Rationality and Mathematical Narrative

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> We dream in narrative, day-dream in narrative, remember, anticipate, hope, despair, believe, doubt, plan, revise, criticize, construct, gossip, learn, hate and love by narrative. (Barbara Hardy 'Towards a Poetics of Fiction: An Approach Through Narrative', Novel 2, 5-14, 1968: 5)

Introduction

It is hardly controversial to emphasise the importance of the role of narrative in our lives. Phenomenologists, such as David Carr, have taken this point most seriously. Through the length of his *Time, Narrative, and History*, Carr argues that we don't just live an unmediated experience to which we later add a narrative gloss, rather our experience is already structured as narrativised. Narrative also provides us with the means to understands each other's actions, as Alasdair MacIntyre remarks:

> We identify a particular action only by invoking two kinds of context, implicitly if not explicitly. We place the agent's intentions, I have suggested, in causal and temporal order with reference to their role in his or her history; and we also place them with reference to their role in the history of the setting or settings to which they belong. In doing this, in determining what causal efficacy the agent's intentions had in one or more directions, and how his short-term intentions succeeded or failed to be constitutive of long-term intentions, we ourselves write a further part of these histories. Narrative history of a certain kind turns out to be the basic and essential genre for the characterization of human actions. After Virtue:208

Now, one way we might look to support the thesis of the centrality of narrative is to search for it in less expected places. So, yes, we know there is a field called the history of mathematics, but do we find that stories lie at the core of mathematicians' working lives? I believe we do, and this is never more clearly revealed than in their broad surveys. For instance, in his 'Mathematics in the Twentieth Century', Michael Atiyah uses the word 'story' or 'stories' seventeen times in as many pages.

Clearly there is a huge amount to say about this point, and a huge amount has been said in the essays making up the forthcoming volume (Apostolos and Mazur (eds.)). But I want to focus in this essay on a further claim. Not only is narrative a vital part of any intellectual discipline, including mathematics, but picking up on MacIntyre's thought, it is also part and parcel of the ways

in which research activity within a field is recognised and critically assessed. In other words, it is an essential component of the rationality of the enquiry.

Now, this is not a claim that originates with me. During the 1970s, philosophers of science debated this question very intensely. Since that time there has been a cooling off in many ways. But I think we can profit by revisiting this question through the eyes of MacIntyre, a philosopher not generally associated with science or mathematics. In a separate article (Corfield, forthcoming) I have tried to show the value of MacIntyre's notion of a *tradition of enquiry* for the philosophy of mathematics. Although MacIntyre is a moral philosopher, famous for calling for a return to Aristotle and Aquinas, his relevance is not so surprising. He tells us himself that a "major turning-point" in his thinking in the 1970s,

> ...was elicited by my reading of and encounters with Imre Lakatos and Thomas Kuhn and what was transformed by that reading was my conception of what it was to make progress in philosophy or indeed in systematic thought more generally. (MacIntyre 2006a, vii-viii)

What MacIntyre had learned was that the engagements of the scientist, mathematician and moral philosopher in their respective intellectual pursuits have much in common with each other, and that they in turn strongly resemble the engagement of an individual within a moral community, seeking to articulate its shared goods. Narrative is key, the narrative of one's life within a community of enquirers, whether intellectual or moral. And so, in the case of the sciences, we read that

> [i]t 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 lifestory, 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. (MacIntyre 1977, 467)

In this essay I shall consider the kind of history MacIntyre requires of mathematicians as practitioners of an intellectual discipline, and pit this against the views of two contemporary historians of mathematics who have wanted to mark the distance between the way they believe history should be written and the way mathematicians tell each other about their discipline, to the extent, in one case at least, of wanting to replace the word 'history' in the case of the practitioners' narratives. Finally, I shall return, with the help of David Carr, to suggest we avoid this two-state solution, and learn to see these endeavours as continuous.

The Role of Narrative in Criticism

The invention of papyrus and the printing press has had a huge influence on mathematics. What about the advent of a fast internet? With the ArXiv, every day we can tune into dozens of papers produced around the world. Turning your web browser there every morning most closely resembles tuning into a daily soap opera. It is not so easy to discern the story lines from this tapestry of articles, but if you do succeed, you find that individual stories range through: daring quests for single gems using whatever resources can be found; Hausmannian reconstructions of higgledy-piggledy slums; beautiful irrigation projects using water from distant streams; attempted take-overs, and resistance to subsumption within a broader theory. Often these story lines are not so apparent, but from time to time we find explicit acknowledgements:

> Ladies and gentlemen, Today I am going to tell you the story of how a chapter of mathematics has been closed and a new chapter is beginning.
>
> Shing-Tung Yau, 'Structure of Three-Manifolds - Poincaré and geometrization conjectures', DG/0607821

If, as philosophers of mathematics, we want to understand mathematics at its highest level of organisation, something I believe we must do, then we should know that mathematics is governed by powerful story lines. The condition of modern mathematics is not one of presuppositionless set theoretic universalism, but rather is scored across by hundreds of stories, which in printed form are easiest to locate in book introductions, book reviews, and other pieces of exposition. My complaint, taking up Lakatos, has been that there has been insufficient of this kind of exposition, although things have improved enormously in recent years, especially with the rise of mathematical blogging.

I have mentioned Atiyah's reliance on the noun 'story' in his survey, but it is clear from his article that it is inseparable from a form of judgement. He is telling us there what he considers to have been the most important developments through the past hundred years, and what he considers to be promising for the future. Recently Terence Tao has written a piece 'What is good mathematics?', which after making a list of what 'good mathematics' can mean, continues by telling us the story of what he assesses to be some good mathematics. The list, consisting of twenty items, received this harsh assessment from Alain Connes:

> It is hard to comment on Tao's paper, the second part on the specific case of Szemeredi's theorem is nice and entertaining, but the first part has this painful flavor of an artist trying to define beauty by giving a list of criteria. This type of judgement is so subjective that I really had the impression of learning nothing except the pretty obvious

fact about arrogance and hubris...

The story, the 'second part', escapes censure.

I think Connes is right to point to what is troublesome about lists of criteria. Criteria seem clumsy. Also in mathematics at least it seems rather easy to do well. In chapter 8 of my book, the Methodology of Mathematical Research Programs, it was too easy to give an account of a program scoring well. What are we left with in Tao's piece then is a good story. Would this be such a bad thing to conclude: Good mathematics is that which can be described by good mathematical stories. But what is a good mathematical story? Can we capture this except by listing qualitities we would expect to find in it? Fortunately assessment is made easier by a phenomenon Tao notes that when good things happen in a piece of research other good things follow in its wake.

> It may seem from the above discussion that the problem of evaluating mathematical quality, while important, is a hopelessly complicated one, especially since many good mathematical achievements may score highly on some of the qualities listed above but not on others. However, there is the remarkable phenomenon that good mathematics in one of the above senses tends to beget more good mathematics in many of the other senses as well, leading to the tentative conjecture that perhaps there is, after all, a universal notion of good quality mathematics, and all the specific metrics listed above represent different routes to uncover new mathematics, or difference stages or aspects of the evolution of a mathematical story.
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> …the very best examples of good mathematics do not merely fulfil one or more of the criteria of mathematical quality listed at the beginning of the article, but are more importantly part of a greater mathematical story, which then unfurls to generate many further pieces of good mathematics of many different types. Indeed, one can view the history of entire fields of mathematics as being primarily generated by a handful of these great stories, their evolution through time, and their interaction with each other. I would thus conclude that good mathematics is not merely measured by one or more of the "local" qualities listed previously (though these are certainly important, and worth pursuing and debating), but also depends on the more "global" question of how it fits with other pieces of good mathematics, either by building upon earlier achievements or encouraging the development of future breakthroughs.

This global/local distinction is very important. Not enough has been said about the global.

The primary problem with the list of criteria is that we should expect what it is to be a good

story to change. We need a meta-level story of how we have moved on from old stories. These days we expect surprise, such as when Vaughan Jones working on von Neumann algebras very unexpectedly realises he has his hands on a new knot invariant. It would be a very worthwhile task to examine the way overviews have changed in style over the years. For example, how does Klein's Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert compare with modern surveys?

MacIntyre on the Place of History in Rational Enquiry

So we have mathematicians with their value-laden stories, but what relation do these bear to history? If, as MacIntyre argues, a central component of rationality requires of an intellectual tradition that it know the history of its successes and failures, what kind of a history does this involve? Already in the 1970s philosophers of science had proposed several ways of writing history, each connected to their conception of rationality. Back at that time MacIntyre had written in *The Essential Contestability of Some Social Concepts*, Ethics 84(1) 1-9, 1973

> 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. (p. 7)

Now, changes in the history of science have made the discipline barely recognisable from the perspective of the 1970s. The question then arises as to whether, were such histories to be written in the way MacIntyre requires, they would fall foul of the ideals of contemporary historians of science, and in particular of the criticisms that historians of mathematics have levelled at mathematicians' histories that they are largely 'Royal-road-to-me' accounts.

The talk which was the seed for this paper I called 'Why and How to Write a History of Higher-Dimensional Algebra'. Observant listeners could have noticed an echo of an address delivered by André Weil, 'History of Mathematics: Why and How?', on the occasion of the International Congress of Mathematicians in 1978. In a sentence, Weil's point was that the history of

mathematics is best left in the hands of mathematicians, as only they will be able to understand properly what was achieved in any epoch by a fellow mathematician. Naturally, historians have demurred. In particular, Leo Corry (forthcoming) has argued that mathematicians' historiographies are outmoded.

Indeed, the history of science has come a long way since the days when triumphalist accounts of steady progress overcoming the limitations of the past were the norm. These were largely about individual white male heroes, and full of anachronisms and inaccuracies. The message was that triumphs ought to be quickly recognised as such by any competent person. There was little said about the social setting of science except as that which could get in the way of the rational individual. Truth was represented as the cause of our discoveries. It was a history which went hand-in-hand with a nineteenth century outlook. Corry quotes Elkana:

> The conviction emerged and grew, leading up to its positivistic absoluteness in the Victorian frame of mind, that not only there is one reality with it immutable laws, but also that we humans are on a sure course to find out all, or at least cumulatively more and more about the reality: one nature, one truth about nature. Science, the chief glory of Western culture since the scientific revolution, is an inevitable unfolding of knowledge; what we know we had to know - if not here, then there, if not now, then at another time, if not discovered by one man, then by another. (Elkana 1982, 205-206)

Now, this characterisation of a certain mind-set is very similar to what MacIntyre calls the 'Encyclopaedist' version of enquiry. Indeed, the Encyclopaedists he had in mind in *Three Versions of Moral Enquiry* (MacIntyre 1990) were the Victorian Scots who put together the *Encyclopaedia Britannica.*

Shapin and Shaffer's *Leviathan and Air Pump* (1985) marked a defining moment in the movement away from Encyclopaedist history. In this book they boldly took on the greatest Encyclopaedist claim, that the Scientific Revolution of the late seventeenth and eighteenth century was part and parcel of Western Europe freeing itself from dogma by bootstrapping itself into scientific rationality. According to them, at that time two models of science were on offer - Boyle's experimental version and Hobbes' deductive one. It was the political climate which decided in favour of Boyle.

In this type of history, it is not the ways things are which brings about victory. Rather the victorious constitute the way things are. History of science in this mode shifts to look at the excluded - laboratory technicians, editors, women scientists, the exterior social milieu. The first port of call in understanding any decision making is the interests of the parties. Indeed, what has swept

into history of science over the past 40 years is the Nietzchean idea that one should look primarily to power relations. This can be done in subtle or blatant ways, but an indication is the asking of questions such as: Who gains from this decision? What are they trying to achieve by presenting this as ‘natural’? How and why are they defining the boundaries of their discipline? Typical ways of reacting to such historical decisions are to unmask them, to show what was really at stake. In its crudest form this might reveal some political advantage of the ruling class. This form of enquiry MacIntyre terms *genealogical*.

An example of a history of mathematics I would classify as genealogical is Pickering (1997), which tells the story of Hamilton's work on quaternions. Genealogical histories of a practice aim to bring contingency to centre stage - things could have been so very different. What is very noticeable in such histories is that often the very early days of a practice are treated. This gives the genealogist the advantage of needing to study only a handful of people with all their idiosyncrasies. The underlying thought is that if so much could have been so different while the course of a practice was being set, how different things could be decades later. And, if we can find a sharp change of direction away from the original pioneer's intentions late on our story, so much the better. Most of the original thinking guiding the practice will be revealed to be just a story. Any number of stories might have governed at that time, leading mathematics in very different directions.

So, in Pickering's paper, with the pace of research so slow, we can dwell on Hamilton's peculiar metaphysical views, and we can tell the story of the quaternions as having "mutated over time into the vector analysis central to modern physics." (p. 45). Hamilton had failed to reach his original goals, only achieving "a local association of calculation with geometry rather than a global one. He had constructed a one-to-one correspondence between a particular algebraic system and a particular geometric system, not an all purpose link between algebra and geometry, considered as abstract, all-encompassing entities." (p. 59). The quaternions could not form the required calculus for reasoning about entities in three-dimensional space. Even after Hamilton had considered multiplication on just the imaginary part, where the product of two lines could be an ordinary number or another imaginary, "...the association of algebra with geometry remained local. No contemporary physical theories, for example, spoke of entities in three-dimensional space obeying Hamilton's rules. (p. 60). "It was only in the 1880s, after Hamilton's death, that Josiah Willard Gibbs and Oliver Heaviside laid out the fundamentals of vector analysis, dismembering the quaternion system into more useful parts in the process. This key moment in the delocalization of quaternions was also the moment of their disintegration." (p. 60).

From this an innocent reader might take it that, by and large, that was that as far as the quaternions were concerned. But what do we make, then, of a paper (Gsponer & Hurni, 2005) which documents the use of the quaternions and allied algebras in physics up to recent times in the

form of an analytic bibliography of 1300 references? With so many man-hours devoted to the extraction of whatever can be found to be useful about quaternions, and their relationships with other mathematical entities, do the first few decades of their lives tell us very much? Although it makes for engaging history, do we learn so much about the ways in which mathematics operates at its highest level of organisation from the quixotic quests of individuals, rather than from an account of droves of workers, most of whom must necessarily remain largely faceless?

So we now have two versions of enquiry, Genealogist and Encyclopaedist, and they 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 1990: 59)

I myself came to MacIntyre as someone seeking to tread a line between the irrationalism of Kuhn, unsuccessfully distancing himself from the Genealogical side, and the rewriting of history of Lakatos, 'rational reconstruction', in his quest for timeless programme-independent criteria for progress, i.e., someone erring on the Encyclopaedist side. MacIntyre offers a third way, which he calls tradition-constituted enquiry.

> ...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 1990: 65)

Histories of intellectual enquiry naturally reflect conceptions of such enquiry. We must be careful then not to conflate the Encyclopaedist and the Tradition-constituted, or Thomist as MacIntyre sometimes calls it:

> The narrative structure of the encyclopaedia is one dictated by belief in the progress of

> reason...Narrative of the encyclopaedist 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. (Macintyre 1990: 78)

> 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 1990: 79)

Practitioners' histories must be truthful then. There is no place for erasing what should disturb them in the historical record. Such narratives may portray decisions made in the past as incorrect, and could suggest why incorrect decisions were taken. They could also include an account of current weaknesses of a program. An account of the programme written a decade later could change its mind about what it had seen as the correctness of a decision. Historical findings could change practitioners' current conceptions.

But still there is a feature of these histories which rankles many contemporary historians, namely, an unquestioned belief that they are engaged on some long-term project with reasonably clear disciplinary boundaries, and that the majority of decisions of the practice through its history were taken for what will always be seen as good reasons. In other words, they object to the idea that practitioners are working towards a *telos.* The question is whether true narratives can be written which accept that a discipline such as mathematics is the movement towards its *telos*, a more adequate understanding of its subject matter. But, to turn the question around, we might also wonder whether it is possible to capture mathematics faithfully without accepting the existence of such a *telos.* Let us consider this latter question in the context of a large-scale research programme.

Higher-Dimensional Algebra

If we follow MacIntyre's conception of rationality, any research programme must articulate a narrative of its path to where it stands today – a history of the problems it has encountered, its resolution of these obstacles, how it has refined its goals, how it views its rivals. Something I note in chapter 8 of my book (Corfield 2003) is that many of the case studies carried out by those seeking to put Lakatos's Methodology of Scientific Research Programmes to the test in mathematics have chosen short-lived pieces of mathematics constructed by individual mathematicians or a small number of compatriots or colleagues within an institution, when far more representative, at least in the past 150 years, are highly interconnected efforts, transcending decade, institution, and country, and weaving across different branches – algebraic topology, analytic number theory, differential geometry, and so on.

Take a movement like higher-dimensional algebra, or *n*-category theory, which I have treated at some length in Corfield (2005). Inherent to the self-understanding of higher-dimensional algebra is a certain story of the mathematics of the past hundred or so years. Its central message is to replace equation by isomorphism, isomorphism by equivalence, and so on. It has an inbuilt drive to understand the past constructions as taking place at lower steps of a ladder and to suggest how these constructions might be developed, or *categorified*. For example, the introduction in algebraic topology of homology groups instead of Betti numbers in the 1930s is seen as a paradigmatic example of this process. Less retrospectively, new constructions are being sought and found by projecting old constructions up the ladder. A related narrative line tell us of constructions we already have, which can better be understood if we see them properly stretched over two or three rungs.

The originator of the term 'categorification' was Louis Crane, who back in 1992 was looking to form 4-dimensional topological quantum field theories by categorifying constructions used in 3 dimensional theories. It has since found explicit applications across many fields of mathematics, and allows for much retrospective application. Mathematicians are aiming to extend this process infinitely far to ω-categories, by defining them at one fell swoop. The idea for doing so was inspired by Alexandre Grothendieck in a famous 600 page letter, *Pursuing Stacks*, written in 1983. Grothendieck realised that a special kind of *n*-category, known as an *n-groupoid*, would allow a way of treating spaces up to homotopy in algebraic terms. Already at the 2-category level there are many choices of shape to paste together. There are thus many ways of defining an ω-category. At present, twelve definitions have been proposed. It is felt, however, that the choice is in a sense immaterial, in that all ways will turn out to be the 'same' at the level of omega-categories, although each may be best suited to different applications. There is no unanimity about the extent of what we should expect from categorification. While some have looked to lift a handful of constructions, one or two steps up the ladder, Baez and Dolan's paper *Categorification* (1998) lays out the scope of their sense of the term:

> It is clear, therefore, that the set-based mathematics we know and love is just the tip of an immense iceberg of n-categorical, and ultimately ω-categorical, mathematics. The prospect of exploring this huge body of new mathematics is both exhilariting and daunting. (p. 46)

Now, how to write a history of higher-dimensional algebra? If we try to give a narrative history of *n*-categories as pure mathematics, inevitably we will include close to the beginning some account of Eilenberg and Mac Lane's introduction of ordinary categories into mathematics around 60 years ago. But then much inspiration for their work comes from earlier German algebra of the

1930s. And where to stop, why not back to Klein and the Erlanger Program, 130 years ago? Already practitioners have provided us with two histories. John Baez and Aaron Lauda (2007) have a sketch of the history of *n*-categorical *physics*, described as a 'highly subjective chronology', going back to Maxwell in 1876, which at this early stage might be described as a list of achievements. Ross Street (2004) has written a review of Australian mathematicians' contributions from the 1960s, but the restriction to one nation is a device to limit the scope of the article to something manageable and to its author's scope of memory.

How does one carve out a contiguous piece of mathematical activity and call it a single entity? Even if we restricted ourselves to the self-conscious assumption that a world beyond ordinary categories is there to be explored, the activity which has resulted from this assumption runs across national and institutional boundaries, and cannot be treated in the loving detail one may reserve for an individual. A history of something like higher-dimensional algebra can't make an individual or an institution or the national community of some mathematicians the central star of the show. What binds the historical events together is an ever-changing continuity, in which goals become more explicit and modified. But to refuse to take mathematics at this level of organisation, is it not to miss something of the truth of mathematical life?

History as Retrospection and Whiggism

Let us continue to develop the MacIntyrean theme of belonging to a tradition of enquiry. The central practice with which MacIntyre has been concerned is the life of a moral-political community. But for any community to operate rationally, it must do so in terms of a common good, internal to the practice of that community, which in turn must engage itself in a quest to better understand this good which constitutes its end. What we are considering here is whether we can see the mathematical community in similar terms. Now, what is it to perform well in a community?

> Since what discriminates one kind of character from another is how goods are rank ordered by the agent, and since each rank ordering of goods embodies some conception of what the good life for human beings is, we will be unable to justify our choices until and unless we can justify some conception of the human good. And to do this we will have to resort to theory as the justification of practice.
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> Rationality however does not necessarily, nor even generally, require that we move to this point. I may on many types of occasion judge rightly and rationally that it is here and now desirable and choiceworthy that I do so and so, without having to enquire whether this type of action is genuinely desirable and choiceworthy for someone such

> as myself. I may on many types of occasion judge rightly and rationally that this type of action is desirable and choiceworthy for someone such as myself, without having to enquire whether the type of character that it exemplifies is genuinely good character. And I may judge rightly and rationally on many types of occasion that this type of character is indeed better than that, without having to enquire about the nature of the human good. Yet insofar as my judgment and action are right and rational they will be such as would have been endorsed by someone who had followed out this chain of enquiry to the end (in two senses of "end"). It is always as if the rational agent's judgment and action were the conclusion of a chain of reasoning whose first premise was "Since the good and the best is such and such…" But it is only in retrospect that our actions can be understood in this way. Deduction can never take the place of the exercise of phronesis. (MacIntyre 2006b: 36-37)

Elsewhere (Corfield forthcoming) I have noted similarities between moral thinking and mathematical thinking. As I have indicated this is unsurprising from the Thomistic Aristotelianism of MacIntyre. In that paper I sketched out some mathematical reasoning modelled on Aristotelian practical reasoning:

> 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. (Corfield forthcoming, 13)

This is highly schematic. For a fuller account we would want to hear what makes 3-manifolds so important, and what it means for the Geometrization Conjecture to point us in the right direction. Now, if we continue the analogy, we can conclude that a mathematician may 'judge rightly and rationally' without having a full understanding of what he or she is doing. And doesn't this accord well with our views of the great mathematicians? We may know more now, and be able to recast what our predecessors just began to glimpse, but still feel they were tuning in to the way things have turned out to be.

How does the historian tell us what, say, Poincaré was thinking in 1890 using the public language available at the time? Even if we had access to his private language, isn't a part of the truth of what he was thinking only expressible in a language unavailable to him, that is, in

retrospect when understanding has improved? Hence, intellectual history must have something of the future perfect to it.

Poincaré was struggling to develop a new language, we cannot truthfully represent his thinking if we restrict ourselves to the language of his day. But our account of how he was thinking will also be distorted if we phrase it in modern language. Historians of science are very sensitive to the latter sort of anachronism - acting as if Poincaré lived, not in his present, but in ours. But could we not say that it is also anachronistic to treat Poincaré as if he lived solely in his present? As one who brought our mathematics into being, can one revive his thinking without some kind of reliance on what that thinking became? At the same time one must beware the pitfall of incorrectly forcing older ways of thinking into a modern conceptual apparatus for fear of shielding off reasoning which could act to challenge our current conceptions.

> "... 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)

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 2000: 5)

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

When we come to read such accounts there should be no 'suspension of disbelief', as Corry (forthcoming) suggests we do in fiction about mathematics, and as we ought in most mathematicians' histories.

The "thing that has been", which is the singular, the idiosyncratic, is the object of

> historical research, and the historian should strive to understand and convey it in her research. The "thing that might be", while of "more philosophical and of graver import", is none of the historian's professional business.

Grattan-Guinness goes so far as to want to give what mathematicians write another name: 'heritage' for the mathematicians' stories, 'history' reserved for what the historians produce. Each can stick to their own genre without the need to criticise. But is this two-state solution forced upon us? Yes, if historians choose to be genealogical, but then this is a clash of philosophies not a case of peaceful coexistence. As soon as their aim is to convey that always things could have gone just as 'well' but differently, they are providing evidence for a philosophical position. If a historian is rejecting the idea that mathematical decisions may be good, and further may be made for good reasons, then this is to adopt a philosophical stance, and one which clashes with the philosophy underlying the mathematicians' accounts. If they believe in a *telos*, the mathematician must believe that some decisions were good, which doesn't mean they had to be taken, but that it was good that they were taken. And they can't all have been down to good luck.

If the historian has no problem with the notion that a decision can be made for good reasons, then their and the mathematicians' narrative activities do not clash. But why propose that they are divorced from each other? Each can complement the other, perhaps opting preferentially for different scales of mathematical activity, or different vintages, the mathematician more likely to write about something relevant to his or her own research. What I want to insist on is that there is a danger that something of the truth of mathematics will be lost if no large-scale dramatic narratives are written.

Carr's reconciliation of historians' and practitioners' histories

Let us now turn from MacIntyre to David Carr, a phenomenologist, to help us with this potentially collaborative relationship. In his *Time, Narrative, and History*, Carr argues that we don't just live an unmediated experience which we give a narrative gloss to, rather we already experience it as narrativised.

He compares the historical narratives of the historian and the practitioner, and finds the former largely cognitive, detached and disinterested in the outcome of their studies, where the latter is in the thick of things, and interested in the outcome.

> ...the narrative structure and narrational activity within communal existence is, as we have insisted, primarily practical in character; historical narrative, by contrast is cognitive and seeks an objective representation. The former is engaged in action and has an interest in its outcome; the latter is detached and disinterested, and aims only at truth. The second difference concerns the temporal standpoints of the narrators in each case.

> Our "practical" narrator is situated in medias res, whereas the historical narrator looks back at actions and events already completed. That gives the latter the well-known (and already discussed) advantage of hindsight over his subjects: he knows how things turned out, knows the difference between the intended consequences and the real consequences of their action, etc.
>
> These differences between narrative agent or participant and narrative historian are operative and important: there is no denying the importance of temporal standpoint and of the difference in attitude (engaged or detached) in relation to a lived or performed sequence of human events. At the same time we should like to emphasize several respects in which these differences are mitigated. And we shall do this not by denying objectivity and hindsight to historical inquiry, but by attributing them to narrative-historical existence.

But this is not to give licence to the latter to falsify the past:

> We have already pointed out ..., with respect to individual action and experience, that the narrativization that goes on there cannot be indifferent to truth where the past is concerned. Indeed, where the issue is not merely the shaping of an open future but the coherence of future, present, and past, it is important to be clear on what really happened; the past may be variously interpreted but it cannot be wished away or forcibly altered by an inventive narrative imagination. So much of one's present capacities are in continuity with, and sometimes result from, past choices and experiences that getting straight one's past can be seem as a desideratum and even a necessary condition for a coherent life. This is, of course, one of the insights on which much psychotherapy is based, as we pointed out.

We have this scheme:

	Histories
Practitioners'	Historians'
practical	cognitive, objective
engaged	detached
interest in outcome	disinterested
in medias res	know outcomes

Important differences, but differences are mitigated

objectivity (truth is important)

hindsight (thinking in a future perfect tense)

And Carr goes on to qualify the disinterestedness of the historian. What they find can make a difference to the present:

> A concern for the truth of the past plays the same role in the case of the community. Members often debate the facts of the past, precisely because they are so important in the constitution of the present and the future. This is not to deny that the past is often manipulated, especially where social story-telling is political and persuasive in character. The personal past is often distorted too, deliberately or not. My point is merely that a genuine interest in the truth of the past is compatible with and indeed important for the practical narrative constitution of communal existence. Equally, objectively-oriented historical enquiry and research are not disqualified from playing a role in the ongoing political and social debates of a community; on the contrary, they can and do contribute to them.
>
> We are not commenting here, it should be noted, on the success with which truthfulness about the past is actually attained. Our point concerns the interest in or commitment to truth, and we are only saying that this is not restricted to history as a discipline. It is true that the discipline has among other things developed techniques for discovering and evaluating evidence in order to implement its commitment to truth. A justified suspicion that partisanship in the events of the day can distort our view of the past has led to the emphasis on detachment and objectivity. But these in turn, once achieved, can be put in the service of engagement in the present and the shaping of the future.

This raises the question “Who are historians writing for?”.

For Carr we all deploy a future perfect tense to understand a passage of history, even if this is a history of the very recent past and we are participating:

> As for the hindsight which is characteristic of historical enquiry, this too is not exclusive to the latter, at least not formally. Socially constitutive narrative, like the narrative structure of individual life and action, has a prospective-retrospective form. In anticipating the future, it aims at, and largely achieves, that quasi-hindsight that we characterized earlier, borrowing Schultz's term, as the future perfect. Far from waiting passively for things to happen, communities negotiate with the future and understand the present in the light of that future. 171-2

Carr's position would require historians and mathematicians to be brought into a much closer relationship, and rightly so:

> Far from dealing with past events which are fixed and whose consequences are clear, historians here deal with events whose consequences are still being felt and are operative in the present. 173

David Carr Time, Narrative, and History 1986 Indiana University Press Bloomington

There's a role for the mathematician helping the historian make sense of the thought of a mathematician of say 1890. If you just avail yourself of what is published until then, you will have

to have an extraordinary ability to be able to say what that mathematician was thinking, had just begun to glimpse, especially as what emerges afterwards is often complicated and not the way you'd learn the subject, which is later tidied up.

Conclusion

I have argued that mathematicians' and historians' histories should not be seen as two unrelated genres of writing. There is a role for the mathematician helping the historian make sense of the thought of a mathematician of an earlier time. And there's a role for the historian in disrupting some of the current understandings of the past. Both must discover the historical truth, even if one is caught up in the movement about which he or she is writing. But my argument in this paper is part of a larger argument carried out elsewhere (Corfield forthcoming) to the effect that the history of mathematics, as of any intellectual discipline, lies at the heart of what is rational about it. A history composed in a genealogical key denies the existence of that rationality.

Finally, with regard to ourselves, philosophers of mathematics looking for a treatment of mathematics which cares about the subject, speaking of a community, cohesive linked, we'd better not neglect our own cohesion and end up merely contributing a bunch of fragmentary contributions, enjoying our distance to the mainstream, or perceiving ourselves as an annexe. Let's establish the canonical texts of our movement.

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