Why the History and Philosophy of Mathematics should not be Rated X*

DAVID PIMM

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In 1974, in the journal *Science*, historian of science Stephen Brush published an article entitled, "Should the history of science be rated X?" In this essay, I should like to adapt a couple of his points to the situation in mathematics and then proceed to discuss one possibility (out of many) in which the history and philosophy of mathematics can be of use to mathematics education, namely that of informing our understanding of mathematics. As an additional point of reference, recall what mathematician René Thom said at the ICME-II Conference in Exeter, in an address which will well bear re-reading. "The real problem which confronts mathematics teaching is not that or rigour, but the problem of the development of "meaning", of the "existence" of mathematical objects." [Thom, 1973, p.202]

Brush argues that one current viewpoint about science among many contemporary historians and philosophers of science is one of cultural relativism, a view he feels which is not shared by many scientists. "I will examine arguments that young and impressionable students at the start of a scientific career should be shielded from the writings of contemporary science historians... namely that these writings do violence to the professional ideal and public image of scientists as rational, open-minded investigators, proceeding methodically, grounded incontrovertibly in the outcome of controlled experiments, and seeking objectively for the truth, ..." [Brush, op cit., p 1164] Thus if you, as a teacher responsible for the education of young scientists, *wished* to promulgate this view of scientific activity, his advice was for you to keep your students away from the proliferating undergraduate courses on the history of science at American universities.

In contrast to this protectionist stance, Brush also argues that scepticism towards established dogma is ostensibly a prized asset in science. Clearly one possible root for this latter attitude will be a broad knowledge of the history of that discipline, in providing an awareness that past dogma has been overthrown. But where is the tradition of criticism and scepticism in mathematics? In the study of English literature, literary criticism is at the forefront as a major component of that study.† Where is the comparable activity in studying the mathematical literature? An obvious first point is that seldom, if ever, do undergraduates see instances of mathematical literature. Textbooks are very much secondary sources

A recent example of conflict in mathematics arose between two published papers in homotopy theory (involving mathematicians Zahler and Thomas, and Toda and Oka), providing one of the few "published" instances of controversy. [Kolata, 1976; Zahler, 1976] Probably coincidentally, Zahler is also one of the protagonists in the Catastrophe Theory dispute which provides another contemporary occurrence of public criticism, though this latter article concerned strong doubts as to the validity and worth of certain applications rather than about the mathematical theory itself. [Sussmann and Zahler, 1978]

Imre Lakatos' *Proofs and Refutations* [1976] provides one notable instance of what might be termed mathematical literary criticism. In it he draws attention to precisely the *reverse* tradition from the usual one of scepticism in mathematics, namely one where mathematical maturity is equated with willingness to suspend disbelief (and questions) until the results are proved, providing a new twist to the doctrine that the end justifies the means. Perhaps Hilbert was referring to this spirit of pragmatism when he said, "In mathematics, as elsewhere, success is the supreme court to whose decisions everyone submits". Copes [1980] has also written about the reversal of the meaningful mathematical order to produce the customary formal one.

However, implicit in Hilbert's statement is the absolute nature of such decisions regarding success. Sociologist David Bloor, in his stress on the 'negotiability' of meaning, and hence of mathematics itself, has highlighted the social nature of mathematics. [Bloor, 1976] The history of the Euler-Descartes Conjecture, described (and 'reconstructed') so lovingly in Lakatos' book, can be viewed as a series of attempts to preserve the insight that $V-E+F=2$ for polyhedra, and the negotiation of definitions and concepts in which mathematicians engaged towards that end.

Lakatos' work on mathematics provides a method of criticism for improving conjectures and proofs, as well as a taxonomy of counter-examples, organised according to their function and point of contact with the above. He stresses the essential role of criticism in mathematical progress and it is a sad fact that this is virtually a non-existent area within mathematics education. Are we aiming to transmit mathematical methods or a body of knowledge? Surely it is equally essential to explain and communicate the nature of mathematical discovery.

† *Whose* literary criticism is itself currently fuelling a fiery debate among the English faculty at Cambridge University. A report on the wranglings involved has made front-page national news recently [Walker, 1981].

Proofs and Refutations also looks at mathematical methodology. We are provided with a description of the way Lakatos believed mathematics developed and an analysis of the process. This work provides a new, exciting interpretation of "ontogeny recapitulates phylogeny", a precept often quoted as of relevance for mathematics education [Hadamard, 1954; Poincaré, 1952; Polya, 1962; Thom,op.cit.] Under this interpretation we should teach according to the method of proofs and refutations in the classroom, though not necessarily the proofs and refutations of a particular historical development. This method augments our insight and enriches our concepts precisely because it reflects the way mathematics was, and is *done*.

As examples to show that even the methodology of mathematics is negotiable, consider the controversy surrounding the alleged computer proof of the four-colour theorem or constructivist Errett Bishop's [1977] rejection of Jerome Keisler's [1976] approach to calculus via infinitesimals. A further example is provided by the reported methodological (monster-barring) exclamation of Gordan when confronted with Hilbert's non-constructive proof of the existence of a finite basis for any system of invariants. He claimed, "Das ist nicht Mathematik – das ist theologie", and thus clearly disagreed with Hilbert about the results of the latter's appeal to the "supreme court of success". The historical development of mathematics since Gordan has not agreed with him (possibly, a Hilbertian might argue, because of the technique's success), but who is to say that it might not do so later, in a move of neo-Copernican, conservative zeal? For example, rampant constructivism may emerge as the next dominant paradigm. After all, infinitesimal methods were banned for at least a century prior to Robinson's discovery of a way to guarantee their legitimacy, *within the current canons of mathematical acceptability*, and their return to some prominence in the last twenty years has been hesitant [Robinson, 1966] "Very early in his career Cantor had denied any role to infinitesimals in determining the nature of continuity, and by 1886 he had devised a proof that the existence of such entities was in his view impossible" [Grattan-Guinness, 1980] The revisionist potential for future mathematics to rewrite that of the past (and along with it all our judgements on validity which seem so timeless to us) has, I feel, been little appreciated. For this reason, among others, I see the phoenix-like rise of infinitesimal methods as one of the most interesting, twentieth-century developments in mathematics, ranking with the discovery and development of non-Euclidean geometries in the attack it presents on absolutism in mathematics

Too often in mathematics one meets the attitude of "We have it right now, no further developments are necessary, or possible." Awareness of the historical life of mathematical ideas gives an awareness of the temporary nature of mathematics and prevents too many arrogant assumptions about having "the best of all possible mathematics". Contrast (my emphasis): "What is wanting is a satisfactory critical account of the filiation of the fundamental ideas from their incipiency in antiquity to *the final formulations* of these in precise concepts familiar to every student of the elements of mathematical analysis", with: "Perhaps even more important is the role that the history of mathematics and science can play in the cultivation among professional workers in the field of a sense of proportion with respect to their subjects. No scholar familiar with the historical background of his specialty is likely to succumb to that specious sense of finality which the student so often experiences." [both in Boyer, 1959, pp.4-5]

Lakatos' writing deliberately moves between the history and philosophy of mathematics. What is the current involvement of philosophy of mathematics in the mathematics curriculum? Overtly, virtually nothing. Yet, as René Thom proclaimed, "In fact, whether one wishes it or not, all mathematical pedagogy, even if scarcely coherent, rests on a philosophy of mathematics". [Thom, op.cit., p. 204] What then is the "hidden agenda" to be learnt about mathematics from the way it is taught? Clearly that mathematics is the formal study of abstract systems presented in strictly deductivist style. Attendance at courses called philosophy of mathematics, which are, most often, courses of logic (identifying the former with formalism) will accentuate this belief. Mathematician Jean Dieudonné has written, "D'où la nécessité absolue qui s'impose désormais, à tout mathématicien soucieux de probité intellectuelle, de présenter ses raisonnements sous forme *axiomatique*, c'est-à-dire sous une forme où les propositions s'enchaînent *en vertu des seules règles de la logique*, en faisant volontairement abstraction de toutes les "évidences" intuitives que peuvent suggérer à l'esprit les termes qui y figurent." [Dieudonné, 1939, p. 225] But surely in mathematics education, even if it is so in mathematics (which I doubt), we are not even predominantly interested in the presentation of formally correct arguments. Thom's concern about the prevailing balance of rigour over meaning is indicated by his parallel reply to Dieudonné: "Tout mathématicien doté de tant soit peu d'honnêteté intellectuelle reconnaîtra que, dans chacune de ses démonstrations, il est capable d'attacher *un sens* à chacun des symboles qu'il manipule." [Thom, 1970, p.229]

Thus the parallel of learning the scientific method as a goal of a conservative, absolutist, scientific education becomes the learning of the axiomatic method. Are we not in mathematics in the same position that science was a couple of generations ago; that is, we have an officially enshrined methodology – at least as regards what I term public mathematics, the formal presentation via the lecture hall, the textbook and the research journal? My point is that the majority of mathematics instruction at the university level produces a false picture of mathematics, *private* mathematics, creative, human mathematics, by the constraints and beliefs concerning *public* mathematics. Even if mathematics had a Bridgman figure arguing that there is no such thing as *the* mathematical method (as Bridgman did for science in the 1920's), will students absorb the "correct" attitude through their mathematical education? Only if mathematicians behave in private as they do in public. But do they?

What is missing from the formal presentation in the customary *Satz-Beweiss* manner, and how can the history and philosophy of mathematics help alleviate this absence? Firstly, one lacks any discussion of the problem background. What questions were the mathematicians involved in trying to solve? How were they viewing the problem, and in what way is this theorem a solution? Theories arise in response to problems and absorbing theories should not be central to mathematics education. More importantly, how do the definitions of the terms involved relate to the theorems as stated and proved? This whole murky but fascinating area has been incandescently lit by Lakatos' work (for instance, in the telling concept of proof-generated definition) which has only recently seen much development or criticism. [Feferman, 1978] I said earlier that history and philosophy of mathematics can inform our understanding of mathematics. With this kind of richness available, illuminated and illustrated by the forces and vagaries of historical development, does not the formal axiomatic presentation seem somewhat stark and yet pale also?

A new epistemology of mathematics for mathematics education is required which evolves as the confluence of mutually-permeable strands of the now-separated disciplines of mathematics, history of mathematics and philosophy of mathematics. Lakatos, paraphrasing Kant, claimed "The history of mathematics, lacking the guidance of philosophy, has become blind, while the philosophy of mathematics, turning its back on the most intriguing phenomena in the history of mathematics, has become empty." [Lakatos, op.cit., p.2] But what hope has mathematics education without either? My aim is the creation and development of personal knowledge of mathematics [see Gordon, 1978] rather than the transmission of "absolute" knowledge. The growth of understanding can only be enriched and encouraged by an awareness of the problem sources which renew and maintain our mathematics and allow us to observe the changes in what it has been as well as informing our creativity with regard to what it might be. A polished, logical presentation of mathematics (that is, a-historic, prepared with hindsight) shows none of the difficulties, errors, guesses, stumblings which went into its creation and attainment of its present form. Unifying concepts and proofs can't unify if there has been no awareness of a previous state of disparateness, and generalizations have to generalize something. In this light, Sarton's claim [1936, p.3] that "nothing suffers so much when divorced from its history as does mathematics" can be seen in its full force.

The beauty of the study of the history of mathematics is that it can give a sense of place (and hence, for me, meaning) from which to learn mathematics, rather than merely acquiring a set of disembodied concepts. At the same time it provides an elevated point of view from which to survey the current position. A sense of history can provide an awareness that it was not always the way it is today, and hence that it might have been (and also might be) otherwise. In this way, history can convey the notion of a culturally-based and culturally-bound mathematics which is open-ended and changing, a challenge to the more prevalent view of mathematics as a static list of accumulated truths.

But while it is an important point that the history is not "final" or "right", but dependent on the philosophies of mathematics which guide our reconstructions, we would be doing ourselves a disservice to suggest that the historical method, or the method of proofs and refutations should merely supersede the axiomatic method as *the* enshrined methodology and epistemology of mathematics for mathematics education. What I would suggest is a disciplined eclecticism toward, and a deep knowledge of the various alternative possibilities from which to choose, when setting out to teach a part of mathematics. Many central points emerge when a broader view is taken which would have been missed given a narrow axiom-theorem-proof format. Questions can be raised with respect to the diverse natures of mathematics itself, as well as with regard to its origins and development.

Teaching should be concerned with helping students to make connections between ideas, something which is unlikely to be achieved by teaching items in isolation. But what could be more isolated than an idea out of context? One often feels a reaction of bewilderment on being presented with some mathematical idea: "Where did it come from?", "Who, why, and how did someone come up with that?", *precisely* because the context is missing, possibly even the actual question the perpetrator was trying to answer. It also adds to the cult of personality and the aura of genius surrounding "name" mathematicians by denying the very information necessary to render their actual achievements comprehensible and, incidentally, none the less laudable.

We need to indicate more of the steps which were taken and the approaches which were followed. Polished proofs eradicate all the clues in an attempt to make the presentation logical. Isn't it amazing that we teach in precisely the *reverse* order from the one people seem to learn in, by naming a theorem, stating it, proving it, then finally going to a couple of examples? Surely a teacher's role is to help clarify, and not hide, how a piece of work was done. Yet in published mathematics the methods of work are entirely absent and too smooth a path is given. If we never have to struggle with an idea or problem, we feel useless and outraged in the face of a real problem, one outside the narrow confines of what we have seen done. And if we are unable to solve problems, often all that will remain are vague memories of "objects" and relationships between them. This is a fate that I believe it is possible and worth trying to avoid, and that it can be avoided through an incorporation of history and philosophy of mathematics as an essential part of mathematics education.

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— I remember when I was a young Oxford Scholar, that [Dr Edward Davenant] could not endure to heare of the *New* (Cartesian) *Philosophy*: For, sayd he, if a new Philosophy is brought-in, a new Divinity will shortly follow; and he was right.

John Aubrey, *Brief Lives*

— [Thomas Hobbes] was 40 years old before he looked on Geometry; which happened accidentally. Being in a Gentleman's Library, Euclid's Elements lay open, and 'twas the *47El libri I* He read the Proposition By G—, sayd he, (he would now and then sweare an emphaticall Oath by way of emphasis) *this is impossible*! So he reads the Demonstration of it, which referred him back to such a Proposition; which proposition he read That referred him back to another, which he also read *Et sic deinceps* that at last he was demonstratively convinced of that trueth This made him in love with Geometry.

I have heard Mr Hobbes say that he was wont to draw lines on his thigh and on the sheetes, abed, and also multiply and divide

He would often complain that Algebra (though of great use) was too much admired, and so followed after, that it made men not contemplate and consider so much the nature and power of Lines, which was a great hindrance to the Groweth of Geometrie; for that though algebra did rarely well and quickly, and easily in right lines, yet 'twould not *bite* in *solid* (I thinke) Geometry.

'Twas pitty that Mr Hobbs had not began the study of the Mathematics sooner, els he would not have layn so open. But one may say of him, as one says of Jos Scaliger, that where he erres, he erres so ingeniosely, that one had rather erre with him than hitt the marke with Clavius.

John Aubrey, *Brief Lives*