What the foundationalist filter kept out

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Essay review of *Towards a philosophy of real mathematics* by David Corfield; Cambridge University Press, Cambridge, 2003, pp. x + 288, Price £45.00 hardback, ISBN 0-521-81722-6.

From title to back cover, a polemic runs right through this book. Corfield repeatedly complains that philosophers of mathematics have ignored the interesting and important mathematical developments of the past seventy years, 'filtering' the details of mathematical practice out of philosophical discussion. His aim is to remedy the discipline's long-sightedness and, by precept and example, to redirect philosophical attention towards current developments in mathematics. This review discusses some strands of Corfield's philosophy of real mathematics and briefly assesses some of his objections to orthodox philosophy of mathematics.

Part I of the book illustrates Lakatos's insight that theorem proving, conjecturing and concept formation are the fundamental components of mathematical research with some novel case studies. Corfield considers some of the proof and conjecture strategies employed by automated theorem provers, emphasising the key distinction between strategies that attempt to emulate human reasoning patterns and those that play to computers' strengths—the ability to undertake powerful searches. He explains that a conjecture known as the Robbins conjecture, which states that one of the axioms in an early axiomatisation of Boolean algebra can be replaced by a weaker statement, was first proved by computers in the mid-1990s. Unfortunately, this computer-generated proof is 'seemingly incomprehensible' (p. 52): it cannot currently be taken in by (human) mathematicians in anything other than a step-by-step fashion. This provides a springboard for Corfield to stress the importance of proof as valuable not merely because it leads to a particular conclusion, but also because of its capacity to yield 'new concepts, techniques and interpretations' (p. 56) for further mathematical uses. The *telos* of a proof may be to establish a particular theorem, but a good proof should open up further vistas beyond it. Admittedly, the familiar observation that mathematicians ask much more of their proofs than merely to provide an in-principle route to a specific conclusion, an observation which has recently been made with particular elegance in Gowers (2000), will not cause the editor of *Philosophia mathematica* to hold the front page. Likewise, it is hardly news that first-order logic 'does not often bring out the grain of mathematics' (p. 79)—roughly, does not point to mathematically interesting concepts. First-order logic forgoes this aspiration, at least these days.[1] Yet although these general points may be familiar, Corfield gives them a new twist with his discussion of automated proof and conjecture formation.

Following some reflections in Part II on the role of analogy in mathematics and Bayesianism's past neglect of plausible mathematics, comes Part III, in my opinion the high point of the book. Corfield argues that Poincaré's seminal work in *Analysis situs* (1895), in particular his duality theorem for manifolds, gives the lie to Lakatos's method of proofs and refutations. He claims that Poincaré developed algebraic topology to provide tools for uses in other parts of mathematics rather than to prove some specific conjectures, as the Lakatosian line would have it (p. 157). Lakatos is more generally taken to task for maintaining that most mathematical conjectures are proved before axiomatisation and formalisation have taken

[1] Corfield subscribes to the view that one of Frege's ambitions for his logic was that it would enable mathematicians to carve out concepts in a mathematically fruitful way.

place. But Corfield also chides the mathematical physicists Arthur Jaffe and Frank Quinn for arguing that Poincaré's unrigorous ('reckless') work on algebraic topology is evidence for the more general claim that insufficient rigour holds back the development of mathematics. On whether rigour in mathematics impedes creativity (as Lakatos implied) or promotes it (as Jaffe and Quinn contend), he takes the sensible ecumenical line that it depends, pointing out that, as with comedy, it's all in the timing. It would be instructive, however, to hear more from Corfield about when, generally speaking, an injection of rigour is beneficial and when it is harmful, or, alternatively, to be told why there is not much to speak of generally here.

Lakatos's own application of his methodology of (scientific) research programmes to the case of mathematics passed irretrievably away when he did. Corfield assesses the prospects for this application, and in particular casts a critical eye on the debate pitting those who maintain that mathematics is a unitary discipline displaying profound interconnections against those who see mathematics as fragmented. He himself inclines towards the unity view and sympathises with Colin McLarty's diagnosis that the fragmentation view arises from an 'excessively direct transcription of common views of postmodernism into the history of mathematics' (quoted on p. 176). If Corfield and McLarty are right, the interdependence between mathematical theories tells against Lakatos's methodology of research programmes as applied to mathematics. The latter artificially segments mathematics into clear-cut 'research programmes'. As Corfield puts it, 'The language of research programmes and projects as clearly delineated entities seems better suited to describe work conducted in the comparatively leisurely atmosphere of earlier centuries' (pp. 199–200).

Part III also contains further sophisticated discussion of other aspects of mathematical methodology. In particular, Corfield tackles the important question of mathematical value. We typically appraise a thesis (not just in mathematics) using a cluster of criteria. We ask: Is it right? Is it original? Is it important or interesting? Mathematics finds it easier than most disciplines to answer the first two questions. All the more reason, then, for an analysis of the third criterion, roughly, the notion of mathematical value, particularly since every mathematician's career (hence, welfare) is deeply affected by this most subjective of the three criteria. As Corfield rightly points out, different opinions about what constitutes important or interesting mathematics are also likely to reflect different views about the nature and aims of the subject. The proposal he puts forward has it that a mathematical development is progressive when it: (a) allows new calculations in an existing domain, possibly leading to the solution of old conjectures; (b) forges connections between existing domains; (c) allows a helpful reorganisation of existing domains; (d) opens up the prospect of new conceptually motivated domains; and (e) leads to successful applications outside mathematics (p. 205). This analysis is then applied to the notion of a groupoid in a case study which takes up Chapter 9. A groupoid consists of two sets A and B and some functions between them. For example, A could be a set of cities, B could be a set of inter-city journeys, and a function from B to A could map inter-city journeys to their city of departure. As Corfield sees it, groupoids score highly on the 'conceptualist' criteria (c) and (d) on his list, but score relatively poorly on the 'practical' criteria (a) and (e). Indeed, Corfield throws his weight behind Ronald Brown's contention that groupoids are conceptually more basic, mathematically speaking, than groups (which can be seen as special cases of groupoids). Along the way, we are given a potentially general analysis of the notion of 'naturalness' as it applies to groupoids. The analysis consists of three components, the first of which is recurrence in many different areas. Unfortunately, the second component, that natural notions 'embody a simple, *non-artificial* idea, which permits them to measure the symmetries of families of objects' (p. 230, my italics) is partly circular, as to a lesser extent is the third component, that natural notions ‘permit one to model situations without requiring that *arbitrary* choices be made’ (p. 230, my italics).

As several passages make clear,[2] Corfield has exalted ambitions for the methodology of mathematics. By methodology of mathematics, let us understand the discipline that aims to catalogue and clarify the methods, norms and principles of mathematical reasoning, the values and organisation of mathematical communities, the social, political, technological, etc., context of mathematics, and so on. One simple-minded way to triangulate the relationship between mathematics, its methodology and its philosophy is as follows:

$$\begin{tikzcd} & \textit{Methodology of Mathematics} \arrow[dl] & \\ \textit{Philosophy of Mathematics} & & \textit{Mathematics} \arrow[ll] \arrow[ul] \end{tikzcd}$$

On this picture, methodologists offer general systematic accounts of mathematical practice. Philosophers exploit these accounts as well as direct reports from the mathematical coal face in thinking about mathematics. Mathematics itself, however, is by and large unaffected by philosophers’ theories or by methodologists’ accounts.

Corfield’s ambition, it seems, is to turn this picture’s one-sided interaction between methodology and mathematics into a two-way street. One possible attempt to justify this that *I* can think of is as follows. If—and this is a big *if*— methodologists are more adept than mathematicians at bringing to light the values and norms employed by the latter in their research, methodologists may justifiably redress mathematicians’ collective errors of judgement. For example, methodologists could reveal to mathematicians that their own values should lead them to prefer groupoids to groups as an organising concept in mathematics. Yet it is hard to accept that a methodologist could be better placed than an algebraist to evaluate the consequences of replacing groups with groupoids in this way. More generally, methodology cannot in practice aspire to redressing mathematicians’ errors all by itself. After all, it is only mathematicians who have the expertise to apply the values correctly revealed (let us assume) by the methodologists in all their ramifications. So presumably it should be the methodologist’s job to hunt out the values, and the mathematician’s to apply them. Methodologists should in practice act as consciousness-raisers, revealing (if they can) to mathematicians their implicit values and presuppositions. But they are well advised to then take a back seat and await developments.

Corfield in fact both overestimates and underestimates the volume of flow along the bottom arrow, from mathematics to philosophy. The tenor of his book is that contemporary philosophers of mathematics are little acquainted with developments in core mathematics since the 1930s. No doubt this accusation is true of some. But to my mind it is false of most. Granted, it is rare to see a philosopher venturing into print with views on the latest developments in Ramsey theory or in homological algebra. But at least two potential reasons suggest themselves for this. The first reason, which Corfield challenges with much passion but not altogether that much success (more on this below), is that philosophers’ concerns can float somewhat free of local trends and advances in core mathematics. The second, which

[2] For instance, Corfield writes approvingly that ‘One of the main reasons for the introduction of the methodology of scientific research programmes was to provide rules (if only retrospective ones) to determine the relative status of competing research programmes’ (p. 186).

Corfield fails to address, is that philosophers' professionalism and academic self-discipline often curb any inclinations they might have to pronounce on areas in which they have a reasonable grounding but lack expertise.

Corfield also overestimates the mathematical background of at least part of his intended readership. For example, it would be hard for anyone but a research-active mathematician to appreciate the claim, here apparently taken on authority, that mathematicians can see analogies between the Langlands programme and topological quantum field theory (p. 250), and moreover to try to spin some philosophical significance out of it. Likewise, the journey in just four pages (pp. 93–96) from Dedekind's introduction of ideals to the conclusion that adèles may be given a topology which turns them into a locally compact group will prove hard going for the previously untravelled, as Corfield anticipates. In general, most readers not employed by a mathematics department will find the book too fast-paced in several places. Still, Corfield's ambition is to mathematically educate today's philosopher. Opening up the treasure chest of contemporary mathematics is certainly an effective way of doing so, even if we might have wished to linger over the jewels a little longer to fully appreciate them. Corfield also has a regrettable tendency to fire quick criticisms at his opponents without reply. A typical example occurs when assessing Teun Koetsier's line of thought that (roughly speaking) a research project or tradition is progressive in proportion to the number of significant conjectures or theorems it generates. Corfield rejects this hypothesis for several reasons, one of them being that 'Other candidates for signs of progress include the reorganisation of existing bodies of work and the production of new techniques to solve problems which need not be theorems, for example, enabling one to solve a new class of differential equations' (p. 197). But does the production of new techniques not bring in its train a host of theorems about the solutions of equations in the new class? And is it not even worth considering the more general retort that ultimately what is of importance in a technique is the number of new conjectures or theorems it generates—that ultimately a technique is *for* something? Likewise, is it so evidently wrong-headed to maintain that reorganisations of existing domains are ultimately valuable only insofar as the reorganisations can be cashed out in the form of generalisations or connecting principles, that is, as more theorems of a certain kind? Corfield owes it to the view under attack to take the dialectic further.

I expect that mathematicians and philosophers will diverge in their opinions of Corfield's book. No doubt it will delight many mathematicians with its abundance of recent, even of-the-moment, mathematics, and its sensitivity to the history of the subject. John Baez, a mathematical physicist whom Corfield cites approvingly, has already returned the compliment by remarking that, unlike other philosophers of mathematics, who are hopelessly out of date, Corfield is someone we might expect to meet on the Internet rather than in a fin-de-siècle Viennese coffeehouse. The tribute is liable to back-handed interpretation, but Baez intends it in the kindest possible way. Philosophers, too, will appreciate Corfield's close attention to mathematicians' quotidian concerns. But they are unlikely to be moved by the arguments for Corfield's anti-foundationalist manifesto. In fact, they might justifiably complain that it is not clear what his anti-foundationalism amounts to. Take the following passage, which contains the nub of Corfield's opposition to orthodox philosophy of mathematics:

> Straight away, from simple inductive considerations, it should strike us as implausible that mathematicians dealing with number, function and space have produced nothing of philosophical significance in the past seventy years in view of their record over the previous three centuries. Implausible, that is, unless by some extraordinary event in

> the history of philosophy a way had been found to *filter*, so to speak, the findings of mathematicians working in core areas, so that even the transformations brought about by the development of category theory, which surfaced explicitly in 1940s algebraic topology, or the rise of noncommutative geometry over the past seventy years, are not deemed to merit philosophical attention. This idea of a 'filter' is precisely what is fundamental to all forms of neo-logicism. But it is an unhappy idea. Not only does the foundationalist filter fail to detect the pulse of contemporary mathematics, it also screens off the past to us as not-yet-achieved. *Our job is to dismantle it*... (pp. 7–8, final italics mine)

The labels 'foundationalist' and 'neo-logicism' are unhappy, but no matter.[3] A weak reading of Corfield's claims about the foundationalist filter (in this passage and throughout) is that mainstream philosophy of mathematics can coexist with so-called philosophy of real mathematics (more or less, methodology) so long as the former realises that its distillation of mathematical practice ignores much of the practice. On a strong reading, however, there can be no room for orthodox philosophy of mathematics, since this kind of philosophy filters out much of mathematical practice—and filters must be dismantled. Indeed, Corfield more than insinuates that the philosophy of real mathematics is what real philosophy of mathematics is supposed to look like. What is at stake between the readings, then, is whether Corfield thinks there can be a philosophical approach to mathematics other than his own brand of methodology.

The ambiguity is unfortunately never resolved. Although proudly heralded, Corfield's metaphilosophical position is never clearly spelled out. Moreover, there are considerable difficulties with both readings. The weak reading appears to have no polemical force. What filtering philosopher is unaware that he ignores the detail of mathematical practice? This reading has Corfield hitting out at a strawman. The strong reading, on the other hand, has the implication that some perennial questions of the philosophy of mathematics, such as 'What is mathematics about'?, 'What is mathematical truth?', 'How do we know mathematics?', and so on, cannot be systematically tackled, at least not in any distinctively philosophical way. That claim, however, is in no way supported by the rest of the book and is *prima facie* highly implausible. In fact, the only way I can see to do justice to the tenor of Corfield's thought is to contradict some of the explicit passages in which he opposes philosophers filtering of mathematical practice. Perhaps he means to recommend his methodology above all but also wants to allow a more philosophical approach that (inevitably) still filters out some of mathematical practice, although not as much as has traditionally been the case. You just have to use a coarser filter, that's all. But I am not sure. The book does not altogether leave me with the sense that the strong reading exaggerates Corfield's intentions.

I also remain unconvinced by Corfield's specific sideswipes at filtering philosophies of mathematics in Chapter 1. Princeton lore has it that Alonzo Church, when in the University's Mathematics Department (Fine Hall), enjoyed remarking that 'there's no argument in this

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[3] 'Foundationalist' wrongly suggests that contemporary philosophers take mathematics to be founded on a basis of self-evident truths (see traditional foundationalism in epistemology). As for 'neo-logicism', Corfield's targets presumably include not just neo-logicism (of the variety, say, espoused by Crispin Wright and Bob Hale) but also any philosophy of mathematics that makes general claims about the nature of mathematics without paying close attention to its practice (for example, the different varieties of structuralism on offer today).

building that cannot be formalised in set theory'.[4] That was true then; and by and large it remains true today. Set theory contains surrogates for all or almost all the entities, arguments and theorems of non-set-theoretic mathematics. Moreover, this fact is recognised by many practising mathematicians (if sometimes rather vaguely). And of course set-theoretic reductions, terminology, techniques, and so on, permeate much of modern mathematics. Anyone who maintains that the fundamental questions of the philosophy of mathematics—questions about the nature of mathematics, mathematical truth, mathematical knowledge, and so on—can be answered by restricting attention to the basic principles of set theory will find nothing in Corfield's long introductory chapter to deflect her from this belief. And anyone who holds the weaker view that the main questions of the philosophy of mathematics can be answered by restricting attention to some set of basic mathematical principles (not necessarily just set-theoretic ones), together with an account of logic and a discussion of how to model mathematical practice using these resources, will likewise not be troubled by Corfield's polemic in Chapter 1. These positions are extreme, to be sure, and it would be an impoverished philosophy of mathematics that is guided by either of these directives, especially the first. But Corfield does not manage here to deliver any blows against them. That is not to deny that some of the examples contained throughout the book, as opposed to the argumentation in Chapter 1, look like they impact importantly on the philosophy of mathematics. Corfield is right to stress that analytic philosophy has flourished through its acquaintance with science, mathematics and logic. Mathematics is indeed a superb resource for philosophy. But although he piques our interest with some intriguing ideas, Corfield never takes the time to set out systematically what the moral for philosophy should be. For instance, he never develops the implications of viewing mathematical theories as attempts to clarify and elaborate certain central ideas rather than as collections of statements (see esp. p. 181). But if this hypothesis is right, what follows for philosophy? Can the ideas underlying mathematical theories in principle be captured by sets of statements? If not, should discussion of the nature of mathematics proceed differently? How exactly? On the other hand, we are offered several observations about mathematical practice that can plausibly be bracketed by, or tacked onto, orthodox philosophy of mathematics. As we have seen, mathematicians ask of their proofs to generate more widely applicable concepts and techniques. But cannot this dimension of mathematical practice be added wholesale to any 'foundationalist' or 'neo-logicist' philosophy of mathematics? The example suggests that often the interests of orthodox philosophy of mathematics and philosophy of real mathematics lie in different places. In such instances, philosophy and methodology have different, yet compatible, intellectual aims. In sum, the book presents us with ideas that seemingly matter to philosophy without giving us a clear indication of why they matter; or it presents us with ideas which, although of considerable independent interest, appear not to lie in the province of philosophy.

At the end of the book comes the one-chapter Part IV dealing with higher-dimensional algebra and the significance of diagrammatic reasoning in mathematics. Corfield has had his finger on the pulse of modern mathematics throughout, but in this last chapter he moves from diagnosis to prognosis. The advertising for the chapter contains some bold claims: he aims to do for the philosophy of mathematics what some eminent philosophers of physics have done for their discipline (p. 233); he puts himself in the analogous position vis-à-vis mathematics of a 'bright spark' who has to introduce quantum gravity to backward philosophers of physics who barely even know about quantum mechanics or general relativity (p. 235); and he maintains that his outline in this chapter should provide sufficient material to launch at least a dozen doctoral theses (p. 269). A striking hypothesis, which certainly whets the appetite, is

[4] Or words to that effect: I haven't seen a reference in print.

that if 1-categories have been a main concern of twentieth-century mathematics, *n*-categories more generally will be the province of the twenty-first. Corfield is right, however, that the chapter can only form the beginnings of a case for higher-dimensional algebras prospects, and that only time will tell.

The comparison with the philosophy of physics and the philosophy of science is one of the argumentative resources Corfield draws upon throughout to back up his claim that philosophers of mathematics should look at mathematical practice with a finer lens. Since the 1960s, and in particular as a result of Thomas Kuhn's influence, philosophers of science have paid much more attention to the details of scientific practice, to the point where many contemporary philosophers of science tend to look for inspiration more to the history or contemporary state of their science(s) of interest than to other areas of philosophy. One reason for this is surely that the potentially non-cumulative nature of science has deep implications for philosophy. For instance, it radically affects our credence in science, our views on science's objectivity, and our ontological outlook more generally. A second reason is that fundamental physics contains two highly-confirmed and deeply entrenched theories, quantum mechanics and general relativity, which are incompatible. *Prima facie*, however, neither reason is transferable to mathematics. Corfield does not give the second reason its due, and although he does square up to the first (pp. 6–7), he doesn't manage to defuse it. The few authors who have debated the question of whether Kuhnian revolutions occur in mathematics—that is, revolutions in which the prior paradigm's results or principles are rendered either meaningless or false in the posterior paradigm—are agreed that no such examples can be found in mathematics.[5] Whether such revolutions have occurred in the modern era may be a live question for science; but surely it isn't one for mathematics. Indeed, Corfield himself concedes this point: 'What distinguishes mathematical transformations or revolutions from their scientific counterparts is the more explicit preservation of features of earlier theories' (p. 7). Yet he then adds that these theories survive 'in a radically reinterpreted form' (ibid.). His examples: Euclidean geometry is now thought of as one species of geometry rather than the geometry of the space we inhabit; and mathematicians today find it meaningful to ask whether two-dimensional Euclidean geometry emerges as the large-scale limit of a quantum geometry, a question that would have made no sense to Euclid. But Corfield says nothing to block the retort that every mathematical theory has a 'hard' part, consisting of theorems, axioms, principles, and so on, which has never been rejected, and a 'soft' part, encompassing presuppositions, applications, motivating ideas, in short, the whole associated disciplinary matrix, which is the part of the theory that has changed over the centuries.[6] What remains to be argued, then, is precisely how and why the radical changes in the 'soft part' are philosophically significant.

Part IV also contains a discussion of the role of diagrams in mathematics, and in particular diagrammatic notation, in connection with higher-dimensional algebra. Corfield's claim is that diagrammatic calculations are springing up in many new areas of mathematics, and that often 'diagrams are not just there to illustrate, they are used to calculate and to prove results

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[5] Pourciau (2000, p. 299) contains a useful collection of quotes on this point from various contributors to Gillies (1992). His article argues that the intuitonistic revolt against classical mathematics, had it been successful, would have been an example of Kuhnian revolution in mathematics (and furthermore that intuitionism failed for contingent reasons).

[6] The hard/soft terminology comes from Steven Weinberg, who applies it to modern physical theories (1998, p. 50). Corfield's terminology on p. 7 suggests that he implicitly recognises this point, but he does not tackle it in his discussion.

rigorously' (p. 254), *pace* the traditional philosopher of mathematics (see, for example, p. 252). But *pace* which philosopher of mathematics? No sensible philosopher of mathematics denies that diagrams are often used to calculate. Likewise, no sensible philosopher of mathematics would deny another of Corfield's claims that without diagrammatic representation it can be exceedingly hard to find proofs of certain mathematical facts. So what about the use of diagrams for rigorous proof, as opposed to calculation or mathematical discovery? First, what exactly is a diagram? Surprisingly, Corfield offers little by way of theoretical elucidation on this point. The criterion he appears to rely on is that diagrams require 'two dimensions' (p. 261) and amount to 'exploiting the freedom of the page' (p. 258), as opposed to being linear. But this won't do: standard alphanumerical notation is also two-dimensional and runs both vertically and horizontally across the page. Well, one might reply, non-diagrammatic notation can in principle be captured in one-dimensional form. But cannot diagrams also in principle be encoded in one-dimensional form? And after all, since this is the philosophy of *real* mathematics were talking about, of what relevance is it what could be done in principle? So we still don't know what a diagram is. We are somehow supposed to take it on trust that our theoretical ignorance on this point doesn't affect any of the issues discussed in Chapter 10.

As for rigorous proof, the relevant point is surely this. Consider the kind of proof that is intended to convince the sceptic, to take on all comers, to find a permanent place in the mathematical archive more or less as it stands. To the extent that any diagrams involved in this proof take us in a direction away from mechanised verifiability, from a proof of the kind that (as the saying goes) if ever two people held it in dispute they could take up their pencils and say to one another 'let us calculate', from, in short, a gap-free proof, then the presence of such diagrams *in this kind of proof* is undesirable. Now this is not to say that the dizzy standard of gap-free proof is ever attained in mathematics, nor that mathematicians ever aim for it—as opposed to (sometimes) aiming for a more practicable approximation to it—nor indeed that this applies to other contexts. What it *is* to say, however, should be nothing controversial. It's a near tautology given our understanding of what the very highest standards of proof are. And notice that the claim doesn't essentially have anything to do with diagrams. The desirability of not slackening current standards when it comes to the very highest form of mathematical justification applies to any notational form whatsoever. In particular, it is perfectly compatible with the thought that diagrammatic reasoning—however exactly this is defined—can be both expedient and rigorous. It remains unclear whether Corfield's intriguing examples of diagrammatic notation are intended to challenge this thought, probably the only entrenched thought about the mathematical use of diagrams in philosophy today. The combination is characteristic of Corfield's book: captivating examples of contemporary mathematics coupled with insufficient philosophical amplification. The book splendidly illustrates what philosophers are missing out on by turning a blind eye to contemporary methodology. It should leave every philosopher of mathematics with more than a sense that there is much here that one could profitably address. But Corfield's book ultimately remains unclear on *why* it should be addressed, and on how to harness this material towards fundamental, perennial philosophical questions about the nature of mathematics.

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