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Argumentation and the Mathematical Process

Introduction

We have reached a position today from which to evaluate judiciously Lakatos' contribution to the philosophy of mathematics. We should applaud unreservedly his decision to take the development of mathematics as a topic worthy of philosophical consideration and admire the first steps he took to elucidate patterns of theory change via his identification of certain mechanisms of concept-stretching. We must recognise, however, that the *method of proofs and refutations* accounts only for the modification of a concept's definition,[1] omitting more fundamental varieties of theory development, and consequently that much remains to be done. The two tasks before us today are to describe an expanded range of varieties of theory production, especially radically innovative mathematical conceptualisations, and to ponder the philosophical issues relating to such a description. Regarding the former, see Corfield (1998a) and Kvasz (1998). Through this paper I shall largely confine myself to the latter, endeavouring to convey a sense of the vast array of possibilities now open to us.

In the first part of this paper I shall survey some current ideas about how the development of mathematics should be studied and show that, despite the wild variations in the approaches adopted by commentators and the absence of any serious engagement with each other's positions, there is still plenty of common ground. I shall attempt to forge a path through some of the literature by constructing links between the contributions to be considered. Some of these links take the form of questions which I raise here not to answer, but to provide markers for the construction of an arena in which the mathematical process may be studied. This is to be seen as suggestive of a much larger project to weave together the very partial and disjointed insights gained to date. In the second part I shall propose some measures for bringing the parties together to allow this weaving to take place.

G. Kampis, L. Kvasz and M. Stöltzner (eds.),
Appraising Lakatos: Mathematics, Methodology and the Man, 115–138.

1. THE PRESENT

Mathematical judgement

For all the attention he devoted to informal mathematics and its heuristics, there still remained in Lakatos a residual streak of logical empiricism, in the sense that the scientific and mathematical knowledge of greatest concern to him, philosophically speaking, was embodied in theoretical statements. Hidden lemmas, he tells us, emerging during the 'proofs and refutations' stage of discovery, turn into the axiomatic contents of the hard core (1978, p. 96) which must be defended at all costs. We also see this stance displayed in the following quotation, where, while criticising Toulmin, Lakatos posits the 'knowing that' component of scientific knowledge as philosophically fundamental and adopts a correspondence theory of truth:

> This stress [on Toulmin's part] on the inarticulable shifts the problem of 'knowing that' to the problem of 'knowing how'; from knowledge expressed in propositions to knowledge expressed in skills and activities. This in turn leads from the classical conception of truth – a proposition is true if it corresponds with the facts – to pragmatism – a belief is true if it gives rise to useful or effective action ... Propositions, and thus the 'third world,' are redundant (1978, p. 228).

Moreover, Lakatos maintained that axiomatisation marks the end of the creative process. He indicated that readjustments might be required if the informal material had not been satisfactorily captured, but gave no hint that an axiomatisation could act as a springboard for further theoretical development. This, coupled with a recognition of the achievement of a considerable stabilisation in standards of rigour, left him with something close to Francis Fukuyama's vision in political theory of the 'End of History' (Fukuyama, 1989): at the level of ideas, all battles are over, the idea of liberal democracy having won. This parallel, no doubt attributable to their Hegelianism, extends to the fact that they bemoan the lack of excitement to be found in the realms of current political and mathematical thought. For Lakatos, once a mathematical theory has entered the axiomatic stage "imagination is tied down to a poor recursive set of axioms and some scanty rules" (1978, p. 68).

However, Lakatos is wrong on both these counts. He should have known that in the axiomatic era mathematics is much larger than its accepted statements. This excess is not only non-linguistic know-how, although essential, but may be expressed in statements: 'This definition captures the concept at the right level of generality'; 'Construction A is a better way of generalising construction B than is construction C'; 'Theory F can be used to produce simple/short/generalisable proofs of results in area G'; 'This notation is extremely efficient'; 'Proofs often prove more than was intended and as such should be sifted through to extract their extra power'; 'That line of research has been disappointing because it has provided no new insights.' Even had he later come to recognise this kind of knowledge as belonging to the heuristic of a mathematical research programme, he still would have had difficulty coupling it to his rigid conception of the axiomatic process.

Axiomatisation is not the end of the road. It acts to support a more refined kind of informal thought, allowing plenty of room for further disagreement. The good news is that there are still arguments being conducted, often unnoticed by outsiders, which concern the direction of research, what constitutes a breakthrough, which style of research to encourage, which way to think about a theory, which way to generalise a concept, and so on. Mathematicians have not relocated to the realm of logic, but are as ever stationed within the realm of dialectic, where there is room for inventive concept-formation and concept-stretching. There is still excitement. What are invoked in these arguments are selection criteria which for the most part are unrelated to issues concerning the correctness or validity of mathematical results. Indeed, you will hear mathematicians claiming about another group of researchers that they have 'gone too far,' with no hint of an accusation of lack of rigour.

In a heuristic-filled textbook for aspiring topologists (Gilbert and Porter, 1994), the authors, having earlier provided the means for the reader to construct a formal proof of Van Kampen's theorem, invite her to do so as an exercise (p. 222). Their next piece of advice is to

> ... go to your library and search out books which contain a formal proof of the Van Kampen theorem. Compare their methods with yours. *Criticize* both your proof and theirs. Try to improve your proof with ideas, phrases, or notation from their version. Do not think their version will necessarily be fundamentally better than yours, but as the authors tend to have had years' more experience of writing proofs, you should expect to gain something, at least in style (Ibid., pp. 22–3, my emphasis).

What is at stake here is not the kind of proof criticism by counterexample that we find in *Proofs and Refutations*, but one concerning conceptual organisation, notation and style. The truth or correctness of the result is not at issue, nor is the rigour of the proof. It is all about mathematical taste and understanding.

To move quickly to the central point I wish to make here, what have typically been ignored by philosophers of mathematics are the judgement skills of mathematicians, their *bons sens* or *finesse*, to return to Pierre Duhem's expressions. In effect, what I have been indicating above is the fact that Lakatos' appreciation of this *bon sens* was somewhat restricted. This is also revealed by his decision to elaborate on the first of George Polya's principal brands of 'plausible reasoning' (Polya, 1954), *induction*, when, as I shall discuss elsewhere, it is the second, *analogy*, which counts for more in the estimation of mathematicians. Yet Lakatos' efforts to learn from mathematicians, historians of mathematics and mathematics educationalists gave him a far greater sense of mathematical culture than those who can think of nothing more interesting to wonder about mathematics than whether numbers exist. Our task then is to persuade sufficiently many people that it is towards an analysis of the varieties of mathematical judgement that our resources should be devoted. We must recognise, however, that we are up against a deep-seated ignorance of phenomena in this realm.[2] Indeed, we appear to be dealing with a widespread form of agnosia and, as such, the difficulty in persuading the sufferers of their deficit should not be underestimated.

For those of us who have long known that there was more to be said about mathematics, our passage has appeared to be blocked by barricades erected at one end by the logical empiricists – mathematics is just a bunch of contentless tautologies and is only of interest as directly applied in science or everyday life, and at the other by Platonists – mathematics is about objects dwelling in some ethereal realm. If these obstructions are confronted on their own terms, one ends up confined to a kind of philosophical ghetto on a diet of Gödel's results, constructivism, or the Benacerraf paradox. Small wonder that some of those wishing to break out have turned to category theory. It possesses the twin qualities of being arguably as 'foundationally' significant as set theory (e.g. topos models for Cohen's forcing and for constructive logic), and yet also of revealing its tighter connections to mainstream mathematics, arising as it did out of concrete problems in algebraic topology studied in the 1940s. But once it is accepted that there is some philosophical juice to be extracted from the development of the central branches of mathematics, we may also be permitted to talk about the careers of the remarkable ideas of Gauss, Cauchy, Abel, Dirichlet, Galois, Riemann, Kronecker, Dedekind, Hilbert, Poincaré, Weyl and Noether, not to mention a host of theorists of the past sixty years. Their achievements should be our bread and butter, just as anyone studying the development of physics is expected to have the essential ideas of Faraday, Maxwell, Helmholtz, Einstein, Bohr and Heisenberg at their fingertips.

Today there is particular cause for hope that after decades in which little interest has been shown in what constitutes mathematics 'done well' the pendulum may be swinging in our direction, since we now begin to find our services required by the practitioners of two disciplines, namely, science studies and mathematics education.

Sociologists, historians and historically sensitive philosophers of physics have until now largely been concerned with the struggle to capture and question the various kinds of *best practice* of the theoretician and of the experimenter, and have treated the mathematics as something already there, implicit within the physics. They may justify their neglect of mathematical best practice by pointing to the fact that in the past physics and mathematics had not separated. As late as 1915, the world's leading mathematician, David Hilbert, was near enough to the cutting edge of theoretical physics to come a close second to Einstein in the race to publish a theory of general relativity. Then they will remind us of the likes of Weyl, von Neumann and Wiener. Even that quintessential 'pure' mathematician Karl Weierstrass did physics.

We should respond by saying that while there is a considerable overlap between the physicists' and the mathematicians' sense of judgement, it is just an overlap, one which may vary over the passage of time as a proportion of the whole. Certainly, von Neumann worried that this overlap can become too small, leading to degeneracy on the part of mathematics, but he was far too good a mathematician to wish to see mathematical research swamped by scientific considerations. Mathematical and scientific conceptual development are not locked together in a tight embrace; their course resembles more a piece of modern dance than it does a waltz. So even if valid objections are raised to the presentation of their encounters as being inexplicably fortuitous, there ought to be just a little more surprise displayed at the fact that Appolonius' work on conics, and then, more efficiently, the differential calculus, could allow Newton and his followers to model the motions of the solar system, or that Einstein did not have to look far for an appropriate calculus for general relativity, given the reasons behind the invention of the mathematics.

Given this non-coincidence, to put things a little naïvely, the more a piece of mathematics has developed due to internal considerations of a mathematical nature, the more there is to explain about its later applicability.

The striking thing is that the non-coincidence is today coming to the fore in that credit is accruing to physical theories if they make *mathematical*, rather than *empirical* predictions:

> ... quantum field theory has had its credibility enhanced by its success in making correct mathematical predictions. Given the lack of rigorous foundations for quantum field theory, these successes provide great encouragement to physicists that their ideas are fundamentally sound (Atiyah, 1995, p. 6070).

If mathematical statements are merely tautologies, it is hard to see the source of this enhancement of credibility.

Another reason the mathematical physics of the past twenty or so years calls for attention is that the recent interaction between mathematics and physics comes after a period when their grip on each other has been at its weakest.[3] Central to the state-of-the-art mathematics which is now intriguing physicists are the ideas formulated by the differential and algebraic topologists, practitioners of branches which have such a good century that "... by the 1970's, the whole of mathematics was saturated with topological ideas" (Atiyah, 1995, p. 6069). While it will be possible to find common considerations governing the development of gauge potentials in gauge field theory, on the one hand, and of connections on fibre bundles, on the other, there will still be more work to be done to think through this type of theoretical convergence.

Some less sanguine about the concept of 'mathematical' predictions will, no doubt, claim that theories such as topological quantum field theory are just mathematics dressed up as physics and not proper empirical science. Be that as it may, there are still good grounds to anticipate a useful partnership between those who study less recent aspects of the scientific and mathematical processes.[4]

Let us turn now briefly to a second group of prospective clients – the mathematics educationalists. Mathematics has always had an intimate relationship with the way it is taught, reflecting its etymology. The Babylonians bequeathed us tablets containing worked solutions for the benefit of their students. In the *Meno* Plato writes about a mathematics lesson Socrates gave to a slave. The pedagogic format of *Proofs and Refutations* may be accounted for by the fact that Lakatos himself worked in the Ministry of Education in the post-war communist government of Hungary and was well versed in the didactic literature.

If we turn the spotlight on the mathematical process as a whole, the training of future mathematicians will certainly need to be investigated. Teaching practices reflect cultural conceptions of the nature of mathematics and have a marked impact on research practices.

The division of labour between those studying the mathematical process and the mathematics educationalist may be put as one between the study of the 'phylogenetic' and 'ontogenetic' development of mathematical understanding. These, of course, are not isomorphic, but as one educationalist tells us:

> The difficulties they [the students] encountered, the tentative understandings of a still very unclear situation were often quite close to those experienced by mathematicians in the past (Sierpinska, 1994, p. xi).

Calls have been made by educationalists to introduce historical information into the classroom. When teaching the quadratic equation, for example, one could contrast the Babylonian formulaic method with the Greek ruler and compass construction and the Arabs' much closer geometric mirroring of the relevant algebra. More recent episodes could be useful for university education. How many mathematics students have turned up to the first session of a course on rings and modules to be told without further ado that "A ring is a set with two binary operations ..."? How can a concept such as this be grasped without a sense of the problem situations which gave rise to the emergence of rings of algebraic integers, polynomials, entire functions, p-adic numbers, etc.?

As with science studies, the relationship with mathematics education should be a dialogue. Ideas concerning the mathematical understanding of students at all levels are germane to the study of the mathematical process. Here, it is noteworthy that many educationalists have turned away from Anglophone thought to other traditions, for example, to Piaget, Vygotski and Bachelard, in their attempts to think about the nature of mathematics and the implications for the way it is taught. This inclination towards psychology is something to which I shall return below.

Phenomenology versus social history

So far I have not said much about the status of this *mathematical judgement.* The first point a sociologist of knowledge might make on hearing the term is that its transmission sounds a lot like indoctrination, the passing on of the values of the élite. Certainly, I am claiming that it has much to do with the production and selection of mathematical concepts and strategies, which taken together form a process governed by comparatively few people. The question is whether, having shifted from a dichotomous discovery-justification model, a rationality may be posited as underlying this process, and, if so, where this rationality resides. The range of options is wide. It runs from seeing the mathematical process largely as a power struggle between groups of mathematicians to control the purse strings and so the direction of research, to seeing it as a messy realisation of the unfolding of disembodied thought in some other realm. Between these extremes are to be found some intriguing options. A further point for those who take rationality to be at stake when choices are to be made in specific problem situations is whether such piecemeal rationality can be patched together to provide a long-term version.

Lakatos' writings have attracted appreciative glances, but comparatively little effort has been expended in building on his work. As I argue elsewhere (Corfield, 1998), his ideas require some radical recasting to have a hope of working. Let us then consider how the ideas of Lakatos' rival, Thomas Kuhn, are faring in the mathematical domain. We can find a dozen or so appraisals of the relevance of Kuhnian concepts to mathematics contained in the volume *Revolutions in Mathematics* (Gillies, 1992). Many interesting points are raised here, yet a failing of this book was a reluctance on the part of many of the contributors to consider whether the rationale for Kuhn's work in science was relevant to mathematics, revolutions being judged for the most part as descriptive devices. Of those who did go beyond the question of whether or not revolutions occur, two contrasting developments are due to Luciano Boi (1992) and Herbert Mehrtens (1976, 1992), a phenomenologist and a social historian.

Their papers do reveal considerable agreement when it comes to their suggestions as to the best devices to describe the mathematical process. Mehrtens gives a subtle descriptive framework of a disciplinary matrix in terms of beliefs in particular models, values, exemplars, concepts, and standard problems (pp. 31–5), and, as we can see from the first two of Boi's "most significant points" (pp. 206–7), they are in accord over this.

1. Mathematical thought undergoes conceptual transformations which embrace at the same time its methods, concepts, 'objects,' symbolism, and techniques; all these elements contribute to the development of mathematical knowledge ...

2. In mathematics there are traditions, problems, and traditions of problems which are just so many ways of giving a reply and a solution to these problems.

That this amount of agreement may be found is impressive. From here, however, they part company, their paths diverging towards a social history and a phenomenology. Boi clearly has no time for the sociological direction of Mehrtens[5]:

No sociological or extra-mathematical reasons could help in understanding the nature of mathematical knowledge and the intrinsic reasons for its development and changes (Boi, 1992, p. 197).

Boi's preference is for the phenomenological construction of a 'genealogy of ideal forms':

3. There is an intrinsic hermeneutics of mathematics which, as we have seen, is characterized by the following two aspects: the inter-translation and auto-interpretation of theories.

4. Mathematical knowledge is indissociable from the establishment of a theory of concepts and a theory of structural analogies which bind the theories together.

5. There are certain fundamental intuitions which inspire and guide mathematical discovery and development; they are alinguistic and cannot be completely formalized. An example is the spatial continuum, which cannot be reduced to any axiomatic construction (Boi, 1992, p. 207).

Returning to Lakatos, each side could claim him as their own. He could be steered down the social history route by taking research programmes in the direction of social representations theory and concentrating on the practical realities of adhering to a programme, or down the phenomenological route by emphasising his Hegelian idealism and by focusing on what gets left out by formalization (cf. Boi's point 5). Unlike Boi, I have no objection to those who choose the former route, particularly if Sal Restivo's point is taken that, at times, the influence due to the external social milieu is much weaker than that due to the internal social milieu, that is, to factors operating within the relevant mathematical research community (Restivo, 1992, pp. 139–141). Indeed, not being a fan of a 'World 3' cast in stark opposition to the two other Worlds, I rather hope that the two approaches may interact creatively.

What appears to be at stake between the approaches is whether the stress should be placed on the development of mathematics as a necessary or as a contingent process. On the side of necessity, we often find, alongside the phenomenologists, mathematicians taking the time to reflect on their discipline as a whole. What they appear to be doing is arguing for a picture of mathematics which reflects the kinds of qualities they find attractive in particular pieces of mathematical theorising. In their attempts to promote the latter they will use the language of necessity: "This concept has been formulated independently in many fields. Its definition appeared natural to several mathematicians. It acts to unify these fields and allows the fruitful transfer of further concepts. There was an inevitability to its discovery."

In view of the complexity of the necessity/contingency question, it is all too easy to discuss aspects of it without meeting head on treatments arising from the opposite orientation. Hence the need to find specific issues to debate. A good place to commence a dialogue concerns what is perhaps the largest bone of contention between the opposing factions, namely, the difference between their respective presentations of modern mathematics as fragmented or as unified.

The unity of mathematics

The key question here is whether this dispute has a basis or whether it is simply a matter of a difference in emphasis. The sociologists and social historians tend to stress disunity: Eduard Glas (1989) outlines the incommensurability between the programmes of Monge and of Laplace and Lagrange; Mehrtens (1990) portrays the demise of the grand programme and the fragmentation into specific concrete problems; Restivo (1992) concentrates on the organisational structure of research over very long time scales where naturally variation will be easy to find.

Opponents will accuse them of a lack of feel for the structure of mathematics and its interacting branches (algebraic geometry, differential topology, analytic number theory, etc.) and a lack of feel for the flow of mathematics and its continuity. The Babylonians posed problems such as that of finding the side of a square field given that eleven times its area added to seven times its side amounts to $6\frac{1}{4}$. It generally comes as a surprise to find them presenting a solution which amounts to the calculation of

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

demonstrating that quadratics were solved 4000 years ago in a very similar fashion to the way we teach our teenagers today. Then, why should the unpublished ideas found in Gauss' *Nachlass* contain so many of the major developments of the first half of the nineteenth century? Furthermore, much current research is hard to classify as belonging to a single branch, blending as it does ideas originating in diverse fields. To pluck out a single example, William Thurston's work in three-dimensional geometry has points of contact with a vast array of branches.

But by this search for continuity and unity, we might inaccurately be presenting today's régime as inevitable. Perhaps the sociologists are justifiably avoiding acting to legitimise the ways things are. Might Boi be seen in the same light as Kant, the legitimator of Euclidean geometry, someone who inhibited the development of geometry by arguing for its necessity?

At times mathematicians certainly do sound like legitimators. André Weil, for example, claims of one of his historical works that

[t]he main thesis will be the continuity of number theory for the last three hundred years and the fact that what we are doing now is in direct continuation of what has been done by the greatest number-theorists since Fermat started it all in the seventeenth century (Weil, 1974, p. 279).

Michael Atiyah goes so far as to make the claim that:

The difference [between modern and traditional mathematics] is more in the manner than the substance and if Newton or Gauss were to reappear in our midst only a short refresher course would be necessary before they could understand the problems being tackled by the present generation of mathematicians (Atiyah, 1977, p. 275).

Elsewhere Atiyah (1984, p. 307) aligns himself, in terms of mathematical taste, with Hermann Weyl. Is this self-aggrandisement or is there a genuine continuity of (successful) personal heuristic? Again, why did Emmy Noether admire Richard Dedekind so greatly?

Perputed continuity is used to legitimise, but so are claims about the cohesiveness of mathematics. Here, as elsewhere, we might learn some lessons from our colleagues studying the natural sciences. In an important contribution, Ian Hacking in his paper *The Disunities of the Sciences* (Hacking, 1996) distinguishes two senses of the term 'unity': unity as *singleness* and unity as *harmonious integration*. We might say that the various foundationalist enterprises and Bourbaki's monolithic *Elements of Mathematics* have aimed at the former, whereas mathematicians today are promoting the second by conveying their sense that when any of their kind worth his or her salt is working on a problem in one area, they cannot help but stumble across ideas central to another, as when Vaughan Jones' studies of von Neumann algebras allowed him to discover a new link polynomial in knot theory. This, it appears, is what Atiyah means when he talks of the 'unity of mathematics' (Atiyah, 1978). Saunders MacLane (1986), meanwhile, prefers the term 'connectivity.' We may then wonder whether it is just part of their attempt to impose their preferred mode of doing mathematics or whether it is in some sense *rational* to aim for harmonious integration?

MacLane also talks of the 'Protean' appearance of ideas, that is, the same idea cropping up in many guises in different branches and epochs. What is this notion of the 'same' idea? It suggests a stability of form, dissociated from the particular temporal instantiation – anathema to the sociologists – closer to the phenomenologists.

In the same paper, Hacking discusses *methodological* disunity while renewing his advocacy of the following classification of scientific styles proposed by the historian 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, p. 65).

To these Hacking adds "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 no other, just so long as this restoration does not lead to its annexation by science studies. Indeed, mathematicians do more than postulate axioms and devise algorithms; it would hardly be figurative to say that mathematicians also engage in styles (b), (c) and (d). Jean-Pierre Marquis (1997) has even made a start on an analysis of the notion that some mathematical constructions are used as apparatus to explore the features of other mathematical entities, for instance, K-theory to probe topological spaces, although he seems inclined to see this as a metaphorical usage (Ibid., p. 254). The fact that mathematicians employ such a wide range of styles suggests we might learn from current studies of scientific argumentation. However, annexation might occur 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.

In sum, we could expect the theme of 'the unity of mathematics' to provide an enlightening topic for debate.

The inevitability of the mathematical process and the applicability of mathematics

Returning to the social historians, their investigations certainly throw up philosophical questions. Consider Andrew Pickering's recent attempt to extend his 'mangle of practice' to mathematics via his analysis of William Hamilton's invention of quaternions (Pickering, 1995, chap. 4). After relating how Hamilton aimed to create a three dimensional number system by transporting certain features of the complex numbers to a new setting, Pickering arrives at the question whether "Hamilton's search for triplets was doomed in advance (or fated to arrive at quaternions)" (p. 141n). Two quick answers are possible here, the second favoured by Pickering:

(a) Yes, as Frobenius later showed there are just three associative division algebras over the reals: the reals, the complex numbers and the quaternions;

(b) No, the acceptance of the notion of an associative division algebra over the reals was a product of sequences of practices taking place after, and largely arising from, Hamilton's work. "Since this concept [associative division algebra] was not available to Hamilton, he cannot have been looking for new instances of it." (Ibid.)

Pickering's question would seem to bring us directly to what I would take to be philosophical questions. These are not so much about the realism of mathematical objects, but more along the lines of 'Are some concepts (here the notion of a division algebra) inevitable?' If along with Mehrtens we hold that "mathematics is about something that offers resistance to the mathematician and calls for treatment" (1976, p. 24), we see that Hamilton did meet resistance. What is its nature?

This episode may also be viewed as illustrating an answer to a historical question with sociological overtones and philosophical implications, along the lines of Peter Galison's (1987) 'How Experiments End': How mathematical experiments to find analogue constructions end. The physicists of the nineteenth century were not nearly so impressed by the quaternions as were Hamilton and Tait, and so hacked off their non-scalar part to form a three dimensional vector calculus. This raises the question as to whether the formation of these vectors could count as the proper end of the experiment Hamilton had begun, despite their lack of resemblance to the complex numbers: two multiplications, each allowing products of non-zero vectors to be zero. This argument could not be resolved purely by logical means, but is a matter of justification and criticism.

The quaternions have gone on to have other uses relevant to modern physics, for instance, via the isomorphisms between the unit quaternions, the three sphere and the the Lie group SU(2), double covers of the Lie group SO(3). This brings me to the question of applicability. MacLane (1986) has briefly sketched an explanation for the applicability of mathematics which begins with our activities in the world (measuring, counting, shaping) and our psychological capacities (generalising, abstracting, analogising). The mathematics thus generated then finds application because the world happens to have structure similarities at different levels (very small and very large). By contrast, David Ruelle pictures mathematics as formed by periodic injections from science coupled to a filling-in process, which is governed by the mind according to conceptual structures inherent in language, since "our 'logical thinking' is linked with visual intuition and tied to illogical 'natural languages'" (1988, p. 266).[6] Thus, for Ruelle, our mathematics is not so very inevitable. Different physical problems might have inspired us, and a different psychology would have led to a different filling-in process.

In that the products of this filling-in process have a human flavour to them, even with Ruelle's picture of scientific injections, we might still wonder at the later applicability of mathematics produced in this way. Atiyah, for instance, claims that he had no idea of the future applicability of the Atiyah-Singer index theorem, and that he even had to be taught the significance of Fourier analysis (1988, p. 2). This kind of comment subtly suggests the theorem's importance. If a piece of theory has unexpected applicational uses, any thought of its contingency vanishes.

In response one might allow an implicit 'intentionality' implanted by earlier workers which adheres to the mathematics as it is transformed. After all, the course on which your dancing partner sets off is devised for later reunion. Remember that in Atiyah's case part of the ground was prepared by those trained in the Soviet Union, where those aspiring to become pure mathematicians were given a thorough grounding in mathematical physics.

Psychology and phenomenology

Notice a common use being made of psychology to explain mathematical development. Is it an accident that mathematicians tend to prefer psychological to sociological explanations? Given our situation as biological and social beings, emphasis on the former would favour the *continuity of mathematics* picture. Sociologists might be happier with the idea that changing mathematical conceptions of space reflect and produce alterations in, say, the means of production, modes of transport and communication, styles of architecture and urban planning, and our lifespace in general, to include the construction of the body image and selfhood. Mathematicians, on the other hand, seem to prefer the idea that they are extracting principles employed by us in very everyday perceptual and motor activity. Just think of the complex visual and proprioceptive feat involved in walking towards a box and picking up a ball contained within it. Can we imagine a culture in which a healthy five-year-old could not do this? Yet researchers constructing machines to do similarly need to pack in a huge amount of differential geometry. Might we say that the cultural environment is what provides the requisite frames of mind for the extraction and development of the principles employed by the mind in its everyday activities?

The idea that mathematical understanding is rooted in our embodied condition has been touted by various cognitive linguists (Jackendoff, Lakoff, Johnson, Talmy), who would have mathematics seen as an elaboration of certain modes of thought, e.g. image schemas arising in us through our embodiment. For instance, Talmy (1988) argues that the experience of the world's resistance to our intended actions lies at the base of our modal and logical concepts. Here we are not so far from Poincaré and Einstein's conception of thinking with the body image or musculature, nor from the variety of phenomenology put forward by a philosopher such as Ferdinand Gonseth.[7]

Phenomenology is something of a blanket term, but generally commendable as an approach which requires an immersion in the subject matter with which it is dealing: to discuss the possible, you need to have engaged with the actual. However, as Gonseth argued, phenomenology can find itself ending up in some rather barren dead-ends, especially when it aspires to new forms of foundationalism and essentialism. A position developed by Joseph Margolis over the course of his trilogy, *The Persistence of Reality*, is that, foregoing foundationalism, we should move towards a naturalised phenemonology, which he sees as equivalent to a phenomenological naturalism.[8]

I can think of no better illustration of the potential for a naturalised phenomenology than the improbable convergence between a recent discovery in cognitive neuropsychology and Martin Heidegger's idea that prior to any propositional knowledge of objects, tools for example, the tool is *ready-at-hand* and will only emerge as *present-at-hand*, i.e. as a 'thing' with properties, when there is a disruption to the action of using the tool. Compare this to the theory of Melvyn Goodale (1995) which distinguishes two streams of visual processing, a ventral stream concerned with visually guided motor behaviour, and a dorsal stream for "identifying objects in the visual world and attaching meaning and significance to them" (p. 176). Subjects with a lesion interrupting the dorsal stream cannot recognise even the most familiar objects, yet are able "to grasp that object under visual control as accurately and as proficiently as people with normal vision" (p. 169). For instance, they don't recognise a pencil when they see one, yet they can still pick it up and draw with it; it does not exist as an identifiable object for them, yet it is ready-at-hand.[9]

To what extent, we may wonder, do mathematicians use theories instrumentally, without propositional awareness? Certainly, much of their activity goes on with tacit understanding, only surfacing occasionally when things go wrong, during foundational crises, for instance, or, more prosaically, when students fail to learn the game. Other elements of their understanding will only need to be made explicit for the purposes of conventional exposition. William Thurston (1994, p. 167) explains how he can skip sections of a paper when he knows "how it will go," since he would find it quicker to construct his own proof rather than wade through someone else's. This he likens to the way you don't bother reading the 16-page instruction booklet for a new toaster when you already know how to use one.

Possibly this kind of linkage between mathematical thinking and phenomenologically sensitive psychology is something new that those studying the mathematical process could bring to science studies. Psychologists themselves have made attempts to offer their services to science studies, but they have encountered a great deal of resistance due largely to the persistent stand-off between sociology and psychology.

Summary

To sum up where we have reached so far, researchers adopting a variety of approaches to the study of the mathematical process are currently addressing interrelated questions. But, whereas science studies provides a meeting ground for sociologists, historians and philosophers to compare notes, there is no satisfactory arena at present for analogous serious and sustained dialogue concerning mathematics to occur. Or, if you prefer, the mathematical wing of science studies is extraordinarily weak. Either way, a community of researchers must learn to engage with each other's points of view and through participation in dialogue adopt a more explicit sense of identity. Since the correlation between the viability of a discipline and its worthiness is not particularly high, strategic planning is not to be decried, even for so good a cause as ours.

2. THE FUTURE

Studying the mathematical process

What I am proposing is the creation of an analogue to science studies. For this we shall first need a name. *Mathematics studies* would be a natural choice, although such titles are now seen by many in a bad light. Another possibility is to wrestle the term *metamathematics* from the formalists. The term is, however, weighed down by a history of foundationalism. A third option would have us stay with *philosophy of mathematics*, while moving away from the standard fare offered up in the English-speaking world under this description.

Does this field not already exist? Certainly many studies could be included under its banner, but there is a lack of self-conscious identity that would help promote conferences and the creation of journals. Aspray and Kitcher (1988, p. 17) have dubbed 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?

Three points may be made here:

(1) The term 'Maverick' I find unhelpful. Samuel Maverick was a Texan cattle-raiser, famous for not branding his calves. His name has been transformed into a word with two meanings: (a) dishonestly obtained animals, anything dishonestly obtained; (b) unbranded by its owner, not belonging to a school. The latter sense is intended by Aspray and Kitcher, rendering the term 'Maverick Tradition' as something of an oxymoron. At a time when co-operation is needed, it portrays isolated scholar-mavericks united only in their common disillusion with the traditional school's questions of the existence of mathematical objects and the truth of mathematical statements.

(2) The list of questions seems to require us to find some ahistorical criteria for judging growth and progress. We hardly need brand ourselves 'post-modernist' to wish to query this outlook. While at first glance this century would seem to present itself as one of enormous growth and progress, listen to mathematicians such as Vladimir Arnold, Réné Thom and Morris Kline for doubts on these scores.

(3) They position this approach solely within philosophy, which is fine just so long as this allows room for contributions to be made by historical writing with a point: Kline (1972) fears for the degeneracy of a mathematics that veers away from applications; Mehrtens (1990) claims that the image of a unity of mathematics put forward by modernist thinkers like Hilbert and Bourbaki has been punctured, leaving us with a welcome multiplicity of practices; McLarty (1990) proposes that the history of topos theory be written in a way to give its roots in algebraic geometry their due and so to challenge set theoretic reductionism.

But we can look beyond philosophy and history to take insights from sociology, psychology and anthropology. In this way, if you wanted to study the relevance of psychological theories of analogy for the development of mathematics, and later do a piece of interpretative history, you would not be accused of drifting out of your field in the form of a 'But-that's-not-philosophy' objection. This, after all, is asking for less of a reversal of the division of labour than did Karl Marx when he held out a vision of our being able to milk cows in the morning, and do philosophy in the afternoon.

Mathematics must be allowed to speak to us through its history and not act as a screen on which to project our philosophical or sociological fantasies. This is what Lakatos intended with the first half of his warning that "Philosophy without history is empty, history without philosophy is blind." Rather than its being empty, instead we see philosophers readily filling the void with their flights of fancy. Lakatos himself courted such dangers with his rational reconstructions of developments in World 3. Witness how the large jump in *Proofs and Refutations* from the nascent algebraic topology of the 1830s to Poincaré's *analysis situs* of the 1890s, made without a mention of the names of Riemann and Betti, induced him to rely too heavily on the method of proofs and refutations as the engine of mathematical progress.

There is a need for a deep engagement with mathematics; a large part of our research time should be spent learning and reading about mathematics and soaking ourselves in its culture. Above our door we might erect a plaque bearing the inscription 'Let no man (or women) enter who is ignorant of Riemann surfaces or Galois theory.' We shall certainly need to attract the interest of mathematicians to help us mathematically, but in a more practical sense we shall also need them to provide the resources to support our work by allowing us to teach, say, a module per year to their students. That, after all, is how most history and philosophy of science departments earn their keep. But to do this we had better not make what to them will appear as laughable mistakes. No doubt errors have crept into my papers, but I hope they including nothing comparable to claims, which may found in the literature, such as that the mean curvature at a point on a surface is an intrinsic property of that surface, or that a group is a product of a normal subgroup and the corresponding quotient group.[10] If we carry on in this vein, mathematicians will not wish to let us loose on their students and we shall remain unemployable.

What needs to be done?

Regarding the present weaknesses of our fledgling discipline, foremost is the pitiful state of our knowledge of twentieth century mainstream mathematics. It is a lamentable fact of modern academia that 'Star Trek: The Next Generation' has considerably more academic research time, conference time, and journal space devoted to it than has the recent development of the central branches of mathematics. The foundationalist legacy has caused far too much time to be spent on mathematical logic and set theory, at the expense of geometry in all its forms. This concern with foundations even carries over into a book such as *Revolutions in Mathematics* (Gillies, 1992) where the only two twentieth century studies concern logic and set theory.

In the history of mathematics, we find a complete imbalance as regards time, as though historians were wondering whether enough time has elapsed to discuss the Crimean War; twentieth century historical work is all too often done by mathematicians. Typically, when Kitcher and Lakatos turned their attention to the history of mathematics of the nineteenth century it was to the foundations of real analysis rather than the glorious nexus of concepts relating to Riemann surfaces (elliptic curves, holomorphic differentials, harmonic functions, topological genus, uniformisation).

The selection process occurs on different time scales and at different levels of commitment: the qualities sought by journal editors, what to teach and how, which problem to recommend to a doctoral student, whom to employ, which line of research to take, whom to be awarded a prize. There is, therefore, a danger similar to one treated by Kuhn in his paper *Mathematical versus Experimental Traditions in the Development of Physical Science* (Kuhn, 1976). Here Kuhn discusses how two 'noncommunicating' historiographical modes have emerged in the history of science: one which describes the development of a particular science, while the other describes the evolution of science taken as a whole. The danger run by the first approach is of artificially isolating a speciality based on present day understanding. For example, to see the antecedants of the modern theory of electricity as comprising a unified endeavour is historically inaccurate. The second approach, by taking on the whole of science, runs the risk of not delving sufficiently into the technicalities of any specific area thus lacking the resolution to explain the development of particular scientific theories. So both in their way underplay the subtle and complex dynamics of scientific evolution whose depiction requires the historian to occupy a 'difficult middle ground' where he or she may enter sufficiently into the technicalities within a field while being aware of the influence of other fields. In mathematics we find a similar situation obtains: *Proofs and Refutations* could be accused of artificially isolating algebraic topology and certain sociological works could be accused of bypassing technicalities.

Questions raised by the applicability of mathematics must feature prominently in our discipline, requiring very strong links to be made to science studies. The latter has made some important advances and we should feel free to try out some of their ideas in our patch, while maintaining the necessary distinctions. This has already been done for Lakatos, Kuhn and Laudan, but there should be insights to be taken from the writings of people like Peter Galison, Ian Hacking and Thomas Nickles. We should remember that applications although deepest in physics do occur elsewhere: coding, control theory, optimization, theoretical computing, etc.

I mentioned above my belief that some strands of psychology should prove important. Besides the suggestions of the cognitive linguists, there have been important debates concerning the nature of mental imagery. Might we not use these to interrogate the claims one hears to the effect that the education system is stacked in favour of the algebraic manipulators over the geometric intuitionists, and, even worse, that our imagery-producing faculties are atrophying due to neglect? A knowledge of styles of mathematical thinking would be useful in this context.

An enormous field is opened up, some of which should be accorded greater priority. In other words, we must worry about our own selection process. One strategy is to follow Lakatos' advice to study episodes in accordance with the basic value judgements of the élite (1978, p. 124). We might start by listening to mathematicians, so long as we do so with caution. Already there has appeared some important work arising from attempts to think through their comments. The treatment by Marquis (1997), which I mentioned above, arose from Alain Connes' claim that some mathematical objects act as machinery, allowing the finer examination of other objects. Leo Corry (1996) has investigated the claim that mathematics is the study of structure, demonstrating the problematic nature of specific assertions to this effect. The claim that mathematics is the 'science of analogy' has emerged as a *leitmotiv* in the writings of mathematicians throughout this century and also needs interrogating.

It should be possible for people from a wide range of backgrounds to contribute to our understanding of phenomena occurring within the mathematical process. Along with any of the questions raised in Part 1, we might pose more specific problems, such as evaluating the significance of the emergence through this century of algebraic topology as a vital core subject. We might hear responses begin as follows:

(1) The extension of the study of equations to several variables yielded solution spaces of dimension higher than two. Whereas the closed Riemann surfaces require a single topological invariant for their classification, viz. their genus, higher dimensional spaces require more subtle topological invariants. These are necessary for the better understanding of the differential, holomorphic and algebraic structures borne by manifolds.

(2) Its course was held up during the nineteenth century by an epistemological obstacle preventing the investigation of higher-dimensional spaces.

(3) Generally speaking, Poincaré's work was taken up slowly due to his unwillingness to form a school. While Birkhoff later took up his dynamical systems theory, three other American residents, Alexander, Veblen and Lefschetz, developed his algebraic topology strongly from 1920 onwards, allowing it to act as a symbol of the vigour of a new mathematical nation.

(4) It marks a new chapter in the centuries long elaboration of the relationship between the two classes of our sense modalities, the visual-kinaesthetic-proprioceptive and the linguistic-syntactic-propositional. The decomposition of spatial objects into subparts and a registering of their intersections is one of the strategies employed by the visual systems of the brain.

(5) Its recent applicability in physics may be related to its role in mediating between the continuous and the discrete, in the guise of the cohomology and homotopy groups of spaces, etc.

Along with the successes, we ought also to study the 'failures.' Of special interest are ideas neglected at the time but later revived: Grassman's exterior algebra and the exterior differential calculus, Kronecker's work on ring extensions and scheme theory, etc.

Studying the mathematical process as useful to mathematics itself

We might wonder what became of Lakatos' legacy. He himself abandoned mathematics for science and none of his students continued his first interest. Philip Kitcher (1983) later attempted to reconcile the two philosophies of mathematics, but the parts of his book were better received than the whole. Much could be learned from this failure to succeed in the impossible task of bridging the gap between a historically sensitive treatment of mathematical development and a philosophy embracing a correspondence theory of truth and knowledge as warranted true belief. Kitcher also later turned to science.

Even within the study of the scientific process, there are signs that the divisions of History and Philosophy of Science departments are moving apart. The historians view the philosophers' historiography as unsophisticated, and are turning towards sociological, postmodernist and phenomenological ideas for inspiration. Meanwhile, even historically-aware philosophers are using logical formalisms to encapsulate an ahistorical vision of the scientific process of model formation.

This divergence was made abundantly clear to me in a recent seminar in which a presentation was made of certain rival positions in the philosophy of mathematics. There were expressions of incredulity and murmurs of 'Scholasticism' from the historians when *modal fictionalism* was discussed. I cannot claim a profound grasp of this curious hybrid, but I take this position to be proposing that there exists some possible world in which mathematical objects exist as fictions. What is so disturbing about our own world that makes highly intelligent adults seek refuge in this style of thought? What is to stop them from going on to consider the existence in a possible world of a organism imagining a possible world in which mathematical objects exist (as fictions) ...? Are we so far from Star Trek studies (or is that *fictional modalism*)?

At least the Scholastics were overt about what they were doing, rather than engaging in a surreptious form of theology which reveals more of the inner phantasy life of the philosopher than anything about mathematics. Surely there is enough work to be done understanding the intricacies of this world:

> To suppose ... that we possess criteria of rationality which are independent of our understanding of the essentials of the scientific process is to open the door to cloud-cuckoo land (Kuhn, 1962/70, p. 264).

As I have remarked earlier, I do not believe we have much a grasp on the essentials of the *mathematical* process.

We may pose ourselves the question as to whether ahistorical philosophies of mathematics have anything to offer us. Consider a fictionalist's attempt to talk about algebraic topology's century. Given their sole concern with applied mathematics, the best we could hope for is that they know that this branch has spent much of its life as a provider for other branches. But what do we gain from the notion that mathematicians have devised some useful fictions, useful to other mathematicians in understanding their fictions, which have helped other mathematicians to produce fictions useful in physics?[11]

One slender indication of there being a potential for dialogue comes in a portion of Hintikka's recent book, *The Principles of Mathematics Revisited* (Hintikka, 1996),[12] where despite dismissing interest in mathematical practice as a 'fetish' (p. 102), he does note that mathematicians do not spend the majority of their time proving theorems, but engage in what amounts to concept-stretching. This I can agree with: the neglect of the act of mathematical description in favour of a near exclusive concern with mathematical proof has been a large mistake.

What should be a cause for concern for philosophers of mathematics is the fact that mathematicians show not the slightest degree of interest in their work. An article (Jaffe and Quinn, 1993), recently appearing in a mathematics journal, argues for the necessity of rigorous proof in the mathematics now being devised for the latest physical theories and expresses the fear that the ungrounded speculation of those romping through a field and attaching their name to all the big conjectured results may stunt the growth of promising new ideas, and hence lead to the loss of potentially important new theory. Their claims may thus be seen as the flip-side of Lakatos' concern with premature axiomatisation. The next issue of the journal included a flurry of replies to what was considered a provocative article. What was immediately noticeable was that not only did no philosopher enter into this discussion, none was even mentioned.[13] Here, mathematicians are carrying on their own debates about production and selection practices,when there is surely a place for critical input. Indeed, the only non-mathematician to contribute to the debate was Jeremy Gray, a historian, who corrected some erroneous historical claims made by Jaffe and Quinn.

So, we do find spontaneous outbursts of debate among the mathematical community concerning ideas of best mathematical practice, but their claims often suffer from a lack of precision. Ours has the potential to be not only an intellectually challenging field, revealing something of the human condition, it could also usefully allow a more richly structured setting for debates within mathematics and provide the resources for issues to be explored from a variety of angles. The findings could then be fed back into the process via modifications in educational methods.

Conclusion

These are critical times for the future course of a discipline which takes as its object the mathematical process. The numbers of researchers and their level of commitment have reached a level at which such a discipline can now become viable, but this will only happen in the event of sufficient accommodation on all sides to allow for meaningful dialogue. In this paper, I hope to have given some indication of the way our efforts might be better coordinated. Even those from opposite ends of the spectrum, radical sociologists of knowledge and Husserlian phenomenologists, should be able to find a sufficiently specific issue in which to engage each other in constructive debate.

A third of a century after *Proofs and Refutations* first appeared, the emergence of this discipline is by now long overdue. In view of his interest in the historical flow of mathematics, it can be no accident that we find Lakatos to be one of the few philosophers about whom mathematicians speak well. I have the impression, but no proof, that his ideas had a real effect on textbook writers, encouraging them to include more of the informal side of the mathematics they were discussing, a little of its history and its motivation. Ultimately, the source of the fascination held by *Proofs and Refutations* rests in the fact that it presented a brief, albeit distorted, snatch of a real mathematical argument. What we need do today is carry out a wide-ranging review of other snatches of mathematical debate. By doing so we shall prepare ourselves to be able to provide support for present-day mathematical conversations, and, who knows, we might even be able to join in.

NOTES

1. This relates to the point made by Feferman (1981) that Lakatos' concept-stretching may be represented by the predicate calculus. Lakatos himself attempted to use logical symbolism to represent the method of proofs and refutations in *The Method of Analysis-Synthesis* (1978, p. 70). On the limitations of Lakatos' philosophy of mathematics see Corfield (1997) and Kvasz (this volume).
2. Witness the fact that Duhem discussed *bons sens* and *finesse* in the field of physics, contrasting them to *géométrie* as an automatic mode of thought to which the mathematician is restricted (cf. Crowe, 1990).
3. David Ruelle, a mathematician as receptive as any to issues raised by physics, claims that "... it is clear that 20th century mathematics now largely produces its own problems, and that physics is only a secondary source of inspiration" (1988, p. 260).
4. Without a doubt, we should allow include all fields of engineering here. The computer scientist Nicholas Metropolis (1993) argues forcefully for more work to be done on the history of computer science. He provokatively maintains that:

    > The relationship between computer science and mathematics scarcely resembles that which exists between physics and mathematics. The latter may best be described as an unsuccessful marriage, with no possibility of divorce. Physicists internalize whatever mathematics they require, and eventually claim priority for whatever mathematical theory they become acquainted with. Mathematicians see to it that every physical theory, sooner or later, is freed from all shackles of reality and liberated to fly in the thin air of pure reason.
    >
    > Computer science, in a very different mode, turns to mathematics in much the same way that engineering always has. It borrows freely from already-existing mathematics, developed for altogether different purposes or, more likely, for no purpose at all. Computer scientists raid the coffers of mathematical logic, probability, statistics, the theory of algorithms, and even geometry. Far from resenting the raid, each of the disciplines is buoyed by the incursion (Metropolis, 1993, p. 123).

5. Cf. in particular section 2.3 of Mehrtens (1976) and his (1992).
6. Contrast with Boi's point 5 above.
7. Jean Petitot (1992) argues for the relevance of Gonseth's philosophical outlook to cognitive science, and in particular to the work of the cognitive linguists I mentioned above. These latter I have discussed in my unpublished PhD thesis.
8. See in particular Margolis (1989). He also advocates some deconstructionist input to stop the relationship becoming too cosy.
9. Cf. Heidegger in *Being and Time*: "The less we just stare at the hammer-thing, and the more we seize hold of it and use it, the more primordial does our relationship to it become, and the more unveiledly is it encountered as that which it is – as equipment" (1962, p. 98).
10. This last claim is true in a trivial sense, but is badly wrong in what I take to be its intended sense.
11. In recent years homotopy theory has been applied directly to account for faults in nematic crystals, so presumably requiring Fieldian nominalisation, the smuggling of structure from one formalism to the next.
12. Cf. I have review this work in Corfield (1998b)
13. While Lakatos was not mentioned in the debate, it is hard to think of any other philosopher with any comparable claim to inclusion. Cf. Stöltzner (this volume) for a detailed interrogation of this debate.

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