CHAPTER 9

Narrative and the Rationality of Mathematical Practice

DAVID CORFIELD

1. Introduction

How is it to act rationally as a mathematician? For much of the Anglo-American philosophy of mathematics this question is answered in terms of what mathematicians most obviously produce—journal papers. From this perspective, the mathematician's work is taken to be of interest solely insofar as in consists in deducing the consequences of various axioms and definitions. This view of the discipline, with its strong focus on aspects of mathematics that do not feature largely elsewhere—its use of deductive proof, its supposed capacity to be captured by some formal calculus, the abstractness of the objects it studies—isolates the philosophy of mathematics from philosophical accounts of other forms of enquiry.

Against this position, some have refused to class as philosophically insignificant readily observable similarities between mathematics and the natural sciences, such as that each discipline has its own very long history. Mathematics constitutes a continuous intellectual effort stretching back through many centuries, "one of mankind's longest conversations," as Barry Mazur beautifully describes it. And, as with the sciences, this is not just any conversation but a series of vigorous, socially embodied arguments as to how the field should progress. Now, one of the few philosophers to make much of these and other similarities with the natural sciences was the philosopher Imre Lakatos. His understanding of what constitutes rational enquiry led him to call on mathematical practitioners not to hide their conceptual thinking behind the formal barrier of journal articles but rather to expose their work in novel ways, telling the stories of the development of their concepts. Indeed, Lakatos went so far as to call 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.

While there is much to admire in Lakatos's philosophy of mathematics, I believe I have shown it to be wanting in several respects (see Corfield 2003, chaps. 7, 8). What I would like to begin with this essay is an attempt to bring to mathematics what I take to be a superior account of rational enquiry, that of the moral philosopher Alasdair MacIntyre. If the reader is surprised that I turn to a moral philosopher, this reaction may be lessened by noting that both MacIntyre and I advocate realist philosophical positions in domains where many have wanted to rewrite the respective objects out of existence. In ethics, "Murder is wrong" is rephrased by the emotivist as "I don't like murder"; in mathematics, "$2 + 2 = 4$" has been rewritten by the logicist as an analytic truth. But there's more than just realism at stake, as MacIntyre and I both look for an objectivity reflected in the organization of historically situated practices, and here we share a common influence in the philosophy of science of the 1970s. MacIntyre's account of enquiry is an intricate one. In this essay I sketch some of its salient features, and intersperse thoughts on their relevance to mathematics.

2. Three Versions of Enquiry

In his *Three Rival Versions of Moral Enquiry*, Alasdair MacIntyre (1990a) distinguishes among the encyclopedic, genealogical, and tradition-constituted versions. The *encyclopedic* 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, and so forth, 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, or 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 the totality of what was known in an encyclopedia, successive editions of which would reveal an inevitable progress. From the introduction of the ninth 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.

Such an optimistic conception of enquiry has all but disappeared among ethicists, but for MacIntyre, its ghost haunts the field's unresolvable debates, as it does other branches of philosophy.

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." For him, the world is an interplay of forces, ceaselessly organizing and reorganizing itself, giving rise to successive power relationships. The task of those adopting *genealogical* enquiry, then, is to discredit received wisdom by the unmasking of the will to power.

Genealogists and encyclopedists agree that their accounts of reason exhaust the possible options, but there is a third possibility, namely, 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. (Macintyre 1990a, 59)

This MacIntyre calls *tradition-constituted* enquiry. We are less aware of this version of enquiry, he claims, because of a rupture in philosophical theorizing that took place between the time of Aquinas and that of Descartes, the rejection of Aristotelianism, resulting in the formulation of philosophy as the search for clear and evident first principles, the patent lack of which has fed skepticism. For Plato and Aristotle, however, philosophical enquiry was conceived of as a craft, requiring something akin to apprenticeship.

Since this conception of enquiry is much less familiar to us, I shall quote at length MacIntyre's description of 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." (Macintyre 1990a, 64–65)

So we have the movement of a community of enquirers toward a *telos*, where the best understanding of this movement is through a narrative account of the path to the present position. Becoming a member of the community, you identify with this story and seek to find your place in its unfolding. The understanding of this story is passed on by teachers, who instruct new members in becoming experts in the community.

> 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. (Ibid., 65–66)

For the encyclopedist there is no need for such lengthy instruction, for the genealogist what is at stake is indoctrination to maintain power.

Now, leaving aside the question 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 later genealogist, Michel Foucault, distinguished the human sciences from mathematics, cosmology, and physics, which he described as "noble sciences, rigorous sciences, sciences of the necessary" where, unlike in 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.

3. Scientific and Mathematical Enquiry

With MacIntyre's trichotomy in hand, we can now try to classify contributions to the philosophy of science. This might run as follows:

- *Encyclopedic*: The Vienna Circle, logical empiricists, most contributors to contemporary realist/antirealist debates.

- *Genealogical*: Sociologists of scientific knowledge, Latour, and other targets of Sokal.
- *Tradition-constituted*: Collingwood ("The Idea of Nature"), Lakatos, Laudan, Polanyi, Friedman ("Dynamics of Reason"), MacIntyre.

We see in the following quotations MacIntyre's advocacy of a tradition-constituted philosophy of science:

> [N]atural 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. (MacIntyre 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. (Ibid., 467)

This position has been hard to sustain, and is frequently taken to be identical to geneaology by advocates of encyclopedic rationality, and vice versa. MacIntyre explains how the tradition-constituted, or Thomist, position 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. (MacIntyre 1998, 193)

This misunderstanding, not just on the part of opponents but also of philosophers who might have happily adopted such a position, may cast some light on the case of Thomas Kuhn. While I have heard him described disparagingly as a "progressivist," he is often taken by "orthodox" philosophers of science to belong to the genealogist 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 own position. The Kuhn of the 1962 edition of *Structure* has appeared to most readers as a relativist. He observes a lack of ontological 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. This, coupled with the thought that paradigm change is a largely irrational process, leads to the relativist charge. The later Kuhn, followed by Laudan, attempted to evade such a charge by arguing that we see improvements in problem- or puzzle-solving capacity as we pass from one scientific theory to the next. This is not sufficiently robustly realist for MacIntyre, and his comment on the question of nonconvergence is via narrative, to insist that no plausible story could be told of how to move from Aristotle straight to Einstein, whereas one clearly could be written that passes via Newton.

Now let's see whether the trichotomy can be made to work in the philosophy of mathematics.

- *Encyclopedic*: Formalism, logicism, intuitionism. Analytic style responses to Benacerraf, indispensability arguments, structuralism, fictionalism.
- *Genealogical*: Bloor (1994) on $2 + 2 = 4$, MacKenzie (2001) on deduction, Pickering (1995) on quaternions—these are mild forms.

Stronger forms come from mathematicians complaining about what they see as wrong directions, or limited viewpoints, but they only extend the unmasking attitude to others' work, protecting their own rationality. 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. (Arnold n.d., 3)

- *Tradition-constituted*: Lakatos, (Kitcher), Maddy, Krieger, McLarty, Marquis. . . .

Where Lakatos called for an equivalent of literary criticism, genealogists would call for an equivalent of some forms of cultural theory, as in some contributions to Herrnstein Smith and Plotnitsky (1997). Kitcher's name I place in brackets because although *The Nature of Mathematical Knowledge* (Kitcher 1984) is concerned with the rational transmission of practices, the larger framework developed over the second half of the book is in the encyclopedic style. I place Krieger in the tradition-constituted camp since with *Doing Mathematics* (2003), he has done more than anyone to emphasize the craftlike nature of mathematics.

That I take Lakatos as a proponent of tradition-constituted enquiry may surprise some people. While he clearly focuses on historically situated research, he is often perceived to deny that we aim at the timeless. 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 encyclopedist uses of the terms "nominalism" and "realism," which are employed in 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 that in his *Collected Works* is tacked on to the end of another letter, Weil likens the mathematician's work to that of a 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).

Lakatos observes in "History of Science and Its Rational Reconstructions" (1971) that inductivist philosophers of science with their limited perspective on what constitutes scientific rationality leave the door wide open for relativist sociological accounts to explain the remainder. Something similar happens in the philosophy of mathematics. Where they give the impression that they are stout defenders of truth in our relativist times, the limited place analytic descendants of the encyclopedist 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. To the response that the way mathematical results are considered is 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, or 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. Rational considerations 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 knowledge wouldn't have to compete to claim the territory yielded to them but instead could start picking away at the tiny residue to which encyclopedists are left clinging.

This insistence on the exclusive philosophical interest in the "established" is damaging in the extreme because it stops us from talking about the historical and societal aspects of mathematical practice, something we must do if we wish to treat the vital decisions of mathematicians as to how to direct their own and others' research as more than mere preferences. Subtract the society 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. They may reply that these are not the concerns of philosophy, but to say so is to exclude from philosophy much of Plato's own writings on mathematics. In *The Republic* (528b–e), during his discussion of the overall shape contemporary mathematics was 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 entirely dissimilar to the historian Leo Corry's body/image distinction:

> 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. (Corry 2006: 3)

But Corry naturally recognizes both body and image as integral parts of mathematics. 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. Such histories are the natural bedfellows of much contemporary encyclopedist philosophy of mathematics. Little can be learned from them.

Corry continues by rightly pointing 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. (Ibid., 4)

But a tradition-constituted philosophical account of rationality cannot rest content with this observation. It requires of mathematicians that they make great efforts to clarify 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 (2001) account of the necessity of prospective metaparadigmatic work makes a similar point.[2]

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 genealogists and exponents of the tradition-constituted approach, both versed in the history of the subject. But how to characterize what's at stake? As a starting point, we might use the following claims as a demarcation:

Lakatos tells us in *Proofs and Refutations* (1976) that "any mathematician, if he has talent, spark, genius, communicates with, feels the sweep of, and obeys this dialectic of ideas" (146), while for Bloor, "Lakatos's discussion of Euler's theorem . . . shows that people are not governed by their ideas or concepts. . . . [I]t is people who govern ideas not ideas which control people" (1976, 155). However, the editors of *Proofs and Refutations* declare that Lakatos would have modified the passage from which his quotation is taken "for the grip of his Hegelian background grew weaker and weaker as his work progressed" (146n2) and that he came to think human ingenuity is required to resolve problems. The editors, students of his, have come in for much criticism for these footnotes, but they may well be right about Lakatos's change of mind, which is not to say that they are also right about the Hegelian grip. In any case, it is quite proper for an advocate of the tradition-constituted version of enquiry to accept Lakatos's modification. If rational enquiry is likened to a craft, evidently it requires diligence and other virtues for its practice. It is not just a matter of not standing in the way of dialectical progress; one must actively engage in the process.[3]

This is not the proper boundary. No, rather it is the notion of progress toward a telos that distinguishes genealogy and tradition. What candidates, then, do we have for a *telos* of mathematical enquiry?

4. 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 else use "truth" and its cognates in an atypical way. Réné Thom, for example, tells us that "Ce qui limite la vérité, ce n'est pas le faux, c'est l'insignifiant" ("What limits the true is not the false but the insignificant") (1980, 127) while Vaughan (Jones 1998, 204) 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." But then, what is progress toward if not some ultimate logical correctness?

One should expect, and welcome, different views about the aims of mathematics. In one of his Opinions,[4] Doron Zeilberger suggests that the discovery by computer of humanly inachievable results is one such aim, but others disagree. I shall follow them here. Good mathematicians don't just know facts as if they were people succeeding on a quiz show. Rather, as MacIntyre claimed about any craftsmen, they know how things behave, they sense promising directions, and they communicate a vision of how things might be. This is surely why mathematics examination questions go a certain way. State a result, prove it, and then apply it in a novel situation. What is being tested is fledgling understanding,[5] and this accords with the views of William Thurston and other mathematicians on the ultimate aim of their field:

> How do mathematicians advance human understanding of mathematics? (Thurston 1994, 162)
>
> 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. (Mac Lane 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. . . . [I]t 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 and Salomon 1995, 339)

> Broadly speaking, 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, 246)

But can't this understanding all be cashed out in terms of the "hard stuff," those stable, "established" facts? Perhaps it depends on what the understanding is of: entities, results, theories, 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 and demonstrate their properties. For Thurston, however, understanding cannot be cashed out in terms of mathematical propositions. We can know our understanding has improved by the propositions we can now prove, but any conjectured proposition may turn out to be a poor indication of 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 that 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. (Ibid., 168)

This raises interesting questions as to whether mathematicians could employ permanent forms of recording to capture understanding. One would imagine that a much better job could be done using different forms of writing. But even if the written word is not the best medium to convey understanding, now we have the technological resources to make lectures available. This would seem to be a pressing problem if our precious understanding can be lost:

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

Some recording sessions by these practitioners giving lectures, talking to each other, talking with graduate students might have allowed this understanding to survive.

But now, what is mathematical understanding? Let's return to MacIntyre for a Thomistic Aristotelian view:

> 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)

So adequacy of mind and object does not characterize a correspondence relation between judgment and judged, as in much of contemporary epistemology. There is a more subtle relationship at play here. Rather than a right/wrong dichotomy, sometimes augmented by an "approximately correct," applied to one's judgments, here we must consider all aspects of what the agent does, which we might place under the broad umbrella of "understanding." Some part of what is at stake here has been termed *cognitive control* by Jukka Keranen (2005). More broadly it will include a larger sense of a field, its history and its future prospects, the kind of thing that allows you to write a survey article.

But MacIntyre insisted on *perfected* understanding:

> 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 1984a, 148)

Is this notion conceivable, let alone required? Well, first, notice that the claim is not that perfected understanding in any branch of mathematics has been achieved or even that it is likely to be achieved. It is rather a regulative principle to make sense of improved understanding. Imagine that here we are with our rival mathematical understandings at time C. You understand earlier theory B to be an advance over even earlier A, while I don't. You think your meta-understanding, which is really just a part of your understanding, is better than mine. Now, our successors will make their own minds up about comparisons between our understandings, and may well disagree with each other. If so, their accounts of the history of the tradition will be very different in the place they accord to us.

So what do I mean when I say, "My understanding, including all that meta-understanding, is better than yours." Do I really just mean, "From my perspective, my understanding is better than yours"? I seem to be saying, "My account of the history of the tradition leading up to today is better than yours. and future generations will judge that my historical understanding was better than yours." But is this enough? Besides a clause to the effect that the future generations assessing us had better be rational, and that needs explicating, I don't just want my ideas to be thought to be right ten generations ahead, only for this judgement to be overturned twenty generations ahead and ever after. I'd also surely be depressed if I ever came to believe that every ten generations opinion would oscillate between thinking my understanding far superior to yours, followed by a regime that made the reverse judgement. By this I don't mean I care about my understanding as *mine*, but rather as what it is about. I'd much rather it be found that your understanding was superior to mine ever after, that what we argued about found some resolution in the future. If we knew no such issue in our field ever found resolution, would we proceed?

So I'm hoping there's a chain of improvements in understanding with a certain stability to it, where successive members of the chain can make good sense of the earlier stages, realise their partialities, and so on. And I'm also hoping there isn't a whole series of other such chains making very different judgments about issues in my field. But is even this enough? If an angel whispered in my ear that there is something built into the human brain that means that in our field of study, however much it seems as though we're getting at the truth, we will always be led astray, and if I believed that voice, would I continue with my work? In other words, I seem to want any future resolution to be arrived at for good reasons, which may not be accessible to us now but which relate to our descendants' minds becoming, through new theories, equipment, and so forth, more adequate to the objects of the field. I don't just want our descendants for all time to judge my understanding better than yours. I want them to be right about it.

Thinking about the possibility of an oscillation in our views of the past may be difficult for us now. Perhaps we have come to rely too heavily on the idea that our understanding steadily improves, give or take the odd loss, as a partial order, or more, as a cumulative improvement in the ordering of understandings from the past until now. And we expect this ordering to be largely preserved in the future. But of course, this is not necessarily so, and in other fields, such as moral inquiry, plausibly this is a hopelessly wrong story. For MacIntyre, we have largely lost a very subtle moral theory and are merely left with the useless fragments in our hands, which we don't understand how to use. In mathematics, we may again be placed in the situation of those in the early centuries of the last millennium, trying to recover classical learning.

With the telos of perfected understanding, we can say about a particular piece of our reasoning today that its significance lies in the role it plays in forming the final organization. To contribute to this final organization 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 she is 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. It should also be updated as the object of enquiry is better discerned, and if need be, 3-manifolds as a concept can be jettisoned.

When encyclopedic thinking dominates, however, it promotes individualistic kinds of research less likely to engender rapid progress. We should expect the corresponding philosophy of mathematics, whose limitations we discussed earlier, to look for reasoning paralleling that of practical reasoning from the Enlightenment onward. 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 of maximum utility. (This suggests judging mathematics ends as external, in the Aristotelian sense.)

Notice also that the Aristotelian approach requires 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, that is, it claims they are arbitrarily grouped together, having nothing more in common than that they happened to be named "3-manifolds." The realist maintains that our best accounts will always find a place for this kind. Something similarly nominalist about the finite sporadic simple groups has been claimed, that they are better seen as belonging to a different class, some of whose members "happen" to be groups. An early venture into such a theory can be seen in my "Mathematical Kinds, or Being Kind to Mathematics" (Corfield 2005b), where attitudes toward 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. Other tropes include glimpses of mushrooms, archipelagoes, peaks in the mist, and dinosaur bones. With greater knowledge may come greater uncertainty. We should expect, then, that the mathematical parallel to Friedmannian metaparadigmatic work is very necessary at this time.

5. Rival Traditions

An important topic for a theory of inquiry is the resolution of rival claims to truth. For genealogists, disagreements are resolved by (masked) force, the will to power. Encyclopedists' disagreements are resolved by debate on neutral ground, one side is simply shown to be wrong. What, though, of the tradition-constituted version? Well, Lakatos worried that Kuhn was advocating a "mob psychology," and tried to find an improved Popperian account. Against Popper's falsificationism he claimed that theories are already born refuted; for example, Newton would have to be counted as a failed scientist by a Popperian, for not having given up his theories. For Lakatos the remedy was to take a larger entity as the right unit to assess a piece of science. This is his notion of a *research program*, a series of theories, with a unifying heuristic spirit that 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 program it is rational to sign up to. To decide this, one should know how one is 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 program—the neutral standpoint is an encyclopedist's dream. Bodies of theories, MacIntyre (1984b, 42) writes,

> 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 programs, 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. Thus, he allows for a stronger form of incommensurability than does Lakatos, each participant acting according to the different rational standards of their own tradition, without being led to a radical relativism.

For MacIntyre, history and rationality are inextricably linked:

> Consider . . . the continuing argument between Kuhn, Lakatos, Polanyi, and Feyerbend, an argument in which what is at stake includes both our ability to draw a line between authentic

> sciences and degenerative or imitative sciences, such as astrology or phrenology, and our ability to explain why "German physics" and Lysenko biology are not to be included in science. A crucial feature of these arguments is the way in which dispute over the norms which govern scientific practice interlocks with debate over how the history of science is to be written. What identity and continuity are recognized will of course depend on what side is taken in these latter debates but since these debates are so intimately related to the arguments about the norms governing practice, it turns out that the dispute over norms and the dispute over continuity and identity cannot be separated. (MacIntyre 1973, 7)

For Lakatos, rational choice of theory 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 programs to be incommensurable. One rational reconstruction is superior to another if it constitutes more of actual history as rational. But MacIntyre argued against this distortion of the truth:

> 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. (MacIntyre 1977, 470–71)

But how can traditions be brought to improve their histories, to make them more truthful, more adequate to their objects? 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 program. 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 vulnerabilities; usually one hides one's incompletenesses. But if one recognizes that these may be the source of what is dynamic to the program, its "progressive problemshifts," for Lakatos, 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.

So 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.

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 coexistence 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 that should be taken. In some ways the promotion of this form of rationality entails not so much trying to beat the other side but rather 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 they are encountering difficulties or even recognizing a failing they did not realize they had. Ultimately, adjudication takes place through adequacy of the rival histories: "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" (MacIntyre 1988, 403).

What, then, of mathematics? At first glance it appears that rivalry between research traditions is infrequent in mathematics. 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)

It would surely be for the good if we had clearer exposition about such differences of opinion, reflecting a willingness to place oneself in a position to be shown wrong.

Chapter 8 of my book (Corfield 2003) describes two rival programs to succeed Kummer's ideal numbers: Dedekind versus Kronecker. I am sure I did not do justice to this, largely because at the time I wrote it I was working within the Lakatosian framework of research programs. The problem is that success on both sides is too easy if you try to mimic Lakatos and look for a neutral standpoint. Indeed, 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* (1940) is indicative that the constructivism versus classical mathematics debate was involved, but this is certainly not the whole story. Dedekind's ideals flourish today, while Kronecker's program can be said (and was by Weil) to be realized 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 my chapter I divide these levels of commitment into three classes: research traditions, research programs, and research projects.

I realized 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 (Corfield 2003, chap. 9). What is noticeable here is that after some initial explicit criticism, the opposition falls silent. 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 organization 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 showing that its adoption would not lead set theorists to their goals. However, these assumed goals are not likely to be ones adopted by $V = L$ proponents. Set theory is taken by her to be foundational, that is, 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. In other words, there is a degree more incommensurability between programs than Maddy allows.

We can see this clearly if we try to run set theory against category theory or even *higher-dimensional* category theory. Now the nature of the foundations of mathematics is precisely thrown into question. Yuri Manin's version of foundations is rather MacIntyrean (or Collingwoodian):

> 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 notice 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)

One burning question at the present time is whether *n*-categories will play this role in twenty-first-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."

There's a strong line of advocacy for n-categories one can adopt. Part and parcel of the movement is a strong narrative framework. We're ascending a ladder where we'll see constructions of which our current ones are merely projections. We're properly revealing structures that are collapsed versions of the truth, that is, they include elements from different levels. We know we are getting to the heart of the matter when the definitions in terms of which we conceive the objects under consideration categorify effortlessly. There's an idea of the program capturing "lawlike" mathematics. Fluky set theoretic truths for which there can be no story are not genuine mathematics.[6]

We don't yet have very many good n-categories histories. Their story has been told in a mythical way (as a Fall from the paradise of omega-categories) and a historical (nonteleological) 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.[7] 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.

> [A]n 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 1984a, 223).

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? Head-to-head rivalry might be more commonly 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 a danger that a diffusion of responsibility for maintaining the position that things should remain the way they are makes it all but impossible to challenge the status quo effectively.

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 is 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? We can arrange responses to these questions as follows:

1. Don't worry that there's little overt sign of rivalry or justificatory narratives:

    a. The demonstration of superiority is usually quite straightforward, and so does not need to be advertised.

   b. Mathematics has an extra dimension, mathematical space is roomy enough that a wait-and-see approach—that is, get on with your own thing until forced to decide—is the most sensible strategy.
   c. Mathematics is connected; if we make a mistake, researchers forging along other paths will correct us. So there's no need for head-to-head clashes, except perhaps occasionally at the highest level (e.g., Hilbert-Brouwer).

2. Do worry that there's little overt sign of rivalry or justificatory narratives:

   a. It goes on surreptitiously, anonymous referees' reports, prize committees, and so on. It spontaneously bubbles over from time to time in unhelpful ways.
   b. 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.

Further reflection might lead us to say that relying on a "truth will out" policy might seriously delay developments. Just because these narratives have not been written does not signify that they could not or should not be written. Conditions ought to be improved 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.[8]

Returning to MacIntyre's story of rivalry, maybe the model of two delineated parties is too simple, taken as it was from 1970s views of science, especially physics, and seen to fit with ethics. Mathematics might offer a corrective. 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.[9] Indeed, the merging of viewpoints is more common than the outright victory of one over another, and a 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 program 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.

6. Varieties of History

Histories of intellectual inquiry naturally reflect conceptions of such inquiry. Obvious targets for historians are the doxologists, or extreme Whigs, who tell the tale of the glorious passage to the present. Grattan-Guinness (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 $\mathrm{Gal}(\bar{\mathrm{Q}}/\mathrm{Q})$. (Manin 2002a, 2)

For Grattan-Guinness this would be fine as a piece of heritage to the extent that Manin is pointing out that what's at stake are maps $x \to x^k$, rather than a reading of inevitable progress toward a contemporary position. But perhaps this distinction is made too quickly. To each of his three versions of enquiry MacIntyre associates a narrative form:

> 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. . . . 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.
>
> 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, 78–79)

Might we say then that tradition-constituting history would be a form of heritage, including the treatment of our failure to make the most of the past—good heritage rather than the bad "royal road to the present" heritage of an encyclopedist's tale? But what then is it to write a Grattan-Guinness history? Can there be such a timeless study of a period in the past? Very often historians' histories are inflected with the notion that things could have gone so differently, that the present state of affairs is highly contingent. But then these histories also bear upon the present.

What we're after is history written with an allowance for some retrospection without the excesses of Whiggism, its self-justification without proper self-examination:

> 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)

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 n.d., 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.

7. Conclusion

Only from the tradition-constituted perspective can we begin to do justice to mathematics philosophically. We can then continue by working on what is characteristic to mathematics, the kind of understanding it aims for. Benefits can accrue for both mathematics and philosophy. 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. In this sense, Thurston is strikingly Aristotelian:

> 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. . . .
>
> [T]he 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 [Jaffe and Quinn 1993] 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. (Thurston 1994, 171–72)

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, such as Jonathan Brundan's statement.[10] More impressive are pages such as Mark Hovey's Algebraic Topology Problem List.[11] This may lead on to substantial sites such as Ronnie Brown's[12] or Barry Mazur's.[13] Someone who can surely claim to have stimulated mathematics is John Baez, who has written an extraordinary amount about mathematics and mathematical physics.[14] In his Web publications you will find both exposition and the elaboration of a philosophy or image, metaparadigmatic work. Although matters have improved even over the course of the few years since I began this essay, with the flourishing of blogs and other Internet resources, 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.

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 is an integral part of their rationality, and that even here, in the paradigmatically rational endeavor that is mathematics, there may be profound disagreement as to the future direction of the field. This is not a cause for desperation but rather for rejoicing. Mathematics would be anemic and lifeless without it.

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NOTES

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."

2. For a criticism, however, of a distinction Friedman sees in the structures of mathematics and mathematical physics, see my "Reflections on Michael Friedman's Dynamics of Reason" (Corfield 2005a).

3. See Langlands (n.d.): "[B]ut it is well to remind ourselves that the representation theory of noncompact Lie groups revealed its force and its true lines only after an enormous effort, over two decades and by one of the very best mathematical minds of our time, to establish rigorously and in general the elements of what appeared to be a somewhat peripheral subject. It is not that mathematicians, like cobblers, should stick to their lasts; but that humble spot may nevertheless be where the challenges and the rewards lie."

4. See http://www.math.rutgers.edu/~zeilberg/Opinion51.html.

5. (Pólya 1954, 144–145):

> A problem is not yet your problem just because you are supposed to solve it in an examination. If you wish that somebody would come and tell you the answer, I suspect that you did not yet set that problem to yourself. . . . You need not tell me that you have set that problem to yourself, you need not tell it to yourself; your whole behavior will show that you did. Your mind becomes selective; it becomes more accessible to anything that appears to be connected with the problem, and less accessible to anything that seems unconnected. . . . You keenly feel the pace of your progress; you are elated when it is rapid, you are depressed when it is slow.

6. This resembles a similar claim about the sciences: "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 1998, 183).

7. See http://math.ucr.edu/home/baez/history.pdf.

8. 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" (Terence Tao, Clay Mathematics Institute Interview, 2003). http://www.claymath.org/interviews/tao.php).

9. "One can and must approach operadic constructions from various directions and with various stocks of analogies" (Borisov and Manin 2006, 4).

10. See http://darkwing.uoregon.edu/∼brundan/myres.pdf.

11. For Mark Hovey's Problem List, see http://claude.math.wesleyan.edu/∼mhovey/problems/index.html. Hovey remarks, "[E]ven 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 forty or fifty 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)

12. Ronnie Brown's work is available online at http://www.bangor.ac.uk/∼mas010/. Especially relevant to this paper is http://www.bangor.ac.uk/∼mas010/quality.html.

13. Barry Mazur's website is http://www.math.harvard.edu/∼mazur/.

14. John Baez's website is http://math.ucr.edu/home/baez/.

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