A Reply to Benjamin Breen

Seventeenth-century theologians and philosophers often wrote long books in which they reproduced sentence by sentence the texts of their opponents, and then at even greater length pointed out how foolish their arguments were. The result was two books in one, their texts shuffled together like two decks of cards. I have no desire to breathe life back into this extinct literary form, but I want to take here a few passages from Benjamin Breen's review of my *Invention of Science* and of Steven Weinberg's *To Explain the World* (chronicle.com/article/VialError/234826) and briefly show how they miss the point. My goal here is not to win or lose an argument with Breen: it is to identify the nature of our disagreement. We each believe the other is confused about the methodological and historiographical issues. It is the issues themselves that matter, not any particular book or any particular review. I will take Breen's comments in his order, rather than reorganising them into my own framework; the result, as in the seventeenth-century polemics I have spent so much time reading, will be a somewhat disjointed argument, but it has the advantage of keeping more closely to Breen's text.

"When we employ present-day knowledge," writes Breen, "about what ideas and knowledge ended up being significant and what fell by the wayside, we risk writing histories that cherry-pick scientists' triumphs and exclude their numerous -- and instructive -- wrong turns." This is absolutely true. But does my approach mean that I am bound to make this sort of error? I stress, for example, Galileo's belief that he could use the tides to prove Copernicanism, which was a major mistake. (Elsewhere I have written a long essay entitled "Galileo: Reflections on Failure" [2011]). I stress that the results of early vacuum experiments were equivocal, and that significant figures, such as Roberval, thought they showed there was no vacuum in the Torricellian space. More interestingly perhaps I discuss an instructive wrong turn that all previous historians have missed: the belief that the earth would, from space, be invisible, a dark star. This belief was held by important people -- Benedetti, Digges -- but no one, as far as I have been able to tell, has commented on it. It is a fundamental part of my method to seek to avoid making the error Breen describes; we are all fallible, but it would be interesting to see an example of where I am supposed to have gone wrong, and only fair to acknowledge that I have made every effort to avoid doing so.[1]

Weinberg and Wootton, says Breen, “mount spirited defenses of teleology in the history of science.” Well no, not Wootton (nor Weinberg, I suspect). As I point out in my book, the word “teleology” has come, under the influence of Foucault, to be very carelessly used.[2] I distinguish between teleological history, which thinks that history has a goal or purpose, which I am against (for the obvious reason that history has no goal or purpose), and retrospective history, which asks how we got from there to here; how for example the Allies came to win the Second World War. Victory was not inevitable nor predictable; but its causes can be identified and debated. I argue that retrospective history is legitimate. I also argue that scientific advance is path-dependent: the Torricellian experiment led rapidly and directly to attempts to construct a primitive Newcomen-type steam engine; Einstein’s theory of relativity led rapidly and directly to attempts to measure the bending of light from background stars as it passes the sun during a solar eclipse; one discovery often leads directly to another. I argue this against the currently fashionable view

1. I do realise that Breen was reviewing two books in a short space, and thus don’t expect him to support every claim with examples. The issue is whether the claim could be substantiated, not whether he should have substantiated it within the limited space of the review.

2. For an example of a more careful usage, Shapin and Schaffer, *Leviathan and the Air-Pump* (2011 edition), xix.

that developments in science are contingent and unpredictable. So, in my terms, I defend retrospective history and path-dependency; I certainly don't defend teleological history, and I say so very clearly. (There is a further complication here that we will come to in a moment.)

Wootton's book marshalls "a tremendous amount of research into primary sources to reframe the history of the Scientific Revolution as a progressive voyage of intellectual discovery" says Breen. It is the word "reframe" here that is problematic. As I show, Bacon imagined a science which would be a progressive voyage of intellectual discovery. By the end of the seventeenth century all sorts of people were announcing with delight that more progress had been made in the last few decades than in the previous thousand years. I don't "reframe" the history of the Scientific Revolution as a progressive voyage of intellectual discovery -- I point out that that is how contemporaries framed it, and that this was because they belonged to a new discovery-oriented culture. Indeed my views are so close to those of William Wotton that Tim Lewens feels obliged to suspect some sort of "collusion across the centuries. Writing close to the field of combat, Wotton detected the very same upheavals in knowledge production that Wootton himself now discerns with the benefit of historical distance."

Breen complains that my book is "thin" (a strange word for a book that Lewens describes as a plum-pudding of a book, full of delicious morsels) because it contains no discussion of topics such as Galileo's bawdy sense of humour. Perhaps the place to look for that would be in my biography of Galileo or in Dava Sobel's admirable *Galileo's Daughter*. He would be wasting his time looking for it in Thomas Kuhn's *Copernican Revolution* or in Robert Westman's *The Copernican Question* for the simple reason that it would be as out of place in their books as it would be in this book of mine.

Breen quotes against me Shapin's review of my book *Bad Medicine*, and then says "Shapin's point... is not that scientific or medical progress does not exist. It is that *understanding the historical trajectory* of progress depends upon an awareness of its gaps, its uneven distribution, and the lived experience of those who participated in it." (For Shapin's text: https://goo.gl/nOubHA; for my reply: https://goo.gl/nOubHA). No, that isn't Shapin's point. And it is precisely because it is not Shapin's point that I engage in extended polemics against positions which critics like Breen claim nobody holds ("straw men"), but which in fact people like Shapin do (or did) hold. Shapin's argument in his review of my book was that progress is real, but historians must not write about it. To write about progress is to write Whig history and not to understand the past in its own terms; writing about progress is an intellectually valid enterprise, but historians must not do it (even though, to repeat, progress is real), because if they do it they cease to be historians. He compares writing about progress in medicine to a history of painting which describes it as "a triumphal progress from Titian to Tracey Emin" or a history of music which celebrates "a linear ascent in compositional quality from Bach to Birtwhistle." He concludes that my book, which takes progress seriously, "isn't bad history so much as not history at all."

Consequently, as I read Shapin -- and I challenge anyone to produce a quotation that shows this is not what he thinks -- it is no part of the historian's function to understand the historical trajectory of progress. Let me quote Shapin on history of science since Sarton: "What 'everyone knew' about the history of science was precisely what academic historians no longer knew, or [a very slippery 'or'], at least, what their writings were no longer predicated on: science progresses, and its successful applications are powerful testimony to that progress."[1] If Breen thinks that elucidating the progress of science is part of the historian's purpose then he needs to understand that he is on my side of the divide, not Shapin's.[2]

What Shapin substitutes for a study of progress, in his review essay, is a relativistic study of what *counts* as progress. What people want medicine to do keeps changing, what counts as disease keeps changing, he argues, and so medicine is always both succeeding and failing -- or at least that must be the historian's point of view. Yes, *in the story doctors tell themselves*, medicine has got much better over time. That is what the medical textbooks say; but that is not the historians' view. From their point of view asking whether medicine has improved over time is like asking whether music has.

---

1. My book, of course, was not predicated on the idea that medicine progresses; indeed it argued that for two thousand years it made no significant progress.

2. Shapin, with Schaffer, returned to the question of progress in the introduction to the 2011 edition of *Leviathan*, xvii-xix (https://goo.gl/UwoFOh). There they reaffirmed their hostility to histories of scientific progress. But they add in an awkward parenthesis (xix) a passage which seems to mean that it is ok to believe in scientific progress as long as you don't focus your work on it (a sort of "don't ask, don't tell" approach, where believing that science makes progress is treated as if it were comparable to an historian believing that God answers prayer). It's ok to focus your work on gender, class, patronage, visual representation, spatial distribution, and anything else, but not on progress -- despite the fact that progress is arguably a peculiar feature of modern science. They appear to believe that the claim that science has this unique feature is incompatible with a naturalistic account of how it functions. But, in the first place, progress is not unique to science: we also see it in athletics, where it would appear to have a similar foundation in competition, specialisation, and the sharing of best practice. And second, even if scientific progress were unique, other social institutions have their own unique features: market economies, for example, or crowds. My view is that the question of progress is difficult and complex, but there is no reason why it should be excluded from debate or restricted to some sort of liminal or ghostly role in historical discourse. Nor do I accept that it might be ok for philosophers to argue about progress, but historians, just because they are historians, mustn't (in the way that one might say that theologians can discuss whether God answers prayer, but historians shouldn't). I take the view that any aspect of human behaviour can be a subject for historical enquiry; nothing is excluded. If progress in science is real, then it's an appropriate (but not compulsory) subject for historians to address.

I am perfectly happy to concede that one can write a history of medicine's changing goals; but it remains the case that doctors have always sought to postpone death and alleviate pain (Shapin acknowledges that these are "basic human wants"); and for two thousand years they failed to do what they were trying to do, which raises the interesting question of what went wrong. The answer turns out to be simple: they weren't counting outcomes and comparing different groups who had been treated in different ways, or had not been treated at all. "Being right" cannot, Shapin maintains, in his laconic reply to my response, be the historian's concern because it is impossible to know where to start. I found that reply very strange because the whole of my book was written to show that one could know where to start: not with Galen, not with Vesalius, nor with Leeuwenhoek, not even with Pasteur, but with medical statistics. The claim that one cannot know where to start depends on the presumption that there is no continuity in what doctors have been trying to do, that the goals of medicine are entirely cultural relative (as the goals of painters and musicians perhaps are) and constantly changing. But that, as Shapin knows, is only one way of telling the story.

The Invention of Science is not *Bad Medicine*. The two books are separated by a decade and by a great deal of reading and thinking. But the basic argument of *Bad Medicine* (that medicine before the nineteenth century should be thought of as being like astrology -- people convinced themselves that it worked, but it didn't) still seems to me sound. And the argument of *The Invention of Science* follows on from it. Aristotle argued that ice floats, despite being heavier than water, because it is flat. Archimedes showed he was wrong, but for two thousand years the Aristotelian doctrine was taught by the philosophers, and Archimedes' arguments were dismissed as mere mathematics, not knowledge of causes. For Aristotelians, Aristotle's argument worked fine; but it would not have gone on working if they had made any elementary attempt to test it out. For two thousand years there was hardly any progress in the understanding of nature; indeed there was no expectation of progress; then people like Galileo embarked on programmes of experimental testing and mathematical modelling, and progress suddenly accelerated.

Let's be clear here: physics is different from medicine. What Galileo or Einstein tried to do is very different from what Aristotle tried to do. And the problems Einstein worked on are only distantly related to the problems Galileo worked on (though Galileo wanted to measure the speed of light). But since the seventeenth century there has been a general understanding that knowledge of nature ought to progress, that it progresses through tests and trials, through establishing reliable facts and building theories. This broad framework has remained stable even while the content of science has changed radically. The argument I am making is not, in its outline, a new one. William Whewell made it in the nineteenth century. What is new is the evidence I use to support it, particularly the evidence of language change, which neatly embodies the fundamental change in what practitioners of natural science (whom we, following Whewell, call "scientists") understood themselves to be doing.

Historians, busy with other things, have stopped making my argument. Worse, they have claimed that arguments of this sort should not be made, that they don't count as history, that they are misconceived in some fundamental way. As a result they have lost any sense of the significance of the changes that occurred in the seventeenth century, and indeed any sense of what science is and what distinguishes it from everything that went before. (Perhaps at this point I might point out that, contra Lorraine Daston, I am not claiming that the Scientific

Revolution was "a big bang": it took place over a period of four lifetimes and was no more a big bang than the Industrial Revolution. But like the Industrial Revolution it was a transformative period in world history; indeed without it there would have been no Industrial Revolution.)

All societies seek to cure disease and alleviate pain. But there are plenty of societies in which the question of why ice floats is not thought of as interesting (see Ash Jogalekar's blog: http://goo.gl/R4BVro) -- in asking that question Aristotle was trying to do science, but he had an inadequate sense of the fallibility of his own reasoning, and so (I know I sound here like Steven Weinberg) he didn't do a very good job. The Aristotelians thought intellectual debates could be settled by appeals to authority: they maintained that Galileo was no philosopher because he disagreed with Aristotle, and a bad mathematician because he disagreed with Archimedes. Unless we acknowledge that attitudes to authority changed, we can't see the point of what Galileo achieved. Indeed we are likely to find ourselves stating (see my earlier post) that Galileo *argued* that ice is lighter than water, when we need to acknowledge that he *proved* it (even if his Aristotelian contemporaries remained completely unconvinced -- more fool them -- because Galileo's proof consisted of an experimental demonstration, not a sequence of syllogisms, and because Galileo failed to cite a single text of Aristotle in support of his case).

The claim that science progresses thus depends on the claim that there are certain cross-cultural standards as to what constitutes reliable knowledge; what science involves is a peculiar intensification and systematization of those standards, and that intensification depends on certain very specific social and cultural preconditions -- an improved flow of information (the printing press); a new scepticism with regard to authority (which may well have something to do with the printing press); a new preoccupation with applied knowledge (the mathematical disciplines of navigation, surveying, fortification, gunnery); a new type of interaction between the learned and the unlearned (itself a consequence of an interest in applied knowledge); a new, post-Columban understanding of knowledge as potentially progressive; and of course (some say my book is about texts not things) new bits of kit -- telescopes and the long glass tubes out of which barometers are made require very specific technologies for their fabrication.

Breen says that I consistently elide the difference between discovering something that nobody knew before, and educated European men 'discovering' things that lots of people already knew. Is this really the case? Much of the time, from the discoverer's point of view, there is no difference: Falloppio discovered the clitoris not by talking to women but by dissection. But when the difference counts I take account of it, as in my discussion of garlic, diamonds, and magnets, where I stress that sailors knew all along that garlic doesn't deactivate magnets, and diamond merchants knew all along that diamonds are frangible even without being soaked in goat's blood: what was new was the willingness of the educated to accept that sailors and merchants were better authorities on such questions than Plutarch and Pliny, and their insistence that in the end they must carry out their own tests to resolve the question.

Let me quote Lewens' review: "The great strength of Wootton's book lies in the demonstration that notions that we might today take for granted -- concepts like 'discovery', 'fact', 'theory', 'experiment' and so forth -- are by no means self-evident or inevitable. Nature does not present us with these ideas, which need only to be taken from their boxes and put to work by any competent observer. Instead, they needed to be constructed." He goes on, thinking he is making some sort of point against me, “In demonstrating in marvellous historical detail how that all happened, Wootton shows the value of the very idea he attacks: the relativism of curiosity.” But of course, that’s the point of my argument. Science is culturally specific, just as technology is; but it does not follow, as proponents of the strong programme think it does, that there is no such thing as discovery or progress, or that (as Breen comes close to arguing) we should take the side of the losers not the winners in scientific disputes, just as socialist historians took the side of the exploited classes, and feminist historians took the side of women. Socialist and feminist historians are seeking to undo past wrongs, or at least make their contemporary perpetuation more difficult; but it is not the job of the historian of science to rescue phlogiston from the judgement of history, for the simple reason that in the class struggle and the conflict between the sexes evil often triumphs; in science it is the good guys -- i.e., to use Kuhn’s terminology, the better puzzle solvers, not the better spouses or teachers -- who (if there is freedom of debate) usually win. (Hasok Chang wants to show this is untrue by going, as it were, back in time, rescuing failed theories, and showing that good science can be made out of them. If he succeeds then he will have demonstrated a very important point; but he hasn’t succeeded yet.)

Breen goes on to make some sort of political point: “To make matters worse, considering the central role he plays in Wootton’s argument, Columbus was not even the first European male to reach the New World, and, regardless, he steadfastly maintained that his voyages had brought him to the East Indies rather than to unknown lands... The early Iberian mariners were intrepid seamen, to be sure -- yet many of them were also religious fanatics, murderers, and slave-traders, a fact that Wootton’s triumphalist vision of their ‘discovery’ of already-occupied lands elides." Surely every high school student knows all this, and I can reasonably assume that every reader of my book knows it. What matters (for the argument of my book) is that according to the established geography and cosmology of Columbus's day there could be no Antipodes -- land directly across the globe from other land. Had Columbus sailed more than half way round the globe and reached China this would have been a remarkable event; but it turned out that, contrary to everyone's expectation, he had discovered land where there ought only to be ocean, a new continent where no continent could be. This is what chapter 4 of my book is about -- how the discovery of America destroyed well-established doctrines. This discovery was celebrated by Vespucci and by everyone after him by the use of a brand new word, the word "discovery." And it led to a new conviction that all significant knowledge was not already known by the ancient Greeks and Romans, that progress in knowledge was possible; this was a precondition for the new science.

My book does not celebrate Columbus; it celebrates the new culture of discovery. (I think knowledge is in itself a good thing, though its consequences, such as the invention of the hydrogen bomb are, as we all know, often dreadful.) Columbus, as I carefully point out, never became part of that culture of discovery; but his voyages brought it into existence. He did not intend to produce a new culture; but he produced one nevertheless. It is a simple error, one I try to avoid, to imagine that because *y* followed *x*, it necessarily follows that *x* was intended to produce *y*.

I can't help but point out at this point that there is a certain irony in Breen claiming to be on Butterfield's side when Butterfield insisted that it was no part of the historian's function to make moral judgements. Butterfield would have protested that to call the early Iberian mariners “religious fanatics and murderers” is to think of them in our terms not theirs, and is incompatible with writing good history. On this, as on other questions, I happen to think Butterfield was wrong; but it is far from clear why Breen thinks his own methodology permits him to pass judgement in this fashion. He certainly can’t appeal to Butterfield for support.

Of Weinberg, who attacks Aristotelian teleology, Breen writes: “if it is problematic to frame the natural world as following an internal logic that pushes it toward a certain end goal, then shouldn’t we pause to reflect when we apply a similar understanding to human history? Weinberg here [when he says he wants ‘to understand how science progressed from its past to its present’] marshals a teleological argument to refute Aristotle’s teleology.” There is a major confusion here. Nature as far as we can tell (without recourse to religious revelation) has no goal, and so no internal logic that pushes it toward that goal. But human beings do have goals, and both individuals and societies pursue their goals.

One of the goals of Western society for the last two hundred and fifty years has been ever increasing prosperity; that must be part of an explanation of why the West has become much more prosperous over time. Since Bacon, one of the goals of a significant group of Western intellectuals, a group we now call “scientists”, has been to develop a form of knowledge of nature which is capable of continuing progress. Bacon did not quite work out what that form of knowledge needed to be, but others soon did. They succeeded because they had a goal: their goal was to make new discoveries. We can’t understand their success if we don’t see that it was related to their goal.

The first move in seeking to understand how science has made such extraordinary progress over the last four centuries is to ask how and why people began to think of the knowledge of nature as being capable of making progress. And this involves grasping that medieval natural philosophers had no concept of progress, and Bacon and his successors had a very clear concept of progress. (This, I hasten to point out, is not teleological history, but rather what is generally thought to be its exact opposite -- understanding the past in its own terms. It is not "history" that has goals, but individuals. Note too that sometimes *y* follows *x* because that was what people intended should happen -- some consequences are intended and some are unintended.)

A little earlier Breen claimed that he wants (and, he mistakenly thinks, Shapin wants) to understand the historical trajectory of progress; but now he attacks Weinberg for wanting to understand "how science progressed from its past to its present". Shapin, to his credit, avoids this sort of self-contradiction.

Science, says Breen, "not only contains error -- it welcomes it." This is "something essential about science". Well yes. Quite so. And when did this become a key characteristic of the knowledge of nature? Aristotle, remember, believed that natural philosophy should consist of a series of syllogistic demonstrations. For Aristotelians there was proof, or persuasion; there was truth, or opinion. There was no scope for welcoming error as the basis for further progress. There were, for Aristotelians, plenty of disputed questions, but those disputes were never resolved; disagreement never led to new knowledge. The textbooks did not change from one generation to the next -- always Aristotle, always Sacrobosco. And so Aristotelians continued to teach as true statements that had been disproved centuries ago: that the nerves originate in the heart not the brain, for example.[1]

But in the late seventeenth century two new terms were introduced to replace "proof" and (logical or mathematical) "demonstration" -- those terms were "hypothesis" and "theory". In my book I show that they represent the emergence of a new concept of knowledge as defeasible; hypotheses and theories are inherently falsifiable (they are falsified by appeals to facts and evidence) and may eventually be replaced by better hypotheses and theories. (Newton was famously against hypotheses, by which he meant claims that were formulated in such a way that they couldn't be falsified; he was all in favour of theories). I argue that this new understanding of what knowledge should be represents "the invention of science". Breen notes that I have "etymologically inflected" chapters on "fact" and "evidence" which are "particularly original" -- but he seems to have failed to grasp the role they, along with the chapter on hypothesis and theory, play in my argument, and he doesn't acknowledge that he and I agree about the essential nature of science. What I am doing in my book is writing the history of how science acquired this "essential nature".

Note that when Breen introduces the category of "error" he is making a retrospective judgement, because error is generally recognized only after the event. It is difficult to see how one can write about error without writing what Breen lambasts as Whig history, history which portrays a journey from ignorance to progress.

---

1. Galileo tells the lovely story of the philosopher who was shown the path of the nerves in a dead body by an anatomist. Asked to confirm that they originated in the brain he answered: "You have made me see this matter so plainly and palpably, that if Aristotle's text were not contrary to it, stating clearly that the nerves originate in the heart, I should be forced to admit it to be true". (Wootton, *Galileo*, 27).

It is of course a central tenet of many historians of science that science doesn't work through the identification of error. The argument of Shapin's and Schaffer's *Leviathan and the Air-Pump* is that Boyle's experiments didn't do the work he claimed they did, and Hobbes said so -- but Boyle succeeded anyway. The argument of Schaffer's "Glass Works" is that Newton made all sorts of false claims about his optics experiments, and contemporaries pointed them out -- but his views triumphed because he had more social clout than his opponents. It follows (or it used to follow, for their views may have shifted somewhat, though if so they have never explained in quite what way they have changed -- remember the slippery "or" in Shapin's review of *Bad Medicine*), that someone doing history without the benefit of hindsight can never identify anything that might sensibly be called progress, let alone something so grand as the historical trajectory of progress. Indeed it used to be argued that Kuhn's doctrine of incommensurability showed that there could be no common standard which established that one paradigm was better than another -- but that argument has fallen out of fashion, and anathemas against Whig history have been mobilised to take its place. Again though, Breen is on my side of the divide. He finds error a useful category.

When Breen says that "something essential about science" is that it not only contains error but welcomes it, he is of course re-presenting a claim made famous by Karl Popper. And good for him; if only others would follow him. He ought now to go on and ask: Has this always been the case? If not, when did it become the case? How and why did it become the case? He would then find himself writing a book on the invention of science -- and if he wants to do that, and write a better book than mine, I'll be absolutely delighted.

But perhaps he doesn't want to write that sort of book. Perhaps he wants to write the sort of book that Shapin argued and still argues one ought to write. In which case here's a challenge: Write a history of the dispute between Galileo and the Aristotelians over why ice floats which is agnostic as to whether it was Galileo who was right or the Aristotelians, just as *Leviathan and the Air-Pump* is agnostic on the question of whether it was Boyle or Hobbes who was right (or sort of agnostic -- by and large that book argues that "Hobbes was right" [p. 332, see my book, pp. 548-9]). That would make interesting reading, and one would certainly learn a great deal from it about Aristotelian philosophy in the early seventeenth century, but it would not (in my view) be good history of science.[1] That's the crux of the matter.

1. Why not? Because the agnosticism as to which side was right could only be sustained right to the end by subtly improving the arguments of the Aristotelians (there's an approbatory term for this: "charitable interpretation") and weakening Galileo's arguments: if you didn't do that Galileo would win the argument. Galileo didn't convince his opponents back then because they were already committed to finding the answer in Aristotle -- you can explain this to modern readers, and you can explain to them how unfamiliar Galileo's style of reasoning would have been to his contemporaries, but you can't persuade them that the Aristotelians may well have been right and Galileo may well have been wrong, unless you put your finger on one pan of the scale. And if you define good history of science as showing that the Aristotelians were just as sensible as Galileo, then you oblige yourself to put a finger on the pan. Tim Lewens thinks I misunderstand the strong programme when I attribute cognitive egalitarianism to it; and Philip Ball thinks I misread *Leviathan and the Air-Pump* when I read it as expressing a relativistic view of scientific knowledge. I have few disagreements with their versions of the strong programme and of *LA-P*; but their versions don't seem to me to match up with the texts we are discussing, nor with the standard interpretations of those texts.

Breen thinks he is on Shapin's side, and opposed to me; but actually it turns out that his categories -- categories like "progress" and "error" -- are the ones I use and proponents of the strong programme oppose. The fact that he doesn't understand this is a sign of the confusion which now pervades the history of science. Every professional historian of science is opposed to Whig history, just as every accountant tells you to pay your taxes. But what is this thing called Whig history? And what exactly is wrong with it? I think Weinberg really *is* a sort of Whig historian -- he does overlook the complex interplay between success and failure, he does underestimate the extent to which science is culturally determined. He's a Whig, I might say, and I'm not. But it seems to me the category of Whig history (invented for quite different purposes, a very long time ago) is getting in the way of the need to make sensible distinctions, between retrospective and teleological history, between path-dependency and radical contingency, between the progress that science makes and the evidently mistaken idea that progress somehow comes naturally, and indeed between words and things (as in the dispute over whether an ancient Egyptian pharaoh could die of tuberculosis). It would seem evident that when, as I have done occasionally, I call myself a Whig historian, and Breen criticises me for being (horror of horrors) a Whig historian, the result is more heat than light. My critics are right: there is lots of scope for agreement. But if we are going to agree on what the history of science is or ought to be we need to make some careful distinctions, so that we are a good deal clearer on where we agree and where we disagree. Surely, we don't agree about everything; but at the moment what we disagree on is above all the nature and extent of our disagreement.