Does Science Need History?

A Conversation with Lorraine Daston

Lorraine Daston, Director Emerita at the Max Planck Institute for the History of Science, Berlin, in conversation with Editor, Samuel Loncar

Introduction

As part of our Meanings of Science in the Modern World project, I had the honor of speaking with Dr. Lorraine Daston. A leading authority on the history of science, Daston is visiting professor in the Committee on Social Thought at the University of Chicago, Permanent Fellow of the Wissenschaftskolleg in Berlin, and Director Emerita of the Max Planck Institute for the History of Science, Berlin.

Daston has published on a wide range of topics in the history of science. Her more recent books include *Rules: A Short History of What We Live By* (Princeton University Press, 2022), *Gegen die Natur* (2018; English edition *Against Nature*, 2019), *Science in the Archives* (University of Chicago Press, 2017), and *How Reason Almost Lost Its Mind: The Strange Career of Cold War Rationality* (with Paul Erikson et al., University of Chicago Press 2014). Her many distinguished lectures, honors, and fellowships include the Sarton Medal from the History of Science Society for "lifetime scholarly achievement."

Over the course of a lively and generous conversation, Daston shared her expertise on science and some of the challenges it faces today. We began with the conception of "science" in Europe and America, which led to a rich discussion about what can happen when we ask scientists to be cultural authorities, the history of science as relates to moral and ethical training, the crisis of peer-review, the perks and problems of working in strictly defined disciplines, and the strengths of an international scientific community.

We began by chatting informally about science and its various meanings in America and Europe, which led to my first question:

Science in America and Europe

SAMUEL LONCAR

What do you make of the dominance of the conception of "science," as someone who's familiar with the European context? "Science" has narrowed its meaning in the English language, moving from the whole of knowledge to just the natural sciences. This narrowness is fairly recent: the mid- to late-nineteenth century is when historians tell us our current idea of "science" and "scientists" originated. So how do you see the current role of the word and concept "science" in our culture?

LORRAINE DASTON

You're right about the contraction of the expansiveness of "science," which in all the European languages that derived some cognate from the Latin *scientia* used to refer to any form of organized knowledge. But it contracts not only in English but also in French, albeit a bit later in the late nineteenth and the early twentieth century. The French term for the scientist or scholar goes from being *savant*, which is still a word you can easily encounter in nineteenth-century French, to *scientifique* to refer exclusively to a scientist. And it surely has to do with the soaring prestige of the natural sciences, which is also the case in Germany.

There were many anxious lectures given at the end of the nineteenth century, the turn of the twentieth century, about how the *Naturwissenschaften* (the natural sciences) were edging out the *Geisteswissenschaften* (what we would call the humanities, although that's a very rough translation). But the reason is the same in all cases, and it's made very explicit in these anxious lectures: the natural sciences, after centuries of promissory notes, have finally come into their own with regard to impressive applications, first in the chemical industries of the mid-nineteenth century, but then in the applications of electricity and magnetism in the latter part of the nineteenth century (the worldwide telegraph system, for example). At that point, you do indeed have a contraction of the word "science" and its use as a closely guarded honorific in French and English.

In German, it's the enormous prestige of at least some of the humanities, particularly classical philology, which, I think, ends up making that contraction impossible—not that there wasn't a movement in the identical direction. One has to remember that in Germany, even the luminaries of the natural sciences, someone like Hermann von Helmholtz or Emil du Bois-Reymond, the great physiologist, would have had a classical education at a Gymnasium. So they had been schooled in Latin and Greek, thought of this as part of shared high culture, and did not wish to sacrifice their credentials as members of a cultural elite. As long as the classics were enshrined in elite secondary education, the place of classical philology as the hardest of the hard sciences was secure. I remember when I taught in the early 1990s at the University of Göttingen—famous for its mathematics and physics in the twentieth century—I gave a talk at their Academy of Sciences, where the President was a classical philologist and the Vice President was an experimental physicist. I was told afterwards that this was because classical philology is harder than experimental physics. I'm not sure this would still be the case, but within living memory it had been.

Science and Culture

SAMUEL LONCAR

That's fascinating because it points then to the importance of culture. For Germans, the idea of *Wissenschaft* is linked to the idea of *Bildung*, and the Gymnasium program is linked to an ideal that comes out of the late eighteenth and nineteenth century with people like Schleiermacher and Humboldt. Do you see that as connected to the distinctive development of, say, German science? Obviously, there are tremendous developments happening in the nineteenth century in French science, but it is the century of Germany. So even just from a competitive standpoint, would you say there's maybe some advantage, if you want to evaluate, to the German broadness? That that system of *Bildung*, that broader vision of education, might have produced? You have scientists and physicists who have a much broader vision, because in their mind they're really thinking *Wissenschaft*, and *Wissenschaft* really is something inclusive that has to do with the whole in its ideal rather than just this very narrow—I guess what we'd call—*Fachmenschen*-type of work, which we would actually think of as a "scientist" in the American or English-speaking context.

LORRAINE DASTON

Yes, I think that's very interesting. There's certainly, for good or for ill, the notion that intellectuals—professors, not just public intellectuals, but academics—are *Kulturträger*, carriers of culture. I say for good or for ill, because that can be very easily abused, and people who are basically citizens like you and I will be asked to pontificate by the media on a topic of which they know not. But the other side of that, for good, is that there is something of an expectation that a scientist in her or his field should be able to present a lucid, well-informed lecture to a *gebildetes Publikum* (a generally educated audience), explaining not only the chief results of his or her research, but also their broader significance.

It's historically fascinating to look at the contrast between, for example, how the pioneers of quantum mechanics in Germany—people like Schrödinger and Heisenberg and also Niels Bohr in Copenhagen—philosophized about the implications of this highly counterintuitive, remarkable new theory, whereas their American colleagues were narrowly focused on the solution of technical problems. So you see the signature of this implicit embedding in the broader landscape of *Wissenschaft*, even in the most technical, scientific work.

SAMUEL LONCAR

When you mentioned Schrödinger, I thought of this quote by him from his 1951 lecture:

it seems plain and self-evident, yet it needs to be said: the isolated knowledge obtained by a group of specialists in a narrow field has in itself no value whatsoever, but only in its synthesis with all the rest of *knowledge and only inasmuch as it really contributes in this synthesis toward answering the demand, 'Who are we?'*

Do you think it's a problem with the current public understanding of science that the vision of science which has come to dominate is one in which the ideal isn't a broadly cultivated person, and yet scientists wield an enormous cultural authority and are asked to speak very generally about the significance of their work, and the culture and the publishing industry wants that and encourages it?

Scientific Authority

LORRAINE DASTON

I think it's a landscape which is mined with all kinds of dangers for scientists. On the one hand, because so much of research is publicly funded—over 90%, I think, still in the United States—not only might the media demand that scientists speak to the public; the public has a right to understand why it's worth supporting science.

So the fact that scientists attempt to explain their work to the public seems to me an entirely gratifying development and long overdue. I'm delighted that the old stigma that used to be attached to scientists who would write a popular book or a textbook is fast disappearing. So that, I think, is an extremely positive development. The problem comes when the scientists are tempted to stray out of an area where they are genuinely experts. For example, although I am a historian, I certainly would never contemplate or countenance offering an opinion on the history of the American West, because I really am not better informed on that topic than your average Jane Q. Citizen.

But the scientists, because of, as you say, capital-S science, are increasingly—particularly, of course, during the pandemic, but also in the context of climate change—being pushed into the limelight with a microphone put in front of them. That's a situation which is dangerous for them and dangerous for the public. There's an additional element that probably erodes what would otherwise be their professional inhibitions about straying from their area of expertise. Because science is publicly funded, to have an article about your work published in the *LA Times* or *The New York Times* or in Germany in *Die Zeit* or the *Süddeutsche Zeitung* is considered a real perk. It's something that could possibly have an impact, however indirectly, on the future funding for your research in a highly competitive field. These considerations also add a motivation, first of all, to go to the popular press before you go to the specialist press, before it's gone through peer review, and secondly, perhaps, to exaggerate the significance of your findings.

That's why I think that the scientists are being forced to walk a tightrope between a laudable desire to communicate with the public and the temptation to hype their results for journalists. This is a situation not entirely of their own making. I think the professional disciplinary organizations or perhaps the American Association for the Advancement of Science needs to offer some guidance on what the professional ethics are in such situations.

How History Can Help Science

SAMUEL LONCAR

And in relationship to such guidance, where would you position your own field? Should scientists know the history of science? Do you see what you do as part of science?

LORRAINE DASTON

I certainly see it as a discipline with rigorous standards. Because I'm a historian of the pre-modern period, the ancient origins of the word *historia* are always echoing in my mind's ear. When I hear *historia*, I hear: we are the people who invented empiricism, all kinds of empiricism; we are the people who study particulars. We historians are not the whole of science, but we are the trailblazers of empiricism.

I think of history as a discipline, one that invented and is still inventing ever new rigorous methods for not only the cross-examination of the sources we have, but even more importantly, the discovery of sources we don't yet have. I look upon the integration of many different strands of evidence braided together into a strong rope of argument in history as identical, philosophically, to the practices of any science. This is one of the reasons why the history of science is of use to science and scholarship.

All of these methods, which taken *in toto*, constitute rigor in any given scholarly or scientific discipline develop at different times under different circumstances. Without knowledge of how differently, for example, in medicine, clinical observation and randomized clinical trials developed, you have no clue, no foothold in the next task, which is: how do you weigh these two kinds of evidence? How do you integrate them? And that holds, I think, *mutatis mutandis*, for all scientific disciplines. So that's one good reason why the history of science is of use to not only the sciences, but all branches of scholarship.

SAMUEL LONCAR

I think you make an extremely compelling case for history and the history of science particularly as a kind of science in the broad sense of a very technical, rigorous discipline. However, as you know, in the natural scientific community, histories of science are not a standard part of their education. I wonder, and this is a large topic, but do you think that it's a mistake in the current natural scientific community that the history of science, based on what you said, is ignored?

The Uses of The History of Science: Ethics, Decisions, and Consequences

LORRAINE DASTON

I do, and I hope that doesn't sound like provincial special pleading for my own discipline. Let me explain why I think it's a mistake. We often have at the institute that until recently I co-directed in Berlin, the Max Planck Institute for the History of Science, scientists, young scientists, coming to us after they have finished a PhD in physics or biology or chemistry, but especially the life sciences, and wanting to do some kind of postdoc with us. It’s certainly of no use to them whatsoever professionally; on the contrary, I’m sure on their CVs, it would stand out like a sore thumb. But the reason is that, because of the combination of the narrowness of research specialization and the intense pressure to produce results quickly, they have no overview of their field. Or perhaps to put it more provocatively, they don’t know why they’re working on what they’re working on. Moreover, they don’t know what the alternatives are.

The history of science has always served two purposes. One purpose has been to give that kind of orientation, really in the Kantian sense: Here’s how the field has developed; this is why it has taken this path rather than another path. In some disciplines—psychology might be a good candidate for this—there were roads not taken, or abandoned, which perhaps are more promising in retrospect because they showed very robust empirical effects. I’m thinking of Gestalt psychology, for example.

So that’s one important use of the history of science: to train scientists. Another use, of course, is for almost any science: to prepare scientists for decisions that no science textbook can prepare them for, namely, ethical decisions. Increasingly, especially in the biomedical sciences, but one thinks also of the Manhattan Project—involving physics and chemistry—scientists will be confronted with decisions about research that have ethical implications. The history of science is not an ethics course, but it can offer case studies of how scientists have dealt well or badly with this in the past and what the consequences have been. One might describe this as a form of sensitization about the importance of making these decisions in a somewhat wider context.

So, I think that’s what the history of science can offer the scientists. I think one reason why the history of science has disappeared from science courses is that it’s no longer offering what the scientists wanted from it, which was a history of triumph, a history of why it is that what we believe now is the only possible, reasonable theory we could embrace. But if there is one moral to the history of science it is: whatever we believe now, we probably won’t believe and should not believe in 10 to 25 years when research has enlightened us further; and to deprive scientists of that triumphal teleology has for many scientists been an enormous disappointment. They've simply stopped reading the history of science.

SAMUEL LONCAR

So that touches on such a deep problem. It brings to mind Kuhn's famous discussion of textbook science when he wrote *Structure*. And if I remember right, I think his first book on the Copernican revolution was taught at Harvard. He had worked with the president of Harvard to develop this course they—

LORRAINE DASTON

Then known as Nat Sci 9 when I took it, yes.

SAMUEL LONCAR

That's wonderful. I often think that is exactly what I would have loved as a science course, and I still think this should be taught, but it seems like it had its time at Harvard and then basically that mindset disappeared from the science courses. But I feel like—speaking of things that weren't picked up—it would have been good if that vision had persevered, but it didn't. You mention some of the reasons, which is that the history of science challenges—how would you want to put it? *Ideology* is a strong word that can be loaded. I don't mean it in a particularly Marxist sense. But you basically suggest a mismatch between the empirical reality of science's history and scientists' preferred mode of understanding their own activity, and that itself is a rather significant thing.

LORRAINE DASTON

Right, and your mention of Kuhn is right on the money because that's where it begins. James Bryant Conant, chemist and President of Harvard but also a very high-level administrator on the Manhattan Project, returns to Harvard after 1945 convinced that democracy will turn into a technocracy unless the world's future leaders (by which he means always, of course, Harvard students) have some understanding of science. They're not going to become scientists, but they have to have some understanding in order as citizens and leaders to make decisions and not to be at the mercy of technocrats. That's the origin of the course you're describing and also Conant's extraordinary *Case Histories in Experimental Science*, a book that can still be read with pleasure and profit. Kuhn was a teaching assistant in that course. He developed some of the modules for that course, and that's the beginning of *The Structure of Scientific Revolutions*.

But what Kuhn ends up thinking is that you've got to historicize science, you've got to understand it on its own terms, you've got to understand it not in terms of what we think now—in which latter case, the history of science is merely a history of errors. You've got to imagine yourself into the rational, if exotic, mindset of the alchemist of the seventeenth century or Aristotle thinking about falling bodies. That kind of historicist program, which has transformed the history of science and made it genuinely historical, has, of course, alienated the scientists because that is not the story they wish to hear.

SAMUEL LONCAR

Yes, and this connects to one of the themes that I have been excited to talk to you about, and, of course, you and Peter Galison discuss it at length at the end of *Objectivity*. For people who maybe don't know, by historicism, we're talking about placing anything, including science, in its own historical context in which you understand it based on that context, based on the way the actors understood themselves and could have understood themselves rather than by, say, standards that are present to us now, but unavailable to them. And when we do that, you end up getting this vertiginous experience in the history of science, much as you do in cultural anthropology, in which you recognize that people weren't just precursors of our current ways of thinking. It's not some linear, progressive history of either people messing up to get to us or helping us get to where we are.

At the very beginning of *Structure*, Kuhn says his goal is to convey the results of a historiographical revolution. And I often reflect that—how many years on?—it seems like it hasn't succeeded yet. In other words, he says we're possessed by an image of science that is going to be changed when this historiographical revolution occurs. But then, as you say, the result of that revolution was a separation of science from the history of science because it wasn't giving scientists what they wanted.

So do you think that that implies something like a long-standing cultural or scientific crisis or revolution at the level of the image of science itself—where scientists want a certain image of science, but the empirical reality actually doesn't supply that image?

I would love to get your input on this because, to me, what it goes back to is the fact that the American, English-speaking context never assimilated, or had their own version of, these debates that you alluded to, in Germany, where the entire Neo-Kantian project was coalescing partly in response to historicism, to figures like Dilthey; and I often worry whether we've made any progress since then because it seems as if the US context didn't assimilate the complexity of that debate. The debate was had in Germany. So how do you see the landscape, given that Kuhn's work now is sixty years old? This earlier context is over a hundred years old. It was over a century ago that Max Weber was already lamenting the sacrifice that the specialization at that time demanded, where he says you just have to accept that you'll be a drop in the ocean.

LORRAINE DASTON

Wissenschaft als Beruf.

SAMUEL LONCAR

Exactly. There is a kind of nihilism in that vision—a stoic nihilism, people might say. But do you think that there's a fear? I was very struck when in *Objectivity* you say all epistemology begins in fear. And so that's part of what I'm hearing as you discuss that. Scientists are—I don't want to put it too provocatively—but frankly they're afraid of the history of their own discipline. What do you think that means?

LORRAINE DASTON

I think it's very important to distribute the blame evenly here. There are three parties who have to pull up their socks, and let me start with my own discipline because we've been talking about Kuhn. It's inconceivable now that someone would write a book with the broad ambitions of *The Structure of Scientific Revolutions*, and that's not just because we know a great deal more about all of the episodes in the history of science that Kuhn discusses. We certainly do, but that is not the point. The point is that with the increasing historicization—we might even say professionalization—of the history of science, we have become more like historians. We always were a kind of strange outpost—underdisciplined and overtheorized—of history. But with that professionalization, which had many, many advantages in terms of the rigor of our work, we've become extremely focused on very detailed, narrow-gauge case studies.

One of the criticisms that Peter and I received for *Objectivity* was that it was presumptuous to cover such a long arc in history and so many different disciplines. The reproach was in some ways understandable: how can you be specialist in all of them? But that specialist stance means that you are condemning the poor scientists who wish to inform themselves about the history of science to slogging through monograph after monograph, each focused narrowly on, say, microscopy between 1830 and 1835 in Manchester. So that's problem number one.

Problem number two are the philosophers who have never risen to the challenge of rethinking what truth might mean if our highest standard for the truths we have—and this I would certainly subscribe to—are scientific truths. That's our highest standard. But those truths change. So we need a philosophical remake of the concept of truth that does justice to the historical dynamism of science. It's not surprising if the philosophers cling to a Platonic, theological notion of eternal, immutable truth, and the poor scientists don't know what to do in terms of reconciling their absolutely sincere belief that they are looking for the truth with the empirical experience of the truth being constantly, as Weber said, surpassed.

And then, of course, the scientists themselves consider almost anything which is not within their discipline, including other sciences, to be blather. So there’s quite enough blame to go around in terms of explaining why it is that we’ve come to this impasse of mutual incomprehension.

SAMUEL LONCAR

That’s wonderfully helpful. So in the first case, the historians have adapted, you could say, a kind of micro-history focus, much like other historians in other fields investigating the legal records of baptisms in Manchester in 1430s. This makes it very difficult to use the history of science. So that’s part of what you’re saying: the history of science is not exactly serviceable?

LORRAINE DASTON

It’s not exactly digestible.

What is True? Science, History, and Philosophy

SAMUEL LONCAR

So there’s the issue of academics’ writing being narrow, focused, and maybe not very inviting. So, it’s hard for scientists to see why they should slog through this. But then related to that is a deeper issue, which you put on the philosophers. Essentially, what you’re presenting is an empirical fact that the history of science reveals, which is that truth isn’t what we think of it as being. If you have an image of truth as an eternal static thing, then you’re going to be extremely unhappy with the history of science because it just doesn’t give you that image. And that is then a pill that’s very hard to swallow for scientists, because they tend to think of themselves as getting at the truth. And I don’t know if we’ve measured this, but my experience of natural scientists is that they’re in practice what philosophers would call “scientific realists”; that is, they tend to think that what they’re getting at is something really like the truth.

LORRAINE DASTON

That's where the philosophers could be very helpful. They could point out the distance between "the Truth," wherever that metaphysical realm is, and actually making contact with reality. Those two are not synonymous.

The Importance and Problems of Disciplines

SAMUEL LONCAR

We can bring in *Against Nature* there. I think this book, to me, is a really wonderful historical essay in philosophy, in the broad sense, and what you said is directly analogous to your argument about nature in the book: nature is much more multiple than any one image of nature, but nature as norm in the background is inevitable for a variety of reasons that you lay out, and maybe truth is like that. Maybe the truth is out there, but like nature, it's much more complicated than we realize. And so we're capable of having different morphologies of truth. That's not going to make everyone happy.

It's a huge problem actually. But we also have this disciplinary problem. The scientists, as you say, often regard anything outside of their own narrow discipline as unimportant, which is how most academics feel. So as a historian or philosopher in the broad sense would you offer any thoughts on that to scientists or others? The disciplinary issue is part of what we're trying to address in *The Meanings of Science Project*. I think it's the fundamental structural technology problem of the academy. Hyper-specialization, I would just say directly, is unscientific unless it's curtailed by some synthetic integrative dimension; it leads to incoherence. So, if we could just start with the disciplinary issue, which I know you have thought a lot about and are even writing about, how do you think disciplines should function?

LORRAINE DASTON

I do not wish to be understood to say that we should blow up disciplines. I believe that disciplines are the repository of intellectual traditions and the slow accumulation not only of knowledge, but of the kinds of methods that we were talking about earlier: in history, for example, of source criticism, archival research, increasingly the use of objects and images as well as text, and the cross-correlation of all these kinds of evidence. The use of maps is becoming an extraordinarily useful heuristic in suggesting hypotheses in history made possible both by computer visualization methods and corpora of data. That is the treasure of disciplines, and to be trained in a discipline is to be trained, first of all, to master that array of methods. But it's also to be trained in an ethos, and I take that ethos extremely seriously, especially in a time in which, unfortunately, scientific and scholarly fraud seems to be on the upswing, which is not unrelated to the Derek de Solla Price exponential curve of the number of scholars and scientists now at work, more than all previous epochs of history put together. It's extremely important that young scientists and scholars internalize the ethos of their discipline. All of that is the indispensable *raison d'être* of disciplines.

In the nineteenth century, a corypheus of the discipline like David Hilbert or James Clerk Maxwell or Wilhelm Dilthey, for that matter, would be asked to give an overview of the field, and so to do exactly the kinds of synthetic work that you point out is now missing and, very important, then to say: I think these problems are the growth areas. The Hilbert problems in mathematics grow out of exactly that kind of effort, which focused the discipline's attention on these problems. That can generate an extraordinarily fertile research program.

Could this tradition be revived now? I don't know whether it's impossible to do or not; that's for someone within those disciplines to judge, but certainly there is no reward for doing it. It's no longer a recognition of one's leading position in the field. It's simply a distraction from your research. I think a lot of that has to do with the frenetic pace of publication, and especially the pressures of applying for grants, which are usually the sole means of financing one's research.

I don't think it's impossible to have this kind of synthetic vision within a disciplinary framework, but it would mean major changes in the incentive system of scholarship and science. It would mean—and I mean this half-jokingly, but only half-jokingly—perhaps a moratorium on the number of articles we can publish in a lifetime. I used to think that we should have a lifetime quota of trees and that after that, that's it. Even with digital publication, the servers are using energy, and we should have a lifetime quota of energy that we can use, which in the same way that certain job applications ask for your five best articles would focus people's attention on the quality of their articles. It would also be very helpful, I think, in saving the peer review system, which has simply collapsed under its own weight. If you have a 4% increase every year in the number of researchers worldwide—and also an explosion of journals, many of them predatory journals—it doesn't take a great deal of math to see that the work of peer review has become impossible. So, I don't think the problem is intrinsic to disciplines. I think it's a problem within disciplines at this moment.

SAMUEL LONCAR

That's very helpful, and I agree. I take it the premise of science is what we would call disciplines. It's learning a kind of ethos, which whether the truth is more complicated than we think, you have to be honest, you have to cultivate some sense of sharing in a community of practice and of research and internalizing those skills—much like the ancient guild structure that the *universitas* was based on, where you learn to become a master craftsperson, as it were.

But then you point out, of course, the challenges with the incentives. I'd just like to pick up on peer review. Do you think peer review itself, as we conceptualize it, is part of the problem? It is recent. Peer review, as we know it, was not part of standard natural scientific practice even in the heyday of these great physicists, such as the founders of quantum mechanics. You could say there was a *de facto* peer review, meaning there were distinguished persons editing a journal: they read an article, they decide to publish it.

But what we currently call peer review is new. The double-blind system, for instance, is not really blind. If it's an elite journal, people know in general who the authors are, and there's actually an extraordinary amount of corruption that that system enables in the way of nepotism, for instance. And that's apart from the scope issues that you mentioned. You say peer review collapsed under its own weight. But do you think that the system as we have it is itself part of the problem? Or do you think that system just structurally can't sustain the volume, and the volume is really the only thing that needs to be dealt with?

Peer Review: Why Einstein Wouldn't Be Published Today

LORRAINE DASTON

I'm of a divided mind about this. The great journals of the late-nineteenth century and the early part of the twentieth did have a form of reviewing, but it's just as you say. Max Planck and Wilhelm Wien, who were editing *Annalen der Physik*, would correspond with one another. I remember looking at their correspondence in Göttingen. The exchanges went something like this: "Well, what do you think of this article?" "I think perhaps this might be interesting to publish with this." But they enjoyed a latitude of discretion, which a conscientious editor these days does not have. Take the case of Einstein's 1905 article on special relativity. This is an article that no respectable physics journal today would print, because it begins with thought experiments such as: "Imagine you were sitting on a train station, and you were looking at the clock." That's the point at which people would stop reading. But Planck said, in effect: "Oh, you know, I think there's a few interesting ideas in here, so why not?" And it appeared in the *Annalen der Physik*. The rest, as they say, is history.

One or two editors with very strong personalities and strong tastes often produce extremely interesting journals with an unusually wide range of articles. But the reverse side of that is that certain people are indeed going to be systematically excluded. And I know for a fact, having looked at the archives, that women were, for example, systematically excluded from that system, and I am sure they were not the only group who were generically excluded by the system. The peer-review system and double-blind reviewing were meant to counteract that kind of prejudice and to create an open meritocracy.

Unfortunately—I can only speak for my own field—I think what it's led to is indeed a system, when it's working well, in which you definitely get rid of the clunkers, but you probably also get rid of the most brilliant and unusual articles. So basically, if you imagine a normal curve, you lop off the two extremes. Because of the enormous pressure on most journals, which always have far more articles submitted than they can publish, if a manuscript gets two reviews (I've certainly seen this happen), one of which says, "This is absolutely brilliant; print it tomorrow quickly," and the other says, "This is trash; it's not worth the paper it's printed on," the editor will take the average of those and say, "C-" and send it back.

It's a system that is very good, as the statisticians say, in eliminating one kind of error, which is truly terrible articles. But unfortunately, it often has the consequence, by the same token, of eliminating the extraordinary, brilliant, and innovative articles. So what is to be done? One solution, which I think is worth experimenting with, are preprint servers. The idea is that you just put up your article on this website, and it gets peer reviewed after it's appeared, so that everyone who's interested in it starts commenting publicly under their own name, and the author can make corrections and edits as the commentary unfolds. This is very useful for specialists in the field who will pounce upon anything that's of interest to them immediately. It's very difficult, I think, for science journalists, for example, because they never know when the article is really finished and refereed.

SAMUEL LONCAR

And this is *de facto* the practice, isn't it, already in fields like physics, where there's a preprint system?

LORRAINE DASTON

And increasingly also in biomedicine, much accelerated by the pandemic, of course.

SAMUEL LONCAR

So in that sense, from a sociological standpoint, do you think peer review, regardless of its merits or demerits, just isn't functional—both for the internal issues you mentioned as well as volume? Do you think that as it is now, if things continue going, even if we wanted to preserve the system as we've had it, do you actually think that's possible?

LORRAINE DASTON

Just looking at the math, just the sheer number of articles being published, it's hard for me to imagine that peer review, as we want it—scrupulous, careful, rigorous review—can be sustained. I don't know anybody in any field who doesn't feel that he or she is absolutely inundated with requests to review articles, and they can't possibly do even half of them. The view that something must be done is widespread; the question is *what*.

It would be useful if there were some international consultation on this, perhaps amongst the national academies of sciences, to lay down guidelines for journals—especially the enormous multiplication of online journals, many of them so-called "predatory journals," which extract fees from authors for publication costs. Desperate authors—particularly, I am told, in China—will do anything because their whole careers depend on publication. There has to be some policing that goes on with regard to the unnecessary proliferation of publications of very low quality.

SAMUEL LONCAR

So it seems like we've identified two or three overlapping problems that occur at different levels of the structure of science. What we've been discussing in the peer-review system actually mirrors as a fractal the broader problems that we discussed, which is that disciplines themselves have a communication problem. So this is one structural motif that's woven into our conversation. Relevant research from a variety of fields isn't known to fields which might benefit from it. This is a very deep problem, and it's only gotten worse over the 100-plus years that it's been recognized to be a problem. A related issue is the actual volume or scope of science has become functionally unmanageable for the systems we have that are notionally designed to regulate its quality. In this case, we're focalizing on peer review. You mentioned that it would be very helpful if there were some international consultation.

But one obvious question is this: Is science, as Price predicted, simply too big? And if it is too big, what might that imply about rethinking it? For example, a natural thing to think—I'm not advocating this, but you might argue that science has always been strongly *de facto* national; that there have been very strong effects of just national scientific cultures, and that the idea of science as a global enterprise, which is, of course, implicit in its ideals of truth, is maybe unsustainable at scope. And so that's a very big issue about the imagination of science.

Do you imagine science as an ever-expanding global enterprise? Or do you think it's worth rethinking in order to make science work, given the problems of volume on their own right? And then given the problems of disciplinary communication, do we somehow need to create systems in which scientists can communicate more effectively—with more qualitative richness—with their colleagues? And is there any way to do that functionally that doesn't involve just saying, here's the problem, but we're not going to do anything serious about the system as we have it?

Can Science Communicate Across Boundaries?

LORRAINE DASTON

I find myself agreeing both to A and not-A simultaneously. I agree with your observation that creating contexts in which scientists and scholars—the problem is identical for many branches of scholarship—can get together, preferably face to face, for intense discussion of results would foster communication across specialist boundaries. That's often the most efficient way of doing it, because you can ask questions immediately. 'Tis devoutly to be wished. Here the main impediment in the age of Zoom is not so much money, but time. This is why one has to think about the incentive system, both the evaluation system and the grants system, which drive everything. If you change the incentive system—to make a radical suggestion, you got rid of the Science Citation Index, which is not a bad thing in itself, but subject to much abuse—there might be greater willingness to indulge in such moments of stocktaking and discussion.

So that's A. Here is not-A. One of the greatest achievements of science, contrary to what anyone would have thought not just circa 1700 but circa 1800, is the creation of the only effective international governance system that we have. In the face of two planetary crises—climate change and a global pandemic—it has not been the UN, it has not been the G8, that got together to diagnose the problem and suggest a solution. It has been the international community of scientists, and I would be extremely loath in any way to undermine the only example of semi-effective international governance we have.

SAMUEL LONCAR

That's a profound observation that science, in a sense, provides our best epistemic model, even as complicated and as flawed as it is. But I agree it's the single most important authority we can turn to in order to answer any questions we have about facts or knowledge—to turn to disciplined domains of serious professionals who have been seeking in a community and a tradition to address these questions as best they can.

And at the same time, as you mention, there's an actual political model that science has provided, which is quite an extraordinary observation. Science as a community, through its pursuit of truth and knowledge, has ended up modeling a form of international cooperation that whatever we do, we don't want to get rid of. So maybe the desideratum is utopian, which is fine, because it's the Republic of Letters, and maybe our job as scholars and scientists is to be utopian. I think of *Marginalia*'s mission as utopian: to bring the Academy and the public together is clearly a utopian ideal. But I think it's worth pursuing, as you say in the book. It's worth pursuing ideals and norms, even if we don't fully realize them.

Thank you so much for your time, for the extraordinary insight and relevance of your remarks and the fact that they embody the relevance of the history of science so well in all the concerns we discussed. I'm so grateful for your time and for your participation in *The Meanings of Science*.

LORRAINE DASTON

Thank you, Samuel, it was my pleasure.

Lorraine Daston is Director Emerita at the Max Planck Institute for the History of Science, Berlin, and regular visiting professor in the Committee on Social Thought. Her work focuses on the history of rationality, especially but not exclusively scientific rationality. She has written on the history of wonder, objectivity, observation, the moral authority of nature, probability, Cold War rationality, and scientific modernity. Her current book projects are a history of the origins of the scientific community and a reflection on what science has to do with modernity. Her most recent book is Rules: A Short History of What We Live By *(Princeton University Press, 2022).*

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