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Special Issue
Leviathan and the Air-Pump
After 40 Years: Reception, Criticisms and Impacts

Interview: Simon Schaffer

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Interviewed by
Ana Carolina Vimieiro[1]; Gabriel Ávila[2] and Mauro L. Condé[3] in May 2024

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Ana Carolina Vimieiro (ACV); Gabriel Ávila (GA) and Mauro L. Condé (MC): Tell us a little about your academic career. For what reasons and at what point did you decide to specialise

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in the History and Philosophy of Science during your degree in Natural Sciences at the University of Cambridge? Who influenced you? Were there any works that were fundamental to this choice at the time?

Simon Schaffer: I'm an historian of science now, so obviously I'm going to give an historical explanation. It's important for this question to understand that in the University of Cambridge, the natural sciences cover a very large number of fields. And in effect, this is still the case: it offers the only science course for undergraduates. So, if you want to study science, you take a course in natural sciences. This includes physics, chemistry, mathematics, bioscience and so on. The course lasts three years. In the middle year, the second year, it's possible to take history and philosophy of science as a minor option. And that's what I did 50 years ago this year. I was still then imagining at that point that I would end up almost certainly studying physics, because that's how I got into the university, and that was what interested me. But the introductory course to history and philosophy of science was very, very interesting for me. And I think it was interesting for about three reasons. The first reason was very selfish. It seemed possible to make something like original contributions when you were still quite young, as a student. In other words, there was not much doctrine that you had to learn. So, the presence of work in the humanities, which was framed around the possibility of doing something original, was obviously very interesting.
Secondly, no doubt, there was a small number of fairly fundamental questions about the sciences that it seemed possible to explore as an historian and a philosopher of science, including some questions of method which became very important for me, as we'll see later on. And also some questions of politics, which became very important for me, as we'll also see later on: notably questions to do with authority, reputation, obligation and so on. This was the middle of the 1970s. And in Western Europe at that moment, there were several major crises in politics, which obviously involved the sciences. There were several issues in the sciences that obviously had a political character. These were issues around what was then still called ecology, and energy policy; and there were also issues about political authority. There was a general crisis in Britain, a very specific one. We will not go into details because that will not be very interesting for your readers. But suffice it to say that it involved a general strike by the entire National Union of Mineworkers in 1974, which was politically highly important in this small country in north-west Europe. The issue of energy and politics and employment was obviously very important.

And the third reason, I think, was that there were some rather charismatic senior members of the department, whose work I found at the time difficult to understand but seemed clearly important. Training in history and philosophy of science 50 years ago in Cambridge was pretty canonical. Historically speaking, it was mainly organized around two crucial periods or conjunctures. One was what was then called the Scientific Revolution: the period from Copernicus to Newton, especially in the physical sciences and astronomy; and secondly, the period of the mid-19th century and the coming of Darwinism. And as it happened, one of the more charismatic teachers at the time, whose courses I audited, but I didn't take – I wasn't then a student – was a very influential figure Robert M. Young, who was working on Darwin and Darwinism. And eventually he published a book, called *Darwin's metaphor* (1985) which came out in autumn of 1985. And that was an extremely important book for students, especially the final paper in the book. This was "The historiographic and ideological roots of man's place in nature" which was, on the one hand, a very interesting meditation on how best to understand the coming of Darwin's work; and how best to use the understanding of mid-19th century British culture and politics. Especially, I would say, the paper addressed the role of Thomas Malthus and the construction of Darwin's views.

And on the other hand, Young's huge chapter was an analysis of the politics of studying the history of science, which included a long reflection on historical materialism, and it included some quite fierce comments on the structure of the department that both he and I belonged to. So that was very interesting. Secondly, that chapter had first appeared as Young's contribution to a very important book which was the *Festschrift* for Joseph Needham. That book was called, *Changing Perspectives in the History of Science,* and it appeared in the spring of 1973, the year before I started my studies in history and philosophy of science. And it included contributions from many of the most eminent historians and philosophers of science working at the time, both in Britain and in the United States, and indeed elsewhere in Europe. The *Festschrift* also helped us understand what Needham had been doing in his huge project on *Science and Civilization in China* (1954-2004). The *Festschrift* included, magnificently, an autobiography by Joseph Needham, written under a pseudonym. Needham wrote his autobiography, but changed the name of the author, so that it didn't quite look as though it was an autobiography, but we all knew it was.

And the title of the autobiography is magnificent, "The Making of an Honorary Taoist", a great title, which was an extraordinary reflection on the relation between Chinese and European philosophy, and Chinese and European technoscience, as we would now call it, a theme we'll come back to. And of course, it included reflections on Marxism, which was very important for Needham. The third figure whose work began, I think, to matter more and more to me, and still does, actually, was a real historian. It's worth remembering that Needham began as a biochemist, not as a historian at all, while Bob Young was trained in science and then philosophy. The real historian in Cambridge at the time, who was interested in the history of science, was a very brilliant young historian called Roy Porter. Roy had written very quickly, as was his wont, his PhD thesis on the coming into being of geology as a science, a book he published under the title *The Making of Geology* (1977). That was a fascinating book for us, certainly for me. It didn't appear until 1977, but it was the basis of his lectures. It was ruthlessly historical. In other words, it asked questions you might have thought were scientific, as though they were historical: what is the relation between eighteenth-century British culture and being interested in questions about the history and structure of the Earth, for example. So those I think were some of the influences.

I was extremely well supervised: sorry, that's Cambridge jargon. In Cambridge, undergraduates attend lectures, and every week they have to write a number of essays, then sit with someone and talk about the essay they've written, and the person they sit with is not called a tutor, but a supervisor. In the Cambridge system, the supervisors are the graduate students. I was extremely lucky because the graduate students who supervised me were really good, and later on became very eminent indeed. Finally, I should mention that there were two philosophers of science, both of whom Mauro [Condé] will know: Gerd Buchdahl and Mary Hesse, who were both teaching in the department at the time. They were both extraordinarily good teachers. Gerd was a Kantian of the most ferocious kind, who would later produce a very large and for us extremely important book, called *Metaphysics and the Philosophy of Science* (1969), which appeared in 1969. But that formed the basis of his lectures, and they were very powerful lectures indeed, and introduced us to the idea that there was an integration between the history of science and metaphysics. And Mary Hesse's work, which of course is extremely well known, was the principal way, and it is interesting to think back to this, in which some historicist analysis of the sciences began to appear in the curriculum: notably, I would say, the work of Kuhn. So, it's interesting the historians of science didn't talk about Kuhn at all, while it was Mary Hesse who talked about his work much more. So, this was a very strong course with some very charismatic people. And in my final year I decided that's what I was going to do and I spent the whole year working on that material. I wrote my finals dissertation. Interestingly to recall, on the search for longitude at sea. I also decided that I wanted to do at least one more year of study, which was very difficult to do in Cambridge. There wasn't really a master's course in this field at that point, so I knew I had to apply somewhere else. I was lucky enough to get funding to go to Harvard in the autumn of 1975. This convinced me that this was probably what I was going to be doing.

Gabriel Ávila: I just want to know, quickly, if this insistence on the making has something to do with Thompson's work.

Simon Schaffer: Oh, that is a great question. In retrospect, yes, though at the time, no. If you mean the titles like *The Making of Geology*.

Gabriel Ávila: Or 'Making of a Taoist'?

Simon Schaffer: Yes: I think that it's overdetermined. On the one hand, clearly, yes, within the problematic that Thompson was addressing in *The Making of the English Working Class* (1963) there was, after all, an attempt to get away from structuralism, to try and work out ways of reintroducing historical process into the analysis of the formation of social systems.

Gabriel Ávila: By Culture and experience, right?

Simon Schaffer: Yes. So that, as we know, this was important for Thompson, especially in that book, and then even more so later on in the collection of papers that's now called *Customs in Common* (Thompson 1991). So, at the time, you know the essay on 'Class Struggle Without Class' (Thompson 1978), the essay on 'The Moral Economy of the English Crowd' (Thompson 1971), and so on. All of those were about class as a form of experience, and there's no doubt that sensibility was shared in interesting ways by some other historians, like Roy Porter. However, one should not exaggerate the role of that sensibility. The group of historians to whom Roy Porter belonged were a group of historians, mainly influenced by a liberal and pretty classical English historian, J. H. Plumb. His students included figures like Simon Schama, Quentin Skinner, Barry Supple and John Brewer. These are not, by and large, people who were in exactly the same camp as Thompson. So, the presence simultaneously of Needham, Young and Porter defined a very particular set of interests that were not entirely historical materialist, to put it mildly. A note there might be worth adding. There's an extremely brilliant analysis of the way in which history of science developed as a field in Cambridge from the middle of the 20th century onwards. The analysis is by Anna-Katherina Mayer. She mainly published in the journal *Studies in History and Philosophy of Science*. [Mayer 2000, Mayer 2004].

And Anna Mayer's analysis makes at least one extremely important point, which is that Needham had just before and then certainly after the Second World War played an extremely important role in the institutionalization of the history of science as a possible course for Cambridge students. However, Needham's argument in the mid-20th century, very interestingly for us, was that the history of science was far too important to be given to historians. Historians, so Needham argued, are conservatives, idealists, reactionaries; whereas natural scientists are materialists, progressives and several of them, of course, were Marxists, like Needham and Desmond Bernal and so on. So, the history of science ended up being taught to scientists, not in the history faculty. There's an irony there, when you ask about Thompson, which I find extremely interesting. And we were, as students, very well aware of that tension. Because our textbooks were written by figures like A. Rupert Hall, who was the principal opponent of Needham's view. And it was he, Rupert Hall, who presented us with the official doctrine about the scientific revolution, in his textbook [Hall 1954]. And certainly, Rupert had absolutely no time for any materialist explanation of scientific change. So when I said at the start that there were political issues in play, it was obvious that there were political issues in play.

ACV; GA; MC: You recognise science communication as an essential part of your career, having worked in science museums and broadcast on various television and radio programmes on Channel 4 and the BBC, such as 'The Day the World Took Off' and 'Light Fantastic'. You even say that your involvement in science communication has inspired new and good ideas.

Specifically, how did this type of activity contribute to your historiographical production? In your opinion, what role does the popularisation of the history of science play in the training of scientists and science education?

Simon Schaffer: This is a very difficult question. However, I will try to answer it. In terms of my own biography, as I'm sure you will be very aware, becoming involved in broadcasting is essentially random, not an aspect of policy. I was just lucky, basically. There were one or two connections that initially helped me get the opportunity to become involved in broadcasting from comparatively early. So, the first television programs that I was involved in were produced in 1980, when I was 25. I presented a television program about Isaac Newton for Channel 4. That was just luck - it would be silly to call that policy. Not only that, but of course, a formal public broadcast is possibly the most inefficient way of communicating an idea or an argument ever invented. It takes an extraordinarily large amount of money and a huge amount of time to make programs like that. And that was even more true 40 years ago than it is now, because everything was analog, including in particular editing, which was done, let's not forget, with scissors: so, not efficient, very slow, extremely time-consuming, maybe not to be recommended. On the other hand, it's very obvious that if the audience and interlocutors for our work are only our colleagues, then it's a waste of time.

History of science is a public activity or else nothing, in my view; partly because I still think that its concerns are extraordinarily important in terms of public life. And we'll come on to that later. Getting it wrong has very major effect, as we know. So, it does seem to me to be important to take part in enterprises which, ironically, it's extraordinarily difficult to join. And it's become, bluntly, more and more difficult to take part in these enterprises, at the level of public network broadcasting. It's no longer difficult to broadcast. It's extremely easy now. At least in industrial society, broadcasting is an extraordinarily widespread activity, for good or ill. That was not true in 1980, nor even to a great extent in 1990. It is true now; and in some respects that's clearly a very good thing. Obviously, there were no blogs, no webcams, no Zoom and there was no feasible form of digital recording: and so on. There was no email, no Spotify and no electronic dialogue or social network. And now there is.

Once again, one has to put this into a material historical context. The complementary question is much easier to answer, which is that once I got involved in thinking about how to make my arguments work on television, on film, I was forced, whether I liked it or not, to see how to make these arguments dramatic. That meant making sure that there were events and objects and material systems that could be shown and seen. So, the single most important effect, for example, of making a TV show about Isaac Newton or about the railways, or guns, or optics, or automata (which is my favorite show), was that it drives you towards material culture, and towards labor, because that can be filmed. And it tends to block certain idealist and over-intellectualist intuitions that you might otherwise have - because there's almost nothing so boring as turning on the television and having someone who looks like me talk. That is not a television program worth watching. What you want is precisely making, and especially the dramatic iconography of making. And that's what I learned. And of course, that then feeds back into the kinds of approach and analysis that the historian of science will offer. So, all philosophy becomes praxis.

Finally, I was extremely lucky again because I happened to get the chance to work with three or four really good directors: notably, David Dugan, who produced *The Day the World Took Off*. He's an extraordinarily brilliant television producer. And I'd especially want to mention Paul Sen, who is an absolutely outstanding TV producer and director with whom I've worked for 25 years now on various projects and programs. These were people who, on the one hand, absolutely understood the need to get away from what in English we call the 'talking head'. And on the other hand, they were prepared to let me do more or less whatever I like doing. Neither David nor Paul were much interested in filming before nine in the morning, which was great because there are some TV producers who are so concerned with the light that they start at sunrise. I do not start at sunrise.

ACV; GA; MC: In the introduction to the 2011 edition of *Leviathan and the Air-Pump*, you mention that in the construction of the book, the technology of the typewriter was an essential device for making historical knowledge of a certain kind. To remember how long ago and with what type of writing technology the book was written and published, could you tell us how four hands put the book together? How did you go about researching sources? What was it like to write the book collaboratively using the writing technology available then? When the book was being produced, you were at Imperial and Steven Shapin was at the Science Studies Unit in Edinburgh. So how did the intellectual Interlocution take place to develop the analyses in the book, and how important was the intellectual atmosphere of the two institutions in defining the interpretative lines of the work?

Simon Schaffer: There's a very close relation within all these questions. Partly, this is just biography. In 1981 I was hired at Imperial College in London, the main science and engineering institution in London. They had at that point one or two posts in history of science and technology, and I was lucky enough to get one. There was no Department of History of Science and Technology, so I was basically on my own. There were no PhD students that I was supervising. There was a very small master's course, and there were no full-time undergraduates. So, the teaching load was extremely light in comparison with what was about to happen.

I had a lot of time; and I was in London, which has extremely good libraries. Not just the British Library, the national library, but in particular, I'd mention that Imperial College is next door to the Science Museum in central London. At that stage the Science Museum Library was part of Imperial College and the Science Museum Library, which was first assembled in the middle of the 1800s, initially based on the library of the Patent Office, was and is an absolutely fantastic library of the history of engineering, the history of technology, the history of scientific instruments, obviously really important for the book. So, in terms of the research resources, it was amazing. One should also add, that it's not just that we had typewriters. Again, we were in an entirely analog world. There were no digitized sources. It was impossible to do what one can do now. If you wanted to access, for example, seventeenth-century printed books, you had to go to the library. There was no other way of doing it. It was extraordinarily undemocratic, elitist, difficult and time-consuming in comparison with the situation now. It's very important to remember that when one's thinking about the condition of work that we were pursuing. Because what we were pursuing was a pretty detailed set of readings of mainly primary printed material, from the middle of the 1600s in English, Latin and French.

In contrast, now almost all of that material is online and can be consulted in a matter of seconds. In those days, it took about three days. So, it's not just that we had typewriters - we didn't have the web. Steve did his PhD in Philadelphia at the University of Pennsylvania. Then he got briefly a job at Keele University in England, and was then hired into the Science Studies Unit, where he worked for most of the 1970s. We met because he was one of the examiners of my PhD, in November 1980. The other examiner was Roy Porter. Steve and I got on extremely well and by the summer of 1981, he and I had decided to write an article, a short article about the fight between Robert Boyle and Thomas Hobbes. Steve was writing a book, which was initially to be called "The Moral Use of Nature", in which there would have been a chapter on Hobbes. So he had become aware, very significantly, of the existence of the controversy between Hobbes and Robert Boyle, and that there was amazingly little study of this controversy, and that it seemed to be extremely relevant and interesting. So, we began to correspond. We began to write letters to each other. We've still got several of the letters.

The first important letter, I think, is dated August 1981. There's one sheet typed by Steve, which sets out the idea that we might write an article; and then there are seven pages of typescript where he set out what the outline of the article would be, and more or less explaining what he would do and what I would do. That was the summer of 1981, and the book was more or less finished three years later. So, it expanded quite fast a lot, and in very unexpected ways, as letter-writing does. Steve decided that in the preface to the 2011 edition, it would be a very good idea to mention the typewriter. There are two reasons for that. Firstly it's true that the typewriter was no doubt the most important machine in our relationship. And secondly, our students have forgotten what manual typewriters are like. They have several features that affect the way in which the work proceeded. And since this illustrates one of the claims of the book, which is the immense role of material technology, it's perhaps worth dwelling on that. In the case of a manual typewriter, if you wish to change something, you have to retype. So, you're invited completely to reconstruct a text, if you want to change it at all. So, it's kind of inevitable that the text is going to expand.

By definition, if there's something in the middle that you are no longer happy with, you have to retype the whole thing from the beginning. And we kept the post office very busy. Every so often, I would visit Edinburgh, and stay in his extremely comfortable apartment. And slightly less often he would visit London because my apartment was not very comfortable. It was also extremely cold. In terms of the culture, that which mattered was the Science Studies Unit, an absolutely remarkable outfit. As you'll know, there were basically four people employed there: David Edge, who was in charge to a certain extent, former radio producer turned historian and sociologist of science, an authority on radio astronomy; David Bloor, who trained as a psychologist in Cambridge, who'd already published one of his masterpieces, *Knowledge and Social Imagery* (Bloor 1976); Barry Barnes, who was a real sociologist, and who was extremely prolific and very good indeed at making arguments better and clear, very much in dialogue with critical theory, on the one hand with Habermas' work and with anthropology on the other, especially structural functionalism.

And then there was Steve, who'd been trained as an historian of science at Penn, and whose PhD was on, interestingly, science in Edinburgh in the 18th century. So, it was a kind of return to the field when he moved to Edinburgh. And he was the historian in the group. We tried out bits and pieces of this article at the Science Studies Unit. There was already a small group of extremely brilliant PhD students, many of whom would later become very eminent, practitioners of science studies like Andrew Pickering and Donald McKenzie. So that was the culture. There was also, as I have mentioned in various other interviews, the importance of a meeting at the University of Bath in March 1980, in effect the first time that the British Sociological Association had held a meeting on the sociology of scientific knowledge, and it was organized mainly by Harry Collins. I went to that meeting when I was still a PhD student. That's where I first met Harry Collins, David Bloor, Steven Shapin and so on. So that was very important, partly because it also brought the work of Harry Collins and his collaborator

Trevor Pinch into dialogue with the kinds of questions that we were interested in in this article.

Gabriel Ávila: Very good, very interesting biography, but I have a little curiosity about this 2011 edition. This edition lacks the translation of *De Natura Ars* that you have made included before. Do you think this is just an editorial choice, or has something to do with the changes of the field between 1985 and 2011? And this maybe, has to do with the next question about the formation of the historian of science.

Simon Schaffer: The French edition (Shapin and Schaffer 1993), which was in effect organized by Bruno [Latour], did not carry the translation either. So already in 1993 it was decided to leave out all of that stuff. Pretty quickly it was decided, to put it bluntly, that this is a book within science studies, rather than simply a work of history.

Gabriel Ávila: Oh, yeah. This kind of historical erudition involved in translating.

Simon Schaffer: Lots of readers treat it like some kind a manifesto; when in fact it was supposed to be an exercise, just as you are about to point out. The relation between an exercise and a manifesto is very interesting. So, I feel and I know that Steve feels, that the book should be read as offering a case, in every sense of that word. It's supposed to be an example: It might be a terrible example or a good example. But it's become something quite different from that, no doubt. Yes, you ask a very good question. To put it very straightforwardly, the historical apparatus in the book has subsequently come to seem to many readers unnecessary, while I don't think it's unnecessary.

Gabriel Ávila: No, it's not unnecessary. Do you think this have to do with Latour's own view of the book, his use of the book.

Simon Schaffer: Well, that was certainly one very important use. I absolutely agree with Bruno Latour that meaning is in the hands of future users. It would be inconsistent for me to complain too much about the hermeneutic shifts of a particular text, and it would certainly be inconsistent to claim that the author's view is the only acceptable view. However, I can certainly report that this was a book written as an exercise. And as I've just indicated, it was initially written as an article, not even a book. So that seems important.

ACV; GA; MC: The book *Leviathan and the Air-Pump* was conceived as "an exercise in the sociology of scientific knowledge" (Shapin and Schaffer 1985, 15), but it ends up making peculiar use of the work of the later Wittgenstein in a much broader way (Shapin and Schaffer 1985, 18, 20, 22, 49, 51, 67). To what extent is the book an epistemological expression of the strong programme? Considering the epistemological controversies that the book has generated, would not this book be an excellent history of science, but without an equally robust epistemological thesis?

Simon Schaffer: The answer is very simple: it wouldn't. The book explicitly announces that it's an exercise, an exercise in sociology of scientific knowledge. This implies that it's an empirical exercise, that it requires a great deal of detailed investigation. The aim of the original article, like the aim of the book, frankly, is to investigate the grounds of obligation, and the book says so. How is it that it's supposed that one is obliged to accept experimental accounts of the world? Where does that idea come from? Is there a history of that claim? And what are the implications? This is the second-order question of asking that as an historical interrogation. If you ask that historical question, you are already in the world of the *Philosophical Investigations*. You're not only in the world of the *Philosophical Investigations* (Wittgenstein 1953 [2007]) but also in the world of the *Genesis and development of a scientific fact* (Fleck 1935 [1979]).

So, this is for Mauro [Condé] now. You are in the world of language games and forms of life. You are in the worlds of *vade mecum* science and in the world of thought collectives, as Fleck put it. Because one has asked the question that according to some cannot or should not be asked. Otherwise, that can't be a legitimate historical question about grounds of obligation, of that form. How could that form of obligation possess a history such that you could write it? So, consider the question, when considering these controversies, as to whether it would be an excellent history of science without an equally robust epistemological thesis? It wouldn't exist without the robust epistemological thesis, because of the historical question to which, for good or ill, the book attempts an answer. It's obviously, to a certain extent, a Hobbesian question. What is the historical basis of obligation is the question to which *Leviathan* is the answer. The artificial man, to quote Hobbes, is the response to that question. It's an historical answer, based on the theory of this original contract in which subjects apparently entered. And so, the question about obligation has this, Steve thought, rather neat and rewarding reflexive quality: it's a question about the views of the people we were going to investigate.

It's also a question that presses on the condition of possibility of the existence of the book itself. The preliminary question was to ask to what extent is the book an epistemological expression of the Strong Programme – I don't think so. The book certainly uses techniques that had been set out in the Strong Programme; it uses them a lot, such as the symmetry principle, for example, which is very often wrongly interpreted in my view. As *Leviathan and the Air-pump* applies the symmetry principle, this is the maxim that the same kinds of explanations should be given for views about the world, whatever one's current attitude to those views. And that meant that this would not be a book that would adjudicate on the outcome of the issues between Boyle and Hobbes about what's going on inside mid-seventeenth century air pumps, or whether there were such a thing as the void. We were not going to take a view on that, for explanatory purposes. Similarly, the principle of causation was used throughout the book. That is to say, whatever the view, we took it as our job to give a causal account of the views and practices that these dead people engaged in. And that raised issues about of the sociology of scientific knowledge as an historical inquiry that went beyond what was in the strong programme.

So, it's obvious, or so it now seems to me, and it was presumably obvious at the time that the book was also using what was called the Empirical Program of Relativism, that's to say, methods due to Harry Collins, Trevor Pinch and others, so not at all just the Strong Programme. For example, the considerable emphasis in the book on replicability and replication as an object of study, because replicability, which is not the same as replication, has very often been taken to be what it is that grounds obligation, in the case of the sciences. If an experimental technique works elsewhere successfully one is apparently obliged to suppose that it works everywhere. Call that the sociological solution to the problem of induction. There is much in the book about that question: there are maps, diagrams and stories which spend a lot of time doing that. Now, one reason, as I say, is clearly epistemic. Replicability is supposed to drive obligation. One of them is methodological. It's important to remember that a claim made by the empirical program of relativism, as it was brilliantly developed by Collins, is that you must, as a sociologist of scientific knowledge, be in a position where you are following a controversy in the sciences before it closes. Or, as Collins put it, you have to look at the ship before it's inserted in the bottle. Otherwise, you have what we used to call a catastrophic crystallization of certainty, through which you can no longer remember that there was any doubt about the outcome or the matter of fact.

Now, if that's true, if it's the case that successful sociological analyses of passages of scientific action have to follow the science as it's being made. You can't do what *Leviathan and the Air-Pump* was doing, because we're too late. All the ships are in all the bottles. It's over. Let's not forget the book took longer to write than the events that it describes. I mean, the book took three and a half years to write, but it only describes eighteen months, in Restoration London and Oxford. So, one way in which it's an exercise is that it's an attempt to see if it's possible to forget. It's extremely ironic or dialectical, depending on which words you prefer: this is a book, you could say, mainly about oblivion, about unlearning. It's about adopting a position that is very difficult to adopt. Which is why the preface begins with a reflection on playing the stranger.

The task of this kind of history of science is like the task that Novalis describes as the task of romantic poetry, which is to make the familiar strange and the strange familiar. That's another version of the symmetry principle. Something very strange, at least very strange for history and philosophy of science, is Thomas Hobbes' position: experiment is not philosophy; the world is full; the air-pump is not a demonstrative device; we only know what we make. All of those claims are strange, and we were trying to make them familiar. Consider what Robert Boyle says, which is experiment is the only reliable ground of authority; that an experiment that is witnessed and replicable can be taken as an unproblematic standing record, his phrase, which we are all obliged to accept as the premise of reasoning. We wanted to make that seem odd, or at least worth asking about. And that is what I mean by saying the book is about oblivion, about trying to forget the two things that I'm sure we all know. Which is that we know that Boyle is right, and Hobbes is wrong.

ACV; GA; MC: Still considering *Leviathan and the Air-Pump*, Shapin and you analyse the socio-technical devices that regulate the conventions necessary for producing experimental knowledge. These devices discipline belonging to a community, access to the laboratory, the ability to carry out experiments, etc. It is a narrative that recounts a fundamental passage in the history of modern science in terms that come close to describing the technoscience that was emerging at that period. To what extent did you realize at the time that you were attributing a past to technoscience?

Simon Schaffer: We certainly realized this. It was not a coincidence that Steven Shapin developed the vocabulary of technologies. If you consult the article that he published in the middle of all this, in 1984, "Pump and Circumstance: Robert Boyle's Literary Technology" (Shapin 2010), a brilliant article it is. He was already developing the idea of three related technologies and the choice of the term "technology" there was quite deliberate. It's a real choice, partly to draw attention to the artful and as we might say (I don't know how this works in Portuguese) the artisanal quality of the skill and the technique that is in play in the work that we're discussing. So, this is *techne*; and *techne* is very often, as in Plato, contrasted with *episteme*. This is exactly the point that enterprises that had classically belonged to *episteme* are here treated as technique, as practice, cunning, the world of skill and the artisan.

In a way, you could summarize the whole argument of the sociology of scientific knowledge as it was in the later 1980s, as the claim that scientific activity is fundamentally the activity of artisans, basically technical. In some respects that itself has been connected, for me, with the work in broadcasting. As I explained, those projects raised the importance of machines, labor, the labor history of knowledge and so on. For Steve, it was absolutely about the work that he was very soon going to continue and pursue: his work on invisible technicians, published in 1989; his work on "The House of Experiment in Seventeenth-Century England" (2010b), published in 1988; his work in *A social history of truth* (Shapin 1994); and so on.

Yet there is a fundamental distinction between the idea that this kind of enterprise is to be understood as technique, as artisanal, embodied, tacit and in class terms as essentially relying on a workforce whose presence or not is recognized or not; and, on the other hand, the notion of technoscience. When the notion of technoscience became fashionable and real in science studies, its chronology was completely different. Consider Ursula Klein's very important book on the topic, *Technoscience in history* (Klein 2020). This crucial analysis would date its institutionalization to the period of the late 18th through the early 19th centuries. She identifies the systematic alliance between organized inquiry, industrial production, artisanal experience being expropriated. This is the period of big new state organizations in Paris or Potsdam or Saint Petersburg, the worlds of mining in Central Europe and Scandinavia and in Potosi and in Mexico or the worlds of chemical manufacture, or you might say, above all the worlds of metallurgy, mineralogy and metal working and so on; and then finally the worlds of textiles. So, there's something very odd going on here: not about the attribution of a past to Technoscience, but the issue of what kind of past is being attributed to it. So, it seems to me that one question that the *Air-Pump* book raises about that chronology is, to put it bluntly, what happens to the artisans? This is to pose the issue in economic and class terms, and is certainly not a new question. For example: the history of the air-pump in *Leviathan and the Air-pump* stops in that book when it was possible to buy an air-pump. Once one could be bought from Francis Hauksbee or from the Dutch makers, notably the Dutch, I would say our story is over.

Because in a very interesting way, the market solves the problem of the experimenter's regress. The problem of the experimenter's regress is, in summary terms, that if I get a different result from you, it might be because I'm not copying you properly; but it might be because you're wrong; and I can't tell which. I can, however, tell which if we buy machines from the same shop. Then it is as though there are available reasons with which to control the problem of obligation. It's of course a convention, not stronger than the other forms of discipline, but it is a form of discipline located in market relationships. We might say it's a form of discipline located in the social relations of technoscience, and that the role of the instrument market in the story that we were telling is very important indeed.

ACV; GA; MC: Throughout your career, the requirements and skills required to train a historian of science have changed a lot. Which of the skills required by historians of science today were overlooked during your formative years? And what kind of skills and knowledge have been lost and are missed by new generations?

Simon Schaffer: Well – in terms of the skills that were overlooked, obviously, as I've just said, there's a close relation between the skills that a community thinks are necessary and the questions it thinks are worth asking. This certainly applies to football teams - even in Brazil. So, if you know what you need to do, you know what skills you have to learn, and vice versa. So, if one thinks about these three communities, France, Britain and the United States in the 1970s and 1980s when I was being trained, the most obvious absence was the location of knowledge and technique anywhere outside Europe. And it's extraordinary to think how much that was overlooked. I underwent eight years of training in history and philosophy of science, and don't think it was suggested I read anything of consequence in the field written by anybody from outside Europe and North America. With the exception of Needham's work on China, I wasn't told to read anything significant about what was happening or what had happened outside Europe and North America. Insofar as there were extra-European materials, they were traditionally confined to events of previous millennia, not even previous centuries. Eurocentrism was the most obvious feature of what was overlooked. None of us were provided with any usable skills or resources to deal with that.

It was rather as if other languages, other traditions, other information, other forms of reading, other kinds of questions didn't exist. And as we know extremely well, even in the Needham case, the whole point was that Needham was saying that China had been massively in advance of Europe in the field of technique, but not in the field of science. By 1550, so Needham claimed, Europe had caught up. And since then, so it seemed, you only really needed to study Europe, with the exception of medicine. This was a highly interesting exception, because Needham was a huge admirer of Chinese medicine. But apart from that, if you worked on the sciences since 1550 or even the technology since 1550, you didn't have to know Mandarin. So even that didn't really change the conversation or the skills. And in a very similar fashion, one could say just the same thing about women's work and achievements. In the middle of the 1970s, obviously, there were very important feminist voices in the field. But I was not taught them; certainly not, for example, by Mary Hesse. It is interesting to think back. Donna Haraway was a close interlocutor of Bob Young, for example. And her earliest work is, you know, in the 1970s. This is the work that would become her really important studies on crystallography and biosciences [Haraway 1976], and then eventually her book on *Primate Visions* (1989). And there were some very important writers on women, feminism and the sciences, very important writers indeed, from the 1950s and 1960s onwards if not earlier. But they were not on the curriculum: at least not on mine, in Cambridge, London, Harvard or Paris.

So those would be two absolutely obvious sets of skills and questions. What kind of skills and knowledge have been lost and are missed by new generations? I'm tempted to say you'd better ask them. Interestingly, some commentators say two things that point in opposite directions. One is that current history of science and science studies is too little engaged with the sciences. That is clearly not true. It's an issue about what counts as engagement, what counts as the content of the sciences, and what counts as the externality. That's our topic. In my view this can become a viciously circular argument. In any case, when I look at the work of people who are doing their PhDs now, there's massive engagement with the practice, argument, beliefs, conduct, and organization of the sciences.

At the same time (and sometimes it's the same critics), there's also the claim that there's not enough training in philology, in hermeneutics, in basic historical skills. That's an extraordinary irony. As I've said, when I was being trained, there were few historians of science in Britain who were trained as historians: almost all were scientists who'd become interested in the past and in the philosophy of science, not the other way around. Mary Hesse was a mathematician, for example, by training, and a very good one. So was my supervisor, Michael Hoskin. He was a mathematician, by training. So, there were very few historians of science who had begun as historians. Rupert Hall, who had a formal training as a professional historian, was extremely unsympathetic to historical materialist approaches to the sciences. That was how the dialectic worked.

I think it's quite hard to answer that question. Disciplinary order is purchased at the price of amnesia: disciplines work by forgetting. Disciplines' members do not always nor frequently have to recapitulate why the questions they now find pressing and urgent are indeed the questions they find pressing and urgent. Many of the issues and questions that concerned us 50 years ago are no longer pressing and urgent but in many cases they're not even remembered. As a result of which, reading that material, insofar as anybody does, is somewhat hard because it's an answer to a question no one now remembers. For example, a further, third example of material of which we seem to have been utterly unaware of in the 1970s, a huge irony, was the climate crisis. There was a large amount of work going on the history of ecology, especially on political ecology, but it was not being conducted under the sign of the climate crisis. In other words, it did not include the physical sciences. If you were an historian of physics (my PhD was on Isaac Newton), one apparently didn't have to ask nor answer any questions about the history of climate and of climate science.

ACV; GA; MC: In recent years, we've seen the rise of denialism and disinformation to the centre of the political arena – as a tool in the political struggle, in fact – in cases like the Brexit referendum. At the same time, the reflection in the scientific community has been a kind of epistemological retreat, almost a return to an atavistic positivism. Do you see a role in the history of science in this context?

Simon Schaffer: Yes, absolutely. Fortunately, it's not just a task for the history of science, otherwise we really would be in trouble. It's worth remembering there are very few of us. We're not talking about a large community of professional specialists here. I'll say three things about this, all of which you know more about than I do, especially given where you live and what you work on. Firstly, there is an absolutely intimate relation between those two developments, in other words, widespread denialism and widespread dogmatism often amount to the same thing. That is a seventeenth-century idea. So the idea that skepticism and dogmatism are two sides of the same coin is absolutely an argument of mid-seventeenth century European philosophy, in Britain, France and the Netherlands. The argument is that if people have a ridiculously exaggerated set of criteria for truth, then they won't believe anything, because no human knowledge reaches that condition. So, you could argue that it's precisely exceptionally dogmatic Darwinism that goes along with creationism. Because if you're extremely dogmatic about evolution by natural selection, then it might be easier to show that you're mistaken. The same applies to a very wide range of skeptical and denialist positions : a position akin to a plague on both your houses.

Or secondly, you could take the view that, for example, Steve Shapin has urged brilliantly, which is that in fact we don't live in political cultures of skepticism, but in political cultures of distributed dogmatisms [Shapin 2004]. It would be better if we lived in a world in which most people were somewhat skeptical and doubted things. Yet we don't. We live in a world of absurd conspiracy theories and absolute faiths. These are not people or views that contemplate any doubt. They've taken the red pill. They know exactly what is going on. And it's not, therefore, that we live in a world of peculiar refusal to be obliged; but instead in a world in which everyone wishes to be obliged. We live in a world in which there's an extraordinary amount of certainty. It's just distributed. That's the second possible response. Notice that both those thoughts, the thought that dogmatism and skepticism are essentially the same thing; or the thought that everybody is dogmatic and we have an insufficiently large numbers of skeptics; are both issues where historians of science have a lot to say and a great deal to do, which is to construct accounts of workable knowledge neither dogmatic nor skeptical. And from that point of view, it's not that the sociology of knowledge is relativist, but that it's pragmatist. I mean, these are the views that one wants to associate with writers like James, Dewey and Peirce, who were writing in very similar times and in a very similar society, that's to say, Gilded Age America.

And the third thing to say is that, raises a peculiarly strong irony. The sociology of science and sociology of knowledge are attacked for claiming that decisions about knowledge are decisions about social and political order, while at the same time those people doing the attacking are also claiming that decisions about knowledge are decisions about political and social order. In my little country Brexit is a pretty good example. If you want an example of how a solution to the problem of knowledge is a solution to the problem of social and political order, the Brexit referendum is an extremely good example. I wish it wasn't quite such a good example, The usual principle applies: there is systematic confusion between description and endorsement, along with the complexity of the position of critique.

On these matters the great, late and much-lamented Bruno Latour, my friend, had very important things to say indeed, about how knowledge and politics relate to each other. He helped us see how the powers of politics and of the sciences rely on a remarkable principle that insists that they're separate and yet that they're simultaneously related. Following Michel Serres, following the La Fontaine fable of the Wolf and the Lamb, Latour reflected on the moral that "la raison du plus fort est toujours la meilleure", that "the reason of the strongest is always the better" [Serres 1982]. And the Point of the Wolf and the Lamb fable, as Michel Serres and Bruno Latour, in their different ways, both pointed out, is, after all, that in order to eat the lamb the wolf gives reasons. It is what La Fontaine at the fable's end calls a trial, a process. The wolf doesn't just eat the lamb; rather, the wolf invents a kind of third position, upstream, which he claims is initially occupied by the lamb. The lamb says, no, actually I'm downstream. And then it's occupied by the lamb's elder relatives, and the lamb says I don't have any. And then it's occupied by the shepherd and his dogs: and then the wolf eats the lamb. The key to that fable is that reasons are given. It's not entirely arbitrary, but it's the arbitrariness of a trial, a process, where reason and force are at stake. That does seem to me to be a good way of capturing our current predicament. Now I'm done.

Ana Carolina Vimieiro: Thanks a lot, Professor Schaffer!

Mauro Condé: This interview is an important moment of our very short history as a journal. So, thank you so much for this interview.

Simon Schaffer: You're very welcome. It's a great honor for me to meet you all and to be interviewed. I don't get interviewed very often, so it's a treat.

Gabriel Ávila: Thanks a lot, Professor Schaffer. It was an honor to us.

Simon Schaffer: Obrigado!

References

Bloor, David. 1976. *Knowledge and Social Imagery*. London: Routledge and Kegan Paul.

Buchdahl, Gerd. 1969. *Metaphysics and the Philosophy of Science*. Basil Blackwell.

Fleck, Ludwik. 1979 [1935]. *Genesis and Development of a Scientific Fact*. Chicago: The University of Chicago Press.

Hall, A. Rupert. 1954. *The Scientific Revolution 1500-1800*. London: Longmans.

Haraway, Donna. 1976. *Crystals, fabrics and fields: metaphors of organization in twentieth century developmental biology*. New Haven: Yale.

Klein, Ursula. 2020. *Technoscience in history: Prussia, 1750-1850*. Cambridge, MA.: MIT Press.

Mayer, Anna-K. 2000. Setting up a discipline: conflicting agendas of the Cambridge History of Science Committee, 1936-1950', *Studies in History and Philosophy of Science* (31): 665-689

Mayer, Anna-K. 2004. Setting up a discipline: British history of science and 'the end of ideology', *Studies in History and Philosophy of Science* (35): 41-72

Needham, Joseph. 1954-2004. *Science and Civilization in China*. Cambridge: Cambridge University Press. 7 vols.

Porter, Roy. 1977. *The Making of Geology: Earth Sciences in Britain – 1660-1815* Cambridge: Cambridge University Press.

Serres, Michel. 1982. "Knowledge in the classical age: La Fontaine and Descartes". In Serres, Michel. *Hermes: literature, science, philosophy*. Baltimore: Johns Hopkins University Press

Shapin. Steven. 2010 [1984]. "Pump and Circumstance: Robert Boyle Literary Technology". In Shapin, Steven. *Never Pure: Historical Studies of Sciences as if It Was Produced by People with Bodies, Situated in Time, Space, Culture and Society, and Struggling for Credibility and Authority*. The Johns Hopkins University Press.

Shapin. Steven. 2010b [1988]. "The House of Experiment in Seventeenth-Century England". In Shapin, Steven. *Never Pure: Historical Studies of Sciences as if It Was Produced by People with Bodies, Situated in Time, Space, Culture and Society, and Struggling for Credibility and Authority*. The Johns Hopkins University Press.

Shapin, Steven. 1994. *A Social History of Truth*. Chicago: Chicago University Press.

Shapin, Steven. 2004. "The way we trust now: the authority of science and the character of the scientist". In Pervez Hoodbhoy, Daniel Glaser and Steven Shapin, *Trust me, I'm a scientist.* London: British Council.

Shapin, Steven and Simon Schaffer. 1985. *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life.* Princeton: Princeton University Press.

Shapin, Steven and Schaffer, Simon. 1993. *Léviathan et la pompe à air. Hobbes et Boyle entre science et politique.* Paris: La Decouverte.

Shapin, Steven and Simon Schaffer. 2011. *Introduction to the 2011 Edition.* Up for air: *Leviathan and the air-pump* a generation on. In Shapin, S. and S. Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Li*fe. Princeton: Princeton University Press.

Thompson, E. P. 1971 "The Moral Economy of the English Crowd in the Eighteenth Century", *Past & Present* (50): 76-136.

Thompson, E. P. 1978. "Eighteenth-Century English Society: Class Struggle without Class?", *Social History* 3 (2): 133-165.

Thompson, E. P. *Customs in Common*. New York. The New Yorker Press.

Wittgenstein, Ludwig. 1953 [2007]. *Philosophical Investigations.* Oxford: Basil Blackwell.

Young, Robert M. 1985. *Darwin's Metaphor: Nature's place in Victorian Culture.* Cambridge: Cambridge University Press.