, the only interesting questions involve the security of our axioms, the reliability of our proofs, and the ultimate truth of our theorems. Of course, this story misses much of the texture of day-to-day mathematical practice: theorems can be deep or shallow, definitions can be natural or unnatural, results can be constructive or conceptual, proofs can be more or less explanatory, etc. The next three sections of Mancosu's book look at some of these less formal—but no less important—ways that mathematicians evaluate their work.

The first two papers, by Mancosu himself and by Mancosu and Johannes Hafner (Berkeley and NC State, respectively), look at the notion of mathematical explanation, both in the sense of using mathematics to explain results in other disciplines—e.g., physics or economics—and in the sense of explanation within mathematics itself. Mancosu's introductory paper provides a short, but very clear, survey of some of the best recent philosophical work on these topics. As with his introduction to the volume as a whole, this paper will be particularly useful as a guide to those mathematicians who would like to read further in contemporary philosophy of mathematics. Mancosu and Hafner's research paper focuses on a model of "explanation as unification" that was developed by Philip Kitcher (and was explicitly intended to be applicable to mathematics). Using a test case from real algebraic geometry, they argue that Kitcher's model fails to capture many of the judgments about explanation that mathematicians actually make.[4] They conclude that, even if Kitcher's model is on roughly the right track, it will need substantial revision, guided by far more detailed analyses of a far wider range of mathematical test cases, before it can provide an adequate account of mathematical explanation.

The next two papers, by Mic Detlefsen (Notre Dame) and Michael Hallett (McGill), involve an issue that has come to be known as "purity of method"—roughly, the desire that mathematicians sometimes express to prove results in a *particular* branch of mathematics using only techniques *proper to* that branch of mathematics. By way of example, consider mid-twentieth-century number theorists' interest in obtaining a purely number-theoretic proof of the prime number theorem—i.e., a proof that does not use complex analysis. Or, consider the desire of group theorists to obtain a group-theoretic proof of Burnside's $p^a q^b$ theorem—a proof that does not appeal to representation theory. Finally, and most famously, consider nineteenth-century concern over the *casus irreducibilis*—the fact that when we use radicals to extract real roots of a cubic polynomial with rational coefficients, we often have to detour through the complex plane (even in cases where all of the roots of our polynomial turn out to be real).

Mic Detlefsen's paper provides a nice history of purity concerns in mathematics. He starts with Greek attempts to eliminate mechanical reasoning from geometric proofs, works forward through nineteenth-century attempts to remove geometric reasoning from analysis, and ends with some contemporary cases where mathematicians have expressed an interest in purity (e.g., the prime number theorem example from the last paragraph or the search for an "elementary" proof of the Erdös-Mordell theorem). Along the way, he discusses the *different* reasons mathematicians have given for pursuing purity and the various formal and epistemological virtues that pure proofs might be thought to have. Michael Hallett's paper follows this discussion with a rich and detailed study of the role purity played in Hilbert's axiomatization of geometry. Clearly, Hilbert had some purity concerns here—e.g., he wanted to eliminate

[4] *The case involves using the Tarski-Seidenberg decision procedure and/or the Tarski-Seidenberg transfer principle to prove general theorems about real closed fields. The transfer principle, for instance, allows us to use special properties of the reals—say, the Bolzano-Weierstrass property or the least upper bound property—to prove theorems about the real field and to then "transfer" these theorems to other real closed fields. This works even when the other fields lack the special properties used in the initial proofs. Many geometers feel that these kinds of "transcendental" arguments are less explanatory than purely algebraic arguments that work uniformly in all real closed fields.*

certain kinds of intuitive and diagrammatic reasoning from geometry. Hallett highlights the subtle and complicated interplay between these kinds of purity concerns and Hilbert's larger project of incorporating metamathematical reasoning into geometry.

Finally, Jamie Tappenden (Michigan) discusses the significance of good definitions in mathematics. His first paper uses the introduction of the Legendre symbol as a case study for exploring the role that fruitful consequences play in explicating the notion of a mathematically "natural" definition.[5] It also discusses the ways definitions can *change* in response to deeper understanding of a field—e.g., the realization by algebraic number theorists that the notion of primality is more fundamentally captured by the property

$$n \text{ is prime iff } n|ab \Rightarrow n|a \text{ or } n|b$$

than by the traditional,

$$n \text{ is prime iff } a|n \Rightarrow a = 1 \text{ or } a = n.$$

Tappenden's research paper traces these kinds of issues through the work of Riemann and Dedekind, and it explores the relationship between natural definitions in mathematics and some recent philosophical discussions of "natural properties". (Along the way, he makes some illuminating criticisms of "structuralist" accounts of the natural numbers as given by, e.g., Benacerraf, Shapiro, and Sider.) I should note that, although these two papers' focus is clearly philosophical, some sections are likely to be more accessible to mathematicians than to philosophers. Tappenden clearly feels space constraints when summarizing the mathematical evidence for his conclusions, and those who already know the mathematical context in detail will find these sections easier going than those who don't.

Who We Talk To

The final three sections of the book concern questions arising from the interaction of modern mathematics with other academic disciplines: computer science, philosophy, and physics. Of course some such questions have been widely discussed among philosophers—e.g., the applicability of mathematics to physics or the relevance of recursion theory to computer science—but the particular questions examined here have been far less commonly addressed.

Jeremy Avigad (Carnegie Mellon) focuses on the growing interaction between mathematics and computer science. The first half of his introduction explores the role computers increasingly play in generating (informal) evidence for mathematical assertions—e.g., in testing conjectures numerically or using computer verification to check a long and complicated proof. The issue here is not the traditional: "do computer proofs count as full-fledged proofs?" Rather, it's the more complicated question of whether we can give *any* philosophical account of the notions of mathematical plausibility that are in play in these more informal cases.[6] The remainder of Avigad's papers involve the relevance of computer science to the study of mathematical *understanding.* The research paper, in particular, provides a richly detailed discussion of some of the difficulties that arise when training computers to follow certain families of mathematical proofs, proofs that most human mathematicians can follow quite readily. By focusing on such cases, Avigad hopes to provide new insights into the ways human mathematicians actually understand proofs and to thereby give a deeper account of the nature of mathematical knowledge.

Colin McLarty (Case Western) explores the relationship between category theory and mathematical structuralism. Very roughly, structuralism is the claim that mathematics is concerned, not with particular mathematical objects, but only with general patterns or structures. Particular objects are defined in terms of their positions within such structures—i.e., their relations to other elements of the structures—but they have no identity conditions outside of those structures.[7] McLarty argues that the *right* way to develop structuralism is through category theory. His paper provides a whirlwind tour through some central episodes in the history of mathematics, leading up to the development of the notion of a *scheme.* He then

[5] *At first blush, the definition of the Legendre symbol is a paradigm of an unnatural and cobbled-together definition. It's only after we do some mathematics with the symbol—to understand its algebraic properties, its useful consequences, and its various generalizations—that we come to understand how natural the symbol really is.*

[6] *To see the problem here, consider a simple Bayesian approach to the issue. Since Bayesian theories assign probability* 1 *to all logical tautologies, they can't be used to model plausible reasoning about whether a given statement is, in fact, a tautology. Similarly, suppose that Goldbach's conjecture is false. Then there is a proof of this fact which uses only elementary arithmetic. So, Bayesian theories would insist that we should reject Goldbach's conjecture with (at least) the same confidence as we accept elementary arithmetic. But, of course, we don't yet know that elementary arithmetic disproves Goldbach's conjecture (even though the relevant proof is out there somewhere, we haven't yet discovered it). So, this kind of Bayesian analysis won't capture the reasoning we actually use when we assess the plausibility of the conjecture.*

[7] *So, for instance, the number* 2 *is completely defined by its position in the ordering of the natural numbers, and there's nothing to say about its nature or essence outside of that pattern. While both* $\{\{\emptyset\}\}$ *and* $\{\emptyset, \{\emptyset\}\}$ *may play the role of* 2 *in some particular set-theoretic reduction of arithmetic, it makes no sense to ask which of them really is the number* 2.

draws some illuminating lessons for philosophical structuralists concerning, e.g., the distinction between morphisms and functions, the significance of the difference between isomorphism and equivalence in category theory, and the role that category theory can play in solving (or dissolving) puzzles concerning mathematical ontology. As in the case of Tappenden's paper, I expect that the mathematical prerequisites of McLarty's paper will make it (far) more accessible to mathematicians than to philosophers.

The final two papers in this collection, both by Alasdair Urquhart (Toronto), concern the relevance of physics to mathematics. Urquhart's primary interest is in the ways physicists shamelessly abuse mathematics and yet, with some regularity, manage to parlay their weird mathematics into strikingly good physics. The core of Urquhart's research paper consists of a series of case studies—involving infinitesimals, the umbral calculus, and the Dirac delta function—in which physicists developed new mathematical techniques that mathematicians were only later—and, in some cases, *much* later—able to put on any kind of rigorous footing. Urquhart urges mathematicians and philosophers to take the lesson of these studies to heart and to view physics as an important source of new mathematical ideas (even, or perhaps especially, when the physicists themselves cannot explain the mathematical foundations of what they are doing). He ends with a final case study involving the Sherrington-Kirkpatrick model of spin glasses, a case in which physicists are currently making surprising progress by employing some extremely dodgy—and at this point inexplicable—mathematics.

Concluding Remarks

This, then, gives us a picture of the details of Mancosu's book. Let me close with two somewhat more general comments. First, I want to emphasize just how good this book really is. The papers are clear and well written; the introductory surveys provide nice introductions to the relevant philosophical literature; the research papers address fresh topics which have been unduly neglected in recent philosophical discussion; and the mathematics in the book is both more varied and more central to mainstream mathematical practice than is typical in philosophical contexts. Taken as a whole, the book provides an excellent introduction to some of the most exciting and mathematically well-informed work in recent philosophy of mathematics, and it lays out an attractive agenda for future philosophical research.

Second, I want to issue a minor caution. In some cases, the papers feel just a bit too programmatic. Although they lay out interesting agendas for future work and take some preliminary steps towards fulfilling those agendas—asking important questions, clarifying key notions, and working through specific test cases—I often found myself wanting just a little more development and a little more argument in order to be fully convinced. Readers who are looking for airtight theses, completely detailed analyses, fully worked out arguments, and rigorously proved claims (in the philosopher's, not the mathematician's, sense of "rigorously proved"!) may find some sections of this book a bit frustrating.

That being said, this *caution* isn't intended as a genuine *criticism.* The purpose of this book is to lay out a vision of what the philosophy of mathematics could someday be—both by sketching the kinds of topics philosophers might turn their minds to and by making enough (local) progress on these topics to convince us that our efforts are likely to be rewarded. On these terms, the book succeeds splendidly. My desire for a more developed version of the project—say, a small bookshelf filled with monograph-length expansions of these papers—is merely a testament to this book's overall success.