For a History of Human Rationality: An Interview with Lorraine Daston, 2024 Balzan Prize for History of Modern and Contemporary Science

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On 21 November 2024, in Rome, the historian of science Lorraine Daston was awarded the Balzan Prize for History of Modern and Contemporary Science, one of the world's most prestigious academic awards. Administered by the International Balzan Prize Foundation, this award honours the work of scholars with internationally outstanding achievements.

The General Prize Committee recognized Professor Daston «for the extent, originality and variety of her work, which has drawn on a wide range of scientific fields to highlight the mental representations and values underlying research activity».[1]

Daston has mainly explored concepts – such as probability, evidence, rationality, objectivity and many others – which shape our practices of knowledge, structure our thought and constitute the conditions of the possibility of our experience. For Daston, these organizing concepts – as we may call them – come into being through specific historical and social processes, and change and get their meaning from the uses we make of them in a certain period of history.[2] Through tracing the trajectory of scientific objects and concepts in this way, Daston has expanded the field of historical epistemology. She attributes a special role to rationality, which could be described as a sort of meta-

[1] Speech by the president of the International Balzan Foundation Prize, Maria Cristina Messa, Rome, Quirinal Palace, 21 November 2024.

[2] The expression «organizing concept» was used by the philosopher Ian Hacking (1936-2023) in Ian Hacking, "Historical meta-epistemology", in Wolfgang Carl and Lorraine Daston (eds.), *Wahrheit und Geschichte*, Göttingen: Vandenhoeck & Ruprecht, 1999, pp. 53-77.

organizing concept that gets its meaning from the interaction of different organizing concepts and other epistemic elements in a given historical and social context. One of her best-known books, *Objectivity* (2007), co-authored with Peter Galison, charts the history of the conceptions of objectivity that emerged in the last three centuries and shows how each of these conceptions is rooted in an epistemic virtue, as the two scholars call the scientific ideal to which scientists are committed in a particular period or circumstance.

In her writings, Daston has often called attention to the ways of knowing that dominate particular historical or social periods.[3] However, Daston does not propose a systematic definition of a way of knowing instead recognizing the extreme variability and the disparate characteristics of the approaches adopted by different societies. In this way, Daston opens up the potential of a history of knowledge that goes beyond that of Western science, ranging from Babylonian astronomy and herbal medicine to Neolithic agriculture. Since her first monograph (*Classical Probability in the Enlightenment*, 1988), Daston has been committed to a vision of the history of science as an integral part of the general history of knowledge.[4]

This effort of revealing the deep links, at the level of the epistemological foundations of different disciplines, is also evident in books such as *Wonders and the Order of Nature* (1998), co-authored with Katharine Park, and in recent books such as *Rules: A Short History of What We Live By* (2022).[5]

The Balzan Foundation gave me the opportunity to interview Daston in Rome, before she was awarded the Balzan Prize in the presence of the president of the Italian Republic, Sergio Mattarella. The interview is structured in four parts. In the first part, Daston discusses the central questions of her work and the context of her research in history and philosophy of science. In the second part, she illustrates her vision of the history of science and explains how the latter is tentatively becoming the history of knowledge. The third part of the interview aims to expand and clarify some of Daston's research findings in the field of

[3] The phrase «ways of knowing» was popularized by the British historian of science John Pickstone (1944-2014) in John Pickstone, *Ways of Knowing: A New History of Science*, Technology and Medicine, Manchester: Manchester University Press, 2000.

[4] Lorraine Daston, *Classical Probability in the Enlightenment*, Princeton, NJ: Princeton University Press, 1988.

[5] Lorraine Daston and Katharine Park, *Wonders and the Order of Nature*, New York: Zone Books, 1998; Lorraine Daston, *Rules: A Short History of What We Live By*, Princeton, NJ: Princeton University Press, 2022.

the history of science, which are used in the last part of the interview to shed light on some problems that afflict humanity today.

On Historicizing Epistemology

Luca Sciortino (LS): Professor Daston, you have published on a wide range of topics, from the history of statistics to the study of epistemological concepts as objects that evolve, and from the analysis of the emergence of scientific facts to the investigation of how objects become the focus of scientific research. What ties together this seemingly diverse set of problems?

Lorraine Daston (LD): Like all intellectual historians, I am aware of the dangers of enforcing a false coherence onto my own historical preoccupations. However, if there is a red thread that runs through my own set of publications, it is an interest in giving rationality a history. I think of rationality as a multifarious object of inquiry, which is instantiated not only in scientific ways of knowing but also in much more general ways of knowing which are part of our everyday knowledge. What interests me is the way in which new forms of rationality emerge in different historical and cultural contexts and the way in which they eventually become integrated with each other or even come into conflict. A great deal of my work has aimed to reveal the tensions between different forms of reasoning. Ultimately, all I have written can be considered as part of a history of human rationality.

LS: Understanding «rationality», as well as other organizing concepts – objectivity, evidence, probability and many others – which undergo mutations and evolve along with the changes of our practices – is the main aim of a field of research called «historical epistemology». Could it, then, be said that all your research falls into the field of historical epistemology?

LD: If it were necessary to define my work with one short label, «historical epistemology' should be part of the definition. However, I do not think that this label would cover everything I have written. As a historian of science, I work at different levels. The expression «historical epistemology» would leave aside the bread-and-butter concerns of all the historians of science: the detailed and in-depth examination of particular cases. The study of individual episodes in the development of science constitutes the indispensable material upon which everything else is built. And it is an essential part of my work as well as that of all the historians of science.

What is meant today by «historical epistemology» and why is its research programme important?
My original inspiration in using this term was a certain vision of the history of science, which necessarily proceeds at different levels and on different timescales: there are the empirical findings which are published at a breathless pace in the scientific journals on a timescale of weeks and months; there are the slower-moving frameworks of explanation and interpretation of these results, which may unfold on timescales of decades or even centuries; and then there are the ways of knowing which emerge at certain points in human history and that accumulate over centuries and millennia. I conceive of historical epistemology as a research programme that concerns this last timescale. In my work, historical epistemology is about the historical preconditions for the emergence of new ways of thinking and knowing.
LS: The expression «historical epistemology» also refers to an older tradition of thought in philosophy of science born in France in the beginning of the twentieth century when different thinkers started to reflect on the historical conditions of knowledge. How does «historical epistemology» as it is practised today differ from this older tradition of thought?
LD: My colleague Hans-Jörg Rheinberger has written beautifully about the tradition of French epistemology.[6] One of the key ideas from that book is that not only does epistemology in the philosophical sense have a history; it has developed through a set of practices, for example that of experiment. But I am not a stickler for labels. I would be glad to call whatever it is that I do by some other name, if indeed labels are needed. It goes without saying that the work of Bachelard, Canguilhem, Foucault, Koyré and other French thinkers has undoubtedly been a source of inspiration in the history and philosophy of science and continues to be so.
LS: Precisely… and how have these scholars been important in your work?
LD: The first thing that comes to my mind are Bachelard's psychoanalytical insights into the history of science, particularly those concerning styles of thinking that have been quite neglected in these days, alchemy, for example. In particular, Bachelard's analytical insights into the secrets of nature and into why they have been so tantalizing for human imagination were really important to me,

[6] Hans-Jörg Rheinberger, *On Historicizing Epistemology*, Stanford, CA: Stanford University Press, 2010.

especially during my research with Katharine Park on the wonders of nature. Canguilhem's contributions to the history of science were also crucial in my research. I think in particular of the insight into how fundamental the medical categories of «normal» and «pathological» are to so many other forms of thinking. Particularly for my early work in the history of probability theory, the double meaning of the «normal» as both descriptive (what usually is the case) and prescriptive (what should be the case) as analysed by Canguilhem was fundamental.

LS: It seems to me that the so-called «science studies' have also been a source of inspiration of your work… I am thinking, for example, of the social study of the experiment and the construction of scientific facts. Did you reach conclusions different from those of the scholars of science studies?

LD: I think one has to look at the relation between the history of science and science studies in historical perspective. In the 1980s and in the 1990s, there was an extraordinary fruitful cross-fertilization between these two fields. As you mentioned, the studies on the history of experiment carried out in that period were revelatory for historians (and some philosophers) of science – they opened our eyes to the fact that empiricism has a history.

As this example illustrates, one of the signal contributions of science studies has been to historicize what had previously appeared to be a self-evident and timeless part of science.

Leviathan and the Air-Pump by Steven Shapin and Simon Schafer was an extraordinary and emblematic exploration of a historical episode, i.e. Boyle's air-pump experiments in the 1660s, which rightly became an instant classic in both science studies and the history of science. Other works from that period that influenced me were those of Harry Collins and those of the theorists of the so-called Edinburgh school, especially the publications on the problems of credibility and trust in science by Steven Shapin. Another work from that period that reoriented my own research was *The Emergence of Probability* by Ian Hacking, with his brilliant, albeit highly idiosyncratic, interpretation of Foucault. All these works gave historians of science a whole set of new research questions. And then I don't know why it happened… but it happened… after the 1990s the history of science and science studies took two separate paths: the former became ever more historical and the latter ever less. Perhaps through the influence of Latour's work, science studies became more devoted to ethnographic studies of how science is practised today and less attentive to earlier historical periods.

LS: Once you decide that you want to study an organizing concept, i.e. a concept that structures our knowledge about the world, how do you know what kinds of authors to look at in detail and what kinds of material do you explore?
LD: This is a million-dollar question! [Laughing] But yes, I absolutely agree it is the right question to ask… and it is a question I myself ask whenever I listen to another scholar presenting her work – what are your sources, and how did you decide on them? I don't think I have the adequate answer to your question because so much goes on at an unconscious level, both as to the choice of topics and also the mode of inquiry. What science studies taught us historians of science (at least my generation, who had been largely trained as intellectual historians) is the importance of focusing on the way abstract ideals like objectivity or precision are crystallized in concrete practices – here we go back again to your previous question and the glorious days when the history of science and science studies were marching arm in arm. There was never an epistemic ideal so lofty as not to be cashed out in everyday practices… and once you think about those practices, you have a clue as to which sources you should look at. When Peter Galison and I began to consider the concept of objectivity, we decided to focus on workaday practices in the making of scientific images.
But obviously we could have chosen different sources. For Peter's and my part, we decided to make it as concrete as possible by looking at many, many atlas images from disciplines ranging from anatomy to physics, chemistry to zoology.

Towards a History of Knowledge

LS: In one of your articles, you suggested that we are no longer «historians of science» but we are «historians of knowledge». Is «history of knowledge» just a field of research or an emerging discipline? And what is its relationship to the history of science?[7]
LD: First of all, let me explain the motivation for expanding the horizons of the history of science into the history of knowledge, which has to do with the very peculiar history of the institutionalization of the history of science after World Wars I and II. The discipline of history of science emerged in the context of a narrative that singled out the West and its process of modernization as a world-historical moment. A

[7] Lorraine Daston, "The history of science and the history of knowledge", *KNOW: A Journal on the Formation of Knowledge* (2017) 1(1), pp. 131-54.

handful of influential books published between 1920 and 1960 put forward the idea that the locomotive of modernity since the seventeenth century has been European science, later diffused to other parts of the world.

LS: Classics such as *Science and the Modern World* by Whitehead or *The Origins of Modern Science* by Butterfeld…

LD: Yes, but also *From the Closed World to the Infinite Universe* by Koyré, *The Metaphysical Foundations of the Modern Physical Sciences* by Burtt. According to all these authors, modern science not only represented a gigantic intellectual leap forward; it was the essence of modernity. The Scientific Revolution was the Big Bang moment of an irreversible and unstoppable modernity. Although these works were deeply ambivalent about the emotional costs of modernity, they were unequivocal in claiming science to be the prime mover of modern history. This narrative, which led to the institutionalization of the history of science as a means of understanding how science had transformed the world, had enormous limitations and has become increasingly untenable in the light of subsequent historical research.

First of all, during the past thirty years, the new globalized perspective and postcolonial critiques have taken the entire discipline of history by storm, including the history of science.

The explicitly Eurocentric focus of the earlier narratives has been profoundly modified by research into knowledge exchanges resulting from cultural encounters as some (by no means all) European powers expanded their commercial and imperial reach. Secondly, the understanding of what science is and who counts as a scientist has broadened and diversified to include many other ways of thinking and knowing beyond the stereotypical image of a man with spectacles in a lab coat that shaped the post-war narratives. Artisans, herbalists, artists and monks, not to mention women in all capacities, are all now part of the dramatis personae of the history of science, to name only a few groups. Thirdly, and in large part in tandem with the first two developments, historians of science, who once saw science as a quintessentially Western achievement, are increasingly decentring the West.

The upshot of all of these advances in research was that we have found ourselves teaching a narrative that we now know to be seriously distorted. The «history of knowledge» is an opportunity to reconstruct that founding narrative about the origins of modern science and indeed to enlarge the geographic and chronological scope of our inquiries. But

this very amplitude presents its own conceptual challenges. The history of knowledge seems to encompass everything – or everything not currently classified as modern science – and runs the risk of becoming a miscellany. In contrast, the history of science, in part because of its early partnership with both the philosophy and sociology of science, has a long and very sophisticated tradition of thinking about scientific knowledge. Clearly, some version of history of knowledge is indispensable, but the category of knowledge will have to undergo a probing conceptual analysis. So this is a cry… I say to you as a philosopher of science: we need the philosophers [laughing]!

LS: On behalf of the category of the philosophers, I say that it is a tempting invitation… [laughing]. In this regard, couldn't we consider historical epistemology as a kind of conceptual basis for doing research in the history of knowledge?

LD: Mmm… It is a very good question. If your perspective is that of historical epistemology, you might be thinking about what Pickstone called «ways of knowing»; there's surely potential there, although there is also the risk of making a miscellany even more miscellaneous. But you might also be thinking about hierarchies of forms of knowledge. What I mean is that in every culture there are different forms of knowledge and also hierarchies of knowledge, hierarchies of more or less valued forms of knowledge. Some cultures may elevate knowledge of ancient texts to the pinnacle of the knowledge pyramid; others may enshrine knowledge of how to navigate by the stars; still others may esteem mathematical prowess above all other accomplishments. Here is where historical epistemology comes in: why is one form of knowledge more valued than another? What are the implicit or explicit criteria that make a certain form of knowledge superior to other forms of knowledge?

LS: I see… One of the advantages of adopting the new perspective of history of knowledge is that some thorny questions that have tormented scholars throughout the twentieth century lose their importance. For example, you pointed out that it does not make much sense any more to discuss the demarcation principle between what science is and what it is not – whether, say, Islamic alchemy or Babylonian astronomy are really «science». However, it is clear that Western scientific thought has been characterized by specific ways of reasoning such as postulational reasoning or statistical reasoning and some others. It would then seem that, precisely by focusing on the notion of way of reasoning, we could find a demarcation criterion – couldn't we just say

that science is that form of knowledge that is characterized by specific ways of reasoning?

LD: First of all, let's not forget that what we call «Western science» is no longer a phenomenon culturally localized. This in itself is a remarkable fact – it is very hard to find another form of knowledge that has the kind of global reach that science enjoys. Now, saying that science is characterized by some distinctive ways of reasoning is an important point to make, but it would not constitute the reintroduction of the demarcation criterion, which presupposes that these distinctive ways of reasoning are fixed and easily identifiable. But science is a moving target: what makes it so creative and dynamic is that it is always inventing new ways of reasoning and knowing. These new ways of knowing might not necessarily be fully compatible with ways of knowing that characterized science earlier. I don't think we would be well advised to simply try to update the demarcation criterion from time to time.

Instead, what we need is a new way of thinking about the dynamism of science in which new ways of knowing continually emerge and evolve and interact with each other – sometimes reinforcing, but sometimes also conflicting with each other. Among other things, we need to understand how these ways of knowing, developed in different historical contexts for different purposes, come to be woven together. For example, in medicine, clinical observation is ancient, while randomized clinical trials were developed in the twentieth century by Fischer.[8] In the best of cases, these two ways of knowing are complementary, but that is not always the case. We would need a systematic account of how to integrate evidence from these two legitimate scientific ways of knowing.

LS: Another idea that, from the perspective of history of knowledge, seems to be losing steam is that of «Scientific Revolution»… What are your thoughts on this? And, more generally, do you believe in sharp beginnings in the history of science, perhaps under the influence of authors such as Bachelard, Kuhn, and Foucault?

LD: No, the problem with the revolution rubric is that it is too coarse-grained. In science there are novelties all the time and what constitutes revolutionary change as opposed to other kinds of change is very ill-

[8] The geneticist Ronald Fischer (1890-1962) was one of the scholars to introduce the use of randomized experiments.

defined. If I had to think in very schematic terms, I would say: science proceeds on different timescales – the timescale of the headlines in weekly magazines such as *Science* or *Nature* is very different from the slow development of ways of knowing such as those based on controlled experiment or those based on the use of the chemical assay. Trying to fit all these historical and epistemological novelties to the Procrustean bed of revolution seems to me a doomed project. Consider the Scientific Revolution of the sixteenth and seventeenth centuries that loomed so large in older narratives in the history of science. The problem is not that there was no novelty, but that there was too much novelty and that its forms were highly diverse. In the year 1543, whose importance every historian of science knows, both Copernicus's *De revolutionibus orbium colestium* and Vesalius's *De humani corporis fabrica* were published. These two extraordinary moments of history of science led to very different developments. Trying to subsume novelties like these under the rubric of the Scientific Revolution raises more questions than it answers.

LS: You have mentioned a couple of times that the evolution of science proceeds on different timescales… I find this idea very interesting. Could you tell me more about how you see scientific progress?

LD: Scientific progress ticks according to three clocks, I think. The fastest of these clocks is that of empirical discoveries reported in scientific journals. In a musical metaphor we could say that this tempo is allegro, sometimes even prestissimo: very fast. The second clock ticks not at the pace of weeks or months but at the rate of decades or even centuries, and, to continue the musical metaphor, at tempo andante, the pace in which the most important scientific theoretical frameworks are formulated, such as those associated with names such as Aristotle, Newton or Darwin, although they are almost always collective achievements.

And finally, there is a third clock that ticks even more slowly, at the pace of centuries or millennia, at larghissimo tempo: the emergence and persistence of new ways of knowing.

This is why scientific progress has been imagined both as a cathedral being built brick by brick over generations but also as a speeding locomotive racing toward who-knows-where.

These two widespread but contrasting scientific images of scientific progress can coexist with one another because each of them captures a different pace of scientific progress.

LS: When you call attention to the existence of different «ways of knowing» one cannot help but think of the notion of «styles of reasoning» introduced by Hacking in the 1980s.[9] It seems to me that this notion is compatible with many ideas you have expressed so far and, in particular, with your study of objectivity.[10] Do you agree?

LD: Hacking's great insight was that, from an epistemological standpoint, science is pluralistic. His view was antithetical to the ideals of monism and unity in science so dominant in the philosophy of science of the 1970s. It was precisely that belief that there was the science, the scientific method, which made discussions of the principle of demarcation possible. The contribution of Hacking and all those who worked in the field of disunity of science was to show that there is a plenitude of legitimate forms of science.[11] For example, the epistemological criteria of evolutionary biology are very different from those of nuclear physics – evolutionary biology explains but does not predict, yet no one questions its credentials as a genuine science. This insight about scientific pluralism is still fundamental today in both the history and philosophy of science. I share these ideas of Hacking, to whose work and person I owe a great deal. Where I think I differ from him is on the claim of incommensurability, according to which, if you are reasoning in a given style, you are inhabiting a hermetically closed world with its own standards of truth and objectivity.

We find a similar view in Foucault and Kuhn, but I think its origins are in Koyré – it is no accident that both Kuhn and Foucault have Koyré as an intellectual grandfather. I reject this view. As I said before, when different styles of reasoning emerge at different moments or in different contexts, then subsequent science faces the challenge of reconciling these styles with one another. In cases in which there is a tension between them, they are not incommensurable; they are in constant dialogue with one another, and they often modify each other.

LS: Right… however, I believe that another crucial difference between your views and those of Hacking is that you think that there are many

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[9] Ian Hacking, "Language, truth and reason", in Martin Hollis and Steven Lukes (eds.), *Rationality and Relativism*, Cambridge, MA: MIT Press, 1982, pp. 48-66; Ian Hacking, *"Style" for historians and philosophers*, in Hacking, *Historical Ontology*, Cambridge, MA: Harvard University Press, 2002, pp. 178-99.

[10] The connection between the notion of style in Hacking and the concept of objectivity is explored in Luca Sciortino, *History of Rationalities: Ways of Thinking from Vico to Hacking and Beyond*, Cham: Palgrave Macmillan, 2023, Chapter 3.

[11] Peter Galison and David Stump (eds.), *The Disunity of Science: Boundaries, Contexts, and Power*, Stanford, CA: Stanford University Press, 1996.

more styles of reasoning than those mentioned by Hacking… and which do not have all the features mentioned by him…[12]

LD: Yes, perhaps I have a thinner idea of these ways of knowing… of these styles of reasoning, as he calls them. But the main point of disagreement is that I do not think that these styles commit you to a world view. It was Koyré's legacy in both Kuhn and Foucault that, I think, Hacking absorbed. I disagree with him on that.

LS: Ludwik Fleck's «thought style», Michel Foucault's «episteme», Thomas Kuhn's «paradigm» and Imre Lakatos's «research programme» are other examples of analytical notions for modelling the evolution of science. What are your thoughts on these notions? Do they capture the richness of the history of knowledge?

LD: The thing that I find more stimulating in these authors, much more than their Hegelian efforts to wrestle the history of science into a grand theoretical scheme, is that they tried to apply their schemata to concrete cases. This is no doubt a personal quirk on my part: I tend to think in terms of examples. I suspect that most of the works of the authors you mention have been produced by generalizing from concrete cases. I am thinking of Lakatos's *Proof and Refutations*, which is an absolutely brilliant work in the philosophy of mathematics based on extraordinary historical examples, each fascinating in itself. If one discarded the entire superstructure of *Proof and Refutations* it would still remain a brilliant approach to the philosophy of mathematics – and a gauntlet thrown down to the history of mathematics.

LS: Is there anything important to learn from the history of science about knowledge? What has it taught us more than, say, epistemology?

LD: First, regarding relations between history and philosophy of science, I belong to a shrinking minority that believes that these are two fields that need each other. For me, philosophy has been a source of probing questions to the history of science and a welcome prod to think beyond the specific case study at hand: it is a way of answering the so-what question – why should anyone except fellow specialists be interested in this case?

For the philosophers, I would hope that engagement with the history of science would bring them closer to the way in which science, all of it, past and present, has actually been done. But I realize that this view is considered eccentric by most of my colleagues. To the broader

[12] On the problem of what should be counted as a style of reasoning see Luca Sciortino, "What is a style of reasoning?", *Transversal: International Journal for the Historiography of Science* (2023) 15, pp. 1-20.

question, what we can learn from history, my answer is one word: surprise. The great problem with abstractions is that they strive to be systematic, and when you strive to be systematic you increasingly foreclose the possibility of surprise. If there is one thing that is characteristic of the dynamism of science, is the continuous production of surprise.

Insights into Human Rationality and Its Practices

LS: Let's now talk about some results of your research… You have described the concept of nature as a three-layers concept: «specific nature» (what makes a thing what it is), «local nature» (a distinct configuration of a particular place), «universal nature» (the order of the universal laws).[13] Do all these different conceptions of nature still coexist today?

LD: I think all these conceptions (and more) of nature are present today. For example, in the discussions about human nature, when we ask what is natural and what is unnatural about us, there is an echo of the «specific-nature» conception, the idea that there are traits that are inborn or spontaneous. Human beings cannot easily imagine a world without specific natures, which would be a world in which everything transforms into something else. Likewise, we can easily detect the conception of «local nature» – more particularly, the disruption of the delicate ecological and meteorological balance that characterizes a local nature – in the discussions with regard to climate change. Finally, nature as «universal natural laws» is also present in today's imagined moral orders: the generality asserted on behalf of human rights, regardless of time and place, echoes the generality of natural laws.

LS: One can't help but remember that in his book *The Veil of Isis*, the historian of philosophy Pierre Hadot argued that there are two contradictory approaches to nature: the Promethean, which uses technology to tear the veil from Nature and reveal her secrets, and the Orphic approach, which instead exploits the resources of poetic discourse and assumes that a denuding of Nature is a grave trespass. Hadot's perspective is significantly different from yours …

LD: Yes, *The Veil of Isis* by Hadot has not been of very much use to me in thinking about nature, although I greatly admire his work on spiritual exercises in ancient Graeco-Roman philosophy. *The Veil of Isis* contains a strong Goethean strain, and Goethe's tradition of natural

[13] Lorraine Daston, *Against Nature*, Cambridge, MA: MIT Press, 2019.

philosophy resonates with the Orphic approach. There is also an unconscious – or perhaps conscious – echo of Nietzsche's opposition between the Dionysian and the Apollonian in Hadot's book. These are not my categories, and I am suspicious especially of the Orphic (or the Dionysian) category, which is so saturated in Romanticism, which has exerted an unhealthy influence on the history of science in its Carlylean cult of heroic individuals and occluding the role of the collective.

LS: Another question you have attempted to answer concerns how «the scientific community» came into existence. Scientific collaborations at an international level seem to work well while other forms of collaboration often do not achieve the results they want to achieve. How do you explain this difference?

LD: I think the first answer to give to your question is to point out what a wonder it is that a scientific community ever came into being at all, in whatever form. Even the most impressive early efforts at international scientific collaboration often ended in disarray. For example, in 1761 and 1769, more than 250 astronomers joined together in what can be considered the first international scientific enterprise: the observation of Venus' transit for the calculation of the Earth–Sun distance (the astronomical unit), which would in turn provide the absolute scale of the solar system. Although these two expeditions cost a great deal of money and even human lives, they brought no conclusive answer to the questions they were meant to answer because of lack of coordination and rivalries among both nations and observers. Against this background, the emergence of the first sustained scientific agreements and collaborations in the latter half of the nineteenth century was remarkable. I wouldn't take the permanence of the current version of the scientific community for granted. There are obvious internal and external pressures: competition and rivalry are examples of internal pressures, wars such the invasion of Ukraine and calls for scientific boycotts are examples of external pressures. The scientific community is an extremely fragile construction: there is no central organization, there is no United Nations, there is no pope, there is no parliament, there is no president. I could well imagine a history in which a scientific community emerges, develops and then disappears. Ultimately, I do not think that scientific cooperation, as compared to other types of cooperation, is so distinctive that its survival is guaranteed.

LS: Another set of questions at the centre of your interests concerns how objects or phenomena become the focus of scientific research.

How does it happen that an object that has not been noted for ages becomes of scientific interest? Can you provide some examples?
LD: The first thing to say is that the history of science reveals that objects of scientific inquiry can come into being and pass away. Not all the efforts aimed at elevating an everyday object into an object of scientific inquiry succeed permanently. The second thing is that there seem to be many ways in which an everyday object can achieve – however briefly or however durably – the status of scientific object. One of them is simply to become an object of social interest. For example, you may say that «race» is a very bad object of scientific inquiry, and indeed there are schools of thought among biologists who argue that «race» is a phantasm that has no place in scientific research. Nevertheless, this sort of phantasm persists in biomedical, and not only biomedical, research. The obvious reason why race, among all the possible dimensions of human variations, became an object of scientific inquiry in the late nineteenth century has to do with colonialism, imperialism and slavery in the United States and elsewhere. Once established as a burning issue socially and politically, it then became an object of scientific inquiry. This is just one of the many ways in which an object becomes the subject of scientific inquiry.
LS: For my part, I was thinking of fossils…
LD: Oh yes, fossils are another very good example. They were regarded as objects demonstrating nature's plastic powers or the seeds of a species that had somehow germinated in stone before being generally accepted as the imprint of organisms preserved in stone. But much more interesting is what happens if you choose the wrong scientific object.
LS: This has to do with the contingentism/inevitabilism debate in the history of science…
LD: Exactly… James Clark Maxwell once said that if Galileo had started off with turbulence instead of falling bodies, physics would still be in its infancy, overwhelmed by the complexity of the phenomenon. Galileo very cannily chose as a scientific object what he could deal with mathematically.

A Glimpse into the Present (As Well as the Future)

LS: Let's move on to questions about issues that are of great importance today… Due to global warming and all the environmental problems, we feel that the human species could become extinct. Can history teach us anything about this?

LD: What is interesting about the religious apocalyptic tradition, and not only that of Christianity, is that the destruction and re-creation of the world are completely independent of human agency. They are predestined events, which unfold on a divinely ordained timescale. Ours is a different situation – we not only know the causes; we are the causes, and we therefore have it within our power to at least mitigate their effects. Apocalyptic frameworks, although they still enflame our collective imaginary, are therefore inappropriate for confronting this kind of emergency. That said, it is worth asking ourselves why we should care about whether or not future generation might survive. In his beautiful book *Why Worry about Future Generations?*, the philosopher Samuel Schefer argued that anything that we do right now would lose meaning if we thought that there will be no human beings in one hundred years.[14] This very powerful thought experiment is a motivator completely outside the realm of any religion.

LS: Once you said, «History never repeats itself»… Can you explain what you meant?

LD: The «never» must have a footnote and become «never exactly». Was it Mark Twain who said that «history does not repeat itself, but it rhymes»? The older I get the more I can see the wisdom of this comment. To give just one example, any historian of early modern Europe who has studied the broadside literature of that period, which abounds in reports of portentous events such as monstrous births, celestial apparitions, rains of blood and other bizarre phenomena of dubious authenticity but polemically useful, will not be surprised by the explosion of fake news on the Internet. Despite many obvious disanalogies between the new print media of early modern Europe and the new digital media today, both offered unregulated opportunities for the propagation of attention-grabbing stories and images in the service of religious or political factions.

LS: Does this reflect the constancy of human nature, or rather, of «specific» human nature?

LD: It has also to do with the possibility of unregulated human technology.

LS: Can intellectuals still influence society?

LD: I think the answers to these questions differ depending on which intellectual tradition is at issue. In the case of the United States, for example, I would be quite pessimistic about the possibility that

[14] Samuel Scheffler, *Why Worry about Future Generations?*, Oxford: Oxford University Press, 2018.

intellectuals can influence the course of history, given that country's long history of vehement anti-intellectualism. And not only in the United States but also in the United Kingdom. Yet in other intellectual traditions – for example, in Germany, France or Italy – intellectuals still command some public attention, for better or for ill.

Surely, there is a public appetite for ways of making sense of the rapid succession of technological innovations to which we are all subjected, which can help us understand how the new technologies are reshaping human reality and how they are enabling, empowering or constraining us. Now, whether or not scholars are the right people to do it is another question. Perhaps because I am American, I have a deep suspicion especially of Nobel laureates who pronounce upon questions very far from their area of scientific competence. If we think of William Shockley and his racist theories, we realize the risk we run when intellectuals abuse the authority given to them.[15] A Nobel Prize, no prize, should not turn the laureate into a prophet. Nonetheless, intellectuals can contribute explanations and interpretations of events that seem to break over us like thunderstorms.

LS: Do emotions still play an important role in science today?

LD: Yes, absolutely. The biologist Medawar wrote a vade mecum for young scientists in which he informed their families that the scientists were in the grip of a powerful obsession that left little room for such occasions as children's birthday parties or holiday celebrations.[16] For Medawar, becoming a scientist demanded an emotional attachment to research bordering on monomania. Another example of strong emotions in science concerns the cases of scientific fraud or any other trespass against an epistemic virtue, which evokes scandalized indignation within the scientific community concerned. Such vigorous responses suggest that while the values of science may not be those of the society at large, they are as emotion-laden as more generally recognized moral values.

LS: Now I would like to ask you a question that comes to mind when thinking about your book *Objectivity*: does the use of artificial intelligence in science imply new forms of objectivity?

LD: It might… though I should once again emphasize that I'm no expert in this area. In the last five years everyone has been made

[15] William Shockley (1910-89), who was awarded the 1956 Nobel Prize in physics, defended racist policies.

[16] Peter Medawar, *Advice to a Young Scientist*, New York: Harper & Row, 1979.

conscious of the epistemic, moral and political risks of certain applications of AI – for example, the prejudices unwittingly built into the facial-recognition programs which were trained only on white male faces and therefore led to real miscarriages of justice for other groups of people. Another risk has to do with machine learning, for example, when programs that must be trained on vast quantities of data simply hoover up whatever is available on the Internet and thereby indiscriminately incorporate falsehoods, bigotry and outdated information.

LS: Do you mean that AI incorporates new ways of reasoning?

LD: I think we have to distinguish between two historical periods of AI: what might be called classical AI was an attempt to imitate human patterns of reasoning such as that you mention; and machine learning, which uses an entirely different way of reasoning based on correlations discerned in immense masses of empirical material in order to find patterns by a kind of brute induction, made possible by high-speed computing power. Classical AI aspires to be deductive and economical, using what were then scarce resources of computer memory and operational speed most efficiently; machine learning is inductive and statistical on an unprecedented scale, and wastrel with such resources (and also, it should be added, with the electricity needed to power projects like large language models). Machine learning may therefore qualify as a form of reasoning qualitatively different from human reasoning.

LS: You made a distinction between «thick rules» and «thin rules». Thick rules come with exceptions and caveats while thin rules have clear boundaries and apply to all the situations uniformly. You have argued that today thin rules prevail. What are the implications for our lives and our freedom?

LD: Everyone has experience of what thin rules entail for our lives. We have probably all experienced the frustration of filling in an online form and discovering that our case fts none of the options provided. This is an example of thin rules, which presuppose a stable, homogeneous, predictable domain of application without exceptions. This form of rule always assumes an implicit default «normal situation», what happens most of the time (but not always) and, by implication, what should happen. This is a classic «Canguilhem situation» of the «normal» and the «pathological»: the algorithms presuppose a definition of «normality» and classify as pathological anything that falls outside of that «normality». Human variability becomes «pathological». Thick

rules, in contrast, come upholstered with examples and exceptions in their very enunciation, in order to prepare the rule follower for the range of variable cases to which the rule will have to be applied in practice.

LS: One final question: what are your current projects?

LD: I'm currently working on a book project tentatively titled *Thinking with Disasters*, based on a series of lectures I gave at Yale University in April 2024. My working thesis is that the way we think about disasters, which in some ways are the prototypical historical event, has deep implications for how we think about both metaphysics and morals. It's a project that has already had me reading medieval annals, early modern sermons and contemporary insurance records, and I've only just begun research. So I look forward to seeing where this vast topic will take me next.