How Mathematicians May Fail To Be Fully Rational
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Abstract:
The possibility that narrative might play a crucial role in the practice of mathematics has been paid little attention by philosophers. The majority of Anglophone philosophers of mathematics have followed those working in the logical empiricist tradition of the philosophy of science by carving apart rational enquiry into a 'context of discovery' and a 'context of justification'. In so doing, they have aligned the justification component with the analysis of timeless standards of logical correctness, and the discovery component with the historical study of the contingent, the psychological, and the sociological. The failings of this strategy are by now plain. In this debating arena there can be no discussion of the adequacy of current conceptions of notions such as space, dimension, quantity or symmetry. Such matters become questions purely internal to the practice of mathematics, and no interest is shown in the justificatory narratives mathematicians give for their points of view. In this talk, I would like to outline the views of the moral philosopher, Alasdair MacIntyre, whose descriptions of tradition-constituted forms of enquiry are highly pertinent to the ways in which mathematics can best be conducted, and allow us to discern the rationality of debates concerning, say, the mathematical understanding of space. An essential component of a thriving research tradition is a narrative account of its history, the internal obstacles it has overcome, and its responses to the objections of rival traditions. To the extent that mathematicians do not contribute to this writing, from the MacIntyrean perspective they are failing to act fully rationally.

Introduction
How is it to act rationally as a mathematician? For much of the Anglo-American philosophy of mathematics this is answered in terms of what mathematicians most obviously produce – the journal paper. The mathematician's work is then taken to be that of deducing the consequences of various axioms and definitions. This leads to a view of the discipline as being very unlike any other. Those who cannot believe that mathematics is so very different in essential ways from other intellectual disciplines thus run up against a majority position in philosophy in the English speaking world, which has seen fit to concentrate heavily on aspects of mathematics that do not feature largely elsewhere: its use of deductive proof, its supposed capacity to be captured by some or other formal calculus, the abstractness of the objects it studies. But what if this were the wrong strategy? What if we started out by seeking what mathematics shares with other intellectual activities, such as the natural sciences? What if we studied mathematics as an example of rational intellectual enquiry, by examining in the following order:
(i) What it evidently shares with other forms of such enquiry;
(ii) What is most evident in mathematics, but occurs elsewhere;
(iii) What is most evident elsewhere, but occurs also in mathematics;

(iv) What is peculiar to mathematics.

One of the few to adopt this strategy was the philosopher Imre Lakatos. His understanding of what constitutes rational enquiry led him to call upon mathematical practitioners to *expose* their work in novel ways. Indeed, Lakatos called for a 'mathematical criticism' to parallel literary criticism.[1] He did so not merely for pedagogical reasons, but also because he believed that this would provide the conditions for mathematics to take its proper course. However, Lakatos's philosophy of mathematics has been found wanting in several respects both justly and unjustly, as I explain in chaps. 7 and 8 of Corfield (2003), lessening the effectiveness of his message. What I should like to outline in this paper is what I take to be a superior account of rational enquiry, that of the moral philosopher Alasdair MacIntyre. With this account in place, pressure can be brought to bear on mathematicians. If philosophers' best conception of rational enquiry requires the employment of certain kinds of mode of expression, currently all too infrequently heard, unless mathematicians can defeat this conception, they will either have to write in these ways more often and more systematically, or else count themselves not fully rational.

Three Versions of Enquiry

As I have said, I shall be helping myself to resources provided by someone trying very hard to reconfigure philosophy from the field of ethics. In his Three Rival Versions of Moral Enquiry, Alasdair MacIntyre (1990a) distinguishes between the following conceptualisations of enquiry:

<u>Encyclopaedic</u>

This version of enquiry presumes a single substantive conception of rationality, one that any reasonable, educated human being can follow. It separates intellectual enquiry into separate domains - science, aesthetics, ethics, etc., architectonically arranging each. It aims to cast theoretical knowledge in the form of transparent reasoning from laws or first principles acceptable to all reasonable people. These laws are derived from facts, i.e., tradition-independent particular truths. The high water mark of commitment to this version of enquiry is reached in the Scottish intellectual circles of the second half of the nineteenth century whose goal was to encapsulate what was known in an encyclopaedia, in which they displayed faith in an inevitable progress. From the introduction of the 9th Edition of the Encyclopaedia Britannica we read:

"The available facts of human history, collected over the widest areas, are carefully coordinated and grouped together, in the hope of ultimately evolving the laws of progress, moral and material, which underlie them, and which help to connect and interpret the whole movement of the race."

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[1] "Why not have mathematical critics just as you have literary critics, to develop mathematical taste by public criticism?" (Lakatos 1976: 98). See also Brown (1994: 50): "Does our education of mathematicians train them in the development of faculties of value, judgement, and scholarship? I believe we need more in this respect, so as to give people a sound base and mode of criticism for discussion and debate on the development of ideas."

Such a conception of moral enquiry has all but disappeared, but its ghost haunts the unresolvable debates of contemporary ethics and other branches of philosophy. The dream of the encyclopaedia has recently been revived by the resources of the Web. Wikipedia, for example, is an excellent collaborative project, but it has recently come to grief over the problem of rival researchers trying to impose their version of a topic on the encyclopaedia. Rules have had to be introduced to stop contributors editing entries just as they like, but it seems hard to believe that the problem of rival traditions can be fully overcome.

<u>Genealogical</u>
Nietzsche certainly did not view the moral theories current in Western Europe in the late nineteenth century as the rational products of mankind's finest minds, emancipated from the yoke of centuries of tradition. "This world is the will to power - and nothing besides, and you yourselves are also this will to power - and nothing besides." The world is an interplay of forces, ceaselessly organizing and reorganizing itself, giving rise to successive power relationships.

Truth is "A mobile army of metaphors, metonymies, anthropomorphisms, a sum, in short, of human relationships which, rhetorically and poetically intensified, ornamented and transformed, come to be thought of, after long usage by a people, as fixed, bunding, and canonical. Truths are illusions which we have forgotten are illusions, worn-out metaphors now impotent to stir the senses, coins which have lost their faces and are considered now as metal rather than currency?" (Über Wahrheit und Lüge im aussermoralischen Sinne I)

The genealogist's tasks involve the discrediting of canons, by the unmasking of the will to power.

Genealogists and Encyclopaedists agree: "Either reason is thus impersonal, universal, and disinterested or it is the unwitting representative of particular interests, masking the drive to power by its false pretensions to neutrality and disinterestedness." (Macintyre 1990a: 59) But there is a third possibility: "that reason can only move towards being genuinely universal and impersonal insofar as it is neither neutral nor disinterested, that membership in a particular type of moral community, one from which fundamental dissent has to be excluded, is a condition for genuinely rational enquiry and more especially for moral and theological enquiry." This MacIntyre calls <u>tradition-constituted enquiry</u>.

We are less aware of this version of enquiry, MacIntyre claims, due to a rupture in philosophical theorising which took place between the time of Aquinas and that of Descartes, resulting in the formulation of philosophy as the search for clear and evident first principles, the patent lack of which has fed scepticism. For Plato and Aristotle, however, philosophy was conceived of as a craft, requiring something akin to apprenticeship. Here MacIntyre describes what it is to work within a craft:

"The standards of achievement within any craft are justified historically. They have emerged from the criticism of their predecessors and they are justified because and insofar as they have remedied the defects and transcended the limitations of those predecessors as guides to excellent achievement within that particular craft. Every craft is informed by some conception of a finally perfected work which serves as the shared *telos* of that craft. And what are actually produced as the best judgments or actions or objects so far are judged so because they stand in some determinate relationship to that *telos*, which furnishes them with their final cause. So it is within forms of intellectual enquiry, whether theoretical or practical, which issue at any particular stage in their history in types of judgment and activity which are rationally justified as the best so far, in the light of those formulations of the relevant standards of achievement which are rationally justified as the best so far. And this is no less true when the *telos* of such an enquiry is a conception of a perfected science or hierarchy of such sciences, in which theoretical or practical truths are deductively ordered by derivation from first principles. Those successive partial and imperfect versions of the science or sciences, which are elaborated at different stages in the history of the craft, provide frameworks within which claimants to truth succeed or fail by finding or failing to find a place in those deductive schemes. But the overall schemes themselves are justified by their ability to do better than any rival competitor so far, both in organizing the experience of those who have up to this point made the craft what it is and in supplying correction and improvement where some need for these has been identified.

The temporal reference of reasoning within a craft thus differs strikingly from that of either encyclopaedia or genealogical reasoning. The encyclopaedist aims at providing timeless, universal, and objective truths as his or her conclusions, but aspires to do so by reasoning which has from the outset the same properties. From the outset all reasoning must be such as would be compelling to any fully rational person whatsoever. Rationality, like truth, is independent of time, place, and historical circumstances." (Macintyre 1990a: 64-65)

Only through a teacher can one come into a position to proceed correctly:
"The authority of a master is both more and other than a matter of exemplifying the best standards so far. It is also and most importantly a matter of knowing how to go further and especially how to direct others towards going further, using what can be learned from the tradition afforded by the past to move towards the *telos* of fully perfected work. It is thus in knowing how to link past and future that those with authority are able to draw upon tradition, to interpret and reinterpret it, so that its directedness towards the *telos* of that particular craft becomes apparent in new and characteristically unexpected ways. And it is by the ability to teach others how to learn this type of knowing how that the power of the master within the community of a craft is legitimated as rational authority." (Macintyre 1990a: 65-6).

"The genealogist has no way of understanding such authority except as one more form of domination imperfectly disguised by its mask of rationality, a mask necessarily worn with a self-distorting lack of self-knowledge. To treat tradition as a resource is similarly one more way of allowing the past to subjugate the present. And the central symptom of the sickness of this type of social existence, from the genealogical standpoint, is that, despite its recognition of the historical situatedness of all reason-giving and reason-offering, it understands the truth to which it aspires as timeless. Hence the rationality of craft-tradition is as alien and hostile to the genealogical enterprise as is the encyclopaedist's to either." (Macintyre 1990a: 66)

Leaving aside the matter of whether a tradition-constituted account of moral enquiry is plausible, let's see how these three versions might translate to more precise forms of enquiry. A latter genealogist Michel Foucault distinguished the human sciences from mathematics, cosmology, and physics, which he describes as "noble sciences, rigorous sciences, sciences of the necessary" where, unlike with economics or philology, "one can observe in their history the almost uninterrupted emergence of truth and pure reason." (1970: ix). This hasn't stopped genealogically inspired studies of science.

Philosophy of science
A classification of contributions to the philosophy of science might run as follows:

(E) The Vienna Circle, logical empiricists, contributors to contemporary realist/antirealist debates.
(G) Sociologists of scientific knowledge, Latour, and other targets of Sokal.
(T) Collingwood (e.g., 'The Idea of Nature'), (the later Popper), Lakatos, Polanyi, Michael Friedman ('Dynamics of Reason'), MacIntyre.

Position (G) is seen by many as the opposition in the so-called 'Science Wars'. Position (T) has been hard to sustain, and is frequently taken to be identical to (G) by advocates of (E) and *vice versa*. Perhaps this is because many of its exponents have not become fully conscious of the differences. Where in this scheme would Kuhn appear? While I have heard him described disparagingly as 'progressivist', he is often taken by 'orthodox' philosophy of science to belong to the (G) camp. I am sure this latter view is wrong. Remember that *The Structure of Scientific Revolutions* first appeared in the *Encyclopedia of Unified Science*, edited by the Vienna Circle member Rudolf Carnap. Perhaps the difficulty in locating Kuhn reflects a problem with the consistency of his position.

We see in the following quotations MacIntyre's advocacy of a (T)-style philosophy of science:

"…natural science can be a rational form of enquiry if and only if the writing of a true dramatic narrative - that is, of history understood in a particular way - can be a rational activity." (1977: 464)

"It is more rational to accept one theory or paradigm and to reject its predecessor when the later theory or paradigm provides a stand-point from which the acceptance, the life-story, and the rejection of the previous theory or paradigm can be recounted in more intelligible historical narrative than previously. An understanding of the concept of the superiority of one physical theory to another requires a prior understanding of the concept of the superiority of one historical narrative to another. The theory of scientific rationality has to be embedded in a philosophy of history." (1977: 467)

MacIntyre explains how position (T) is consistently misunderstood:

"...to introduce the Thomistic conception of enquiry into contemporary debates about how intellectual history is to be written would, of course, be to put in question some of the underlying assumptions of those debates. For it has generally been taken for granted that those who are committed to understanding scientific and other enquiry in terms of truth-seeking, of modes of rational justification and of a realistic understanding of scientific theorizing must deny that enquiry is constituted as a moral and a social project, while those who insist upon the latter view of enquiry have tended to regard realistic and rationalist accounts of science as ideological illusions. But from an Aristotelian standpoint it is only in the context of a particular socially organized and morally informed way of conducting enquiry that the central concepts crucial to a view of enquiry as truth-seeking, engaged in rational justification and realistic in its self-understanding, can intelligibly be put to work." (1990b: 193)

Against Kuhn's point that there's little sign of convergence in the historical record, that for example Einstein is closer in some ways to Aristotle in their common reliance on notions of a field than he is to Newton, instead we say that no plausible story could be written of how to move from Aristotle straight to Einstein, whereas one clearly could be written which passes via Newton.

How would the trichotomy work in philosophy of mathematics?

Philosophy of mathematics
(E) Formalism, Logicism, Intuitionism. Analytic style responses to Benacerraf, indispensability arguments, structuralism, modal fictionalism.

(G) Bloor on 2 + 2 = 4, MacKenzie on deduction, Pickering on quaternions – these are mild forms.[2] Stronger forms come from mathematicians complaining about what they see as wrong directions (Arnold), or limited viewpoints (Mandelbrot), but they only extend the unmasking attitude to others' work.

For example, Arnold declares:
"In the middle of the twentieth century a strong mafia of left-brained mathematicians succeeded in eliminating all geometry from the mathematical education (first in France and later in most other countries), replacing the study of all content in mathematics by the training in formal proofs and the manipulation of abstract notions. Of course, all the geometry, and, consequently, all relations with the real world and other sciences have been eliminated from the mathematics teaching."

(T) Lakatos, (Kitcher), Maddy, Krieger,...

Where Lakatos called for an equivalent of Literary Criticism, (G) would call for an equivalent of some forms of cultural theory, as in some contributions to Herrnstein Smith

[2] Bloor 1994; MacKenzie 2001; Pickering, 1995.

and Plotnitsky A. (1997). Kitcher's name I place in brackets because although *The Nature of Mathematical Knowledge* is concerned with the rational transmission of practices, the larger framework developed over the second half of the book is in the (E) style. I place Krieger (*Doing Mathematics*) in (T), since he has done more than anyone to emphasise the craft-like nature of mathematics. That Lakatos should be seen as a proponent of (T) surprises some people who are too quick to cast him as (G). But consider these claims:

"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 one achieves the real through dialectical reasoning, perfectly well-defined entities being discarded along the way. This points to a much more interesting distinction than is covered by contemporary (E) uses of the terms 'nominalism' and 'realism', which are employed in a blanket fashion. Either all mathematical entities exist, or none do. Instead we can seek to locate this distinction in the opinions of a single mathematician, such as André Weil. In the fragment of the letter to his sister which in his Collected Works is tacked on to the end of another letter, Krieger's translation of which recently appeared in the Notices of the American Mathematical Society, Weil likens the mathematician's work to that of the sculptor working on a hard piece of rock whose structure dictates the emerging shape. This marks the perfect contrast to the passage in the full letter where Weil describes the experience of formulating axioms for uniform spaces as follows: "When I invented (I say invented, and not discovered) uniform spaces, I did not have the impression of working with resistant material, but rather the impression that a professional sculptor must have when he plays with a snowman." (Krieger 2003: 304).

There is a curious pact between (G) and (E) which prevents (T) from flourishing.[3] Where they give the impression that they are stout defenders of truth in our relativist times, the limited place analytic descendants of the (E) position accord to rationality in mathematics is in fact quite simply dangerous. They like to drive a wedge between mathematics and science by pointing to the cumulative nature of mathematical truths, where physics seems to involve frequent overhauls. When you reply to them that the way mathematical results are considered gets radically transformed over time, they may then invoke a hard/soft divide. The hard facts are permanently established, while the soft ways we think about them, such as the position they might come to hold in a completed system, or the new light they cast on our conceptions of symmetry, dimension, quantity, for example, may change. But the drawing of the hard/soft distinction ought to be seen for what it is – a huge concession to the genealogist. Rationality must apply to the soft stuff, or else all those decisions made by referees to reject logically correct, but not terribly interesting papers, and all those decisions to award prizes to promising young mathematicians, are purely whimsical choices, or worse mere politicking. Genealogical sociologists of

[3] Steve Fuller points in a similar vein to the "*entente cordiale* that currently exists between history and philosophy [of science]" (Fuller 2001: 571).

knowledge wouldn't even have to turn up to claim the territory yielded to them, but instead could start picking away at the tiny residue (E) exponents are left clinging on to.

This insistence on the "established" is damaging in the extreme because it stops us from talking about the psychological, educational, and institutional aspects of mathematics. Subtract the community of mathematicians' *indwelling* in their theories, to borrow a term from Polanyi, and all you have left is a lot of black ink on a lot of pages. Finding that the vast majority is in some sense orthographically correct, is little more exciting than finding that all English poetry needs only 26 letters. But the liberal successor to Enlightenment thinking can do no other than to treat as mere preferences the vital decisions of mathematicians as to how to direct their own and others' research.

They may reply that these are not the concerns of philosophy, but for myself, I need no other warrant to study the supposedly "soft" than that Plato himself treats the soft part of mathematics in The Republic (528b - e), where in his discussion of the overall shape mathematics is taking he complains of the underdeveloped state of three-dimensional geometry, bemoans the lack of willing students, and suggests that if the state showed interest and funded it things would improve.

The hard/soft distinction is not totally dissimilar to Leo Corry's body/images distinction:[4] "For the purposes of the present discussion it will suffice to point out that this is a flexible, schematic distinction focusing on two interconnected layers of mathematical knowledge. In the body of mathematics I mean to include questions directly related to the subject matter of any given mathematical discipline: theorems, proofs, techniques, open problems. The images of mathematics refer to, and help elucidating, questions arising from the body of knowledge but which in general are not part of, and cannot be settled within, the body of knowledge itself. This includes, for instance, the preference of a mathematician to declare, based on his professional expertise, that a certain open problem is the most important one in the given discipline, and that the way to solve it should follow a certain approach and apply a certain technique, rather than any other one available or yet to be developed. The images of mathematics also include the internal organization of mathematics into sub-disciplines accepted at a certain point in time and the perceived interrelation and interaction among these. Likewise, it includes the perceived relationship between mathematics and its neighbouring disciplines, and the methodological, philosophical, quasi-philosophical, and even ideological conceptions that guide, consciously or unconsciously, declared or not, the work of any mathematician or group of mathematicians." (2006: 3). A history of mathematics required to remain at the level of the body would be unimaginably tedious, and worse still misrepresentative. Some histories have been written approximating to this remit, and indeed are extremely dull. But such histories are the natural bedfellows of much contemporary (E) style philosophy

[4] See also Corry 1989 "Linearity and Reflexivity in the Growth of Mathematical Knowledge", SIC 3, 409-440. Corry, L. (2001), "Mathematical Structures from Hilbert to Bourbaki: The Evolution of an Image of Mathematics", in A. Dahan and U. Bottazzini (eds.) Changing Images of Mathematics in History. From the French Revolution to the new Millenium, London: Harwood Academic Publishers, 167-186. Corry, L. (2003), Modern Algebra and the Rise of Mathematical Structures , Basel and Boston, Birkhäuser, 2d revised edition (1st ed. - 1996).

of mathematics. Little can be learnt from them.

Corry rightly points out that, “The images of mathematics of a certain mathematician may contain tensions and even contradictions, they may evolve in time and they may eventually change to a considerable extent, contradicting at times earlier views held by her. The mathematician in question may be either aware or unaware of the essence of these images and the changes affecting them.” (2006: 4). But a (T) type philosophical account of rationality cannot rest with this observation. It requires of mathematicians that they make great efforts to bring out these images and to refine them by learning from the internal tensions revealed within critical discussion with other practitioners. For the mathematical sciences, Michael Friedman’s account of the necessity of prospective meta-paradigmatic work makes a similar point.[5]

In view of the yielding up of so much of mathematical activity to irrationalism by the modern descendants of the Encyclopaedists, the interesting battle line would seem to be between (G) and (T) exponents, both versed in the history of the subject. The following claims might be thought then to demarcate the line between (G) and (T):

Lakatos tells us in *Proofs and Refutations* 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)

But the editors of *Proofs and Refutations* declare that Lakatos would have modified the passage from which it is taken “for the grip of his Hegelian background grew weaker and weaker as his work progressed.” (p. 146 n2) and that he came to think human ingenuity is required to resolve problems. The editors have come in for much criticism for these footnotes, but they may well be right about this, after all they were his students. In any case, it is quite proper for an advocate of the tradition-constituted version of enquiry to accept this. If rational enquiry is likened to a craft, evidently it requires diligence and other virtues for its practice. It’s not just a matter of not standing in the way of dialectical progress, one must actively engage in the process.

This is not the proper boundary. It’s the notion of progress towards a *telos* which distinguishes Genealogy and Tradition. What candidates do we have for a *telos* of mathematical enquiry?

The *telos* of mathematical enquiry

What is the aim of mathematics? What are the internal goods it seeks? The production of as many mathematical truths as possible? Mathematicians typically point us elsewhere, or

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[5] See my ‘Reflections on Michael Friedman’s Dynamics of Reason’, http://philsci-archive.pitt.edu/archive/00002270/.

else use "true" in an atypical way. Rene Thom tells us “What limits the true is not the false but the insignificant.”, while Vaughan Jones remarks:

“...the "truth" of a great piece of mathematics amounts to far more than its proof or its consistency, though mathematics stands out by requiring as a *sine qua non*, a proof that holds up to scrutiny.” (Jones 1998: 204)

But then what is progress towards if not some ultimate logical correctness? One should expect, and welcome, different views about the aims of mathematics. In one of his Opinions,[6] Doron Zeilberger suggests that the discovery of humanly-inachievable results is one such aim, but others disagree. I shall follow them here. Recalling MacIntyre's comments about the master craftsman, good mathematicians don’t just know facts like people at a pub quiz, they know how things behave, they sense promising directions. They communicate a vision of how things might be. This is surely why mathematics exam questions go a certain way. State a result, prove it, then apply it in a novel situation. What is being tested is fledgling understanding. For William Thurston and others human understanding is the aim.[7]

How do mathematicians advance human understanding of mathematics? (Thurston 1994, 162) (Note that this certainly doesn’t stop him using computers in his research.)

It cannot be too often reiterated that the aim of collegiate mathematics is the understanding of mathematical ideas *per se*. The applications support the understanding, and not *vice versa*. . . . (Mac Lane 1954: 152)

The desire to understand is the most important dynamic for the advance of Mathematics. (MacLane 1986: 454).

... a proof is important as a check on your understanding. I may think that I understand, but the proof is the check that I have understood, that's all. It is the last stage in the operation - an ultimate check - but it isn't the primary thing at all.
…it is hard to communicate understanding because that is something you get by living with a problem for a long time. You study it, perhaps for years. You get the feel of it and it is in your bones. (Atiyah 1984, 305).

It is also not hard to find the goal of understanding appearing in the stated aims of branches:

A major aim of functional analysis is to understand the connection between the geometry of a Banach space X and the algebra L(X) of bounded linear operators from the space X into itself (Bollobas 1998: 109)

Symplectic topology aims to understand global symplectic phenomena... (McDuff & Salamon 1995: 339)

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[6] http://www.math.rutgers.edu/~zeilberg/Opinion51.html
[7] What it is for understanding to be human needs spelling out.

Broadly speaking, the goal of the theory of dynamical systems is, as it should be, to understand most of the dynamics of most systems... The ultimate goal of the theory should be to classify dynamical systems up to conjugacy. This can be achieved for some classes of simple systems; but even for (say) smooth diffeomorphisms of the two-dimensional torus, such a goal is totally unrealistic. Hence we have to settle to the more limited, but still formidable, task to understand most of the dynamics of most systems. (Yoccoz 1995)

Is there just one sense of 'understand' here? Perhaps it depends on what understanding is of: entities, results, concepts. If you aim to advance the understanding of, say, finite groups, then classification is a big step; to advance understanding of a result may require a new proof; for symmetry, perhaps you need to define new entities such as groupoids or Hopf algebras. Can't it all be cashed out in terms of the hard stuff?

For Thurston, the answer is "No". Any suggested result may prove to a poor guide to progress in a field:

"just as Poincaré's conjecture, [The Geometrization Conjecture] is likely not to be resolved quickly, but I hope it will be a more productive guide to research on 3-manifolds than Poincaré's question has proven to be." (Thurston 1982: 358).

As understanding improves, of course, more results will be discovered, but the former must be taken as primary. The importance of the results rests on their revealing to a greater or lesser extent what the understanding has accomplished. Elsewhere, Thurston makes clear that he distinguishes the activities of proving results which are employed in classification situations and the promotion of understanding:

"What mathematicians most wanted and needed from me was to learn my ways of thinking, and not in fact to learn my proof of the geometrization conjecture for Haken manifolds." (Thurston 1994: 176).

He discusses how as he started out in mathematics he found that:

"Mathematical knowledge and understanding were embedded in the minds and in the social fabric of the community of people thinking about a particular topic. This knowledge was supported by written documents, but the written documents were not really primary." (Thurston 1994: 168)

Is the minor role of written documents necessary? Don't unrecorded forms of communication privilege certain people? Doesn't this way of carrying on allow understanding to be lost, progress reversed?

"Today, I think there are few mathematicians who understand anything approaching the state of the art of foliations as it lived at that time..." (Thurston 1994: 173)

Now we have the technological resources to make available lectures, the excuse not to communicate since textual recording is not best suited to conveying understanding is no longer sufficient.

Let's return to MacIntyre for a Thomistic Aristotelian view:
“It is a central feature of all crafts, of furniture making and fishing and farming, as much as of philosophy, that they require the minds of those engaged in the craft to come to terms with and to make themselves adequate to the existence and properties of some set of objects conceived to exist independently of those minds. The embodied mind, in and through its activity, has to become receptive to forms (*eide*) of what is other than itself and in being constituted by those formal objects becomes, in the appropriate way, them. It is not therefore judgments which primarily correspond or conform to those realities about which they are uttered; it is the embodied mind which conforms adequately or inadequately to the objects, the *res*, the subject matter, and which evidences this adequacy in a number of ways, one of which is the truth or falsity of its judgments. It is in becoming adequate to its objects that the embodied mind actualizes its potentialities and becomes what its objects and its own activity conjointly have been able to make it.” (Macintyre 1990a: 68)

“...it is important to remember that the presupposed conception of mind is not Cartesian. It is rather of mind as activity, of mind as engaging with the natural and social world in such activities as identification, reidentification, collecting, separating, classifying, and naming and all this by touching, grasping, pointing, breaking down, building up, calling to, answering to, and so on. The mind is adequate to its objects insofar as the expectations which it frames on the basis of these activities are not liable to disappointment and the remembering which it engages in enables it to return to and recover what it had encountered previously, whether the objects themselves are still present or not.” (1988: 56)

Adequacy does not characterise a correspondence relation between judgement and judged. A claim of truth is a claim that one will never be revealed to have only a partial adequacy to one’s objects. Knowledge of this kind takes the form of a deduction from first principles, but these are only understood by someone who has worked through the dialectic of their establishment, they are not things that are clear to any rational being. There is a final truth of one who has achieved perfected understanding, but this is not a truth that a person can know they have achieved. The *telos* of mathematical enquiry is an adequacy of the mind to its object which does not suffer from any partiality of viewpoint. It is always possible such partiality will be revealed.

But then what can we say about a particular piece of reasoning? Its significance can only be understood through our best account of its place within the whole:

“...enquiry can only be systematic in its progress when its goal is to contribute to the construction of a system of thought and practice - including in the notion of construction such activities as those of more or less radical modification, and even partial demolition with a view to reconstruction - by participating in types of rational activity which have their telos in achieving for that system a perfected form in the light of the best standards for judging of that perfection so far to emerge. Particular problems are then partially, but in key ways, defined in terms of the constraints imposed by their place within the overall structure, and the significance of solving this or that particular problem derives from that place." (MacIntyre 1984: 148)[8]

To contribute to this final organisation is the end of an Aristotelian mathematician. If told that in ten years time a new approach would come along and make their work permanently unnecessary, in that their ideas would not have contributed to this better approach, would have left no trace, and would have led their students away from more promising courses, would a mathematician not want to stop what they are doing? So, a piece of mathematical reasoning written in full by an Aristotelian, such as it seems Thurston might be, should go something like as follows:

> Since perfected understanding of its objects is the goal of mathematics,
> and since 3-manifolds are and plausibly will remain central objects of mathematics, with deep connections to other central objects,
> and since seeking sufficient theoretical resources to prove the Geometrization Conjecture will in all likelihood require us to achieve an improved understanding of 3-manifolds, and indeed yield us reasoning approximating to that of a perfected understanding,
> it is right for us to try to prove the Geometrization Conjecture.

Of course, we should not expect premises of this form to be mentioned at the beginning of every article, but our best reasons for taking 3-manifolds to be objects for a perfected mathematical understanding, and our best account of the place of the Geometrization Conjecture in a perfected understanding of 3-manifolds ought to be given somewhere, as Thurston (1982) himself did. Also note that we need a notion of mathematical kinds, in this case that of 3-manifolds. A possible nominalist position holds that the definition of 3-manifolds does not cut out a natural class of entities, i.e., claims they are arbitrarily grouped, having nothing more in common than that they happened to be named 3-manifolds. An early venture into such a theory can be seen on my 'Mathematical Kinds, or Being Kind to Mathematics',[9] where attitudes towards groupoids are divided into three classes: they form a natural kind; they are useful but not essential; they are useless. From above, we can now gloss the second of these classes as: groupoids may currently usefully expand our understanding of certain fields, but would not feature in a perfected understanding of those fields.

Clearly we are very far from achieving perfected knowledge at the present time. Tips of icebergs are being sighted everywhere. Also glimpsed are mushrooms, archipelagos,

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[8] Aquinas in Disputed Questions on Truth, I, 2 draws a distinction between the adequacy of the divine intellect and of the human intellect. Also, against Aristotle he argues that we only know essences through their effects, i.e., through the *quidditas* of the existent particular, Disputed Questions on Spiritual Creatures II, replies to objections 3 and 7. Might this account for the Gelfand Principle, expounded at the conference by Gowers: give the simplest non-trivial example of any concept you are explaining?
[9] http://philsci-archive.pitt.edu/archive/00001960/

peaks in the mist, and dinosaur bones. Pierre Cartier confirms this impression:

“When I began in mathematics the main task of a mathematician was to bring order and make a synthesis of existing material, to create what Thomas Kuhn called normal science. Mathematics, in the forties and fifties, was undergoing what Kuhn calls a solidification period. In a given science there are times when you have to take all the existing material and create a unified terminology, unified standards, and train people in a unified style. The purpose of mathematics, in the fifties and sixties, was that, to create a new era of normal science. Now we are again at the beginning of a new revolution. Mathematics is undergoing major changes. We don't know exactly where it will go. It is not yet time to make a synthesis of all these things - maybe in twenty or thirty years it will be time for a new Bourbaki. I consider myself very fortunate to have had two lives, a life of normal science and a life of scientific revolution.” (Interview with M. Senechal in Mathematical Intelligencer)

We should expect then that the mathematical parallel to Friedmannian meta-paradigmatic work is very necessary at this time.

Rival traditions
An important topic for a theory of enquiry is the resolution of rival claims to truth. For genealogists disagreements are resolved by (masked) force, the will to power. Encyclopaedists' disagreements are resolved by debate on neutral ground, one side is shown to be simply wrong. What of the tradition-constituted version?

Lakatos worried that Kuhn was advocating a 'mob psychology', and tried to find an improved Popperian account. Against Popper he claimed that theories are born refuted, e.g., Newton would have to be counted as a failed scientist by a Popperian. For Lakatos the right unit to assess a piece of science at is at the level of a research programme, a series of theories, with a unifying heuristic spirit which provides the resources for deciding which path to travel, how to react to obstacles, and so on. Rationality is not about which proposition to believe, but about which programme it's rational to sign up to. To decide this one should know how they are progressing or degenerating. The criteria he terms heuristic, theoretical, and empirical progress.

For MacIntyre these criteria cannot work if they are taken to be employable by people from outside the programme, the neutral standpoint is an Enlightenment dream. Theories “...progress or fail to progress and they do so because and insofar as they provide by their incoherences and their inadequacies - incoherences and inadequacies judged by the standards of body of theory itself - a definition of problems, the solution of which provides direction for the formulation and reformulation of that body of theory.”

MacIntyre is not so far from Lakatos, invoking shades of the latter’s notion of degenerating research programmes, but he insists that to gauge the progress of a tradition you need to be trained in it, as criteria of success are specific to a tradition.

“...just because at any particular moment the rationality of a craft is justified by its history so far, which has made it what it is in that specific time, place, and set of historical circumstances, such rationality is inseparable from the tradition through which it was achieved. To share in the rationality of a craft requires sharing in the contingencies of its history, understanding its story as one's own, and finding a place for oneself as a character in the enacted dramatic narrative which is that story so far. The participant in a craft is rational qua participant insofar as he or she conforms to the best standards of reason discovered so far, and the rationality in which he or she thus shares is always, therefore, unlike the rationality of the encyclopaedic mode, understood as a historically situated rationality, even if one which aims at a timeless formulation of its own standards which would be their final and perfected form through a series of successive reformulations, past and yet to come." (MacIntyre 1990a: 65)

This allows for a stronger form of incommensurability than Lakatos allows, without leading to a radical relativism, each participant acting according to the different rational standards of their own tradition, as we'll see below.

For Lakatos, rational theory choice is possible to the extent that an "internal history" or "rational reconstruction" can be formulated according to which one rival wins out over the other. This allows for a departure from actual history, which generally shows programmes to be incommensurable. One rational reconstruction is superior to another if it constitutes more of actual history as rational. MacIntyre disagrees: "it matters enormously that histories should be true, just as it matters that our scientific theories make truth one of their goals."

"I am suggesting, then, that the best account that can be given of why some scientific theories are superior to others presupposes the possibility of constructing an intelligible dramatic narrative which can claim historical truth and in which such theories are the subject of successive episodes. It is because and only because we can construct better and worse histories of this kind, histories which can be rationally compared with each other, that we can compare theories rationally too. Physics presupposes history and history of a kind that invokes just those concepts of tradition, intelligibility, and epistemological crisis for which I argued earlier. It is this that enables us to understand why Kuhn's account of scientific revolutions can in fact be rescued from the charges of irrationalism levelled by Lakatos and why Lakatos's final writings can be rescued from the charges of evading history levelled by Kuhn. Without this background, scientific revolutions become unintelligible episodes; indeed Kuhn becomes - what in essence Lakatos accused him of being - the Kafka of the history of science. Small wonder that he in turn felt that Lakatos was not an historian, but an historical novelist." (1977: 470-1)

For rival traditions willing to engage with each other we can propose the following agenda: Provide the context for an extended debate. Remind both sides that there's no spot rationality to decide which of the rivals it is most rational to join, but that we can strive to give the best ongoing assessment of their relative strengths. Ideally, there would be an account of what is the common ground between rivals, then a recognition that each tradition has its own criteria to decide progress. What we can expect of each rival is a clear statement of its principles, what it considers to be the path by which it overcame obstacles, which are its greatest successes and what in its terms are the largest open problems confronting it. Also, we need an account of what it takes to be the strengths of the rival, and whether it can understand these in its own terms, and of the weaknesses of the rival and how it understands why they should arise. And it ought to encourage some members to learn the other language as a second language, or even a second first language.

If genuine incommensurability "…can only be recognized and characterized by someone who inhabits both conceptual schemes, who knows and is able to utter the idiom of each from within, who has become, so to speak, a native speaker of two first languages, each with its own distinctive conceptual idiom", then surely it is unfortunate that "…such persons are rarely numerous. They are the inhabitants of boundary situations, generally incurring the suspicion and misunderstanding of members of both of the contending parties." (Macintyre 1990a: 114)

The outcomes we may expect are: no result, pressure on a rival, acceptance of the explanation of a rival's resourcelessness, a merging of traditions. There is no problem with the co-existence of rival traditions. Indeed, rivalry should be seen as an opportunity to rethink one's own principles, a chance for a form of falsification, potentially leading to a creative reformulation, in sum an opportunity which should be taken. In some ways the promotion of this form of rationality is not so much one of trying to beat the other side, but rather one of holding up a mirror. The other party might claim that your mirror is distorting, but they might also have a moment of insight into why the are encountering difficulties or even into a failing they did not realise they had.

"The rival claims to truth of contending traditions of enquiry depend for their vindication upon the adequacy and the explanatory power of the histories which the resources of each of those traditions in conflict enable their adherents to write." (1988: 403)

I am trying to put this conception of rationality into practice in my work at the Max Planck Institute on machine learning: Bayesian versus Statistical learning theory. The novel feature suggested by MacIntyre, a culture of confession to go alongside dialectical questioning, is to seek out and be honest as to problematic or insufficiently worked out areas of one's programme. One should render one's tradition maximally vulnerable, running it up against the best points of the opposition. Some have found it hard to expose these, usually one hides one's incompletenesses. But if one recognises that these may be the source of what is dynamic to the programme, (progressive problem shifts) rather than something to be embarrassed about, this need not be the case. What are required of the participants are certain virtues not always to be found in researchers, including sufficient justice not to exploit unfairly one's rivals' admissions of incompleteness.

We see these kinds of discussions occurring elsewhere in science, although perhaps most noticeable in domains where no single programme dominates. A handful of the participants of the string theory versus loop quantum gravity rivalry argue well. A few exponents from each side are willing to admit current deficiencies, learn each other's language. What about where one tradition dominates, might there be a role for the philosopher? Here's Rene Thom's opinion: "Je voudrais dire mon étonnement que la collectivité scientifique ne possède pas en son sein des critiques, à la manière des critiques littéraires ou artistiques. Sans doute les barrières à la publication (la <<*peer review*>>) jouent-elles – partiellement – ce rôle. Mais dès qu'un groupe – un <<paradigme Kuhnien>> - a conquis sociologiquement une position dominante, ces barrières tendent à perdre leur pouvoir de discrimination; bénignes pour les tenants du paradigmes, féroces pour les étrangers. Je suis convaincu qu'il y aurait, dans l'appréciation globale de la production scientifique, place pour un type d'esprit indépendant, non exempt de préjugés, mais soucieux en tout premier lieu de rigueur intellectuelle. Si la philosophie n'avait pas divorcé d'avec la science depuis longtemps, on aurait dû trouver chez les philosophesdes sciences, les épistemologues, des individualités capables de tenir ce rôles." ('Vertus et Dangers de l'Interdisciplinarité': 636-643). What of mathematics?

Rival traditions in mathematics

At first glance it appears that rivalry between research traditions is infrequent in mathematics. One piece of evidence for this is that the newsgroup sci.math.research is much less lively than its counterpart sci.physics.research. Yet there are plenty of disgruntled mathematicians out there, fed up with anonymous referees reports, or with the way a field is going, exemplified by certain campaigns mounted by Rota:

'What can you prove with exterior algebra that you cannot prove without it?" Whenever you hear this question raised about some new piece of mathematics, be assured that you are likely to be in the presence of something important. In my time, I have heard it repeated for random variables, Laurent Schwartz' theory of distributions, ideles and Grothendieck's schemes, to mention only a few. A proper retort might be: "You are right. There is nothing in yesterday's mathematics that could not also be proved without it. Exterior algebra is not meant to prove old facts, it is meant to disclose a new world. Disclosing new worlds is as worthwhile a mathematical enterprise as proving old conjectures." '(Rota 1997: 48)

Some like Rota can do something about this. Or like Connes,

"The scientific life of mathematicians...often begins by an act of rebellion with respect to the existing dogmatic description of that reality that one will find in existing books. The young "to be mathematician" realize in their own mind that their perception of the mathematical world captures some features which do not fit with the existing dogma." (A view of mathematics, Connes)

Chapter 8 of my book describes two rival programmes to succeed Kummer's ideal numbers: Dedekind versus Kronecker. I'm sure I didn't do justice to this, largely because at the time I wrote it I was working within the Lakatosian framework of research programmes. The problem is that success on both sides is too easy if you try to mimic Lakatos and look for a neutral standpoint. There's plenty of progress for both sides. To tell the story from inside each tradition, one would need to cover a huge amount of ground. Weyl's chapter 'Our disbelief in ideals' in his book *Algebraic Theory of Numbers* is indicative that the constructivism versus classical mathematics debate was involved, but this is certainly not the whole story. Ideals flourish today, while Kronecker’s programme can be said (and was by Weil) to be realised by Grothendieck. To write this story well would require enormous resources. Identifying a single entity to call a tradition is far from obvious here, the interweaving of the many strands is highly complex. Levels of commitment are more fluid than suggested by an image of simple rivalry. In chap. 8 I divide these levels of commitment into three classes: research traditions, research programmes, and research projects. This raises an is/ought issue. Would it be more conducive to the advancement of mathematical understanding if mathematicians organised themselves more clearly in terms of long-term commitments.

I realised there were problems with a Lakatosian history, and went looking for a more focused current controversy. I chose the debate as to whether the extension of the group concept to groupoids is a good thing (chap 9 of my 2003). What is noticeable here is that after some initial explicit criticism, the opposition doesn't show up. One could say that in this case this dispute was taken over by a larger battle between those who believe category theory has a lot to say about the proper organisation of mathematics and those who do not.

Elsewhere, Penelope Maddy (1997) has given us an account of the debate as to whether to adopt the V = L axiom in set theory. She comes down on the side against V = L by assuming goals which are not likely to be adopted by V = L proponents. Set theory is taken to be foundational, i.e., as providing surrogates for all mathematical entities, requiring a maximally large and unified theory. But there is considerable scope to question the necessity of these goals in such a way that V= L becomes a more viable rival.

Perhaps it would be more interesting to run set theory against category theory or even *n*-category theory. This would throw into question what foundations are. Manin's version of foundations is rather MacIntyrean:

"I will understand 'foundations' neither as the para-philosophical preoccupation with the nature, accessibility, and reliability of mathematical truth, nor as a set of normative prescriptions like those advocated by finitists or formalists. I will use this word in a loose sense as a general term for the historically variable conglomerate of rules and principles used to organize the already existing and always being created anew body of mathematical knowledge of the relevant epoch. At times, it becomes codified in the form of an authoritative mathematical text as exemplified by Euclid's Elements. In another epoch, it is better expressed by the nervous self-questioning about the meaning of infinitesimals or the precise relationship between real numbers and points of the Euclidean line, or else, the nature of algorithms. In all cases, foundations in this wide sense is something which is relevant to a working mathematician, which refers to some basic principles of his/her trade, but which does not constitute the essence of his/her work." (Manin 2002b: 6)

Something similar is indicated by the category theorist William Lawvere, although note how much better integrated are foundations and practice in his version:

“In my own education I was fortunate to have two teachers who used the term "foundations" in a common-sense way (rather than in the speculative way of the Bolzano-Frege-Peano-Russell tradition). This way is exemplified by their work in Foundations of Algebraic Topology, published in 1952 by Eilenberg (with Steenrod), and The Mechanical Foundations of Elasticity and Fluid Mechanics, published in the same year by Truesdell. The orientation of these works seemed to be "concentrate the essence of practice and in turn use the result to guide practice".” (Lawvere 2003: 213)

The burning question at the present time is whether *n*-categories will play this role in 21st century mathematics. Manin believes so. After sets came categories, he tells us, and then *n*-categories:

"The following view of mathematical objects is encoded in this hierarchy: there is no equality of mathematical objects, only equivalences. And since an equivalence is also a mathematical object, there is no equality between them, only the next order equivalence etc., ad infinitum.
This vision, due initially to Grothendieck, extends the boundaries of classical mathematics, especially algebraic geometry, and exactly in those developments where it interacts with modern theoretical physics." (*ibid.*: 8)

If right, it suggests that *n*-categories will be more than just “relevant to a working mathematician”. Note that working mathematicians, such as Peter May, have provided some of the existing dozen definitions of *n*-categories. There’s a strong line of advocacy for *n*-categories one can adopt. Against the belief that mathematicians is the study of structure up to isomorphism, we find categories treated as the same “to all intents and purposes” (is there anything else?) which are not isomorphic, but merely ‘equivalent’ (e.g., Morita equivalence), as they should be if seen as objects of a bicategory. Fluky set theoretic truths for which there can be no story are not genuine mathematics (*n*-categories against Chaitin?). [cf. "The regularities of coincidence are striking features of the universe which we inhabit, but they are not part of the subject matter of science, for there is no necessity in their being so." (MacIntyre 1990b: 183)] Bicategories are being taken seriously in some parts of mathematical physics and in theoretical computer science. Higher dimensional algebra, one name for the study of *n*- and omega-categories, represents “a unification of logic, singularity theory/topology, and physics” (Baez, personal communication). Two forms of 2-vector spaces exist, one relates to the String group and Lie 2-algebras, the other is effective in yielding stable homotopic information at chromatic filtration level 2 by playing the role that vector spaces play in K-theory, but this time for 2-K theory. A version which captures the best of both of these versions will point to a very important monoidal bicategory.

Will this movement be the subject of a well-organised debate? As things stand it’s hard to imagine a concerted "stop at 1 (or 2)-categories" campaign. On the other hand, voices have been heard suggesting that bicategories are far enough. It would be interesting to see then what people will take as evidence for tricategories.

We should also mention Connes and Grothendieck, both good storytellers, and their rival conceptions of space. MacIntyre's prime example of a rational merger of traditions is Aquinas's reconciliation of Aristotelian and Augustinian philosophies. By reconciling the ideas of his two colleagues, Cartier in his 'Mad Day's Work' article might then be thought of as the Aquinas of mathematical theories of space. (Connes recently continues the dialectic by giving the advantage to noncommutative geometry, "... the algebra associated to a topos does not in general allow one to recover the topos itself in the general noncommutative case." (A view of mathematics: 21))

So we can observe some forms of debate in mathematics, but should we still expect MacIntyre's picture to be better fitted to the natural sciences with its many disputes? Is the apparent scarcity of disputes in mathematics how things really are, or are they just more hidden there? If it is how things are, is this because that's what the nature of mathematics requires, or could things be better? Is it perhaps the case that only justificatory narrative accounts of one's own work are required, without the need to demonstrate superiority over other accounts. Aren't even these accounts in short supply?

(a) Don't worry that there's little overt sign of rivalry or justificatory narratives:
(i) The demonstration of superiority is usually quite straightforward so does not need to be advertised.
(ii) Mathematics has an extra dimension, mathematical space is roomy enough that a wait-and-see approach, i.e., get on with your own thing until forced to decide, is the most sensible strategy. Mathematics is connected, if we make a mistake, researchers forging along other paths will correct us.

The really fundamental point in that respect is that while so many mathematicians have been spending their entire life exploring that world they all agree on its contours and on its connexity: whatever the origin of one's itinerary, one day or another if one walks long enough, one is bound to reach a well known town i.e. for instance to meet elliptic functions, modular forms, zeta functions. "All roads lead to Rome" and the mathematical world is "connected".

In other words there is just "one" mathematical world, whose exploration is the task of all mathematicians and they are all in the same boat somehow. (Connes, A View of Mathematics)
So there's no need for head-to-head clashes, except perhaps occasionally at the highest level, e.g., Hilbert-Brouwer.

(b) Do worry that there's little overt sign of rivalry or justificatory narratives:
(i) It goes on surreptitiously, anonymous referees' reports, prize committees, etc. It spontaneously bubbles over from time to time in unhelpful ways.
(ii) There's a flaw in the training of mathematicians. They don't understand what it is to belong to tradition-constituted enquiry. They just are not expected to be expert in mathematical criticism.

Comments:

(a) Even if it is accepted that there should be more justificatory narratives, it might be argued: You don't need to argue at length against the use of groupoids, get on with your own thing. Nobody's going to devote much time to an anti-groupoid cause. Positive accounts have been written, either they will persuade or not, there is no need to rebut them. But it must be possible to take wrong turns. Rota complains of theories being subsumed within more embracing theories and something being lost in the process. There need to be mechanisms to debate wrong turns, rather than just getting angry. Relying on a 'truth will out' policy might seriously delay developments.

(b) Just because these narratives haven't been written doesn't signify that they couldn't or shouldn't. Conditions ought to be right for them to be written and attended to. A tradition in which this were the case would be more likely to thrive, both because these conditions are conducive to good research, and because these narratives would maintain these conditions. The surveys of Klein and Hilbert played an essential part in establishing the dominance of Göttingen mathematicians. We might expect mathematics to be thriving where this sort of activity takes place in the open. Perhaps the Moscow School would reveal similar traits.[10]

It could be argued that a discipline such as mathematics would thrive all the more if practitioners worked within schools of research, and that when the liberal successor to encyclopaedic thinking dominates, one where reasoning begins implicitly "I want to study X", it promotes individualistic kinds of research less likely to engender rapid progress. We should expect (E) type philosophy of mathematics, whose limitations we discussed earlier, to look for reasoning paralleling that of practical reasoning from the Enlightenment onwards. These would include appeals to universal rationality, to utility, to personal preferences, and so on. I study X because:

X is a universal truth expressible in ZFC. (But then why not just turn your automated theorem prover on?)

I want to study X. (Why should you be supported?)

X is or will be useful. This suggests judging mathematics ends as external, in the Aristotelian sense:

"Every art and every inquiry, and similarly every action and pursuit, is thought to aim at some good; and for this reason the good has rightly been declared to be that at which all things aim. But a certain difference is found among ends; some are activities, others are products apart from the activities that produce them. Where there are ends apart from the actions, it is the nature of the products to be better than the activities. Now, as there are

[10] Terence Tao speaks of the importance of "being exposed to other philosophies of research, of exposition, and so forth", and claims that "a subfield of mathematics has a better chance of staying dynamic, fruitful, and exciting if people in the area do make an effort to make good surveys and expository articles that try to reach out to other people in neighboring disciplines and invite them to lend their own insights and expertise to attack the problems in the area." (Clay Mathematics Institute Interview, http://www.claymath.org/interviews/tao.php).

many actions, arts, and sciences, their ends also are many; the end of the medical art is health, that of shipbuilding a vessel, that of strategy victory, that of economics wealth. But where such arts fall under a single capacity—as bridle-making and the other arts concerned with the equipment of horses fall under the art of riding, and this and every military action under strategy, in the same way other arts fall under yet others—in all of these the ends of the master arts are to be preferred to all the subordinate ends; for it is for the sake of the former that the latter are pursued. It makes no difference whether the activities themselves are the ends of the actions, or something else apart from the activities, as in the case of the sciences just mentioned." (Aristotle, Nichomachean Ethics)

By taking mathematical ends as external, one becomes hostage to a bureaucratic mentality seeking to maximise your production. In any case, utilitarianism has never been able to overcome the fact that usefulness cannot be judged neutrally. Finally, isn't there something internally glorious about mathematics?

Returning to MacIntyre's Aristotelian picture, maybe the model of rivalry between two parties is too simple. Insights from a large array of approaches may be germane to a particular problem area, the oversight of any one being a cause for partiality of outlook. Indeed, the merging of viewpoints is more common than the outright victory of one over another, and an historical account will reveal complicated patterns of such mergers. Might there be a middle path between extreme individualism and a bloc-like rigidity, blending a Kuhnian or Lakatosian loyalty to paradigm or programme with a Feyerabendian freedom to choose one's short- and mid-term commitments quite flexibly? Just so long as there is collective responsibility for mathematical decisions. None of this takes away from the thrust of this essay which is to demand that much more by way of justificatory exposition is needed.

Head-to-head rivalry might be more encountered in what one would call arguments over 'foundations', where challenges to entrenched views need to present a unified front:

"This situation, like so often already in the history of our science, simply reveals the almost insurmountable inertia of the mind, burdened by a heavy weight of conditioning, which makes it difficult to take a real look at a foundational question, thus at the context in which we live, breathe, work – accepting it, rather, as immutable data. It is certainly this inertia which explains why it took millennia before such childish ideas as that of zero, of a group, of a topological shape found their place in mathematics. It is this again which explains why the rigid framework of general topology is patiently dragged along by generation after generation of topologists for whom "wildness" is a fatal necessity, rooted in the nature of things." (Grothendieck 1984: 259)

Of course it may turn out correct to resist change, inertia has its place, but only within the rational development of a tradition. At present there is such a diffusion of responsibility for maintaining the position that things should remain the way they are, so that it is all but impossible to challenge the *status quo* effectively. In view of Thom's comment earlier, perhaps there is a role for the philosopher to act as a gadfly.

Recognising mathematicians as participants in a thriving tradition, we can indicate how their practice might be improved. What is clear from conceiving mathematics, just as any systematic form of enquiry, as a tradition is that debates about the proper ordering of the goods of mathematics should be organised, not just allowed to happen in unsatisfying ways, as outbursts and releases of tension. The Jaffe-Quinn debate was the most public of these outbursts in recent years (Jaffe & Quinn 1983). There was no role for a philosopher in this debate, which, I like to think, was the poorer for it.

Much discussion, one presumes, carries on in face-to-face meetings and e-mail conversations. Perhaps the latter will eventually be published, as correspondence (e.g., Serre-Grothendieck) has been in the past. If this is read so eagerly, why not make it available sooner? When we do come across contemporary available discursive texts, they may reveal one side of a discussion (e.g., May's 'Philosophizing'[11]). While this is certainly better than nothing, how much more interesting to read the whole dialogue.

What kind of history?

History of science departments are tending in either genealogical or broader historical directions. There's a certain joining of forces in that both are keen to move from the great white males who make up the heroes of the canon. Both are interested in tying science to broader concerns, e.g., empire building of 19th century Britain. The latter has no overt reason to study what today's scientists now take to be key developments of a period. If you want to write about the role of mathematics in 4th century BC Athens, why look at the tiny number of mathematicians whose work resulted in Euclid's *Elements*? Why not include the arithmetic of the hundreds of accountants who worked in Piraeus, as Sarah Cuomo does in *Ancient Mathematics*? You can do this with no view to saying anything genealogical about today's mathematics.

"The social and historical analysis of science poses no threat to the institution's core assumption about the existence of an accessible "real world" that we have actually managed to understand with increasing efficacy, thus validating the claim that science, in some meaningful sense, "progresses." Rather, scientists should cherish good historical analysis for two primary reasons: (1) Real, gutsy, flawed, socially embedded history of science is so immeasurably more interesting and accurate than the usual cardboard pap about marches to truth fueled by universal and disembodied weapons of reason and observation ("the scientific method") against antiquated dogmas and social constraints. (2) This more sophisticated social and historical analysis can aid both the institution of science and the work of scientists-the institution, by revealing science as an accessible form of human creativity, not as an arcane enterprise hostile to ordinary thought and feeling, and open only to a trained priesthood; the individual, by fracturing the objectivist myth that can only generate indifference to self-examination, and by encouraging study and scrutiny of the social contexts that channel our thinking and the attracted and innate biases (Bacon's idols) that frustrate our potential creativity." (S. Gould, 'Deconstructing

[11] See http://www.math.uchicago.edu/~may/MISC/ToDrinfeldSept27.pdf . May begins "Thought I would take an hour off to just write nonsense about points of view, what is "21st Century mathematics". Please be indulgent." Why this self-deprecation?

the "Science Wars" by Reconstructing an Old Mold')

Many examples of this kind of historical research have chosen nineteenth century evolutionary theory as their subject matter. There one can learn about many institutions (church, universities, British empire, navy, gentlemen, science, the public), without any very difficult theory. Many historians here are not forming generalisations, but using the science as a way to illuminate the specificity of empire-building Britain in the throes of an industrial revolution. This raises the problem of whether much of contemporary mathematics can receive a similar treatment. On the one hand, the theory is extremely hard to understand, on the other, mathematics appears to be somewhat remote from contemporary culture. Can more be done than examine the public conception of mathematics, say, by examining the way Wiles was represented as a solitary genius, beavering away in his attic?

Histories of intellectual enquiry naturally reflect conceptions of such enquiry. Obvious targets for historians are the doxologists, or extreme Whigs, but again we must be careful not to conflate (E) and (T):

"The narrative structure of the encyclopaedia is one dictated by belief in the progress of reason...Narrative of the encyclopadist issues in a denigration of the past and an appeal to principles purportedly timeless." (Macintyre 1990a: 78) "So the encyclopaedists' narrative reduces the past to a mere prologue to the rational present."

"For the genealogist this appeal to timeless rational principles has, as we have seen, the function of concealing the burden of a past which has not in fact been discarded at all." They must write a history of that to be undermined, and a history of how to avoid the pitfalls of the taken-for-granted.

"The Thomists' narrative...treats the past...as that from which we have to learn if we are to identify and move towards our *telos* more adequately and that which we have to put to the question if we are to know which questions we ourselves should next formulate and attempt to answer, both theoretically and practically." (Macintyre 1990a: 79)

What we're after is history written retrospectively without the excesses of Whiggism, i.e., its self-justification without proper self-examination. History should be used to expose one’s partialities:

"Despite strictures about the flaws of Whig history, the principal purpose for which a mathematician pursues the history of his subject is inevitably to acquire a fresh perception of the basic themes, as direct and immediate as possible, freed of the overlay of succeeding elaborations, of the original insights as well as an understanding of the source of the original difficulties. His notion of basic will certainly reflect his own, and therefore contemporary, concerns." (Langlands: 5)

We can confront the past not to seek a confirmation of the present, but to 'falsify' it, or better to challenge the 'naturalness' of contemporary ways of viewing a problem. So a narrative must be truthful. It needs to use the past to explain how partial viewpoints were overcome, or how we have acquired new partialities, and have failed to learn from our predecessors.

"... the history of all successful enquiry is and cannot but be written retrospectively; the history of physics, for example, is the history of what contributed to the making in the end of quantum mechanics, relativistic theory, and modern astrophysics. A tradition of enquiry characteristically bears within itself an always open to revision history of itself in which the past is characterized and recharacterized in terms of developing evaluations of the relationship of the various parts of that past to the achievements of the present." (Macintyre 1990a: 150)

Things are no different in mathematics. Something is important for what it will have been:

...while in the natural sciences the feeling of making contact with reality is an augury of as yet undreamed of future empirical confirmations of an immanent discovery, in mathematics it betokens an indeterminate range of future germinations within mathematics itself. (Polanyi 1958: 189)

So it requires a sense of time which is not apparent in many narratives to describe the decision as to what gets taken as mathematics. Consider Hardy's comment on von Neumann after a talk on operator algebras in the 1930s, “Obviously a very intelligent man, but was that mathematics?”. But in the 80s and 90s you could get a Fields medal for working on them, as Connes and Jones did. A powerful narrative could be written about how von Neumann algebras have flourished in the hands of Connes, and how they may feature in the best path to the Riemann Hypothesis, Hardy's Holy Grail. Of course, this form of narrative can also take failure as its subject matter.

Gratten-Guiness (2004) introduces a distinction between history and heritage, one dealing with the context of an event without invoking ideas from the future, the other studying the impact a discovery has on later times. This extract from Manin's 'Von Zahlen and Figuren' would presumably be counted as heritage:

"One remarkable feature of Gauss' result is the appearance of a hidden symmetry group. In fact, the definition of a regular n-gon and ruler and compass constructions are given in terms of Euclidean plane geometry and make practically "evident" that the relevant symmetry group is that of rigid rotations SO(2) (perhaps, extended by reflections and shifts). This conclusion turns out to be totally misleading: instead, one should rely upon Gal(Q^{-}/Q)." (Manin 2002a: 2)

This is fine to the extent that Manin is pointing out that what's at stake are maps x --> x^k, rather than a reading of inevitable progress towards a contemporary position. The traditionalist's history would be a form of heritage, including our failure to make the most of the past - good heritage rather than the bad “royal road to the present” heritage of an encylopaedist's tale.

We don't yet have very many good *n*-categories narratives. Their story has been told in a mythical way (as a Fall from the paradise of omega-categories) and a historical (non-teleological) way (Street 2004). A sketch of what may be construed as a tradition-constituted way of narrating the role of *n*-categories in physics has also been given (http://math.ucr.edu/home/baez/history.pdf). Perhaps it's too early, but we don't seek a definitive history. We would hope that the narrative might shape in some respects the future direction of the field.

The institutional/moral dimension of enquiry

Before concluding, let us put these considerations into a broader context. Polanyi warned us around fifty years ago in *Personal Knowledge*,

"The transmission of mathematics has today been rendered more precarious than ever by the fact that no single mathematician can fully understand any longer more than a tiny fraction of mathematics. Modern mathematics can be kept alive only by a large number of mathematicians cultivating different parts of the same system of values: a community which can be kept coherent only by the passionate vigilance of universities, journals and meetings, fostering these values and imposing the same respect for them on all mathematicians. Such a far-flung structure is highly vulnerable and, once broken, impossible to restore. Its ruins would bury modern mathematics in an oblivion more complete and lasting than that which enveloped Greek mathematics twenty-two centuries ago." (1958: 192-3)

This warning need not be taken merely to suggest that certain scholarly standards must be maintained, the worry of Jaffe and Quinn. As I have argued, much greater efforts should be made to explain visions of a field, where it has come from and where it is heading. But this is just to point to the need for certain virtues to be practiced:

"A living tradition then is an historically extended, socially embodied argument, and an argument precisely in part about the goods which constitute that tradition. Within a tradition the pursuit of goods extends through generations, sometimes through many generations. Hence the individual's search for his or her good is generally and characteristically conducted within a context defined by those traditions of which the individual's life is a part, and this is true both of those goods which are internal to practices and of the goods of a single life. Once again the narrative phenomenon of embedding is crucial: the history of a practice in our time is generally and characteristically embedded in and made intelligible in terms of the larger and longer history of the tradition through which the practice in its present form was conveyed to us; the history of each of our own lives is generally and characteristically embedded in and made intelligible in terms of the larger and longer histories of a number of traditions. I have to say 'generally and characteristically' rather than 'always', for traditions decay, disintegrate and disappear. What then sustains and strengthens traditions? What weakens and destroys them?

The answer in key part is: the exercise or the lack of exercise of the relevant virtues. The virtues find their point and purpose not only in sustaining those relationships necessary if the varieties of goods internal to practices are to be achieved and not only in sustaining the form of an individual life in which that individual may seek out his or her good as the good of his or her whole life, but also in sustaining those traditions which provide both practices and individual lives with their necessary historical context. Lack of justice, lack of truthfulness, lack of courage, lack of the relevant intellectual virtues - these corrupt traditions, just as they do those institutions and practices which derive their life from the traditions of which they are the contemporary embodiments. To recognize this is of course also to recognize the existence of an additional virtue, one whose importance is perhaps most obvious when it is least present, the virtue of having an adequate sense of the traditions to which one belongs or which confront one. This virtue is not to be confused with any form of conservative antiquarianism; I am not praising those who choose the conventional conservative role of *laudator temporis acti*. It is rather the case that an adequate sense of tradition manifests itself in a grasp of those future possibilities which the past has made available to the present. Living traditions, just because they continue a not-yet-completed narrative, confront a future whose determinate and determinable character, so far as it possesses any, derives from the past." (MacIntyre 1984: 222-3).

Much might be achieved by a merger of MacIntyre's and Polanyi's positions, the latter with his greater emphasis on the tacit and passionate elements of personal knowledge.[12]

Conclusion

Only from the Tradition-constituted perspective can we begin to do justice to mathematics. We can then continue by working on what is very characteristic of mathematics, the kind of understanding required (very possibly to include a role for conceptual blending of image schemas). Already we can support suggestions for change. Once we've accepted the Aristotelian view of justice as receiving what is due to you for your contribution to the vitality of the community, we can see room for improvement:

"I think that our strong communal emphasis on theorem-credits has a negative effect on mathematical progress. If what we are accomplishing is advancing human understanding of mathematics, then we would be much better off recognizing and valuing a far broader range of activity.

…the entire mathematical community would become much more productive if we open our eyes to the real values in what we are doing. Jaffe and Quinn propose a system of recognized roles divided into "speculation" and "proving". Such a division only perpetuates the myth that our progress is measured in units of standard theorems deduced. This is a bit like the fallacy of the person who makes a printout of the first 10,000 primes. What we are producing is human understanding. We have many different ways to understand and many different processes that contribute to our understanding. We will be more satisfied, more productive and happier if we recognize and focus on this."

[12] See John Flett's 'Alasdair MacIntyre's Tradition-Constituted Enquiry in Polanyian Perspective' for a start. Pages 184-193 of Polanyi's *Personal Knowledge* (Routledge 1958) contain some of the best philosophical reflection on mathematics of the twentieth century.

(Thurston 1994: 171-2)

As a mathematician one should aim to be able justly to claim with Thurston: “I do think that my actions have done well in stimulating mathematics.” (Thurston 1994: 177). Surely as a basic minimum it isn’t too much to ask of each established mathematician to place a brief research statement on the Web, see, e.g., Jonathan Brundan’s statement.[13] More impressive are pages such as Mark Hovey's Algebraic Topology Problem List.[14] This may lead on to substantial sites such as Ronnie Brown’s.[15] Someone who can surely claim to have stimulated mathematics is John Baez who has created the mother of all mathematics sites.[16] In his Web publications you will find both exposition and the elaboration of a philosophy or image, meta-paradigmatic work. It is a measure of the irrationality of the way mathematics is practiced that his efforts have not been officially recognized. Much more narrative expository writing should be encouraged. Acts of amanuensis, eliciting narratives from the elders, should be promoted. All authors should be instructed to write in a way that people can learn from, to confess weaknesses, to explain their struggles, to expose students to disagreement. Perhaps $1 million could be better spent funding someone, or some team, to explain the significance of the Hodge conjecture to a wide range of audiences, than merely rewarding the one who puts the final piece into the first proof. Why turn mathematics into a commodity? Why tempt people to be secretive, or to trade partial insights?

The best way to argue for the account of rationality in mathematics outlined here would be to write the kind of history I have been discussing. The more self-consciously tradition-constituted a discipline, the easier it is to write the appropriate kinds of history, a history of the successive improvement of the versions of the life-story of the tradition, without hiding its reversals and instances of resourcelessness. Philosophers might learn from this that the organization of community-embodied intellectual practices are an integral part of their rationality, and that even here in this paradigmatically rational endeavour, even here there may be dissent. But that this is not cause for desperation but for rejoicing as this is a major source of rationality itself.

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[13] http://darkwing.uoregon.edu/~brundan/myres.pdf.

[14] http://claude.math.wesleyan.edu/~mhovey/problems/index.html. Hovey remarks “…even if the problems we work on are internal to algebraic topology, we must strive to express ourselves better. If we expect our papers to be accepted in mathematical journals with a wide audience, such as the Annals, JAMS, or the Inventiones, then we must make sure our introductions are readable by generic good mathematicians. I always think of the French, myself--I want Serre to be able to understand what my paper is about. Another idea is to think of your advisor's advisor, who was probably trained 40 or 50 years ago. Make sure your advisor's advisor can understand your introduction. Another point of view comes from Mike Hopkins, who told me **that we must tell a story in the introduction**. Don't jump right into the middle of it with "Let E be an E-infinity ring spectrum". That does not help our field.” (my emphasis).

[15] http://www.bangor.ac.uk/~mas010/. Especially relevant to this paper is http://www.bangor.ac.uk/~mas010/quality.html.

[16] http://math.ucr.edu/home/baez/.

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