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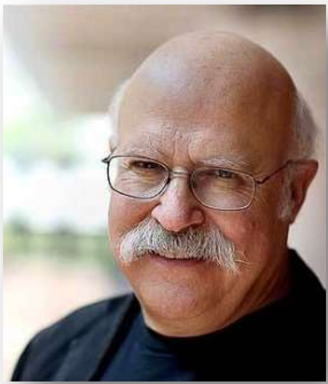
Special Issue
Leviathan and the Air-Pump
After 40 Years: Reception, Criticisms and Impacts

Interview: Steven Shapin



Steven Shapin (born 1943) is an American historian and sociologist of science and professor of the history of science emeritus at Harvard University. Shapin was trained as a biologist and did graduate work in genetics before taking a Ph.D. in the History and Sociology of Science at the University of Pennsylvania in 1971. From 1972 to 1989, he worked at the Science Studies Unit at the University of Edinburgh, and from 1989 to 2003, he was Professor of Sociology at the University of California, San Diego, before taking up an appointment at the Department of the History of Science at Harvard. One of the most essential and creative historians of science of all times, Shapin has won many awards, including the 2014 George Sarton Medal of the History of Science Society for contributions to the field. He has authored numerous chapters, articles, and books and co-authored with Simon Schaffer the influential book *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*.

Interviewed by

María de los Ángeles Martini[1] and Mauro L. Condé[2] in July 2024

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María de los Ángeles Martini (MAM) and Mauro L. Condé (MC): We would like to begin with your academic career. For what reasons and at what point did you decide to specialize in the History and Sociology of Science? Who influenced you?

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Steven Shapin: My entry into this field is a story of drift and accident. I studied biology as an undergraduate, and then I did one year of graduate work in genetics, at the University of Wisconsin. So, a year after taking my first degree, all the signs were that I would be a natural scientist, and I was, at that time, quite interested in evolutionary genetics and questions about speciation—addressed using some of the methods of molecular biology. There are no scientific publications to my name, but I did a reasonable amount of work on experiments that did eventually see the light of day.

There was a major department of the history of science at Wisconsin, but I did not then know it existed. I gave up any ambitions to be a geneticist, but I did not do so to take up the history of science. I did not then have a clear notion of what I might eventually do, but I had developed a strong, if poorly informed, interest in both the politics of science and in scientific journalism.

Moving to Washington, DC, I worked briefly—as an intern—in the US Food & Drug Administration, in an environmental toxicology unit—on possibly mutagenic and carcinogenic substances. At around this time, I presented myself unannounced at the office of the News Editor of *Science* magazine—the great Daniel S. Greenberg—and asked whether there was a junior position open. I had nothing to show Greenberg—I had done some "creative" writing but no journalism. However, for reasons I cannot understand and that Greenberg subsequently could not remember, he showed me into the office of Philip Abelson—the former Manhattan Project physicist and then editor-in-chief of *Science*, which was and is the major periodical of the American Association for the Advancement of Science—and said that, if I could convince Abelson, then he would take me on, since, by coincidence, there was an entry-level position about to open. At that "sliding-doors" moment, if I persuaded Abelson, I could well have become a science journalist. I think I might have been reasonably good at it. But, quite understandably, Abelson saw no potential in me, and, tail between my legs, I went down to say goodbye to Greenberg.

Another "sliding-doors" moment: Greenberg agreed that I had had little chance of talking Abelson into approving me, but Greenberg then picked up the phone to a friend—the sociologist Norman Kaplan, working at George Washington University and the recipient of a large NASA grant to compile reports on the history of large-scale Federal government decision-making about the support of science. He asked Kaplan to give me an audience; days later, I saw Kaplan; he took me on; and soon I was working on a year-long project with him about the history of episodes like the development of government support for agricultural research, for educational research, and for what soon became "the wet NASA"—the National Oceanographic and Atmospheric Administration (NOAA). That is, I was doing—without any significant historical training—a kind of history of science, albeit a history of scientific institutions and the political direction of science. (I still have those reports—and they're not terrible.)

Some of that research was "archival"—especially the agricultural story, which goes back to the mid-19th century—but much involved going around Washington and interviewing politicians and bureaucrats—at the Bureau of the Budget, the Library of Congress, the Department of Health, Education & Welfare, and in the Senate and House of Representatives. I was then particularly interested in what might be called "the engine room" of politics—not electoral politics but the people and organizations working invisibly in the background but who had an outsize role in shaping what actually happened. I got to the point where people were telling me that I understood pretty well how government decision-making worked and suggested that I might have a career somewhere in that area—perhaps in the government bureaucracy (though that didn't appeal to me); perhaps as a congressional legislative assistant (helping to draft legislation and organize hearings); perhaps in a think-tank; perhaps as a journalist after all—and the last three did have some appeal.

Anyway, a number of people who thought I might do these sorts of things also told me that I needed the "credentials" that would open doors. I didn't get much sense what, exactly, these "credentials" might be, but there was general consensus that a PhD of some kind would be helpful. A PhD in *what*—I asked—and the answer seemed to be that it didn't much matter, as long as there was some arguable connection to science politics—so, political science, sociology, law, something like that.

That was the main reason, after a year or so in Washington, that I applied to graduate school. But the history of science was not the only graduate course for which I applied: I also applied to political science departments and to the Department of Sociology at Columbia. I went there for an interview with the great Robert Merton and I sat in the anteroom of his office for three hours until his secretary made it clear that he was not going to appear. That's another "sliding-door" moment: I could have been a Merton student, and who knows how that might have worked out.

Only one of my applications was to a history of science department. The Pennsylvania department was then called the department of the history & philosophy of science (the "sociology" element appeared only after I had been a student for two years, and I am—I believe—the first recipient of a "history & sociology of science" doctoral degree, although there was at the time no actual instruction in sociology). The reason that I applied there at all was because I had read some fine papers on the history of American agricultural research by a young historian of medicine named Charles E. Rosenberg; he was at Pennsylvania; and I had diffuse thoughts that he might be interested in supervising thesis research in the area. (It didn't work out that way, but we remained friends for many years and at Harvard Charles Rosenberg and I were departmental colleagues.)

But, while I can't say I had definite plans what I wanted to do, I still assumed that I might go back to science politics work of some kind, and I had made no commitment to being an academic historian—even assuming I could ever get some sort of university position. What happened is, as they say, "life happened"—and circumstances changed. I found that I enjoyed the day-to-day work of researching and writing history of science. I think it was Ralph Waldo Emerson who said that "life is a journey, not a destination"—and the "journey"—the doing—began to engage me even though I had no idea about a destination. The life of a historian was not something I reflectively chose; it *happened to me*, and when it happened to me, I found that it suited me and that I liked it.

I suppose there are also questions about whether I really *am* a historian of science. I've worked in the interdisciplinary Edinburgh Science Studies Unit; then I worked in a sociology department, though it was so atypical of North American sociology that they hired me *as* a historian; and it was not until I was over 60 years old that I was employed in any kind of history department—Harvard's department of the history of science. Some people refer to me as a sociologist—though I wasn't trained in sociology; some refer to me as a philosopher—though I can't think why. When I arrived in Edinburgh, there was one resident historian of science in the History Department, and he made it clear to me that I was *not* to teach his discipline. We compromised, and the undergraduate course I proposed in the Science Studies Unit was labeled the *Social History of Science*, which satisfied him that it was not the real thing.

I suppose this account seems a bit like a random walk, and, compared to Simon's, I think it is much more a story about drift, accident, and circumstance. At my small liberal arts college—Reed College in Portland, Oregon—I recall that there were two or three lectures in the history of science in the foundation-year "Humanities" course. I found them very boring and they made no impression on me. That said, I still have copies of three books which I read as an undergraduate—out of choice, not as requirements. One was Arthur Koestler's *The Sleepwalkers*—a semi-popular history of astronomy; the second was C. P. Snow's *The Two Cultures*; and the third was Thomas Kuhn's *The Structure of Scientific Revolutions*. These did make an enormous impact on me, but I did not think of them as emerging from an academic discipline called the history of science, nor as token of a discipline that I might ever think to join.

Although I wasn't focally aware of the history of science as an organized academic pursuit, there were things about these books that appealed to me at some temperamental level. I can't say why this was the case, but, while I respected the specialized knowledge that many academics had, I always worried about what seemed to me the narrowness and obliviousness of the specialized way of life—the assumption that *these things* were important and worthy of one's attention and *those things* were not, that you should know a lot about some things and nothing about others. I reckoned—and I think this was the reason I chose a liberal arts college with a common foundation year—that you could not call yourself educated, or even consider yourself a serious sort of thinker, if you accepted the conventional sortings of the academic culture—if, for example, you knew a lot of science but nothing of the arts or humanities, or a lot about the arts and humanities but nothing of science. At that time, and as a biology student, poetry, fiction, and film were important parts of my life and I did not want to set them aside. (By the way, the college's foundational Humanities course was—with the exception of those few lectures on medieval science—innocent of the natural sciences.) This was the same college attended by Steve Jobs and accounts of his time there, I think, pay insufficient attention to the highly developed design and calligraphy culture that was part of the college's aesthetic landscape and that affected the science as well as the arts students. The college produced several very important calligraphers and typeface designers and it did not encourage the extremes of specialization.

I was initially an enthusiast for *The Two Cultures* as some sort of argument for a broad liberal education, but that was a bit of a careless reading, and I later realized that Snow basically wanted to reverse the prejudices of what he took to be a purely literary conception of cultural competence. I can't recall much about the appeal of *The Sleepwalkers*—probably some diffuse fascination with the mixing-up of science, religion, and mysticism. (Remember this was the 1960s.) But what Kuhn offered—as it seemed to me—was a notion of *science-making* as a thoroughly human endeavor. Or, put extravagantly, of science *as* one of the humanities. I don't know how many times I've read *Structure* or how many times I've offered an interpretation of the book, but the "influence" question doubtless tracks back to that book.

One other point about "influence"—and here my story looks very different from Simon's: it was no part of my experience as a student to have had personal contact with important thinkers. Simon *knew* many of the people who arguably "influenced" him. He studied at Cambridge, for a year at Harvard, and he attended Foucault's lectures in Paris. In my own formation—such as it was—I encountered books off library shelves but not the sorts of people who wrote them. I spent only three years and a bit as a graduate student, mostly because I "slipped through the cracks" of a new department which had not yet formalized firm curricular requirements, and, while my thesis supervisor had been a student of Mary Hesse at Cambridge, he supervised me with a very light touch—for which I was grateful—and he was not a conduit for learning much of what was going on in the field in England. One way of putting it would be that Simon knew books *and* many of the people who wrote them; I knew only the books, some of which came to my attention more or less by accident—wandering along library shelves; pursuing stray thoughts; the accidents happening to a badly disciplined but curious sort of student.

(MAM; MC) Tell us a little about your move to the UK and what it was like to be part of the Science Studies Unit in Edinburgh with other talented young researchers. What was the research dynamic like at that time?

Steven Shapin: After finishing my PhD at Pennsylvania, I applied for, and, to my surprise, got, a one-year post-doctoral fellowship at Keele University (in the English Midlands). I had already spent a little time with the Edinburgh Science Studies Unit group as I was researching and writing my dissertation (on aspects of science in the Scottish Enlightenment). While at Keele, I did some writing about the social history of science in industrializing Britain, having made the decision not to turn the thesis into a book. (Two or three published articles came out of the dissertation research in the first few years I was in Britain, but I thought that my thesis was a mediocre production and I did not see myself carrying on work in that genre—basically institutional history of science.) Several British university lectureships were advertised during that year, and I was hoping that I might get the one at Edinburgh—which I did, and where I began teaching during the last few months I was based at Keele.

My "education" didn't stop with completing graduate school—patchy as that training was. I owe an enormous amount to my Edinburgh colleagues David Bloor and Barry Barnes, who kindly included me in their ongoing discussions about the sociology of scientific knowledge (SSK). They never represented themselves as speaking "in the name of sociology" or "in the name of philosophy". Rather, they were committed to the open-ended pursuit of a project on the social making of knowledge which they had no expectation would ever have much impact on the established disciplines but which interested me very much—even if I really do not know whether they recognized my work as contributions to either the "Strong Programme" or to SSK. I learned much from my colleagues; I collaborated several times with Barnes; but I was a bit to the side of the discussions between Barnes and Bloor, even though both were generous with their time and both were kind enough to profess some interest in my emerging historical concerns.

It was a good time: colleagues' doors were almost always open; I felt free to drop in for whatever discussions I had in mind; there were pubs across the street where discussions continued. The language of career-making, of what would "sell" in the academic marketplace, was no part of the local culture. The Thatcher Era audit culture had not yet squeezed academic time into quantifiable bureaucratic boxes—that was soon to come—and I felt, even if my colleagues might not, that we had adequate time to think. The atmosphere was—so to speak—intellectually serious but not professional in any disciplinary sense. The Unit's Director, David Edge, was a radio astronomer-turned-BBC talks producer, who had developed a diffuse interest in issues to do with "science and values" and about "appropriate technology". David Bloor read mathematics as an undergraduate, then took a degree in experimental psychology. Barry Barnes's first degree was in chemistry, and, while he did master's level graduate study in sociology, he never did his doctorate. (It was "Mr. Barnes" not "Dr. Barnes"—and this was a time when there were a number of very distinguished British historians without a doctoral degree.) I've spoken already about my background—and I personally would have no objection to the Unit being described as a small group of "amateurs" and I would be happy to be called one myself. An example: as a graduate student, I never took a course on "the Scientific Revolution" and, despite now having written three books on early modern science, I have never *taught* a course on that subject. It wasn't possible to do that at Edinburgh; at the University of California at San Diego, I was in the sociology department, so it wasn't possible to do that there either; and, when I arrived at Harvard's history of science department, there was an early modern historian already in place who assured me that the subject was already being taken care of.

At Edinburgh, I felt no pressure to publish a book, even though it was eventually clear that I would probably never get promotion beyond Lecturer grade (that is, to Senior Lecture, Reader, or Professor). I spent 16 years at Edinburgh, and three years after publishing *Leviathan and the Air-Pump*, without being put up for promotion from Lecturer, the lowest British academic grade. This didn't especially bother me; I didn't think my thesis research was terribly interesting, and I spent maybe eight or nine years casting around for something that did interest me, that might engage my colleagues' interests, and that merited book-length treatment. I had already developed a small line of research in issues attending the Scientific Revolution, and that was before I met Simon Schaffer. (We both offered papers, but I do not recall that we spoke, when we attended a joint sociology of science/history of science conference in 1980.) When I was asked to be external examiner of his Cambridge PhD thesis on Newtonianism, I was in a position—because of that earlier research—to appreciate what he was doing, and, perhaps, he was in a position to recognize me as someone "sympathetic". Simon was one of the small group of British historians of science who were just beginning to show interest in SSK and related strands of sociology of science (associated with workers like Harry Collins and Bruno Latour). Within a year after meeting, Simon and I were kicking around plans for a long article—definitely *not* a book—on the Hobbes-Boyle controversy—something I had stumbled on, more or less by accident, a year earlier. Simon, of course, already knew about this controversy, but when I stumbled upon it, it's possible that my enthusiasm for what might be said about it was infectious.

(MAM; MC): In 1992, you stated (Shapin 1992) that the problem of internalism vs. externalism had not yet been resolved in the history of science at that point. Could we say that *Leviathan and the Air-Pump* still echo this question? How does this book position itself about internalism vs. externalism? And today, is this a purely dated historical question, epistemologically resolved?

Steven Shapin: The concluding passages of *Leviathan and the Air-Pump* explicitly address the internalism/externalism question, and the 1992 paper you mention is my attempt to give an account of how that question became historically foregrounded, what sense it might coherently have had, why I thought it was sloppily framed, and why it seemed—at that point—to fading away as *the* focal concern for the history and sociology of science. Remember that Thomas Kuhn's influential 1968 essay on "The History of Science", and many other writings, instructed students entering the discipline—as I did in the late 1960s—that the field was fundamentally divided into internalists and externalists, that entrants had to make a choice between the two, and that, for the most part, externalism was a failed way of accounting for the history of science.

In the *Introduction* to the 2011 edition of the *Air-Pump* book, one way of describing it as an historical object was to draw attention to its concluding internalism/externalism passages. We had tried there to make several things clear: that the book belonged neither to the internalist nor the externalist genre; that we need not speak of "the social" as "outside" of knowledge nor of knowledge as "outside" of society; but that the boundary-drawing done by historical actors was nevertheless worthy of serious attention. Did *they* make distinctions between "the social" and "the intellectual" (or "scientific" or "natural"), and, if they did, how did they do so, with what consequences, and with what stability? Perhaps we underestimated the power of the internal/external distinction at the time we wrote, and for some years thereafter. As we noted in the *Introduction*, a number of the book's reviewers saw it as just another form of externalism, giving too much weight to "the social" and not enough to "the intellectual." That reading was, to a degree disappointing, but it was not wholly surprising.

It's not for the authors to say whether or not the book played any role in the subsidence, or even the disappearance, of talk about the internal and the external. For my part, I think this subsidence mainly had to do with institutional and political changes—the professionalization of the history of science, as owned by historians rather than scientists, and the powerful projection of science in the Cold War years as an arm of national and industrial power. It was harder to see science as a delicate flower needing protection from "social influences" when science—in both capitalist and socialist settings—was so securely nested in the institutions of power and profit. And, by the late 1980s, the role of Bruno Latour, and his appreciation/appropriation of the *Air-Pump* book surely had much to do with a growing realization that the internal/external, intellectual/social framing was neither legitimate nor interesting. Or, at least, that historians and sociologists ought not take its terms and sortings as self-evident.

(MAM; MC): In chapter 1 of *Never Pure* (2010), you characterize the changes in the history of science in the last decades of the twentieth century and the beginning of the twenty-first century as a "lowering of the tone". This postulate invites us to focus on daily and material aspects of scientific practice. Do you consider this postulate still valid in the work of the historian of science? Has this postulate opened new lines of research in the history of science in the 21st century?

Steven Shapin: I'm told that the notion of "tone-lowering" is more of a British than an American expression, so perhaps—despite how I expand on the notion in that 2010 chapter—it may not be widely understood. In colloquial speech, you might be "lowering the tone" if you told a joke in a meeting about "serious things" or if you asked for ice in your wine at a Michelin-starred restaurant. So "tone-lowering" might have the implication of a social *faux pas*, but better would be the idea of "bringing down to earth".

There's something both right and wrong about the *faux pas* sentiment. What's right is that many academic and popular pictures of science do portray it precisely as "other-worldly," as not properly to be accounted for in the same way as "lower" forms of knowledge. To describe science as a fully human and historically situated endeavor can be seen not just as an intellectual *faux pas*, but as *lèse majesté*—so that lowering the tone counts as dangerous denigration and denial. You might say that lowering the tone in this way is a form of iconoclasm. But the icons to be smashed are not, in my view, *science*; the icons are certain highly influential *accounts of science*. The authority of science, I think, does not depend upon particular philosophical or sociological *accounts of science*. "Otherworldly" accounts of science—rightly to be subjected to tone-lowering—include, for example, the notion that scientific knowledge is *discovered*, rather than *made*; that science-making has to do with *thought* but not *labor*; that scientific truths are universal, circulating among cultures without effort or friction, not marked by their place of production; that there is a coherent, globally accepted, *Method* whose unproblematic application produces scientific knowledge; that science-making is done by individual *geniuses*; that the passions, interests, social circumstances, and bodies of people making scientific knowledge *do not matter*; that scientific truths can be spoken of as *disembodied knowledge*, its acceptance, maintenance, as well as its making, existing in a transcendent Realm of Ideas.

I say in that piece that tone-lowering of these sorts has characterized much history and sociology of science over recent years. I think this is a good thing, and I'd like to think that I've done some of it myself. It's a good thing for several reasons: for one, tone-lowering has opened up a series of noticeable aspects of science that were previously neglected or whose relevance was denied. So it's a good thing in terms of modes of concrete empirical work. The proof is in the pudding, as it's said, and many fairly recent achievements in the field track back to tone-lowering sensibilities—the labor processes of science-making; the presence and patterns of controversy and the concrete ways in which assent is achieved; the significance of the places in which science is made and transmitted; the means by which scientific knowledge and technique move about, around the world and in the culture; the social worlds and modes of interaction, including the role of "invisible technicians," of domestic environments, of relations between the genders; of meetings, conferences, and travel; of patterns of civility, assent, dissent, and authority; of the *personae*, bodies, body-management, and self-presentation of science-makers.

I've been particularly impressed by recent historical work on domains whose "worldliness" has been least presumed—the production and dissemination of mathematical knowledge—through, for example, "blackboard work" and proof-checking—and the production and dissemination of physical standards—how measures of weight, length, electrical resistance, and the like were decided upon. (Here, as in so many things, Simon Schaffer has produced some of the most compelling work and has influenced many historians.) And, taking notions like "disembodied knowledge," how are disembodiment, abstraction, certainty, and universality practically *performed* and made authoritative by embodied and historically-situated *people*? The appearance of disembodiment is achieved through embodied work.

I think that tone-lowering in these modes has no substantial bearing on the *values* placed on scientific knowledge. It has, it seems, been traditional to pour value over science by representing it as disembodied, timeless, effortlessly universal, a direct product of a coherent Scientific Method; made by solitary individuals and geniuses; Pure Thought. That would be, in part, to consider the scientist in the same frame as ideas of the priest or the Romantic artist. But, if value-pouring is a concern—and for historians and sociologists it need not be—value might also be linked to the figure of the artisan, to craft knowledge, to certain sorts of situated social networks. You can respect artisans and their products more than priests and their products, and, indeed, that is probably a sentiment more congenial to 21st-century culture. Tone-lowering in the history and sociology of science has achieved a lot already, and I don't see any reason why it shouldn't achieve more. I suppose the only foreseeable limit to the empirical fertility of the tone-lowering impulse is if the traditional image of disembodied science itself disappears. Then there would be no High Tone to be Lowered. If there are no icons, there can be no iconoclasm.

(MAM; MC): *Leviathan and the Air-Pump* was a historical work that focused on the materiality of scientific practices, the scientist's body, and science's place (space). These topics are still very relevant, as we see in neo-materialist perspectives. How did these themes emerge on the scene of your historical work? How did artifacts and epistemic objects, their generation, their biographies, and their agency take center stage in your work?

Steven Shapin: It's been said that the "hero" of this book—the center of its attention—is a material artifact—the air-pump—with the social, technical, and linguistic practices arrayed around that artifact. That's true enough, and I suppose it does mark out the book's focus from much then-current history of science, which was, at the time we wrote, primarily a history of *ideas* whose connection with the materiality of their production was not commonly addressed. I have no clear answer to the question of why we came to adopt that focus. Speaking for myself, I can think of two considerations. One is the specific form of iconoclasm we just talked about. The materiality postulate might be said to flow from a disposition to attend to the work practices of knowledge-making and, relatedly, to consider the range of actors relevantly present in knowledge-making scenes. You should not, I think, say that the focus was on work *as* *opposed to thought*; better to think of scientific thought *as* work. So that one thing that the book tried to do—and which later got branded as "historical epistemology"—was to take notions that had traditionally belonged to philosophers and that had sometimes been treated as self-evident, and to describe them as the products of situated work-processes—notions like fact, theory, inference, demonstration, replication, and, of course, experiment itself. How did people establish the fact, show its distinction from theoretical accounts, make inferences, and the like? What *work* was done, and by whom, and in what places? The sensibility was not particularly well developed in the air-pump book, but in subsequent writing I was interested in how knowledge and skills are distributed across knowledge-making scenes: who did manipulative work, who did interpretive work, who was an *author* and was the basis of the author's *authority*?

For my part, I did have some background—as a genetics graduate student and an intern in a government laboratory—of performing scientific experiments. I found them hard to do, hard to make repeatable, hard to interpret—and I did not find this difficulty in doing and disciplining well represented in the writings of historians of science. I wondered what a history of scientific knowledge-making would look like if those types of intractability and the overall materiality of knowledge-making were better acknowledged and confronted. I encountered the work of Michael Polanyi initially through the—I think insufficiently appreciative—account in Kuhn's *The Structure of Scientific Revolutions*; then I read both *Personal Knowledge* and some other Polanyi writings; and then I was happy to see the themes of both *tacit knowledge* and of *controversy* figuring in some early essays by the sociologist of science Harry Collins and his students.

More than that, I admit to a personal fascination with the "backstages" of all sorts of practices: rehearsals for theatrical performances, restaurant kitchens, trade union-industry negotiations, the performance of magic tricks, and, of course, the backstages of science-making whose workings are smoothed out in formal accounts of how science is produced. I realized that lots of practices labor hard to keep their backstages hidden from public view, and I realized that you wanted access to those backstages if you wanted to show how the performance was made to *work*. Early on, I had read Erving Goffman's *The Presentation of Self in Everyday Life*—about how ordinary social interaction trades in the boundaries between frontstages and backstages, and Goffman's sensibilities have accompanied quite a lot of the work I've done. I think it was Bismarck who said that "laws are like sausages; it is better not to see them being made." But he was speaking as a politician, not as a historian, and, for historians of science, it is important to see scientific performances *in the making*, being prepared—and also to see the work-practices involved in maintaining the *stability* of scientific things. It's been pointed out that sociologists can have direct access to knowledge-in-the-making and that historians cannot. Yet, while conceding the virtues of sociological contemporaneity, historians can, in fact, retrieve an enormous amount of evidence of science-being-made—and, in an optimistic mood, I might say that *Leviathan and the Air-Pump* had some success in establishing that point.

(MAM; MC): Beyond the field of the history of science, *Leviathan and the Air-Pump,* together with *A Social History of Truth,* became works that stimulated new lines of research. As part of social epistemology, Martin Kusch's epistemology of testimony (Kusch 2002) is recognized as indebted to your work. What value do you assign in your work to the themes of testimony and trust?

Steven Shapin: As you suggest, my interest in trust and testimony, and their role in science, seems to have developed early on, and these things feature in much of my work. I can't think of a coherent account of how this came about. In terms of material that I read when I was starting out, Kuhn and Polanyi must have had something to do with this, though I don't recall that either writer engaged with these categories in any focused way. There is Polanyi's stress on both tacit knowledge and on the craft-like nature of science and Kuhn's on pedagogy and authority. They both erode the credibility of fully *rationalist* accounts of science-making, but, again, that's not quite the same as the place of trust and testimony in books like *A Social History of Truth*. Inspired by your question, I searched Kuhn's *Structure* and I found one mention of "trust"—that in connection with confidence in paradigms—and two of "testimony"—neither concerning its role in science-making. (That's something I just now did, and I wouldn't want anyone to accept just my word for that.) I also think of Mary Douglas's semi-oracular, but powerful, remark that "the colonization of each other's minds is the price we pay for thought" (Douglas 1975, xx)—which I took to mean that the categories of thought, and the tools we use to think, come to us from others, and that, as we interact with others, our thoughts—virus-like—infiltrate their minds. The "social" bit of knowledge is not to be thought of as an "influence" but as constitutive. And, in this matter, as in much else, I acknowledge conversations with my Edinburgh colleagues Barnes and Bloor. It was by way of Barnes, especially, that I began to read some pertinent 20th-century sociological literature: it was through him that I encountered important work by, for instance, Howard Becker, C. Wright Mills, Mark Granovetter, and Dennis Wrong.

I suppose—though I haven't thought much about it—my interest in trust and testimony developed along with my discontent with both rationalist and, especially, with individualist presumptions about science. I also suppose that I was encouraged to reflect on my own stock of scientific knowledge and how I came to have it—and that story you find rehearsed in the early parts of the *Truth* book. On reflection, I must have found these orientations increasingly fruitful in all sorts of connections as time went on—notably questions to do with the public credibility of science; the role of the face-to-face and of familiarity in acquiring and maintaining knowledge; and the *travel* of science not only between people but around the world.

(MAM; MC): Simmel, Luhmann, and Giddens see modernity as a complex shift from reliance on individuals and face-to-face interactions to reliance on abstract capabilities and systems, analyses with which you may have differing views. Do you consider *Leviathan and Air-Pump*, *A Social History of Truth*, and *The Scientific Life* to be parts of a trilogy around trust in familiar people or, in other words, a trilogy about how "The world of face-to-face and the familiar still figures in making and warranting knowledge" (Shapin 2008, xvii; Shapin 1994)?

Steven Shapin: I certainly never thought to produce a series of works that advanced a coherent theoretical and methodological agenda of that sort or, indeed, of any sort. From my point of view, finishing one piece of work suggested what the next might be. That's to say, I don't think of myself as consistently advancing and defending such a thesis. I accept that my work seems to have sociological and philosophical bearings; I have read in these, and other, areas; but I have never been a "discipline-warrior"—caring about the boundaries of academic disciplines. That said, I have always considered that anything I might have to say that is of theoretical interest should be firmly grounded in detailed accounts of how knowledge is made, how it is sustained, and how it changes. That's not, as it were, to say what is "proper for historians to do," but it does flow from a high value I place on the concrete and a correspondingly low value I place on purely abstract formulations. I know that "case-studies" have recently become a bit unfashionable—represented as some sort of failure of nerve compared to full-blooded and fully-general theoretical formulations, but I think such a judgment is unfortunate. Case-studies, and reasoning from case to case, belong to long-standing and wholly legitimate intellectual traditions, and we should have more, rather than less, of them. But what are wanted are case-studies that are framed and presented so that readers are encouraged to reason from one rich and detailed case to an indefinite number of other cases. Perhaps that's one way of describing what *Leviathan and the Air-Pump* sought to do.

With those qualifications, it is true that I find problematic much about the "system trust" story about modern arrangements. It strikes me as taking a part for the whole, as applying an abstract account of some undeniable changes to the complexity of modern sensibilities and predicaments. It's not just that "trust-in-familiar-people" *survives* in late modernity. Of course, it does, and I am, in general, skeptical of "light-bulb-switching-on-and-off" accounts of historical change. Rather, I find that in parts of our society most associated with technoscientific and institutional *change* trust, familiarity and the face-to-face have a *heightened* importance. Where rules are broken with such rapidity, abstract standards of right conduct have little force, and embodied authority assumes great power. And this is what I tried to argue about the world of entrepreneurial science in my book *The Scientific Life*.

So that is a sentiment of some scope that I do embrace. Still, I do not feel that the body of work I've done over time has come from any sort of thought-through commitment to making general arguments of this type. Perhaps other people kind enough to read my work have seen some sort of underlying coherent theoretical commitment running through it, but I feel—as with becoming a historian of science—that I rather "fell into it" and, having fallen into it, one thing followed another.

I have been very fortunate in finding myself in academic positions where I could take on a wide variety of topics—not just those thought to "belong" to sociologists or philosophers but also topics widely dispersed in historical periods and in cultural domains. So I very much wanted to, and was fortunate in being allowed to, write about late modern entrepreneurial science as well as the 17th-century Scientific Revolution; about aspects of folk culture as well as science; to a lesser extent about human science, commercial science, pseudo-science as well as academic natural science. And, more recently, about "the sciences of subjectivity," about taste, and food and eating. (The book that appeared late last year is a history of ideas about food and human identity.) So that "freedom to roam" has meant a lot to me—even if it seems to have come at the cost of a clearly coherent intellectual agenda. Maybe other people looking at the writing that I've done might discern some underlying agenda or coherence in it, but that's not something I would claim.

I should also say that this freedom to address many different subjects and to write about them for an educated, but non-disciplinary, readership is something for which I should also thank the editors of publications like *The London Review of Books*, for whom I have written many "long-form" essays over the years, and I am grateful to them for giving me the chance to do so, and, occasionally, for allowing me to suggest topics that I would like to write about. I've enjoyed doing that very much, as I've enjoyed the opportunities to write about a range of topics, in a variety of outlets, and for different audiences.

(MAM; MC): Thank you so much, Prof. Shapin!

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