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History of mathematics
and
the new historiography

I don't think it is a particularly controversial thing to note that mathematics is, in some sense, a science. I don't want to get involved in tedious philosophical questions of what I mean by mathematics, or science, or "is" ... we can leave the former two questions to Oxford philosophers, and the latter to President Clinton*. This idea that 'mathematics is a science' is not new, and has probably been uncontroversial since something like the time when (something you might want to call) science was first talked about, in the region of the beginning of the 5th century BC. So if maths is a science, then you would think that trivially the history of mathematics is part of, or an aspect of history of science. You would think that was obvious, but to many practitioners, it is not. It ought to follow that the methods of studying and writing history of science [*historiography*] would be adopted and followed by historians of mathematics. You might also think this, too, is obvious, but to many practitioners, it is not. It seems a simple conclusion that history of mathematics should be done as part of history of science and obviously past mathematics should be understood intertwined with the same contextual matrix within which its contemporary science is understood. You might even go to the extent to think that was also obvious, but to many practitioners, it is not.

This has interesting consequences. I do history of mathematics using – at least to some extent – my own interpretation and take on the historiography of modern history of science. This is not at all the norm, and it is something that many mathematicians find – when they see what I mean by historiographically up to date history of maths – completely unacceptable. I'm not *doing* the maths, and that – for many – seems not to be proper history.

That history of mathematics should be done like any other science strikes me as trivially obvious, but it would only take the most cursory examination of modern history of science and history of mathematics to see that the authors of each sort of history are both distinct and appear to live on different methodological and narrative planets. And sadly, it is most – but not all – historians of mathematics who are on the *wrong* planet. Most history of mathematics as it is done today follows an analytical or interpretative agenda and methodology that is, to be polite, out of date and naïve and

* and if you don't get that reference to Bill Clinton's testimony about his activities with Ms Lewinsky, then you are (a) too young or (b) culturally challenged.

unsophisticated, compared to the kinds of methodological complexity and philosophical sophistication to be found amongst most writers of modern history of science.

There are many reasons for this extraordinary disjunction between the two subjects. It is in part caused by professional distinctions: most historians of science are now people who make an academic career out of being historians of science (or historians and philosophers of science) whereas most historians of mathematics are former mathematicians who for one reason or another get interested in history of mathematics, and think that because they understand modern mathematics, they can do history of past mathematics. Most historians of science live next to philosophers of one sort or another, most historians of mathematics live in mathematics departments and don't think that philosophy of science and history of science have anything to teach them as far as doing the history of mathematics. In the modern academic world there are very few history of science groups or departments where there are historians of mathematics; Cambridge is no different from the norm.

As a result, much history of mathematics does not use the modern style of historiography of history of science because, in part, there is little personal or academic contact between its practitioners. Sometimes, it has to be said, this doesn't matter all that much ... but overall, it has left the history of mathematics intellectually impoverished and historiographically limited because the full range of analytical approaches and tools that are available to other historians of science are not exploited as much as they might be within history of mathematics.

As you can probably guess from the way I have written the paragraphs above, I do not hold to this isolation of historiography, and I hope that the kind of history of mathematics that I do – and therefore I teach – reflects to some extent modern historiography of science.

So you may want to understand a bit about how history of mathematics is now done, and the modern historiography of history of mathematics. Like everything in life, it is not quite as simple as what you might have seen as “history” at about GCSE level: by the time you get to professional or academic history – history of anything – there are a lot of nuances that need to be thought through, and a sophistication of analytical tools that are not always simply trivial. It always come down to one simple issue: what you think you are doing when you do history and therefore what sorts of explanations you think are sensible or valid explanations of what actors in the past did. The “norms of explanative adequacy” are not fixed in stone, and are very much an issue that the historian needs to grapple with – sometimes explicitly, sometimes inexplicitly – whenever any sort of explanation or understanding of the past is proposed.

If you go over to the history faculty, or to academic history conferences and journals, you will find there is a great plethora of (sometimes vacuous, sometimes incredibly profound and interesting) arguments about historiography. Just as there is Marxist historiography, or feminist historiography, or hermeneutical historiography, and no doubt someone will be doing what they call post-modern historiography ... so there have been styles and developments in the historiography of history of science, and these have caused developments and a new style of doing history of mathematics.

At least, they should have caused a new style of historiography in history of mathematics. In general, however, they have not, and in general, history of mathematics is written with an antiquated and naïve historiography that is now getting on for half a century out of date. You need to understand this, because the style of history that I will be teaching in this course, and that you will find in the notes that accompany this course is one that reflects my own take on what I will call the modern or new historiography of science.

Modern historiography

History of the sciences – as done by professional academic historians of science these days – ain't what it used to be. It may not be quite like the stories you were taught at school or may have had told to *en passant* as part of science courses at university.

You need to understand this: history of science is not done the same way, with the same attitudes or ideas, as used to shape how it was written: over the 1960s and 70s a new way of doing history of science emerged, or became the generally accepted style of doing history... and while those outside the profession of history of science may not know about or understand the methods and terms of interpretation of the new historiography, you need to – because it is the style of thinking about the past that I use. I want to talk about this new historiography because it informs profoundly the kind of history of science – any science – that I do, and because it provides me with a whole toolbox of useful ideas and terms of analysis that I will be using when looking at past science and scientists. And it is the way I shall expect you to think (and write, if you are doing the course for assessment) about past science, as well.

This 'new' historiography ... I put the *new* in quotes because it wasn't at all *new* to ordinary historians, but very old hat ... this 'new' historiography made a deep intellectual break with traditional history of science, and caused both considerable ructions at the time, and has been the source of continued controversy and argument ever since.

At heart, the 'new' historiography is just one where one avoids the errors of what we call Whig historiography – I'll explain that in a moment – and where the historian avoids seeing and understanding past science in terms of what we now, today, think are the correct scientific answers. The traditional way history of science was done, or the story told, was one that had as its goal (very roughly speaking) to see how what we now think is correct came about. In other words, it was the story of how what we have or what we know in science today was created, or discovered, or invented*. The 'new' historiography, rather, asks the question of why – or what it was – in the terms of the past historical actors that made it reasonable for them to do what they did. The new historiography is not interested in *today's* correct answers or what science knows today, but rather is interested in understanding the past in its own particular context.

* whether scientific ideas are invented or discovered is a hugely complicated philosophical question, and one that I have no intention at all of even beginning to discuss.

This new historical method or style or approach is not by any means a singular doctrine, and there have been and continue to be endless debates amongst adherents of various schools or strands or factions of the new historiography. Some of these have been significant and interesting, and have revealed a great deal about thinking about history of science and have imported loads of useful tools from the philosophy of sciences (and elsewhere, like the social sciences) ... but most of these squabbles have been more about politics and about academic trivia than really solid and interesting issues. There have been loads of arguments about doing 'internal' and 'external' history, about social history and whether scientific knowledge is uniquely the product of social and political circumstances, about psycho-history and scientific texts as narratives, about feminist issues and feminist historiography, and so on. Frankly, I regard most of these arguments as otiose and uninteresting, as the proof is really only in the pudding, and that is in actually doing the history and seeing what sorts of explanative tools or schema seem to explain the past in a way that we find satisfactory.

I have a strong aversion to any claims of universality – and when someone tries to tell me that this or that is the correct way to understand the past, I immediately suspect that there is an absolutist political (or religious) agenda afoot. Human beings don't fit into single schema – we are all far too eccentric and ornery to always work in the same way.

No schema of explanation is going to be perfect and complete, and there is never going to exist a final and complete explanation of anything in the past. That much, at least, we can be pretty sure about. What some people will find a satisfying explanation of past events will not seem so satisfying to others, and not only is that inevitable, it is right and proper and a good thing. We should be under no naïve misapprehension that there exists a 'correct' or 'complete' history of the past: we can only have a partial understanding of the past. We only have access to a small amount of the documents and thoughts and details of what went on, and any story we construct is at best partial and incomplete, and inevitably involves the historian making choices and judgments as to what s/he includes and how s/he assesses the material. Whilst historians are not supposed to make up facts and make up history, there should never be any illusion that some sort of supra-human objectivity exists: all histories are stories constructed by the historian, and are only that particular person's assessment and reconstruction of the past from what evidence they have considered. These stories aren't *lies* just because they are imperfect, incomplete, and cannot be purely objective ... they just need to be understood as never final and never of the status of absolute and complete truth. That doesn't make them purely political claims or statements, but it does mean that the 'authorial voice' cannot be ignored: my history is different from the next person's history because I'm a different person and will think in my own way, and she or he will think in their own unique and particular way. Thank goodness!

My own position, when it comes to the array of different approaches and positions, doctrines and theological dogmas, methods and styles that there are amongst those who practice history using the 'new' historiography is one of a methodological immoralist, or perhaps a methodological (or is it ideological?) *anarchist*. I don't have any conviction that any of the many different ways of approaching the past is uniquely correct: rather, I am happy to pick and choose and use in a perfectly inconsistent and opportunistic way from the various tools and various approaches that the new

historiography offers ... my only criterion being what seems to me to work, or to give me useful or understandable information or analytical tools about the chaps in the past. Explanative power and historical sense is what interests me, not ideological or political purity. Where a feminist approach seems to explain things, or the tools of feminist historiography seem to help me to understand why people in the past did what they did, then I'm perfectly happy to use those tools; when something in the past seems to be explainable in terms of social issues, or sociological analysis, then that's just fine with me. When what happens or some particular issue seems to me to be explainable in terms of the technicalities of 'internalist' history of science, then I am comfortable taking that approach, in those circumstances. Different historical phenomena will be better explained using different terms of historical analysis. My point is simple: I don't think there is a single explanative scheme that will explain all of what humans do in the past, and I don't think any explanative scheme will satisfy any particular reader's desire for explanation or understanding. So it seems to me to be unproblematic to pick and choose in a completely opportunistic manner what explanative tools, analytical tools, and narrative schema seem to be satisfying ... in any given situation.

The "strong programme"

That science consists of more than just the technical results or theorems is, I hope, something of a trivialeity to any reader; scientists exist in a social context, get paid to do their work by someone or some institution (or *don't* get paid, of course), have to have their work accepted (published, whatever) by some group of peers or colleagues, will be working a problem or something for some set of reasons that may be purely technical or purely social or anything in between ... and so on. The very language a scientist uses will be part of his or her social context, and the style and foundational intuitions that they bring to bear on any work will be part of that larger context. There can be no such thing as knowledge or scientific work that is completely devoid of social context ... nor devoid of intellectual, theological, scientific or technical, institutional, economic, philosophical, or personal context. All scientific endeavour, all scientific results, occur with a social context because the actors are human beings, and exist in some sort of society. This is a pretty trivial observation, really. We cannot usually say a great deal about much of this panorama of possibly influential context in which any scientific work is done, but it is obviously present because, trivially, scientists are people working in a social context (no matter how geeky, they are still social beings). That there is a social context is, I hope, obvious; what is not obvious is to what extent what within that human context will influence the historical actor, or may explain why it was reasonable for them to do what they did.

However, there have been some sociologically and politically minded historians of science who have taken this obvious fact, and drawn from it the conclusion that since scientific knowledge comes from processes that are social and socially (or "societally") embedded, all knowledge is *only* a social construct, purely a social construct, nothing but a social construct, and therefore is *only* political. All science is political, all scientific statements or claims are ultimately socially constructed and therefore political in nature. This argument – called the "strong programme" of the social reconstruction of science – is something to which I do not adhere, and regard as profoundly antithetical to any hope of understanding the past.

The strong programme is, in my opinion, just a political stance on the part of those that hold it: its adherents argue that all other claims to knowledge or to methods of analysis of knowledge are themselves political, but I would suggest that the same analysis can be made of the claims of the strong programme, and that it is itself a purely political stance – and indeed, a political stance characteristic of late 20th century Anglo-American academics – and needs to be understood as nothing more than a political argument in the pursuit of academic power by a certain of scholars in search of an avenue to academic influence or power. In its dogmatic form it is not a useful way to understand the actors in the past. Apart from the repeated slogan that everything is political and all knowledge is political, it is quite hard to use these doctrines to explain many of the activities of past historical actors, except to assert that its political. Of course, it is always potentially helpful to look at the social context and political pressures that might be acting on a past actor individually or as part of a community, and this analytical tool is going to be very useful indeed in explaining the behaviour of an actor in the past. But it is not a universal explanandum, and is not, I suggest, a complete explanation of the actions and the discoveries of past scientists.

It also fails to explain the unreasonably successful nature of the technical aspects of certain scientific theories, and the huge technological consequences some scientific theories have had: to suggest knowledge is only social and only socially determined makes a great slogan, but it does not explain why this medical procedure works and that treatment does not, and it will not explain why this machine is efficient and that one does not work. It will not explain why this calculating model tells us where Saturn will be tomorrow morning at 4 a.m., either (although why I want to know where Saturn is going to be tomorrow morning may well be explained in a social context).

It would be equally foolish to suggest that any such forms of knowledge or any such scientific claims can be understood without taking into some consideration, and including in the matrix of things that explain how a scientific claim comes to be, some consideration of a wide range of possible social influences and the actions of a social context. The social context may, for example, explain why this medical procedure, although more efficient, is not adopted for 30 years by contemporary medics, or it may explain why nobody cared about the success rate (or what *we* mean by success rate) of medical procedures. Explanations that include the social context may be very helpful, but the strong programme makes a much stronger claim: all knowledge is only socially constructed. And that is a claim that I do not accept, and is a historiographic programme and style of explanative schema that I find inadequate.

Doing good history: avoiding Whig historiography

However ... these are the sorts of arguments about what may appear to be nuances within what I have called the ‘new’ historiography. What really matters, I would suggest, is to avoid doing Whig history, or avoid the errors of Whig historiography. That is the basic premise of all of the ‘new’ historiography, and that that is the first rule that any adequate modern history of science must obey.

The insult of ‘doing Whig history’ (and the understanding of this unacceptable error) stems from the title of a short book by Sir Herbert Butterfield; *The Whig Interpretation of History*, published in the late 1920s (Butterfield was Professor of history at Cambridge, eventually Master of Peterhouse, and even Vice-Chancellor of the University). Butterfield’s analysis of the political history of 18th century England that had been done up to then revealed two related and (everybody not-brain-dead now agrees), horrible historiographical or analytical errors.

The *first* was to see the political conflicts of the 18th century as the conflicts between a ‘good’, liberal, and progressive Whig party (in the sense of wishing-to-progress-towards-today’s-liberal-democratic values) and a ‘bad’, reactionary, and anti-progressive Tory party (in that they didn’t hold modern liberal and democratic social, political values). The error here is to see the past as a conflict between good (progressive) and bad (conservative) forces, where the good force is apparently trying to work towards the modern world: modern values, ideas, whatever, and the conservative forces resist these changes. Thus those who make this error analyse the past in terms of today’s values or criteria or terms, seeing the past as a conflict with good guys struggling towards today, or struggling towards the (correct, in today’s terms) discoveries that lie in the future, and their opponents as bad guys opposing progress and the advancement of society, science, or whatever.

It doesn’t take an imagination the size of Jupiter to realise that the actors of the past were not struggling towards today’s values (which today? The end of the 19th century? End of the 20th century? 27th century?) for the simple reason that they didn’t know today’s values or ideas or truths, and cannot have been struggling towards them. It is a simple and gross, crass historical error to see actors in the past seeking out, or struggling towards something that the future holds, or trying to get-to-today. The past actors just don’t know the future, any more than you do, so they are not – cannot – be trying to achieve, discover, or have something they know nothing about. By definition, don’t know about.

The *second* and related error in historical thinking that Butterfield pointed out was to think that it has anything to do with the historian’s job to sit in judgement of the past. Assessing the past, passing judgement on the past, is historical nonsense, and completely pointless. This traditional idea that what a historian is supposed to do is somehow judge the past, awarding some sort of prize or points to those that are ‘better’: this is better than that, this chap is great but that chap is greater, and the greatest of them all / that century / in that town / whatever was so-and-so; this is the most significant discovery, or this scientist is the most important, etc. etc.. As if anybody cared how people at some arbitrary point in the future would judge the past by the criteria of ‘good’ and ‘bad’ or whatever of that arbitrary point in the future. Mere judgement – this is right, that is better, this is good, moral, justified, sensible – of the past, the awarding (or detracting) of points is pretty pointless on its own, but is completely stupid (and a great vanity) to do it with respect to today’s values of right, good, progressive, or whatever. Inevitably when we judge the past we judge it with respect to today’s values, and there is nothing particularly special about the values of today (or 10 years ago, or 1904, or 1876, or 1763, or any other arbitrarily chosen date). The injunction against using Whig criteria of analysis and judgement applies equally to all schools or styles in history of science: Whig judgements of technicalities

are no less meaningless than Whig judgements of political motivation and social prejudice.

In science it is all too easy to engage in Whig history, because it is so easy to see the past as working towards, deliberately trying to progress towards what we know is the 'right' answer, or trying to overcome what we now know to be 'errors'. It is all too easy to tell an edited story of the past which makes the past look like a progression of correct ideas, discoveries, and theorems, as people 'discover' today and science triumphantly overcomes ignorance and superstition in its glorious march towards the wisdom and theories of today. It is so easy to engage in simplistic Whig historical errors in sciences like medicine and mathematics because we know so well what is 'right' and we think we can re-do past calculations or past diagnoses of disease and ascertain whether or not it was 'right', or what the past actor was 'really' trying to do in his clumsy way. Always ask yourself the question of such an argument, be it a calculation or a diagnosis: "So what? Is this how the chap in the past thought?"

Ahh ... the comfortable safety of such nonsense as seeing past science as the triumph of empirical rationalism over superstition and philosophical speculation.

The past is not a progression towards today; the historical actors of the past did not know they were developing towards what we know, they were not trying to achieve what we have, nor were they working on the prototype of what we now know or have, seeing what they did as the first step (or a further step) towards what we now can see was to follow their work. Trivially, nobody knows the future, so past mathematicians did not know what theorems would be proved at some point x years in the future (especially so for x larger than a couple of years). Put this way, it seems quite obvious, but you will find almost every textbook on the history of 17th century mathematics treats the 17th century study of the mathematics of curves and the development of quadrature and tangent finding methods as a "struggle" to invent the calculus, or anticipations of the calculus, or even near-misses to discovery of the calculus ... something that was "finally achieved" by Newton and Leibniz in the 1660s-80s. Talk about teleology!

Unfortunately (for them) the chaps before Newton and Leibniz didn't know that the differential and integral calculus was sitting around out there waiting to be discovered, and so they were not trying to discover it (in fact, they were trying to do something completely different). So seeing their work as unsuccessful attempts to discover what they didn't know was there to be discovered, or the whole period to be a "period of confusion" because they hadn't yet made this discovery, is clearly completely alien to what was going on in the minds of the historical actors of the time, and no way to come to an understanding of what they were trying to do. If you want to understand why a mathmos was doing this or that in 1650, the most certain way to *not* understand it is to see what he was doing as an anticipation of, or a failed attempt to discover the calculus, something about which the mathmos knew absolutely nothing, by definition. It is all a brutal and misleading anachronism ... and the historical use of which (in understanding the past) is pretty much exactly zero.

In a similar vein, you will find that the century (or two!) of biological and geological thinking before Darwin is often portrayed as people struggling to discover evolution, or

nearly getting there ... or singling out a particular group of chaps as important or worth telling about because they are closer to evolutionary ideas, or have some of the ingredients, or some of their discoveries or ideas are later to act as ingredients that Darwin uses. It's a good way of picking out a group of scientists and scientific ideas and discoveries that exactly does not tell you about contemporary scientific thinking and scientific debates - because the theory of evolution is just not in people's minds a century before Darwin.

It is not the job of history – history of anything – to sit in judgement of the past, awarding 'good' or 'bad', 'progressive' 'reactionary' or 'right' and 'wrong' points, and particularly not when judged by the criteria of today's values. These judgements just don't tell us anything about the historical actors, about why it was reasonable for them to do what they did. For the history of science in general this has radical consequences: we cannot accept or reject, judge as good or bad, as significant or not, past scientific work on the grounds of what we, today, know what is right or wrong, significant or insignificant. We have to try, as best we can, to understand it in the terms of the contemporaries, and its significance to contemporaries. Trivially, the perspective of today is irrelevant because there is nothing special about today (except that it is dear to you and me because of the accident that we are alive today, but otherwise our knowledge and values today are not distinguished, nor final), and because it is completely unknown to the historical actor. The scientist in 1500 or 1850 did not know what we would judge or know to be correct or right or whatever, did not know what we know, so any Whig judgement as to the scientist in 1500 being right, wrong, or better-than-something-else is only a measure of our own values, and not about the past scientist ... and reveals no sensible assessment of his own contemporary role, innovation, or significance.

Thus we find that past enterprises like astrology or alchemy, which we today regard as 'wrong' or 'not-good-science' were once regarded as valid inquiries into and explanations of the nature and causes of physical phenomena – valid parts of natural philosophy. And much as we might today regard them as using wrong methods and false premises, if at the time these were regarded as adequate methods and were thought to be based on sound and sensible premises – premises that contemporaries accepted to be reasoned and reasonable – then the work of the historian is only to attempt to understand why such methods, explanations and such an understanding of the world seemed adequate and reasonable to people that we would want to call 'scientists'. There is nothing wrong with, say, Newton holding beliefs that look like occult alchemical nonsense to us: this is very likely to be our problem, not a problem for Newton himself, nor a problem for Newton's science.

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