

The Power of History: an interview with Lorraine Daston

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Lorraine Daston arrived in Montevideo in the last carnival holidays. Although it was her first time not only in this city but in Latin America, her books are well known among historians and philosophers of science from this part of the world. In her visit, she gave a Seminar about Historical Epistemology – the historical practice that she develops in the Max Planck Institute for the History of Science, in Berlin –, a way to understand scientific past in order to comprehend the science of the present. She also offered a lecture at the National Museum of Arts named “Big Calculation and the History of Intelligence”, where she presented – for a big audience – what the roots of nowadays fashioned studies in machine learning and artificial intelligence are. Finally, she participated in the reunion of the General Archive of the Universidad de la República de Uruguay [the academic Department of the archive was the main responsible for her visit to Uruguay], and discussed the role of archives in the constitution of future science.

In-between those activities, she made the time for this interview. We were mostly interested about the kind of thoughts that she beholds behind the program of Historical Epistemology, and what was the History (her story) that made possible the kind of research she develops. The result, therefore, is a combination of personal life, historical moments, situations and institutions, and – of course, how could it be otherwise – scientific ideas.

In other interview you have said that you wanted to be an astronomer but then you changed to History of Science, during your student years. Could you recognize when exactly that happened?

I cannot point out exactly when it happened but I certainly know when I was first exposed to the history of science. Like most students who go to college I had no idea that such a discipline existed. I wanted to study astronomy for really rather foolish reasons. My Greek family had named me after the muse of astronomy, Urania. So I took Harvard’s introductory astronomy class, which was taught by an astrophysicist named Owen Gingerich, who was also a very distinguished historian of astronomy. I think that I speak for many students in his class in saying that, forty years later, if there’s anything we remember from our undergraduate studies, are Owen’s class. He was a magnificent teacher, and he presented astronomy through the history of astronomy. It was an absolutely thrilling story as he told it... unforgettable! I learned from that course that Harvard

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had, quite unusually among American universities at that time, a Department of the History of Science. You couldn't major in it as an undergraduate, but there was a combined major in History and Science, with coursework in the sciences, history, and the history of science. I took classes in mathematics and Astronomy, as well as in mostly European history and intellectual history and the philosophy of science. After I graduated from college I went to Cambridge, England with a fellowship to study philosophy of science with Mary Hesse [who had written a remarkable book called *Models and Analogies in Science*.³ When I arrived at the Cambridge Department of History and Philosophy of Science, first of all, I discovered that it was a war going on between the historians and the philosophers. And secondly, I discovered that my idea of philosophy, which was something that would be perhaps have been suited to the seventeenth century, something like Leibniz's *Monadologie* but was certainly not the kind of philosophy that was done in Cambridge at that time. After a year I returned to Harvard and completed my PhD in history of science. But I remembered thinking the very day I received my doctorate: "Well, If this doesn't work out, I could study something else"... and remembered thinking about Egyptology or something like that. I think that's quite typical for historians of science of my generation who come from many different fields, who are fascinated by the history of science but do not see it as a discipline of the same established variety as philosophy or history. I think I was probably around forty-five or fifty before I realized that I probably wasn't going to study Egyptology after all...

You talked about a war going on in Cambridge between historians and philosophers of science, but the sixties at Harvard seems that was also a place of confrontations... Did you feel that way when you were studying there in those years?

Not at Harvard, in part because in a sense the war had already concluded. I took courses from Hillary Putnam and Israel Scheffler. The philosophers did not speak with the historians of science, and the historians of science did not speak with the philosophers. At Cambridge they were still together in one Department, and the colloquium was a battlefield. Particularly among the historians of Medicine, Robert Young and Karl Figlio, a Marxist approach was dominant, which was anathema to the philosophers. That was the early seventies, when the battle lines were political, and to some extent I think also national. Both Karl Figlio and Bob Young, that made their careers in Cambridge, were Americans. Among the philosophers, Mary Hesse was British and Gerd Buchdahl was German. It was in part a clash of different national, intellectual traditions and formations. I certainly never regretted the work that I did with Mary Hesse nor what I learned about Kant and Goethe's *Naturphilosophie* from Gerd Buchdahl. However, it was quite clear that a reconciliation among the warring parties was not in the offing, and for the graduate students who were there at the time it was like being in the midst of divorcing parents: you had to declare which side you were on. It was at that point that I decided that I was not going to continue my PhD at Cambridge.

How do you feel about the idea that "every historian of science is a converted scientist"?

I think it was once true. Perhaps forty or fifty years ago, the majority of historians of science did indeed come from a scientific background and wanted to use the history of science as a way to reflect about their science. There were people who understood that the current state of science was to some extent not an inevitability. They were curious and perhaps also subversive in their interest in using history of science, not in a textbook fashion to reaffirm the science of the present, but

3 Hesse, M. B. (1970). *Models and analogies in science*. Paris: University of Norte Dame Press.

perhaps to unsettle some of its certainties. And they were also scientists, especially in the 1960's in the United States, who were politically active. Someone like Everett Mendelsohn, a historian of biology at Harvard, was very active in the protest against the war in Vietnam. History of science in those days played an important role in helping scientists reflect politically about their role. But I think it is no longer as true, although many historians of science still have a formation in the sciences. When I now see the people that come to the Max Planck Institute for the History of Science in Berlin (MPIWG), at all stages in their careers and, coming from all parts of the world, some of them are indeed scientists, but others have backgrounds in philosophy, history, sociology, anthropology, art history, and general history., I think that's an enormous enrichment for the field of the history of science. The history of science has, in the past two decades, become much more like a regular discipline, become much more professionalized, and in many ways this has contributed to raising standards. But we still profit greatly from the stimuli provided by colleagues with other disciplinary backgrounds.

But do you defend this kind of nature of History of Science? Because in 2009 you wrote an article in Critical Inquiry

Yes, *Science Studies and the History of Science...*⁴ I'm ambivalent about it. I must say: I think the graduate students who are now trained in History of Science are so much better trained that I was. For example, it was quite common for dissertations to be written without any archival research, which is now unthinkable. So in that sense I think is a very positive development. I have absolutely no objections to this kind of professionalization in the history of science; quite the contrary, I applaud it. But on the other hand, the openness that we've always had toward other disciplines, that any of you – who may have very different formations – can be welcomed at an institute like the MPIWG, seems to me equally valuable.. Such diversity, including that contributed by science studies and also philosophical perspectives, has made the history of science theoretically sophisticated, and I will be very sorry if we lost the kind of fermentation that that such interactions make possible. The kind of probing questions that we get, especially from people who are coming from, say, psychology or anthropology, take us by surprise in a salutary way. No historian of science would willingly answer questions about the scientific creativity. The psychologist insists on posing those kind of questions, a salutary challenge. Without the anthropologist we would probably not be talking about botany, the study of flora and fauna, as a pan-human cultural phenomenon; we will be still talking only about Linnaeus, a quite parochial view.

There is always a question surrounding the formation of historians of science... how much the historians have to know about the science they decided to study to be a proper historian of science?

It is a very serious question. My motto is: more knowledge is always much better than less knowledge. Always try to learn as much as possible. In my own case, for example, it was certainly very useful for me to study modern probability theory. But only up to a point; eventually I had to understand the science of the time in its own terms. And in many ways, although at the beginning knowing modern probability theory was a great help, in the end it was an obstacle because I continued to try to translate what I was reading in seventeenth and eighteenth century texts into the modern language, and thereby of course losing the distinctive content. I think you need to know as much as possible about the science of the time. And that could be extremely technical and extremely difficult.

4 Daston, L. (2009). "Science studies and the history of science". *Critical Inquiry*, vol. 35, No. 4, pp. 798-813.

Do you agree that History of Science had reached a specialized situation – almost based on works of study cases – that threatens any general perspective?

I think in general that is true, but I think that the more important point is why it is true. In part is because of the professionalization that we were talking about a moment ago. History of science has become more like history, and the standards of historians for deep research are considerably more rigorous than those of the historians of science used to be. It's understandable that people attempting to meet those rigorous standards therefore address narrower problems. But I also think it's because historians of science have increasingly lost contact with the other fields that pushed them to ask big questions. In the 1980s, when science studies and the history of science were interacting on a regular basis, some excellent work emerged. I'll mention emblematically, *Leviathan and the Air-Pump*⁵ by Steven Shapin and Simon Schaffer, as an example of such cross-fertilization. However, such interactions have almost ceased for any field in the history of science except contemporary science. Historians of contemporary science, i.e. late 20th century and 21st century science, are still in lively and fruitful dialogue with their colleagues in science studies. But this is no longer the case for those who work on the 17th and 18th centuries, much less on the 12th century.

So yes, I do think that individual articles in the history of science have become more specialized. However, I think that the field taken as a whole has become much broader. When I look, for example, at the research that is published by all of the visiting graduate students and the post docs who come to the MPIWG in Berlin, I am impressed by the broad spectrum of topics: from Mesopotamian divination and the astrometeorological sciences to the use of film in modern biology, and everything in between. If you survey the entire field of History of Science, we've become much broader, but the individual studies have indeed become more focused.

But then, who could be responsible for assuming that broader view represented in the field?

That's a question that I ask myself a lot. At the MPIWG I have been in a very fortunate position and was able to form this working groups – teams of scholars – to take a broader view of a big question: for example, *Does scientific observation has a history?* No one scholar could write that history but a collective of scholars can at least make a start. At least for me, such working groups have been enormously stimulating. It's hugely enlarged my own intellectual horizons to be working with people who are experts on everything from natural philosophy in Ancient Greece to psychoanalysis in the twentieth century. And for all of us, the collective challenge of making sense not just of this or that case study but of all of them together.

We know for certain that you are read among philosophers, historians, anthropologists, and studies of the image... but what about scientists? Do you have an idea if they use your work?

I think that intellectual traditions really make the difference in this case. What is interesting to me is how much more interested scientists are in the history and philosophy of their fields in France, in Germany, and I suspect that here in Uruguay – judging from the people I've met so far – than their counterparts in the anglosphere generally are.

I remember during the 1990's, during the so-called Science Wars – perhaps you will remember this episode – in which some scientists, especially in the United States, felt that developments in the history of science, and science studies concerning social constructionism,, threatened to

5 Shapin, S. and Schaffer, S. (1985). *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*. New Jersey: Princeton University Press. Spanish translation: *El Leviathan y la bomba de vacío. Hobbes, Boyle y la vida experimental*. Bernal: Universidad Nacional de Quilmes.

undermine sciences. In the United States, some physicists actually believed that somehow the history of science and science studies were responsible for Congress cancelling the superconducting supercollider. The idea of an American congressman reading Bruno Latour is hilarious. I recall conversations with German physicists who were quite perplexed by the idea that physics was not as much a part of culture as Bach or Mozart.

I think that there are real national differences here, in intellectual traditions, due in part to the fact that elite education in France or Germany still includes mandatory philosophy in the last years of *lycée* or *Gymnasium*.

Do you think the kind of formation that History of Science could offer is truly relevant to a scientist?

I believe that the history of science has a responsibility to scientists, quite a few of whom visit the MPIWG. Many are coming from molecular biology or genetics, often very advanced in their studies, with every prospect of success in their chosen fields. But they say over and over again: “We have no idea why we are working on the problems we are working on, and moreover, we have no idea where this is going”. That is, they have turned to the history of science not because they want to know about the past, but rather about present research. They feel that they have an ant’s eye-view, and they want to have the bird’s eye view on their fields. The history of science can provide that kind of orientation. The history of science can also provide a sense of intellectual possibility. It’s very important especially for younger scientists to realize that the current reigning orthodoxy in science is not without alternatives. We did not always think the way we do now and in all probability will think differently in the future. Finally the history of science can help scientists reflect on their social and political responsibility by providing case studies of how even the best of intentions have sometimes resulted in terrible human tragedies. Few scientists have any tools whatsoever for thinking about the implications of their research; their training has rarely equipped them to think about making choices – often terrible choices – about whether or not they should continue in the line of research they are pursuing. Such systematic reflection is particularly important in research on ideologically laden topics such as race and gender. Historians of science have shown that scientists often take their own categories from the colloquial categories of the society in which they were raised, – how could it be otherwise? – without further critical reflection. Philosopher of science, Helen Longino, at Stanford, has provided strong arguments about why for just that reason is important that the researchers themselves have a diversity of identities. It makes a difference whether or not someone who is doing research on race is not white, and has some sense of the construction of those categories. Or that those doing research on gender are not exclusively male or female. I don’t mean in any way to blame the scientists. The curriculum that has trained them has no place for reflection on those kinds of issues. Here the history of science could also play a helpful role.

Could you tell us what did involve the reconstruction of the German conditions of scientific research, not only from the Max Planck, but almost for the entire period that began after World War II?

It is a very interesting question about the possible parallels between the continuities and discontinuities in a country that had been divided for over forty years. I should explain a little bit about the nature of the Max Planck Society: It is a research consortium of approximately eighty research institutes, most in the natural sciences, dispersed throughout Germany, with a few extra-territorial institutes and centers. After German reunification, the Max Planck Society established new institutes in the former East Germany. Incorporating East Germany into the Federal Republic of Germany also meant to incorporate them scientifically. It was moreover an

opportunity for the Max Planck Society to establish institutes in new field. There were certain fields, such as ethnography and demography, which had previously been tabu, because they had been politically disgraced by their participation in Nazi political projects. Amongst the new Max Planck institutes was one on ethnographic research in Halle, and one on demography in Rostock to re-introduce the latest developments in these fields back into Germany. The MPIWG was one of these new institutes and was originally located in East Berlin. We had an unusual history because there was a department of the East German Academy of Sciences that had been dedicated to the history and philosophy of science, and as part of the foundation of our institute we took over some of the members of the East German Academy. Discussions with these colleagues were a kind of shock treatment for those of us who had come from the anglophone world – and vice versa – think it was good for both sides.

When now you are talking about the ideological and political aspects involved in research institutions, we would like to know how do you manage this dimension. It doesn't seem that the political aspects of your historical epistemology are explicitly present in your research, is that right?

I think that's largely true. One criticism sometimes made of my kind of historical epistemology is that is 'tone deaf' to power. I think it's some justice in that, but I think it also depends on a very narrow idea of what power is. This may sound naïve, but I believe that ideas are powerful. I believe in the causal efficacy of ideas. They make things happen in the world. I think there's no power greater than the power to make something unthinkable or thinkable, and that is what historical epistemology is about. However, the critics are right to say: it is not about power in the conventional political sense.

Baconian thoughts: "knowledge is power"...

Yes, but I mean something more specific than that. Knowledge is power, I agreed with that, but the most powerful form of knowledge is not this particular fact or that particular fact, but a whole structure of knowledge. Think of the difference that the great critical movements, starting in the Enlightenment and continuing through Marxism and feminism, have made. Marx's insight was that what had been propounded as a universal truth was in fact a truth for one class; or Simone de Beauvoir's insight, that what had been propounded as a universal standard, was in fact a male standard. I can't think of knowledge more powerful than those shifts in perspective. When I think now about what's happening in my own country, the United States, I think you can't understand the election of Donald Trump without the intellectual tools of gender studies. It's that critical knowledge that constitutes true power.