

Towards a Philosophy of Real Mathematics – Review of a book by David Corfield

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David Corfield starts his interesting book with a radical rejection of much that has been written about the philosophy of mathematics. He has no interest in ontology, epistemology, logicism, Platonism, Kantianism, nominalism, fictionalism etc., and does not mention most of them. Taking his cue from Nietzsche, Lakatos and a few others, he likens the traditional approach to the examination of a dead body, [pp. 3-5]. He criticizes the attitude that has led many philosophers of mathematics to imagine that everything of interest to their subject occurred between 1880 and 1930, contrasting this with a very different attitude among the philosophers of physics. We shall see that events since the publication of his book make it even more relevant now than it might have seemed in 2003.

One of Corfield's strongest arguments for turning away from the philosophy of dead mathematics is that it focuses on logical correctness, and is powerless to explain why some topics are regarded as crucial to the subject while others are considered irrelevant, no more than technical games. He argues that if one wants to discuss *real* mathematics, one has to place the subject in its historical context, and focus on the value judgements that mathematicians make, and what lies behind them.

Chapter 1 is fairly demanding philosophically, and is written to explain his rejection of the standard approach and to lay out his stall. Put briefly, he is interested in teasing out philosophical questions from the many ways in which real mathematics has been done. He castigates philosophers for presenting the subject as if it were a purely abstract entity that can and should be discussed in a historical and social vacuum. He has to avoid going to the other extreme, letting his book become no more than a study of the history of mathematics.

But, being a philosopher, he succeeds in this task.

The first word of the title of the book is ‘Towards’ and Corfield admits that he intends to cover a wide range of topics, each of which needs to be fleshed out in much more detail. Many of his examples come from the fields he knows best, but this is inevitable. Its range is wider than the expertise of most mathematicians and Corfield knows what he is talking about, at least as far as I could tell. An important merit of the book is its large number of quotations. These help to keep the arguments anchored in the real world - to the extent that mathematicians live in the real world.

Before I continue, a small digression is in order. In most circumstances a reviewer should not read other reviews, in order to assure independence. I followed this procedure, but, after acceptance, it was suggested that I might care to make some comments on a much earlier review by Timothy Bays. I do this in an appendix.

Corfield greatly admires Imre Lakatos, who was one of the pioneers in this approach to the philosophy of mathematics, [Chapter 7]. However, he emphasizes that Lakatos provided little in the way of examples, and that he relied excessively on an analysis of the history of Euler’s polyhedron theorem. Lakatos maintained that the most interesting part of a mathematical investigation was the struggle to find the best way of expressing one’s intuitions about it, and that the final formulation of the relevant axioms inhibited further advances. Corfield rejects this as a universal rule, and demonstrates, by giving examples, that the development of mathematics can proceed in many different ways, [p. 152]. To illustrate this, he discusses the Eilenberg-Steenrod axioms at some length, [p. 169]. These put homological algebra on a much firmer foundation when they appeared in 1945, and were the starting point for later research. They led to a vigorous research programme and needed few changes subsequently, except for the removal of the dimension axiom in some applications, such as K-theory.

One might add the classification of the finite simple groups, most of which was completed by 1983; see [2] for an account of some issues arising from the nature of the proof. Although it is still the largest ever collaborative project in pure mathematics, the axioms defining finite simple groups had been known for many decades before it started. These examples do not show that Lakatos was totally misguided, only that a developed philosophy of real mathematics must go far beyond the issues that he considered, which most mathematicians would recognize as embodying an important aspect of their work.

There are entire fields in which the production of new axiom systems seems to be largely irrelevant, provided one is willing to make a distinction between axiom systems and mere definitions. To the despair of those philosophers who

seek precise meanings for words, there is a continuous scale between these two notions. Corfield mentions the formula of David Bailey, Peter Borwein and Simon Plouffe in 1997 that enables one to calculate very remote digits in the expansion of π without having calculated all the earlier ones, provided one works base 16. While he states that this is not in itself a fundamental piece of mathematics, [p. 66], some fields advance by the accumulations of large numbers of individually modest pieces. The cumulative effect over fifty years can be transformative. One instance is the progress in understanding – rather than just computing – the spectra of differential operators of the type that occur in non-relativistic quantum mechanics.

Nobody could deny that Bayesian statistics is of considerable philosophical interest. A subject with 18th century origins, it was widely considered disreputable, right up to the 1970s, because its dependence on human judgement was considered incompatible with the Baconian philosophy of science. In the formulation due to Jaynes this problem was minimized by the use of maximum entropy priors, but in applications outside physics it is often necessary to use expert judgement to set the priors. Over the next thirty years Bayesian statistics was rehabilitated to such an extent that in 1996 Bill Gates could claim that ‘Microsoft’s competitive advantage is in its expertise in Bayesian networks’. Today it is fully recognized as the most appropriate technique for analyzing certain types of highly complex data sets. It would be interesting to explore the relationship, if any, between this and the demise of the Baconian philosophy of science under the onslaught of Karl Popper’s fallibilism, now regarded by scientists as the natural philosophy of science. See [8] for a review of a book of Jaynes, published posthumously in 2002.

Corfield discusses the issues surrounding Bayesianism and points out that leading Bayesian statisticians have disagreed about the prospects of using its methods to study mathematical discovery, even in a qualitative manner, [p. 110-119]. One of the problems emphasized by John Earman is the possibility of ‘non-Bayesian shifts’, in which some minor observation, or even a dream, might cause one to completely reassess one’s view about the nature of a problem. There is a well-established literature attesting to the frequency of such events, and many mathematicians have personal experience of them. Corfield goes on to ask what type of evidence persuades mathematicians to change their minds about the likely truth of a proposition. Checking some identity in a finite number of cases cannot be enough, because there are well-known examples of arithmetic identities whose first counterexample occurs for an extraordinarily large integer; Corfield mentions a certain bound on the number of primes less than x in terms of a function $li(x)$, for very large x , [p. 127]. One might also mention the Pólya conjecture for prime factorizations, which holds for all natural numbers up to, but not including, 906,150,257; in spite of its falsity, it

appears that the conjecture may well be true for ‘most’ numbers, in a certain well recognized sense.

On the other hand when John McKay noticed a very small number of coincidences between the dimensions of representations of the monster group and functions arising in the study of elliptic curves, this attracted great attention, [pp. 125-126]. Eventually Richard Borcherds found a logical thread connecting these two very different facts in 1992, for which he was awarded a Field’s medal in 1998. Corfield explains the compelling nature of the coincidences as based on the deep background knowledge of those in the field. The experts felt that it should be possible to use the links between the two subjects to provide a proof. It is not easy to see how intuitions about the significance of such a coincidence could be formalized, but there is no doubt that they exist and that they are important. To ‘explain’ them as arising from mysterious Platonic perceptions of an ultimate reality is simply to refuse to address the issue seriously, [7].

Chapter 3, on uncertainty in mathematics and science, discusses a range of interesting interconnections between the subjects. I will not spoil his tale by listing these, but Corfield has missed an opportunity in his brief discussion of quantum field theory, [pp. 137-138]. He is not referring to this field as it was understood in the 1970s, which has extremely impressive experimental support in spite of the fact that it is not coherent by the standards of pure mathematicians, but to the circle of ideas related to superstring theory, mirror manifolds, duality, etc. He should have drawn the reader’s attention to the fact that a large number of physicists do not regard this as physics, because over a period of thirty years it has not produced any even remotely testable prediction. Even if evidence for supersymmetry appears in the current round of experiments at the Large Hadron Collider, this will not validate string theory itself. The fact that it has led to some extremely interesting mathematical conjectures, some of which have been proved in the accepted rigorous sense, does not say anything about its status *as physics*. These are very controversial issues, with some very big names on both sides. At stake is the very definition of science, and how long one is justified in suspending judgement.

There are many other problems that merit the type of treatment that Corfield is interested in. One arises in the spectral theory of operators on Banach spaces. By the 1970s numerical analysts had already realized that the eigenvalues of medium sized matrices could depend on the matrix entries in a very unstable manner. Nick Trefethen took up this issue around 1990 and introduced the notion of pseudospectra, which makes sense in infinite dimensions as well as for matrices, [11]. This notion gathers under one roof a plethora of different elements, which had previously been studied largely independently. Some researchers reacted negatively to the subject, and, indeed, one can do without it, but it has been accepted by steadily increasing numbers of mathematicians over

the last ten years because it helps to clarify the nature of problems that had been faced in various contexts. Although motivated by problems in numerical analysis, its influence has now penetrated to the theory of pseudodifferential operators. Perhaps it lies near the higher end of Corfield's scale from crucial theories to those that are 'not worth a jot', [p. 11].

A second issue relating to analysis is its heavy dependence on inequalities. When an analyst states that an equality is merely a pair of inequalities that go in both directions (with different proofs in the two cases), this is precisely what makes many other mathematicians feel that analysis is an unnatural subject. Unfortunately for them, it shows no sign that it is about to abandon this 'abhorrent' methodology. Analysts also seem to be more sympathetic to constructive mathematics, particularly the version of Errett Bishop, [4], than are algebraists and geometers. Perhaps this is because they are closer to applied mathematics and see that an existence proof that relies on a nonconstructive element, such as a compactness argument, may carry very little useful information about the properties of the solution. These differences are partly aesthetic and partly driven by necessity, but they certainly create a barrier between analysts and many other pure mathematicians.

In 2004 Peter Swinnerton-Dyer made a distinction between structural statements such as the Riemann Hypothesis and accidental statements such as Goldbach's conjecture, [9, p. 2439]. In purely logical terms, the word 'accidental' is meaningless in this context, but somehow one knows what he means. I suspect that his distinction involves a value judgement: far more people care about the truth of the Riemann Hypothesis, and for reasons that a philosopher might wish to understand. But value judgements are dangerous; if either conjecture were to be proved false, it would suddenly seem much less interesting.

In 2001 Alain Connes, a committed Platonist, who has spent a lifetime working on C^* -algebras and their applications, nevertheless excluded the theory of Jordan algebras from the Platonic world of mathematics, [6]. How do mathematicians make such value judgements, and are their opinions more than prejudices? Corfield gives a lengthy account of another example, the theory of groupoids, which are still nothing like as well accepted as he thinks they should be, in spite of strong support from Alexander Grothendieck, Ronald Brown, Alan Weinstein, Jean Bellissard, Connes and others, [pp. 208-214]. Philosophers should try to understand the issues that underlie the acceptance, or not, of such concepts. Category theory has an even stronger claim to being an essential part of mathematics, perhaps as an alternative to set theory, but many pure mathematicians have no interest in either subject, and do not seem to suffer from that fact, [p. 201]. Corfield also points out that the apparently accepted set-theoretic definition of a function as a set of ordered pairs satisfying certain axioms is not well adapted to computation, where the older idea of a

function as an algorithm that produces an output given some value of the input is more relevant. This reminds one of the warning of Lakatos that one should not confuse a set of axioms with the intuitions that gave rise to it, [p. 201-202].

Since Corfield's book was published, the ingress of computers into the preserve of mathematicians has proceeded apace, and a meeting of the Royal Society was convened in 2004 to discuss 'The Nature of Mathematical Proof', [9]. Some of the issues that arose were anticipated by Corfield, but not all. One was Thomas Hales' solution of Kepler's sphere packing problem. Robert MacPherson, the Editor of the Annals of Mathematics, described why the editors had felt compelled to accept the paper even though a team of experts had eventually abandoned the effort to check all the details after several years of intense work. (Unfortunately MacPherson's contribution was not published.) Perhaps the notion that pure mathematics revolves around concepts that mathematicians can settle with certainty will one day be consigned to the dustbins of history.

In 1998 David Ruelle wrote that 'human mathematics is a sort of dance around an unwritten formal text, which if written would be unreadable', [10]. In spite of this, the Royal Society meeting needed to discuss the slow but steady growth of formally verified mathematics. This arose from the development of computer languages and programs that can check every step of a purported proof for correctness, and also fill in moderately small gaps in accordance with accepted formal rules. Applications of this process still require much effort, but a formal proof of the four colour theorem was announced by Georges Gonthier and Benjamin Werner in 2004. Such proofs are not intended to be read, and they do not provide the intuitive insights of traditional rigorous proofs. Formal proof techniques and other developments, such as the growing use of interval arithmetic in rigorous computer-assisted proofs, seem to be taking us across the Rubicon. This fate can be avoided if enough mathematicians insist that they will accept only proofs that can be understood without artificial aids, however the proofs might have been generated. There is no evidence that this will happen.

The task of a reviewer is to whet readers' appetites, not to milk the contents so thoroughly that nothing worthwhile is left. I have said little or nothing about Corfield's lengthy discussions of the role of analogy in mathematics, the mutual interactions between mathematics and physics, and higher dimensional algebra. The book abounds with thought-provoking ideas and well repays the effort needed to understand the subtleties of its subject. Much remains to be done, and one can only hope that others will join Corfield in taking up the task.

Appendix. Corfield's book has been discussed in several places on the web,

but I will restrict attention to a review written by Timothy Bays in 2004, [3], which represents some of the reactions of traditional philosophers. Some of the differences between us can be traced to the fact that Bays is a professional philosopher whose speciality is formal logic and related fields, while I am an analyst, whose specialities are operator and spectral theory. Bays describes Chapter 1 as polemical, while I describe Corfield as setting out his stall. It is indeed the case that, in doing so, he argues that the other stall-holders are selling ‘dead mathematics’, but one need not be put off by this rhetoric if he is selling something new and interesting, as I believe he is.

Bays is right when he states that Corfield dismisses mathematical foundationalism without exploring its faults or explaining why many philosophers have found it attractive. Corfield, however, is advocating a radical expansion of the range of philosophical debate, and this cannot be achieved by yet another discussion of old issues, even if they are important. Today, one might refer to the Royal Society meeting, for example the articles of Michael Aschbacher and Paul Cohen [2, 5], as evidence of the steadily increasing range of quasi-philosophical issues that mathematicians now have to cope with.

Bays does not consider that Corfield provides substantial new philosophical insights, but accepts that there probably are new insights available to those who are prepared to do the work of fleshing out the programme that Corfield is outlining, [3, endnote 6]. Corfield covers too much ground to please many professional philosophers, who are well aware that attractive ideas may be very difficult to develop in detail. One might liken his approach to those of Popper, Lakatos, Kuhn and others, who gave overviews of new areas that needed to be explored. Only if this is done with conviction will others start to travel over the unfamiliar ground that he is describing. This review is written for mathematicians, and it seems appropriate to adopt their standards, whether or not they mesh with those of philosophers of mathematics.

Andrew Arana has written yet another review of Corfield’s book, [1]. This discusses a number of further issues of great interest, and is strongly recommended to readers.

References

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