TOWARDS A PHILOSOPHY OF REAL MATHEMATICS

DAVID CORFIELD

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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 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 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

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 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 led 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 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 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

$$\{\sqrt{[(7/2)^2 + 11\cdot(6^1/_4)]} - (7/2)\}/11 = 1/2,$$

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.

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 $H \to G \to G/H$, 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 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.

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, vow 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'.

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 access to them? It just so happens, they can claim, that these questions do not lead on to further questions relevant to what takes place in mathematics departments. Where the realist beliefs of a philosopher of physics may dictate that she holds that electrons exist, but lines of magnetic force do not, or those of a philosopher of psychology that the Freudian unconscious exists, but IQ does not, mathematics treats things made of the same stuff – sets, extensions of concepts, possible constructions, fictions or whatever – so the philosopher of mathematics cannot make similar kinds of distinction.

If we pause to think about this, however, should we not consider it a little strange that whatever our 'ontological commitments' – a notion so central to contemporary English-language philosophy – *vis-à-vis* mathematics they can play no role in distinguishing between entities that receive large amounts of attention, Hopf algebras, say (see appendix), and some arbitrarily cooked up algebraic entities. If I define a *snook* to be a set with three binary, one tertiary and a couple of quatenary operations, satisfying this, that and the other equation, I may be able to demonstrate with unobjectionable logic that all finite snooks possess a certain property, and then proceed to develop snook theory right up to noetherian centralizing snook extensions. But, unless I am extraordinarily fortunate and find powerful links to other areas of mathematics, mathematicians will not think my work worth a jot. By contrast, my articles may well be in demand if I contribute to the understanding of Hopf algebras, perhaps via noetherian centralizing Hopf algebra extensions.

Surely, the philosopher ought to be able tell us something about the presuppositions operating in the mathematical community today which would account for this difference. Resorting to the property of having been used in the natural sciences will not do, since there are plenty of entities deemed crucial for the life of mathematics that have found no direct applications. On the other hand, it is hard to see how the property of being deemed thus crucial can be salient to dominant philosophical modes of thinking. For this, questions of conceptual meaning and shared understanding would have to come to centre stage. The Hopf algebra concept possesses a cluster of interrelated meanings, one of which allows for descriptions of interaction between processes of composition and decomposition in many situations. These meanings are implicated in the uses to which Hopf algebras are put.

Returning to the philosophy of science, is it the issue of realism as opposed to instrumentalism – whether we should think of unobservable theoretical entities as really existing – which can be said to relate to the most penetrating analyses of how the natural sciences work? One recent endeavour to escape the realist/instrumentalist impasse in the philosophy of science is *structural realism*, the thesis that science is uncovering only the mathematical structure inherent in the world. But the move to structural realism does not free us from having to make a stark choice as to whether mathematical entities exist or not. Indeed, the choice for the 'ontic' structural realist (see Ladyman 1998) lies between, on the one hand, some mathematical structures existing as actualised in the universe and, on the other, all mathematical structures existing, the ones we self-conscious human structures encounter being deemed physical. Now, at least, mathematicians may be said to be studying something real, rather than merely creating fictions, but still we gain no sense of mathematical thinking as part of mathematical practice. We may have been led to use specific Hopf algebras to allow us to perform calculations with Feynman diagrams (Kreimer 2000), but it cannot be right to say that they are structures instantiated in the world. Still we cannot distinguish between snooks and Hopf algebras.

An attempt to encourage the reorientation of philosophy of science towards debates better grounded in scientific practice has been made by Ian Hacking (1999). These debates are fuelled by the work emerging from science studies and sociology of scientific knowledge, which for him are 'where the action has been in the philosophy of science over the past few years' (Hacking 1999: 186). The first of the 'sticking points' on which the debates depend is related to structural realism, although without its physical foundationalism. Hacking points to an older sense of realism – the thesis that opposes nominalism – and because of the baggage associated with the term realism, he opts for the expression *inherent-structurism* (1999: 83), the position that the 'world may, of its own nature, be structured in the ways in which we describe it'. To understand what is at stake here we don't have to turn to esoteric physical theories, but rather may think through the issue by way of a question such as: To what extent is it the case that the world is structured of its own nature in such a way that it is correct to designate as 'swans' those black feathered things swimming on the Swan River in Perth, Australia, and those white feathered things swimming on the River Thames in England? Note that this is not an all or nothing kind of question. Answers will invoke ideas from anatomy, physiology, genetics, evolutionary theory, the history of ornithology, the history of colonial science, etc.

Could a parallel move work for mathematics? At first glance it might not look promising. How can we talk of a mathematical 'nature' possessing joints to carve? But this, in essence, is how many mathematicians do talk. Rather than anything contained within the doctrine currently referred to as 'Platonism', the sense they have is that something much stronger than logic offers resistance to their efforts, and that when they view matters 'correctly' things fit into place. Whereas Hopf algebra theory is an established part of real mathematics, snook theory is not, they would say, because it is not the result of carving 'conceptual reality' at the joints. This notion of conceptual reality is independent of how we might describe the nature of the stuff talked about by mathematics. It could inhabit Plato's heaven or it could be what results from the process of postulating rules or it could concern operations, actual or idealised, that we can perform on the physical world.[3]

Lakatos is aiming at Hacking's nominalist-inherent structurist distinction when he maintains that:

> As far as naïve classification is concerned, nominalists are close to the truth when claiming that the only thing that polyhedra have in common is their name. But after a few centuries of proofs and refutations, as the theory of polyhedra develops, and theoretical classification replaces naïve classification, the balance changes in favour of the realist. (Lakatos 1976: 92n.)

For Lakatos, if human inquiry allows the dialectical play of ideas to occur with sufficiently little interference, it will eventually arrive at the *right* concepts. In this respect, vast tracts of logically sound, but uncritically generated, mathematics should be cast out as worthless. In response, the nominalist might say that there is nothing which intrinsically determines whether mathematical concepts have been produced correctly. What provides resistance to the mathematician are the conventions operating in her community brought about by the contingencies of history. And so we arrive at a sticking point. Out of this disagreement it might be hoped that the production of a rich picture of mathematical thinking will ensue.

Let us continue with the other two 'sticking points' Hacking sees at the heart of the science wars. These concern the inevitability or contingency of the science we have, and whether external or internal explanations should be given for the stability of our knowledge. What I find attractive about these questions is the possibility to escape polarised answers. Indeed, Hacking amusingly suggests that one locate oneself on a scale from 1 to 5. These ratings are presented in absolute terms as though we have to give a single answer to, say, how likely we reckon it is for specific scientific developments to have occurred. It seems to me more reasonable to take it as a measure of the tendency within one to take a certain side in a series of arguments. We all know of colleagues who tend to take up more contingentist or necessitarian views than ourselves on just about any question.

[3] These last two are, of course, distinguishable: you can physically move a knight forward one square, but the rules of chess do not allow you to do so.

Each of these additional sticking points is relevant to mathematics in the sense that we may argue about the following kinds of question: Is it the case that had a successful mathematical discipline been developed to a level of sophistication comparable to our own, then it would have to involve something equivalent to X, where for X we may substitute the natural numbers, the rationals, the complex numbers, complex analysis, Riemann surfaces, finite groups, Lie groups, Hopf algebras, braided monoidal bicategories, etc.? Why do we still adhere to, and teach undergraduates about, certain ways of thinking of X?[4]

We can find examples of these debates already happening. Indeed, on the question of contingency, Lakatos and Bloor use the same material, Lakatos's case study of the Euler conjecture from *Proofs and Refutations*, to argue different sides. Lakatos tells us that:

> any mathematician, if he has talent, spark, genius, communicates with, feels the sweep of, and obeys this dialectic of ideas. (Lakatos 1976: 146)

While for Bloor:

> Lakatos's discussion of Euler's theorem . . . shows that people are not governed by their ideas or concepts . . . it is people who govern ideas not ideas which control people. (Bloor 1976: 155)

Now, to Hacking's trio of sticking points I would like to add two more. First, there is the issue of the unity or connectivity of mathematics. This is nothing to do with all mathematical entities being seen as constructible within set theory, but much to do with cases of unexpected discovery such as finding that when using Hopf algebras to calculate expansions in perturbative quantum field theory, answers depend on values of the Riemann zeta function. There is an inclination to rebel against such a story and so to latch on to an image of mathematics as thoroughly fragmented as Mehrtens (1990) chooses to do, but then we need explanations of cases of surprising connectivity. For instance, how is it that a geometry devised after a failed *reductio ad absurdum* argument, starting out from the negation of Euclid's fifth postulate, could provide a useful classifier in knot theory in that it allows for the measurement of the volume of the hyperbolic space that typically remains when a knot is removed from the space in which it sits? For those who admit a considerable degree of unity, the further

[4] For an attempt to answer the question 'What kind of combination between the "natural" and the historically contingent led to our conception of modern logic?' by arguing that first-order logic is 'no natural unity' see Ferreirós (2001).

question arises of its causes: social pressures to keep to certain ways of thinking, the way our brains work, or encounters with inherent structure.

Second, there is the issue of the explicability of the applicability of mathematics. Usually this is polarised into 'it's an inexplicable miracle how mathematics, developed for aesthetic reasons, applies to the world' position opposed to one asserting 'it's not surprising because mathematics has been thoroughly shaped by the concerns of physicists'. Think how much more we might learn from a debate between, on the one hand, someone at point 3 on the scale, who recognises mathematics as arising from what the world allows us to do it, and who knows how intricately linked mathematics and physics were in the nineteenth century, but who still thinks there is something to explain about how Riemannian geometry was there for Einstein, and on the other hand, someone at point 4 who reckons in addition that physicists configure their theories to allow for the use of available mathematics. Mark Steiner (1998) has provided a start for us, but there are many more subtleties to discover. Just read a mathematician on the subject to feel the contemporary richness of this issue (e.g. Klainerman 2000).

These debates are not just about getting our description of mathematical practice right, but bear on ideas about how things ought to be. Just as there is a normative element to Lakatos's remarks about realism – we ought to follow his methodology to arrive at 'real' classifications, with the suggestion that we may, and indeed often do, fail to do so – so each of the other sticking points can be made to bear some normative load. For instance, we hear that mathematics may be fragmented today, but along with physics, it could and should be unified by adopting the language of geometric calculus (Hestenes 1986).

These kinds of questioning are to be addressed by an understanding of mathematical knowledge as historically situated rather than timeless. Lakatos understood this, but his work was only a start. To move on we shall need a revolution of sorts. In the 1960s Kuhn was able to revolutionise the philosophy of science partly because there was already a considerable body of history and sociology of science in existence, the product of professionalised disciplines. Philosophy of physics was already a much larger affair than its mathematical counterpart, with ahistoricist philosophers well grounded in mainstream theories and experiments connected with general relativity and quantum mechanics. We should remember, for instance, that Reichenbach worked for a time with Einstein. On the other hand, the logicism expounded by Reichenbach, Hempel and others of that generation was too deeply ingrained in the philosophical psyche to be overcome easily. By the 1960s, there was no philosophical tradition requiring extensive mathematical knowledge, and the history of modern mathematics was still largely an amateur affair stuck at the stage of 'Men of Mathematics', and so the conditions were not right for *Proofs and Refutations* to have its effect.

Forty years on, few philosophers of mathematics have been prompted to gain anything approaching the level of historical and theoretical knowledge that philosophers of natural science are expected to have. This is partly owing to the state of the history of mathematics. We still have nothing to compare with the sophistication of contemporary history of modern physics, the history of twentieth-century mathematics remaining largely the preserve of mathematicians. But these factors would be of little importance were the philosophical agenda to require serious engagement with the thinking of mathematicians through the ages.

How radical a change is required? It often seems that anyone wishing to take the history of a science seriously in their philosophy requires what to many in the English-speaking world of philosophy is an unorthodox philosophical background. This Lakatos certainly had. For Kuhn, on the other hand, it was implicitly fed to him via the historians he studied, Koyré, etc.:

> the early models of the sort of history that has so influenced me and my *historical* colleagues is the product of a post-Kantian European tradition which I and my *philosophical* colleagues continue to find opaque. Increasingly, I suspect that anyone who believes history may have a deep philosophical import will have to learn to bridge the longstanding divide between the Continental and English-language philosophical traditions. (Kuhn 1977: xv)

Without the resources of a dialectical philosophy, Kuhn came unstuck. In the rigid epistemological framework he inherited from the logical empiricists, sameness and difference were polarised, a concept could not evolve into another while retaining something of its past. And so he was guilty both of underestimating diversity within a paradigm and of overestimating incommensurability between paradigms.

One of the last of the English-language philosophers not to be cut off from Continental thinking by the rising tide of analytic philosophy was R. G. Collingwood. Collingwood had the notion that a discipline in any particular epoch possesses its own constellation of absolute presuppositions, and that discovering these is the task of the metaphysician. The fact that these absolute presuppositions change is sometimes seen as having as its consequence that there exists between the stages of development of a discipline an incommensurability akin to Kuhn's. This, however, is a misunderstanding of Collingwood's position.[5] Aside from the possibility of there being absolute presuppositions which have been maintained since the Greeks, when change does takes place it need not be construed as a discontinuous rupture, but rather as a dialectical change in which something about the earlier presupposition is retained in whatever it turns into:

> The problem of knowledge is therefore everywhere and always the same in its general form: when we are presented with something which we do not understand . . . we are to reach an understanding of it by finding out how it has come to be what it is: that is to say, by learning its history. (Collingwood 1999: 178)

This kind of understanding of change was part and parcel of Lakatos's thinking, as his desire to become the founder of a dialectical school in the philosophy of mathematics reveals (Larvor 1998: 9).

For Collingwood, along with this dialectical sensitivity, a capacity to experience the force of the absolute presuppositions of the contemporary form of the discipline about which one is philosophising is vital. While describing which qualities someone should possess to be able to answer the questions of philosophy of history, he remarks acidly that:

> No one, for example, is likely to answer them worse than an Oxford philosopher, who, having read Greats in his youth, was once a student of history and thinks that this youthful experience of historical thinking entitles him to say what history is, what it is about, how it proceeds, and what it is for. (Collingwood 1946: 8)

A similar conclusion could be formulated for philosophy of mathematics, and indeed Kant is praised for dealing with the presuppositions of mathematics 'rather briefly' for 'he was not very much of a mathematician; and no philosopher can acquit himself with credit in philosophizing at length about a region of experience in which he is not very thoroughly at home' (Collingwood 1940: 240).[6] Returning to history, he continues:

> An historian who has never worked much at philosophy will probably answer our four questions in a more intelligent and valuable way than a philosopher who has never worked much at history. (Collingwood 1946: 9)

Evidence for the equivalent statement about mathematics is provided by the very many important contributions made by mathematicians thinking about their discipline, several of which I shall lean on in the course of this

[5] See Oldfield (1995) on this point.

[6] Collingwood is being rather unfair to Kant in that, as Friedman (1992) argues, Kant's engagement with mathematics and especially physics was what gave depth to his philosophy. But that then only supports Collingwood's thesis that to do philosophy of a discipline well one must be 'thoroughly at home' with it.

book. These include the thoughts of Weyl, Weil, Mac Lane, Rota, Atiyah, and from the current generation, Gowers and Baez.

1.5 TOWARDS A PHILOSOPHY OF REAL MATHEMATICS

Aspray and Kitcher (1988: 17) dub as belonging to the 'Maverick Tradition' those philosophers of mathematics who pose such questions as:

> How does mathematical knowledge grow? What is mathematical progress? What makes some mathematical ideas (or theories) better than others? What is mathematical explanation?

While their portrayal of such philosophers as non-conformists may not be far off the mark, it clearly does not represent a desirable state of affairs. Language has a performative role as well as a descriptive one, and we should be looking to inspire a new generation of philosophers to sign up to the major project of understanding how mathematics works. Maddy has opted with her *naturalist methodology* not to use the word 'philosophy', which seems to me an unnecessary concession. Larvor (2001) has described a movement he terms the *dialectical philosophy of mathematics*, and kindly refers to me as one of its three leading exponents. Then again my *philosophy of real mathematics* may provide a louder clarion call for a time.

One way to proceed with this programme is to return to two of the founding fathers of the philosophy of real mathematics: Pólya and Lakatos. This I shall do, but in full consciousness of a problem we face. Back in the early 1960s, Lakatos and Kuhn were able to take risks with their pioneering historicist philosophies of mathematics and science, where bold theses were defended on the basis of a handful of sketchy historical reconstructions. Now, from the perspective of our current sophisticated science studies we look back on Kuhn's *Structure* as being rather simplistic, if understandably so, and we may agree with Peter Galison (1997) that it would be extremely naïve today to maintain that there is a unique structure to scientific revolutions. Our discipline has not had the same opportunity to grow up, and so forty years on we find ourselves in an awkward situation. We wish to propose striking theses, since tentatively expressed claims are hardly likely to energise our field, and yet with so little to build on it is likely that our efforts will appear immature by comparison to our sister discipline. For instance, historians, philosophers and sociologists of science may wonder whether it is necessary to rake up all the paraphernalia of Lakatos's *research programmes*, as I do in chapter 7, when they now have little time for them.

And isn't the Bayesianism of chapters 4 and 5 beyond the pale? Hopefully, they will allow us a period for recapitulation.

An alternative strategy would be to claim sanctuary under the protection of philosophy of science on the pretext that mathematics be seen as a science. Now, one of the varieties of disunity treated by Ian Hacking in his paper 'The Disunities of the Sciences' (Hacking 1996) he terms *methodological* disunity, which concerns the diversity of styles of scientific activity. For several years he has expressed support for the following classification of scientific styles proposed by the historian of science A. C. Crombie:

> (a) postulation in the axiomatic mathematical sciences, (b) experimental exploration and measurement of complex detectable relations, (c) hypothetical modelling, (d) ordering of variety by comparison and taxonomy, (e) statistical analysis of populations, and (f) historical derivation of genetic development. (Hacking 1996: 65)

To these Hacking wishes to add 'laboratory science . . . characterized by the construction of apparatus intended to isolate and purify existing phenomena and to create new ones' (*ibid.*). Hacking applauds Crombie's inclusion of (a) as 'restoring mathematics to the sciences' (*ibid.*) after the logic positivists' separation, and extends the number of its styles to two by admitting the algorithmic style of Indian and Arabic mathematics. I am happy with this line of argument, especially if it prevents mathematics being seen as activity totally unlike any other. Indeed, mathematicians do more than postulate axioms and devise algorithms; it would hardly be figurative to say that mathematicians also engage in styles (b) (see chapter 3), (c) and (d),[7] and along the lines of (e) mathematicians are currently analysing the statistics of the zeros of the Riemann zeta function.[8] As for Hacking's additional scientific style – the construction of apparatus – Jean-Pierre Marquis (1997) made a start on analysing the notion that some mathematical constructions are used as machinery or apparatus to explore the features of other

[7] Cf. John Thompson's comments: 'the classification of finite simple groups is an exercise in taxonomy. This is obvious to the expert and to the uninitiated alike. To be sure, the exercise is of colossal length, but length is a concomitant of taxonomy. Those of us who have been engaged in this work are the intellectual confreres of Linnaeus. Not surprisingly, I wonder if a future Darwin will conceptualize and unify our hard won theorems. The great sticking point, though there are several, concerns the sporadic groups. I find it aesthetically repugnant to accept that these groups are mere anomalies . . . Possibly . . . *The Origin of Groups* remains to be written, along lines foreign to those of Linnean outlook' (quoted in Solomon 2001, 345).

[8] Hacking (1992: 5) remarks that 'A great many inquiries use several styles. The fifth, statistical, style for example is now used, in various guises, in every kind of investigation, including some branches of pure mathematics.'

mathematical entities, as when, for instance, K-theory was constructed to probe topological spaces. But let us remember that these styles of mathematical activity arise in particular epochs and evolve over the centuries. After all, Hilbert's use of axiomatisation differs quite considerably from Euclid's.

The fact that there is such a degree of overlap between the styles of mathematical and scientific activity suggests we might learn from current studies of scientific argumentation. However, were we to ignore the differences between, say, classifying finite simple groups and tabulating their properties, and doing similarly for the chemical elements, fundamental particles, or zoological phyla, we would lose what is unique about mathematics. For one thing, these styles of activity work in a more interactive fashion for mathematics, owing to the greater homogeneity of mathematical material. Pieces of mathematical machinery, such as homology and cohomology theories, although used as 'black boxes' by some consumers, are themselves mathematical entities and so the possible subject matter for mathematical classification, as for instance when the so-called *spectra* representing extraordinary cohomology theories are gathered together to form a category, and one of them – the sphere spectrum – shown to be maximally difficult to compute with. Of course, there are theories of instrumentation in the natural sciences, but nobody seriously contemplates the space of all possible machines of a certain kind.

As I have said, I see no intrinsic reason why we should not succeed in drawing connections between developments in mathematics, including those which have occurred in recent decades, and recognisably philosophical concerns. Indeed, we can point to a considerable number of important studies already in existence as evidence, the vast majority in the mould of descriptive epistemology. But to emulate philosophy of physics we need to make a more systematic effort to engineer space for ourselves to work with a wide range of issues. Alongside descriptions of how research mathematicians have worked, we should also allow philosophy to treat interpretational issues interior to branches of mathematics in such a way as to provide us with insight into reasonably large portions of mathematics, on the assumption that we will miss something important if we only look for features relevant to mathematics as a whole.

Not only do we need to free ourselves from the requirement that we treat simultaneously all of the space of mathematics, we also need to work out varied ways to liberate ourselves from the appeal of timelessness. In doing so temporality needs to be introduced at many scales, since mathematics