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Paolo Mancosu (ed.), *The Philosophy of Mathematical Practice*, Oxford Univ. Press, Oxford, New York, etc., 2008 (XII + 448 pages), ISBN 978-0-19-929645-3.

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The philosophy of mathematical practice is not only a research topic, it is more generally a disciplinary field which is extending its importance and attracting the professional interest of an increasing number of scholars coming from different scientific communities.

There are several reasons for this.

Some of them are negative, *i. e.* they depend on a reaction against other intellectual trends. A major example is the reaction against the persisting interest of many philosophers of mathematics and logic in foundational questions, which, after the 1960s (mainly as a consequence of the influential works by P. Benacerraf), has partially shifted toward more metaphysical and epistemological themes, like the opposition between Platonism and nominalism, and (in the Platonist camp) the access problem, or (in the nominalist camp) the problem of reformulating mathematical theorems and theories without making reference to abstract objects. Another related example is the reaction against a picture of mathematics focusing almost exclusively on formalisation, axiomatisation, and proof, the latter being accounted for through an exclusive appeal to the tools of mathematical logic. A third example is the reaction against a growing sociological decay of many studies in the domain of history of mathematics, representing the latter as a system of institutional, political, or academic dynamics essentially independent of the specific content and characteristic features of mathematics itself. In all these cases, the philosophy of mathematical practice is seen as a program aimed at a reflection on this content and these features. This goes often together with the idea that mathematics is more a human activity than a logically organised repertoire of results, and that this activity is quite peculiar and depends on inner interactions and motivations.

Some other reasons, by contrast, are positive. They depend both on the growth, within the communities of logicians and philosophers and historians of mathematics, of different competences, interdisciplinary expertises, and awareness of new problem areas, and on the emergence, within mathematics itself, of new modalities of discovery, justification, selection and solution of problems, application to and connections with other sciences and disciplinary domains. An example of the former phenomenon is the growing historical competence and interest in the history of mathematics of many logicians. On the one hand, this increased attention to the history of mathematics has made these logicians aware not only of the existence of a residual content of mathematical theories that a formal logical analysis is unable to account for, but also of the significance of this content as an essential ingredient of the relevant theories. On the other (and somehow opposite) hand, this has promoted historical studies that make use of formal logical tools in order to reveal aspects of previous mathematical theories and views that would have hardly been addressed by studies based on the more traditional historiographical methods and devices. An example of the latter phenomenon is the deep transformation of mathematics as a consequence not only of the development of computer-assisted procedures, and of the possibility of appealing to algorithms and classificatory methods whose feasibility depends on the massive use of automatic

computation, but also of the changes in the conception of rigour, reliability, proof, and problem-solving brought about by the development of computer science and by the interaction with physics and other sciences, like biology or economy. This transformation directly influences, among many other things, the idea that professional mathematicians have of the appropriate settings for their theories, and of the modalities through which mathematics applies to other sciences, leading thereby to the emergence of new philosophical problems whose solution can hardly depend on the forms of arguments used in more traditional metaphysical and epistemological discussions.

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The book edited by Paolo Mancosu provides a comprehensive and quite vivid exemplification of all that the aforementioned issues. It especially displays the extension and importance taken by the philosophy of mathematical practice by showing it at work on a set of different and multifarious topics. But it does much more than this. For, it also and overall suggests a substantial agenda and a momentous program for the future development of this field, and, consequently, for a reorientation of philosophy of mathematics that is connatural to it. As Mancosu points out at the very beginning of his Introduction, the book has the ambition of “bringing some fresh air into the philosophy of mathematics” (p. 1). Doubtless, it succeeds in this. But it also sketches several lines of research, which will possibly ensure that this fresh air will continue to circulate henceforth. Furthermore, it gives voice to a community of scholars working in this area and undertaking its development (this community does not only include the authors of the essays contained in the book, but also a large group of scholars sharing their approach and often actively collaborating with them).

The very structure of the book is appropriately conceived in order to succeed in this challenge. In addition to a short Preface (pp. V-VI) and an Introduction, both written by Mancosu himself (pp. 1-21)—on which I shall come back to below—, it includes sixteen chapters devoted to eight different topics: two chapters for each of them. Whereas the first chapter of each pair aims to provide “a general introduction to the subject area” it is concerned with, the second consists of “a research paper in that area” (p. 14).

The first two chapters, written by Marcus Giaquinto, concern the role of visualisation or, more generally, visual thinking in mathematics. The introductory chapter (“Visualizing in Mathematics”, pp. 22-42) describes different cases in which this sort of thinking occurs in proof, discovery, understanding and calculation. The more striking thesis that Giaquinto defends is that visualisation can be an irreplaceable ingredient of some proofs: the basic idea in his argument is that eliminating visualisation from these proofs amounts to getting essentially distinct proofs. Though these new proofs can be taken to support the same theorems, they result in, or depend on a reformulation of these theorems that produces a significant mathematical change. The research essay (“Cognition of Structure”, pp. 43-64) deals with cognition of structures, namely with the way in which visual representation allows “our cognitive grasp of structures” (p. 43). Though Giaquinto is mainly concerned with small finite structures known by acquaintance, he also explains how visualisation can help in knowing some simple infinite structures.

Chapters 3 and 4, written by Kenneth Manders, address the role of diagrams in some forms of geometric argumentation, especially in Euclid’s. Chapter 4 (“The Euclidean Diagram (1995)”, pp. 80-133) reproduces a former paper, written in 1995, that, though never published hitherto, has largely circulated as a manuscript in academic circles, both in North and Latin America and in Europe, and has had an important role in promoting a

great amount of research on this topic. Chapter 3 (“Diagram-Based Geometric Practice”, pp. 65-79) provides a survey of these studies and sketches some “tasks for the future” (p. 75). Manders’ 1995 paper endorses, among others, a thesis that has been very influential. According to him, Euclid’s arguments (Manders refers in particular to proofs and solutions of problems or constructions occurring in books I and III of the *Elements*) include a “verbal part” or “discursive text”, and a “graphical part”, i. e. the diagram. The former “consists of a reason-giving ordered progression of assertions, each with the surface form of an ascription of a feature to the diagram [...] licensed by attributions either already in force in the discursive text or made directly based on the diagram [...] or both” (pp. 86-87). Both the attributions and the corresponding attributes can be either “exact” or “co-exact”: “exact attributes are those which, for at least some continuous variations of the diagram, obtain only in isolate cases”; “co-exact attributes are those conditions which are unaffected by some range of every continuous variation of a specified diagram” (p. 92). Typically (but not always), the former are expressed through statements of equality, whereas the latter are, broadly speaking, topological in nature. Manders’ basic point is that whereas exact attributions can only be licensed by appropriate previous entries in the discursive text, diagrams play an indispensable role in licensing many co-exact attributions. Both the very nature of these attributions, and an appropriate discipline in producing diagrams and in dealing with them, warrant the claim that Euclid’s arguments are both valid and rigorous. This thesis has not only provided the ground for further developments in the analysis of Euclid’s arguments. It has also suggested different ways for formalizing these arguments without expunging diagrams, but rather making manifest how the latter can enter inferences that are both formal and valid even according to modern standards. These developments, due, for example, to Nathaniel Miller and John Mumma, are part of those that Manders surveys in chapter 3 (where the appropriate references are offered).

Chapters 5 and 6, written by Paolo Mancosu, and by Johannes Hafner and Paolo Mancosu, respectively, are about mathematical explanation. Chapter 5 (“Mathematical explanation: Why it Matters”, pp. 134-150) offers a survey of the recent discussion on the topic, by distinguishing between mathematical explanation of scientific facts and explanation within mathematics. The former specifically concerns applied mathematics, namely those forms of explanation “in the natural or social sciences where various mathematical facts play an essential role” (p. 134). In connection with it, Mancosu also addresses a recent discussion on a form of the so called indispensability argument based on the essential explicative power of mathematics in the explanation of certain scientific facts. The second sort of explanation concerns mathematics as such, namely the explanation of mathematical facts provided within mathematics itself, for example through appropriate proofs. In this context, explanatory proofs are those that make clear the (or a) reason why the corresponding theorem holds, as opposed to those that merely ensure that it holds. The chapter is especially concerned with Mark Steiner’s and Philip Kitcher’s models of explanation (and offers of course the relevant references). Steiner’s model explicitly concerns mathematics and connects to each other the two aforementioned forms of mathematical explanation, by arguing that a mathematical explanation of a scientific fact obtains when the relevant argument is such that a mathematical explanation of a mathematical fact is obtained if the specific scientific part of the argument is removed. Kitcher’s model (which develops and crucially transforms a former idea of Michael Friedman’s) is put forward as an account of scientific explanation, but it is supposed to account also, possibly through appropriate extensions, for both forms of mathematical explanation (of scientific facts and within mathematics

itself). The crucial idea of Steiner's model is that explanatory proofs within mathematics are those that involve a "characteristic property" of the relevant entities or structures, that is, a property which is "unique" of these entities or structures, "within a family or domain" of entities or structures (p. 143). By contrast, the crucial idea of Kitcher's model is that explanation is got through unification by generalisation: roughly speaking, a scientific argument is explanatory when it applies a small number of general argument patterns, and it is all the more explanatory the smaller number of patterns it applies. Mancosu's discussion of these models is openly critical. His criticism is mainly supported by the argument offered in chapter 6 ("Beyond Unification", pp. 151-178), where a particular case study from real algebraic geometry (concerning Gregory W. Brumfield's treatment of real closed fields and his way of proving theorems about them) is considered. This case shows that Kitcher's model is not appropriate for accounting for the actual attributions of explanatory power to some proofs made by working mathematicians. Of course, I cannot enter the details of the argument. The basic idea it conveys can be stated quite simply, however: mathematical explanation is a matter of pluralism: no unique and general model can hope to account for it in all its possible forms; hence only a detailed analysis of mathematical practice can reveal some of these forms, and possibly, as Hafner and Mancosu have argued in previous publications, classify them.

Chapters 7 and 8, respectively written by Michael Detlefsen and Michael Hallet, are devoted to purity conceived as an ideal of proofs and methods. The concern of both chapters is mainly historical, though the history of mathematics provides here the appropriate material for philosophical reflexions and classifications. Chapter 7 ("Purity as an Ideal of Proof", pp. 179-197) describes different forms taken throughout history of mathematics by the ideal of purity of proof. It opens by considering Aristotle's principle prohibiting "crossing from one genus to one other in the course of a proof" (p. 179), which is taken as a precept of topical purity: proofs should not transcend the topic of the corresponding theorem. In the most extreme form, this ideal requires that proofs only rely on the conceptual resources needed for understanding the statement of the theorem. This manifests the epistemic nature of such a precept. According to Detlefsen, this is connected, in Aristotle, with a "causal conception of proof" (p. 181). This conception is echoed by Leibniz and significantly developed by Bolzano, who chiefly promoted a sharp separation between geometric and analytic arguments. The basic idea, here, is that proofs should be pure insofar as they should "recapitulate[...] a segment of the natural, objective ordering of truths concerning a given subject" (p. 182). Bolzano's idea were not isolated, and Detlefsen discusses the way in which similar concerns also emerge in Gauss, Dedekind, Frege, and Hilbert, before closing the chapter by considering more modern forms of the ideal of topical purity, which raise, of course, the quite delicate issue of topical aboutness (not to be confused with ontological aboutness, a typical Platonistic concern). The case of Hilbert's attitude towards purity of geometrical methods provides the subject matter of chapter 8 ("Reflections on the Purity of Method in Hilbert's *Grundlagen der Geometrie*", pp. 198-255). Though this appears, at first glance, as a mere appropriateness requirement—namely what means of proof are appropriate in the proof of a certain statement—the detailed analysis of Hilbert's arguments (both in his famous book and in the lectures connected to it), especially of his meta-theoretic proofs, shows that Hilbert's concern is overall for a critical discussion of appropriateness, showing that this cannot depend on "intuitive or informal assessment" (p. 248), to the effect that, typically, "higher mathematics" (usually analytic methods) is shown to be appropriate "to instruct or adumbrate intuition, or, at the very least to

instruct us about it and what it entails” (p. 249). In other terms, appropriateness need not conserve either elementariness or intuitiveness.

Chapters 9 and 10, written by Jamie Tappenden, concern fruitfulness and naturalness of concepts and definitions. The former (“Mathematical Concepts and Definitions”, pp. 256-275) propound what Tappenden calls ‘the Port Royal Principle’, because it is stated through a quote from Arnauld and Nicole’s *Logique*: “nothing is more important in science than classifying and defining well [...] [but] it depends much more on our knowledge of the subject matter being discussed than on the rules of logic” (p. 256). In Tappenden’s understanding (which parallels that of Arnauld and Nicole themselves), this principle is not against logic. It is rather in favour of the necessity of detailed and context-driven analyses as appropriate supports for judging whether a definition is the “right”, “proper”, “correct”, or “natural” one, and it is suitable for promoting a “significant advance in knowledge” (*ibid.*) To achieve this purpose, two examples are considered: the case of the Legendre symbol and that of the appropriate definition of prime numbers. The Legendre symbol is, in fact, a handy notation for a function defined on natural numbers whose consideration allows a compact statement of the law of quadratic reciprocity. Broadly speaking, the question is here whether the introduction of this function only results in an advantage of simplicity or it has significant epistemic virtues. Prime numbers are, on the other hand, usually defined as natural numbers greater than 1 which are evenly divided only by 1 and by themselves. This is a well-known and simple definition. It can be extended to domains other than the domain of natural numbers. But, if it is limited to the primitive context in which it arose, this definition happens to be equivalent to an apparently less simple and less natural one: $a \neq 1$ is prime if, whenever a divides a product bc , it divides either b or c . The question here is which of these definitions is more appropriate to fix the concept of a prime natural number. The answer to both these questions, Tappenden argues, is not immediate and depends on subtle mathematical considerations. A similar strategy for arguing in favour of the Port Royal Principle is also followed in chapter 10 (“Mathematical Concepts: Fruitfulness and Naturalness”, pp. 276-301). It deals however with a single, but much more general example, which is studied in much more details. It concerns “the Riemann-Dedekind approach to ‘essential characteristic properties’” (p. 278). The core of this approach lies in the research of ways for singling out some classes of functions by minimizing the information relative to a certain function that is needed for deciding whether this function is or not part of these classes. Typically, this information depends on some singularities of the relevant functions. The crucial opposition here is that between a global description and a local behaviour. The Riemann-Dedekind approach advocates characterising and classifying functions on the basis of the latter, rather than on the former. Clearly, this cannot be cashed out in terms of a general logical principle. Hence, the example is perfect for urging the Port Royal Principle.

Chapters 11 and 12, written by Jeremy Avigad, discuss the role of computers in contemporary mathematics. The matter could be tackled from different perspectives. The most obvious of them would be that of listing a number of possible ways in which computers can help in calculation, classification and proof. Though Avigad considers some of these ways, he does not follow this direction. He rather wonders whether the use of computers in mathematics results in some significant epistemic gain, and how these advances can be accounted for, in general. In chapter 11 (“Computers in Mathematical Inquiry”, pp. 302-316), two sorts of such gains are considered: they respectively concern “the ability of computers to deliver appropriate ‘evidence’ for

mathematical assertions” and “the ability of computers to deliver appropriate mathematical ‘understanding’” (p. 302). The main question is neither that of discussing some ways computers can be used to get these gains, nor that of showing how significant can be, for these purposes, some results that computers allow us to reach. Rather, the emphasis is on the way evidence and understanding should be conceived in mathematics in order to provide an explanation of the way in which computers can help acquire them. The enquiry is admittedly preliminary, but it is enough to support the following conclusion: “what we need now is *not* a philosophy of computers in mathematics; what we need is simply a better philosophy of mathematics” (p. 315). Chapter 12 (“Understanding proofs”, pp. 317-353) pursues, in some more details, the same aim, with respect to understanding. The main thesis is that understanding should be conceived in terms of the ability of doing something. This leads to a position quite close to a functionalist account according to which understating is manifested by appropriate behaviour. Avigad discusses some usual criticisms against such a behaviourist approach and the way they can be rebutted and dismissed in the special case of mathematics. In the second part of the paper, he presents four case studies in order to test such an approach to understanding carried out using formal verification through the use of computers.

Chapters 13 and 14, written by Colin MacLarty, deal with structuralism conceived in relation with category theory. Structuralism is a position that many contemporary philosophers of mathematics advocate as a response to general ontological and epistemological questions related to the Platonism-*versus*-nominalism debate. It could then appear quite strange to find it among the topics specifically addressed in Mancosu’s book. Still, MacLarty’s aim in these chapters is not only, and not overall, that of advocating structuralism, but rather that of arguing that mathematical practice does not force the admission of a “classical ontology” (p. 356), but can be accounted for by appealing to the sort of structuralism which is typical of category theory, or “working structuralism”, as MacLarty calls it (p. 360), by insisting that it is embodied in mathematical practice. This aim is pursued in general in chapter 13 (“What Structuralism Achieves”, pp. 354-369), through a comprehensive discussion of such a form of structuralism, and in a more specific way in chapter 14 (“‘There is no Ontology Here’: Visual and Structural Geometry in Arithmetic”, pp. 370-406), through the consideration of a single case study. This is relative to Alexander Grothendieck’s theory of schemes, a theory pursuing André Weyl’s idea of an “algebraic geometry over the integers” (p. 385). Details cannot be presented here. It will be enough to say that this chapter offers a philosophy-driven survey of a large and crucial fragment of 20th century mathematics, especially concerned with topology.

Chapters 15 and 16, written by Alasdair Urquhart, are about the connections between mathematics and mathematical physics, in relation with the methods used to prove theorems, or more generally to reach theoretical conclusions in both fields. In chapter 15 (“The Boundary Between Mathematics and Physics”, pp. 407-416), Urquhart argues for the following theses. Modern mathematical physics typically deals with models which is tempting to describe as “mathematical objects in their own right”, insofar as their relation with physical reality is very rough; indeed, so much so that they appear to be nothing but “mathematical caricatures of physical systems” (p. 409). However, he continues, such a way of conceiving these models “brings with it considerable difficulties”, the major of which is that these models do not appeal, typically, to “normal mathematical methods”, and it is often doubtful that “the objects themselves are even mathematically well defined” (p. 410). This difference of methods has increased in the

mid-20th century, where the mathematical and physical communities increasingly diverged. The very last years have seen, instead, an inverse movement of reconvergence, due, on the one hand, to the growing attention of physicists for theories like string theory, whose empirical content is quite weak, and, on the other hand, to many mathematicians' disregard of too abstract methods and their renewed interest for the solution of "more concrete problems" (p. 412). This raises the issue of the way in which the methods of physics, often lacking in rigour with respect to the usual mathematical standards, can be integrated within mathematics and become acceptable for these standards. In Urquhart's parlance, this is the question of the "applicability of physics to mathematics" (p. 413). This is the question that Chapter 16 ("Mathematics and Physics: Strategies of Assimilation", p. 417-440) is devoted to. Four examples are considered in some details: the use of infinitesimals and the development of non-standard and smooth infinitesimal analyses; the umbral calculus (a reinterpretation, mainly due to Gian-Carlo Rota, of a quite obscure method of formal calculation firstly developed in 19th century, using the rigorous tools of modern linear algebra), the theory of distributions; and the replica method (a statistical mechanical method based on mathematically doubtful passages to the limit, which is used to deal with a problem in finite combinatorics that arose in studying some systems of magnetic spin variables). According to Urquhart, these are examples of "nonrigorous mathematics" derived from a beneficial interaction with the physical practice. The chapter closes with a plea for extending philosophy of mathematics so as to include the consideration of similar forms of mathematics.

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The advocacy of a philosophy of mathematics that—in contrast to the standard or mainstream one—is closer to the effective way in which mathematics is done and evolved through history is not new. In his Introduction, Mancosu discusses two recent traditions embodying this aim. The former is shortly identified as the "maverick tradition". People familiar with philosophy of mathematics in the second half of the 20th century immediately identify it as the philosophical movement principally promoted (though in quite different forms and with quite different emphases) by Imre Lakatos and Philip Kitcher (the latter being mentioned overall for his book *The Nature of Mathematical Knowledge*, Oxford, 1984), and more recently followed, according to Mancosu, by David Corfield. No need here to describe this tradition: Mancosu does it shortly but very efficiently. The latter tradition is rooted in Quine's empiricism and naturalism and has its major exponent, in more recent times, in Penelope Maddy. Also in this case, no description is needed.

As influential as the works belonging to these traditions might have been, there is no doubt that they failed in "substantially redirect the course of philosophy of mathematics" (p. 5). Hence, an obvious question arises: what makes the approach promoted by Mancosu and his collaborators different from these two, so as to license the hope of a different and more successful outcome? Mancosu answers this question in the last section of his Introduction.

According to him, the authors in the maverick tradition were still largely "concerned with metaphysical issues" (p. 17), *in primis*, the nature and modalities of mathematical progress. These are quite general issues, which were tackled, however, by pointing to a small number of case studies, taken as paradigmatic. The approach promoted by Mancosu is opposite: a much vaster spectrum of topics and particular studies are taken to serve more restricted aims. On the other hand, in Mancosu's book, by contrast with the works like those of Lakatos and Corfield, no polemic attitude is

struck against logical analysis, analytic philosophy, or the foundationalist tradition. More generally, the interest and importance of major current discussions inspired by logical analysis, analytic philosophy and/or foundationalist worries, are never denied. What Mancosu calls for is, rather, an extension of current philosophy of mathematics, so as to make it possible to address topics that have been ignored or scarcely considered by philosophers following these approaches.

Similar considerations also apply to the second tradition, which is overall concerned with a reflection on the nature and (foundational) role of set theory. Mancosu and his collaborators also avoid making any case against set theory and the reflections on its history and virtues (or defects). They merely argue that “while set theory is a very important subject of methodological investigation, there are central phenomena that will be missed unless we cast our net more broadly and extend our investigations to other areas of mathematics” (p. 19).

So far, so good. Still, as important as they might be, these differences are not enough, as such, to warrant that Mancosu’s effort will result in a long-running success, so as to “redirect the course of philosophy of mathematics”. The perspectives are excellent, overall because this effort does not present itself—as it was, for example, the case with Lakatos—as the struggle of an isolated and factional, though enormously bright, scholar, but involve, rather, a quite large community of scholars (as it is also attested by the recent foundation of an Association for the Philosophy of Mathematical Practice: APMP). Intellectual dynamics mainly depend on the force of argument, however, and, though the arguments advanced in the different papers included in Mancosu’s book are quite strong and encompass a large area of philosophical thought about mathematics, many of them are still programmatic in nature, as it is to be expected by the very nature of the enterprise. So, only the future development of these arguments, and, in general, of philosophy of mathematical practice as such, will be able to establish the long-running success that these scholars (and myself) hope for. It is thus important to wonder which lines of research are the most appropriate for this purpose. This cannot but be a collective effort. Nevertheless, let me make two remarks on this matter.

The first is based on a critical appreciation of the introductory papers included in Mancosu’s book. These papers succeed in delineating a vast agenda of issues that philosophy of mathematical practice is required to address. The research papers that follow them cannot but tackle a small part of these issues and leave many aspects of them still open. Hence Mancosu’s book already contains the indication of many lines of research for future promising development. Still, it seems to me that, barring some exceptions, the introductory papers fail in fixing a compact disciplinary content for the philosophy of mathematical practice, *i. e.* a systematic net of competences, pieces of information, and notions that a philosopher of mathematical practice is required to have and transmit. This is not surprising, for given the programmatic nature of the book one would have been surprised if things had turned out differently. Nevertheless, I think this is an important weakness to overcome in the next future. The prospects of the philosophy of mathematical practice largely depend on this.

The second remark is more specific. Mancosu opens his Introduction (p. 1) by remarking that “contemporary philosophy of mathematics offers us an embarrassment of riches”, which he shortly but perspicuously describes. He points out that much of the work that is presently done in philosophy of mathematics “can be seen as an attempt to address a set of epistemological and ontological problems that were raised with great lucidity in two classic articles by Paul Benacerraf”. Then he observes that, though largely beneficial for the development of philosophy of mathematics, the influence of

Benacerraf's articles "has also had the unwelcome consequence of crowding other important topics off the table". An explanation immediately follows:

In particular, the agenda set by Benacerraf [...] was that of explaining how, if there are abstract objects, we could have access to them. And this, by and large, has been the problem that philosophers of mathematics have been pursuing for the last fifty years. Another consequence of the way in which the discussion has been framed is that no particular attention to mathematical practice seemed to be required to be an epistemologist of mathematics. After all, the issue of abstract objects confronts already at the most elementary levels of arithmetic, geometry, and set theory. It would seem that paying attention to other branches of mathematics is irrelevant for solving the key problems of the discipline. This engendered an extremely narrow view of mathematical epistemology within mainstream philosophy of mathematics, due partly to the over-emphasis on ontological questions.

I totally agree with all that. But I also maintain that the future success of the philosophy of mathematical practice will largely depend on its relations with Benacerraf's agenda (and I think Mancosu would agree with me on this). Though recently so clearly and efficaciously set forth by Benacerraf's papers, this agenda is, in fact, a quite classical one. It is, *mutatis mutandis*, that already suggested by Plato (which explains that it confronts already at the most elementary levels of mathematics), and it is largely with respect to it that the discussion on the foundations of mathematics, so influential since the end of the 19th century up to the second half of the 20th, had developed (which explains why the discussion on the issues raised by Benacerraf's papers has in some sense replaced the foundationalist discussion after the sixties). This connection with the ontological and epistemological questions revived by Benacerraf has even been a peculiar mark of this discussion, to the effect that the foundationalist programs advanced alongside it differed from many others, which have been advanced in other periods of the history of mathematics, also because of their ontological and epistemological imports. Hence, though Benacerraf's agenda can be hopefully supplied by other topics and problems, it is hard to think that philosophy of mathematics can leave it on a side in the next future, as it is also hard to think that philosophy of mathematics can avoid any foundationalist concern.

A crucial problem that the philosophy of mathematical practice should then face is whether there is a way of addressing and trying to respond to the questions included in Benacerraf's agenda and the unavoidable foundationalist concerns that are closer to such a practice. To speak quite broadly, I think, for example, that there is room for hoping that the sense in which one speaks of abstract mathematical objects and of our epistemic relation with them would be clarified if these objects were conceived as being historically constituted by the very mathematical activity. My guess is that the future long-running success of philosophy of mathematical practice will crucially depend on its capacity to integrate these themes within its own approach to philosophical questions. If it is going to achieve this aim, the fresh air that philosophy of mathematical practice is bringing to the philosophy of mathematics, will not only result, I guess, in an extension of the latter, but also in substantial philosophical progress.

I consider this book not only highly welcome, since it addresses the invaluable program of philosophy of mathematical practice, but also a significant contribution to philosophy of mathematics as such. The structure of the book, the vastness of the topic considered, the clarity of Mancosu's Introduction and of the different essays included in it, the significance of the topics dealt with and of the theses advanced, as well as the robustness of the arguments that support these theses make this book an indispensable

companion for contemporary philosophers of mathematics, whatever their approach might be.