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CHAPTER 1

Introduction: a role for history

> To speak informatively about bakery you have got to have put your hands in the dough. (Diderot, *Oeuvres Politiques*)

> The history of mathematics, lacking the guidance of philosophy, has become *blind*, while the philosophy of mathematics, turning its back on the most intriguing phenomena in the history of mathematics, has become *empty*. (Lakatos, *Proofs and Refutations*)

1.1 REAL MATHEMATICS

To allay any concerns for my mental health which the reader may be feeling if they have come to understand from the book's title that I believe mathematics based on the real numbers deserves singling out for philosophical treatment, let me reassure them that I mean no such thing. Indeed, the glorious construction of complex analysis in the nineteenth century is a paradigmatic example of what 'real mathematics' refers to.

The quickest way to approach what I *do* intend by such a title is to explain how I happened upon it. Several years ago I had been invited to talk to a philosophy of physics group in Cambridge and was looking for a striking title for my paper where I was arguing that philosophers of mathematics should pay much closer attention to the way mathematicians do their research. Earlier, as an impecunious doctoral student, I had been employed by a tutorial college to teach eighteen-year-olds the art of jumping through the hoops of the mathematics 'A' level examination. After the latest changes to the course ordained by our examining board, which included the removal of all traces of the complex numbers, my colleagues and I were bemoaning the reduction in the breadth and depth of worthwhile content on the syllabus. We started playing with the idea that we needed a campaign for the teaching of real mathematics. For the non-British and those with no interest in beer, the allusion here is to the Campaign for Real Ale (CAMRA), a movement dedicated to maintaining traditional brewing

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techniques in the face of inundation by tasteless, fizzy beers marketed by powerful industrial-scale breweries. From there it was but a small step to the idea that what I wanted was a Campaign for the Philosophy of Real Mathematics. Having proposed this as a title for my talk, it was sensibly suggested to me that I should moderate its provocative tone, and hence the present version.

It is generally an indication of a delusional state to believe without first checking that you are the first to use an expression. The case of 'real mathematics' would have proved no exception. In the nineteenth century Kronecker spoke of 'die wirkliche Mathematik' to distinguish his algorithmic style of mathematics from Dedekind's postulation of infinite collections. But we may also find instances which stand in need of no translation. Listen to G. H. Hardy in *A Mathematician's Apology*:

> It is undeniable that a good deal of elementary mathematics – and I use the word 'elementary' in the sense in which professional mathematicians use it, in which it includes, for example, a fair working knowledge of the differential and integral calculus – has considerable practical utility. These parts of mathematics are, on the whole, rather dull; they are just the parts which have the least aesthetic value. The 'real' mathematics of the 'real' mathematicians, the mathematics of Fermat and Euler and Gauss and Abel and Riemann, is almost wholly 'useless' (and this is as true of 'applied' as of 'pure' mathematics). It is not possible to justify the life of any genuine professional mathematician on the ground of the 'utility' of his work. (Hardy 1940: 59–60)

Overlooking his caveat (1940: 72), many have enjoyed reproducing this quotation to point out Hardy's error, that the mathematics of Fermat and Euler and Gauss and Abel and Riemann has turned out to be extremely useful, for esoteric physical theories such as string theory, but also more practically for the encryption systems which we trust keep our financial dealings secure. But this is not my concern here. I wish rather to pay attention to Hardy's use of 'real'. Elsewhere he talks in a similar vein of pieces of mathematics being 'important' and even 'serious'. I have dropped his scare quotes. It is hard to see that they can achieve very much in our times.

Hardy is being extremely exacting here on mathematicians who want to join the real mathematicians' club. I think we can afford to be considerably more generous. Where second-rate mathematicians are given short shrift by Hardy, I am willing to give even computers a fair hearing, and, although I shall not be speaking of them, people employing 'dull' calculus are not to be excluded. But that having been said, Fermat and Euler and Gauss and Abel and Riemann, along with Hilbert and Weyl and von Neumann and

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Grothendieck, are right there at the core of what I am taking to be real mathematicians.

What then of the *philosophy* of real mathematics? The intention of this term is to draw a line between work informed by the concerns of mathematicians past and present and that done on the basis of at best token contact with its history or practice. For example, having learned that contemporary mathematicians can be said to be dealing with structures, your writing on structuralism without any understanding of the range of kinds of structure they study does not constitute for me philosophy of real mathematics. But, then, how exacting am *I* being?

1.2 THE CURRENT STATE OF PLAY

Ian Hacking opens his book *Representing and Intervening* with a quotation from Nietzsche's *The Twilight of the Idols*:

> You ask me, which of the philosophers' traits are idiosyncracies?
> For example: their lack of historical sense, their hatred of becoming, their Egypticism. They think that they show their respect for a subject when they dehistoricize it – when they turn it into a mummy.

He then continues: 'Philosophers long made a mummy of science. When they finally unwrapped the cadaver and saw the remnants of an historical process of becoming and discovering, they created for themselves a crisis of rationality. That happened around 1960' (Hacking 1983: 1).

If this portrayal of mid-twentieth century philosophy of science strikes a chord with you, you may well then ask yourself whether mathematics was faring similarly at the hands of philosophers at that time. Hacking's reference to the year 1960 alludes, of course, to the rise within philosophy of science of a movement which took the history of science as a vital fount of information, epitomised by Kuhn's *The Structure of Scientific Revolutions* (Kuhn 1962). Imre Lakatos, with his motto 'Philosophy of science without history of science is empty; history of science without philosophy of science is blind' (1978a: 102), made his own distinctive contribution to this movement. And yet, as the second epigraph of this chapter suggests, we should remember that the rationalist theory of scientific methodology he proposed and developed in the late 1960s and early 1970s derived from ideas developed in his earlier mathematical text *Proofs and Refutations*, which had appeared as a series of journal articles at around the same time as Kuhn's *Structure*. There we find sharp criticisms of a process similar to

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mummification, the treatment of an evolving body of knowledge as lifeless, levelled now at formalist and logicist philosophers and mathematicians:

Nobody will doubt that some problems about a mathematical theory can only be approached after it has been formalised, just as some problems about human beings (say concerning their anatomy) can only be approached after their death. But few will infer from this that human beings are 'suitable for scientific investigation' only when they are 'presented in "dead" form', and that biological investigations are confined in consequence to the discussion of dead human beings – although, I should not be surprised if some enthusiastic pupil of Vesalius in those glory days of early anatomy, when the powerful new method of dissection emerged, had identified biology with the analysis of dead bodies. (Lakatos 1976: 3n.)

Someone working closer to the 'glory days' of early logical reductionism was Ludwig Wittgenstein. Employing imagery similar to that of Hacking and Lakatos, he writes of Russell's logicist analysis of mathematics, 'The Russellian signs veil the important forms of proof as it were to the point of unrecognizability, as when a human form is wrapped up in a lot of cloth' (Wittgenstein 1978: 162, remark III-25). But Lakatos went further than Wittgenstein in reporting to us what lay under the cloth. He exposed much more of the physiology of the mathematical life-form. So did his revelations lead to a parallel 'crisis of rationality' in the philosophy of mathematics?

To provide us with the means to gauge the situation, let us briefly sketch the current state of a central branch of philosophy of science – the philosophy of physics. Now, the first thing one notices here is the extensive treatment of recent and contemporary developments. Consider, for instance, the volume – *Physics meets Philosophy at the Planck Scale* (Callender and Huggett 2001). As this striking title suggests, philosophers of physics may interest themselves in specific areas at the forefront of physics research and yet still ask palpably philosophical questions about time, space and causation. By contrast, elsewhere one finds less specific, more allusive, studies of the way research is conducted. For instance, a book such as *Models as Mediators* (Morgan and Morrison 1999) analyses the use of models over a wide range of physics as a part of the general programme of *descriptive epistemology*. Issues here are ones just about every physicist has to deal with, not just those striving to read the mind of God. So, on the one hand, we have philosophical and historical analysis of particular physical theories and practices, while, on the other, we have broader treatments of metaphysical and epistemological concerns, grounded on detailed accounts of physicists' activities. There is a creative interaction between these two strands, both of which are supported by the study of physical theories, instrumentation and experimental methodologies of earlier times, and there is even a specialist

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journal – *Studies in History and Philosophy of Modern Physics* – devoted to physics after the mid-nineteenth century.

Now, certainly one can point to dissension in practitioners' visions of what philosophy of physics activity should be like. Indeed, one can construe passages of Cartwright's *The Dappled World* (1999a, see, e.g., pp. 4–5) as a call for a philosophy of real *physics*. Nevertheless, there is a strong common belief that one should not stray too far from past and present practice. How different things are in the philosophy of mathematics. While there is a considerable amount of interest in the ways mathematicians have reasoned, this is principally the case for the nineteenth century and earlier and is usually designated as *history*. By far the larger part of activity in what goes by the name *philosophy of mathematics* is dead to what mathematicians think and have thought, aside from an unbalanced interest in the 'foundational' ideas of the 1880–1930 period, yielding too often a distorted picture of that time. Among the very few single-authored works on philosophy of recent mathematics, perhaps the most prominent has been Penelope Maddy's (1997) *Naturalism in Mathematics*, a detailed means–end analysis of contemporary set theory. We shall return to Maddy's work in chapter 8, simply noting for the moment that its subject matter belongs to 'foundational' mathematics, and as such displays a tendency among practice-oriented philosophers not to stray into what we might call 'mainstream' mathematics. This tendency is evident in those chapters of *Revolutions in Mathematics* (Gillies 1992) which address the twentieth century.

The differential treatment of mathematics and physics is the result of fairly widely held beliefs current among philosophers to the effect that the study of recent mainstream mathematics is unnecessary and that studies of pre-foundational crisis mathematics are merely the historical chronicling of ideas awaiting rigorous grounding. Now, there are two ways to try to counteract such notions. First, one just goes ahead and carries out philosophical studies of the mainstream mathematics of the past seventy years. Second, one tries to confront these erroneous beliefs head on. Those who prefer the first strategy may wish to skip the next section, but anyone looking for ways to support the philosophical study of real mathematics may profit from reading it.

1.3 THE FOUNDATIONALIST FILTER

Various versions of the thought that it is right that mathematics and physics be given this very uneven treatment because of inherent differences between

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the disciplines have been expressed to me on several occasions when I have been proposing that philosophers could find plenty of material to mull over in post-1930 mainstream mathematics (algebraic topology, differential geometry, functional analysis, analytic number theory, graph theory, . . .). They have taken two forms:

(1) Mathematics differs from physics because of the retention through the centuries of true statements. While scientific theories are continually modified and overthrown, many true results of Euclidean geometry were correctly established over 2,000 years ago, and mankind has known arithmetic truths much longer even than this. Thus, contemporary mathematics possesses no philosophically significant feature to distinguish it from older mathematics, especially when the latter has been recast according to early twentieth-century standards of rigour. Arithmetic and its applications will provide sufficiently rich material to think through most questions in philosophy of mathematics. And even if one wished to take a Lakatosian line by analysing the *production* of mathematical knowledge and the dialectical evolution of concepts, there is no need to pick case studies from very recent times, since they will not differ qualitatively from earlier ones, but will be much harder to grasp.

(2) The mathematics relevant to foundational questions, which is all that need concern philosophers, was devised largely before 1930, and that which came later did not occur in mainstream branches of mathematics but in the foundational branches of set theory, proof theory, model theory and recursion theory. Physics, meanwhile, is still resolving its foundational issues: time, space, causality, etc.

As to point (1), I freely admit that I stand in awe of the Babylonian mathematical culture which could dream up the problem of finding the side of a square field given that eleven times its area added to seven times its side amounts to $6\frac{1}{4}$ units. Their method of solution is translatable as the calculation of what we would write

$$\{\sqrt{[(7/2)^2 + 11\cdot(6\tfrac{1}{4})]} - (7/2)\}/11 = 1/2,$$

suggesting that quadratics were solved 4,000 years ago in a very similar fashion to the way we teach our teenagers today. But, from the perspective of modern algebra and the contemporary study of algorithms, think how differently we interpret this calculation of the positive solution of a quadratic equation. As for the geometry of the Greeks, again it goes without saying an extraordinary achievement, but out of it there emerged a discipline which has undergone drastic reinterpretations over the centuries.

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Today, one way mathematicians view Euclid's Elements is the study of a case of n-dimensional Euclidean geometry, the properties of the principle bundle $H \rightarrow G \rightarrow G/H$, where G is the Lie group of rigid motions of Euclidean n space, H is the subgroup of G fixing a point designated as the origin, and G/H is the left coset space. From being the geometry of the space we inhabit, it has now become just one particular species of geometry alongside non-Euclidean geometries, Riemannian geometries, Cartan geometries and, in recent decades, non-commutative and quantum geometries. Euclidean space now not only has to vie for our attention with hyperbolic space and Minkowski space, but also with q-Euclidean space. What distinguishes mathematical transformations or revolutions from their scientific counterparts is the more explicit preservation of features of earlier theories, but, as several contributors to Gillies (1992) have shown, they survive in a radically reinterpreted form. There are meaningful questions we can ask about Euclidean geometry which could not have been posed in the time of Riemann or even of Hilbert, and which would have made no sense at all to Euclid. For example, does two-dimensional Euclidean geometry emerge as the large-scale limit of a quantum geometry? The fact that we are able to ask this question today demonstrates that the relevant constellation of absolute presuppositions, scene of inquiry, disciplinary matrix, or however you wish to phrase it, has simply changed.

Moreover, to the extent that we wish to emulate Lakatos and represent the discipline of mathematics as the growth of a form of knowledge, we are duty bound to study the means of production throughout its history. There is sufficient variation in these means to warrant the study of contemporary forms. The quaint hand-crafted tools used to probe the Euler conjecture in the early part of the nineteenth century studied by Lakatos in *Proofs and Refutations* have been supplanted by the industrial-scale machinery of algebraic topology developed since the 1930s. And we find that computer algebra systems are permitting new ways of doing mathematics, as may automated theorem provers in the future. No economist would dare to suggest that there is nothing to learn from the evolution of industrial practices right up to the present, and neither should we.

An adequate response to (2) must be lengthier since it arises out of core philosophical conceptions of contemporary analytic philosophy. In the remainder of this section I shall sketch out some ideas of how to address it, but, in some sense or other, the whole book aims to tempt the reader away from such ways of thinking. 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

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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 non-commutative 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, in the process demonstrating that philosophers, historians and sociologists working on pre-1900 mathematics are contributing to our understanding of mathematical thought, rather than acting as chroniclers of proto-rigorous mathematics.

Frege has, of course, long been taken as central to the construction of this foundationalist filter, but over the past few years new voices have been heard among the ranks of scholars of his work. Recent reappraisals of his writings, most notably those of Tappenden, have situated him as a *bona fide* member of the late nineteenth-century German mathematical community. As is revealed by the intellectual debt he incurred to Riemann, Dedekind and others, his concern was with the development of a foundational system intimately tied to research in central mathematical theories of the day. In this respect his writings are of a piece with the philosophical work of mathematicians such as Hilbert, Brouwer and Weyl. By contrast, in more recent times philosophers have typically chosen to examine and modify systems in which all, or the vast majority, of mathematics may be *said* to be represented, but without any real interest for possible ways in which distinctions suggested by their systems could relate to the architectural structure of the mainstream. Even distinctions such as finitary/infinitary, predicative/impredicative, below/above some point in the set theoretic hierarchy, constructive/non-constructive have lost much of their salience, the latter perhaps less so than the others.[1] How much less relevant to mathematics are the ideas of fictionalism or modalism.

A series of important articles by Tappenden (see, for example, his 1995) provides the best hope at present of bringing about a *Gestalt* switch in the

[1] This is largely through the reinterpretation of constructiveness by those working in computer science, but also through the desire of mathematicians to be more informative, as when a constructive proof of a result in algebraic geometry permits it to be applied to a parameterised family of entities rather than a single one. Both kinds of reinterpretation are well described by category theory.

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way Frege is perceived by the philosophy community, thereby weakening the legitimising role he plays for the activity of many philosophers of mathematics. Frege should now be seen not merely as a logical reductionist, but as someone who believed his logical calculus, the *Begriffsschrift*, to be a device powerful enough to discern the truth about what concepts, such as number, are really like, sharp enough to 'carve conceptual reality at the joints' (Tappenden 1995: 449). With considerable justification Tappenden can say:

> The picture of Frege which emerges contains a moral for current philosophical study of mathematics. We appear to have arrived at a stultifyingly narrow view of the scope and objectives of foundations of mathematics, a view we read back into Frege as if it could not but be Frege's own. (Tappenden 1995: 427)

For the moment, however, I choose to take a closer look at a similar reinterpretation of Frege appearing in an article written by Mark Wilson (1999), since it reveals clearly, although not altogether intentionally, the fault lines running through contemporary philosophy of mathematics. To prepare ourselves to draw some morals for our discipline from his exercise in the methodological exegesis of a hallowed ancestor it will help us to conceive of contemporary research activity in philosophy of mathematics in terms of a Wittgensteinian family resemblance. From this perspective, Wilson is aware that he is putting into question the right of a prominent clan, which includes the Neo-Fregeans, to claim exclusive rights to the patrimony of a noble forefather. Indeed, he writes 'I doubt that we should credit any Fregean authority to the less constrained ontological suggestions of a Crispin Wright' (Wilson 1999: 257). As someone who identifies with this clan ('our Frege'), he naturally finds this result unwelcome. He then continues by introducing his next paragraph as a 'happier side to our story', which oddly he concludes by indicating, in effect, that another clan – the category theorists – may now be in a stronger position to stake their claim to be seen as Frege's legatees. Interpreting this in my genealogical terms, we might say that some new shared family traits have been discovered. Just like Frege, the category theorist is interested in the organisation of basic mathematical ideas and looks to current 'mainstream' research for inspiration. In the case of Frege it was, according to Wilson, von Staudt's geometry and Dedekind's number theory,[2] while in the case of the category theorists, algebraic topology and algebraic geometry have provided much of the impetus.

[2] Currently, the best piece on Frege's mathematical milieu is Tappenden's unpublished 'A Reassessment of the Mathematical Roots of Frege's Logicism I: The Riemannian Context of Frege's Foundations'.

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We should also note, however, that Wilson's interest in the methodological resources available to Frege and his awareness of their continued usage into more recent times is indicative of the work of yet another clan within philosophy of mathematics, the practice-oriented philosophers, or what I am calling philosophers of real mathematics. Continuing Lakatos's approach, researchers here believe that a philosophy of mathematics should concern itself with what leading mathematicians of their day have achieved, how their styles of reasoning evolve, how they justify the course along which they steer their programmes, what constitute obstacles to these programmes, how they come to view a domain as worthy of study and how their ideas shape and are shaped by the concerns of physicists and other scientists. Wilson, allied with one clan, has conducted some research in the style of a second clan, whose effect is a reduction in the legitimisation of the activities of the first clan in favour of those of a third clan.

There are traits suggesting considerable kinship between the latter two clans, the philosophers of real mathematics and the category theorists, an obvious reason for which being that category theory is used extensively in contemporary practice. Thus, the boundary between them is not at all sharp. Tappenden in his (1995) effectively casts Frege as a precursor of the former approach, but interestingly gives an example (p. 452) using category theory to illustrate how a mathematical property can be said to be mathematically valuable.

The rise of category theory will most likely be treated in different ways by the two clans: on the one hand, as the appearance, or the beginnings of the appearance, of a new foundational language; on the other hand, as an indication that mathematics never stops evolving even at its most fundamental level. In the broader context of general philosophy, the category theorist may also be led to find further roles for category theory within philosophy, for instance, to think category theory semantics should replace Tarskian set theoretic semantics in the philosophy of language (see Macnamara and Reyes 1994 and Jackendoff *et al.* 1999).

1.4 NEW DEBATES FOR THE PHILOSOPHY OF MATHEMATICS

Even were they to lose the endorsement of Frege, neo-logicist philosophers of mathematics could still claim that they are acting in accordance with current conceptions of philosophy. After all, they typically start out from the same or similar philosophical questions as those asked in philosophy of science – How should we talk about mathematical truth? Do mathematical terms or statements refer? If so, what are the referents and how do we have

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