gagement with "the real world" and thus ran the risk of being challenged. As such, she once again sought to develop a standpoint that avoided the pitfalls of both relativism and reductionism.[6]

The Invention of Modern Science touched on various themes that over the past decades have become increasingly popular among historians of science. The latter have shown, for instance, a growing concern for the intricacies of science in nonlaboratory contexts and for the role nonhuman actors can play in historical narratives. Historians are reassessing the notion of "whiggishness," and there has also been considerable soul-searching about the relation historians of science should maintain with practicing scientists. Yet, unlike her intellectual fellow traveler Latour, Stengers has not proven to be a major influence on these new historical directions. Whereas Latour provided historians of science with metaphors for understanding historical reality (ranging from "immutable mobiles" to "black boxes"), *The Invention of Modern Science* particularly invested in a concept that could influence their attitude and self-understanding ("humor"). Among most historians, the former seemed more appealing than the latter.

Although *The Invention of Modern Science* originated in the intellectual climate of the 1990s, its key concerns are still relevant twenty-five years later. The rise of "post-truth politics" and the neopositivist backlash it engenders in some circles threaten a "humorous" approach to science once again. In such a context, it can be fruitful to reread *The Invention of Modern Science*. Historians of science might struggle to understand all of Stengers's philosophical subtleties (I readily admit that I was lost on more than one occasion), but they will also find a lot worth reflecting on. Or, as Latour argued: "You grind your teeth on her argument and you feel much better afterward."[7]

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[6] Isabelle Stengers, "La guerre des sciences: Et la paix?" in *Impostures scientifiques: Les malentendus de l'affaire Sokal*, ed. Baudouin Jurdant (Paris: La Découverte, 1998), pp. 268–292.

[7] Latour, "Foreword" (cit. n. 2), p. viii.

Taking Scientists (More) Seriously

Valérie Leclercq
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What makes one theory stand out among others? What makes one observation more legitimate, more "scientific," than another? According to Isabelle Stengers, the element of "risk" is essential. To establish legitimacy, scientists have to present natural phenomena in such a way that they challenge established opinions, exposing themselves to (and at the same time antic-

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ipating) the critiques and counterarguments of fellow scientists. The question "What makes something scientific?" is at the heart of Stengers's 1993 book.[1] And her reflections on the matter may still inspire historians of science today. To be clear, Stengers does not answer this question. Rather, she suggests that the question itself is part of the machinery of modern science, as it has developed since the seventeenth century. The lack of a satisfactory answer in the philosophy of science is, for her, less important than the centrality of the question itself: it requires one to take a position. Whether one agrees or not, whether one defends or criticizes science, we all refer to it somehow. To put it differently: it is almost impossible to escape from the frame that science imposes on us. In that sense, Stengers argues, we are all "heirs" to the invention of modern science.

Despite these major historical claims, *L'invention des sciences modernes* never attained the status of a "classic" in the history of science. The book was rarely picked up by historians, unlike other works from the early days of science studies (e.g., Bruno Latour's books). Stengers's study is above all a piece of philosophical scholarship (and—be warned—quite a difficult read). But its disciplinary grounding alone offers insufficient explanation for the lack of attention it has hitherto received. For theory, whether based in philosophy or in sociology, has often inspired historical research. Latour's work, to point to an obvious example, contributed to the success of an "anthropological" perspective in the history of science. Rereading Stengers's book, we feel that it deserved more attention from historians. What intrigued us most is that the book does not take such an anthropological (what Stengers would call an "outsider") perspective on science. Rather, it considers science as the product of a historical and political process in which not only scientists but also philosophers—and perhaps historians—are themselves embedded.

Science, in Stengers's view, does not exist outside the flux of human history. It is the use of scientific theories and concepts by scientists, scholars, and laypeople that stabilizes and validates them as scientific facts, making them more and more indisputable until a new way of seeing things emerges. Here Stengers suggests an alternative to T. S. Kuhn's "paradigm shift": the concept of "event." An event, such as Galileo's inclined-plane experiment or the discovery of the double helix by Francis Crick and James Watson, determines a "before" and an "after," transforms our worldview, and opens a new infinity of questions and possibilities. But it is not the crystallization of a scientific truth. An event is only an overture, a debate space. What is scientific, ultimately, is the theory that will hold through time, resisting controversies and attempts to reduce it to an opinion; it is what will later be accepted as having made history. Historians of science and medicine today are familiar with the notion that scientific concepts are unstable and have a historical thickness. But if, as Stengers asserts, the very "scientificity" of scientific facts is created not by a methodological but by a historical process, what could this mean for our discipline?

But that's not all. This historical process of science production is also a political process. Scholars and scientists have long since demonstrated that science is never pure, never isolated from social and political forces. But Stengers goes a step further when she argues that making science is a way of making politics. To her, science is political, not because it is subservient to or influenced by politics but because the (historical) process through which it is created is political in nature: it is a collective process that entails debates, leads to the veto or ratification of scientific propositions, and, ultimately, affects society as a whole. Both science and politics are engaged with the question of how to conceptualize and represent the world; both seek a form of generalization. Politicians aim to speak not for themselves but for a community of people; scientists seek universal acceptance of their theories as truth rather than as mere personal opinion. If we apply this stance to our own research on the workings of medical and scientific institutions, Stengers's claims stand as an invitation to study these commonalities anew and to scrutinize the smaller, more tangible

[1] Isabelle Stengers, *L'invention des sciences modernes* (Paris: La Découverte, 1993).

ways in which science resembles politics. Parliamentary tradition in the Western world, for instance, developed parallel to traditions of debate in learned societies and scientific academies. It is tempting to push these comparisons further and look at scientific speech as a political act.

The fact that these ideas, to some degree, still sound new to us is an indication that Stengers's work is worth returning to. However, *L'invention des sciences modernes* is also clearly of its time and so, in some aspects, proved unexpectedly familiar to us. One example is the way the book tried to navigate a middle way between two antagonistic views that, up to that point, had heavily influenced the way scholars and scientists thought about science. The "classic" positivist philosophers, whom Stengers first refers to, understood science as being a unique form of knowledge and practices with the ability to uncover universal truths. Relativist sociologists from the second half of the twentieth century, on the other hand, approached science as an unstable product of human interpretations and conventions, no different from any other form of knowledge. Stengers's intermediate position echoes historians' attempts, in the same period, to find a middle path between the early positivist and whiggish history of science and the constructionist, structuralist, and sometimes stringently anti-institutional approach that defined most pioneering historical works of the 1950s through the 1980s.

In her act of rebalancing, Stengers points out that skepticism, as it is expressed by sociologists, among other scholars, is a good thing. In fact, it is an essential component of scientific practice, including in the field of history. But it also led, in the humanities, to a facile habit of denouncing the subjects of our studies—for example, by analyzing their "strategies" or scrutinizing their arguments as a form of "rhetoric." Stengers enjoins sociologists and philosophers of science to take scientists' words seriously and rediscover a little bit of the admiration that they inspired in the past. In the last decades, historians have striven to do something akin to what Stengers prescribes: to understand more fully—in all their nuances and without overlooking the problematic systems they contributed to—the doings of historical actors that had been grossly caricatured by antiestablishment and structural analyses of the 1950s–1980s. In the field of the history of psychiatry, for instance, we remember being warned during our training years against dismissing psychiatrists too quickly as either medically ineffective "quacks" or Foucauldian agents of social control. Trying to assess their work fairly meant seeking to understand their ideas and motives (and doing the same with other historical actors, such as nurses and patients). By proceeding in this way, it became possible to complement (not replace!) the narrative of domination with an alternative history of subjective psychiatric effectiveness, of shared struggles between physicians and patients to find solutions to the pressing issue of mental illness.

Stengers goes more deeply into this relation between scholars (philosophers, sociologists, historians) and the subjects (scientists) whose work they investigate. She proposes several strategies for diminishing the distance between observers and actors and for taking the latter more seriously. One of these is humor, an approach she distinguishes from irony. If irony creates distance—historians may write with a certain irony about the past precisely because they have the privilege of hindsight—humor creates correspondence. The humor Stengers proposes is related to perplexity, to a type of bewilderment about the dynamics of science as something shared between philosophers and scientists, between historians of science and scientists in the past. The idea is not to laugh at scientists but, rather, to acknowledge—by way of humor—that we cannot escape the process through which science is constantly being renegotiated and reinvented. Scientists, philosophers, historians—we all take part in it.

But how to put this into practice? In our own research in the history of medicine, we have found that physicians in the past sometimes tried to answer the same questions we face as historians. This is true, in particular, for the debate over the status of medicine as an "art" or a "science"—a debate that has been going on among doctors at least since the eighteenth century and that was later picked up by historians. This shared interest across time, we feel, creates a proximity

between historians and their subjects that sits somewhat uneasily with historians' self-images as distant, critical observers. Or, to use the metaphor that became popular with the success of science studies, it does not fit well with the image of historians as anthropologists studying distant tribes. Stengers proposes a different metaphor: that of the political scientist. The latter comments on, breaks down, and dissects politics but also regards himself as part of the political milieu. Even if this metaphor does not apply fully to the relation between historians and science, it is challenging to think about how historical scholarship impacts science, about the way historians, by studying science in the past, contribute to the shaping of its present form. In that sense, Stengers's philosophy empowers those who plead for a more self-confident stance on the part of historians in the debate about the future of the sciences.

As is always the case with philosophical theory, the ideas and concepts put forth in *L'invention des sciences modernes* are not directly "importable" into our work as historians. As a result, Stengers's book is in equal measure frustrating and inspiring. But even now, her focus on processes, her philosophical take on history, and her will to laugh with scientists force a displacement of perspective that could be fruitful. Moreover, Stengers's erudition and knowledge of scientific theories feel like an invitation to get closer not only to scientists but to the content of their work as well. As John Harley Warner pointed out two decades ago, historians' long-standing feeling of illegitimacy when writing about science, as well as their rejection of positivist history, has often led to an exploration of the context of science rather than to an analysis of scientific theories.[2] Stengers's work, in a way, is a reminder that form and content make up an indivisible whole. Stengers has no clear answers to present, no shortcut, no easy definition—and she does not seek any. What she offers is what she hopes for from her peers, scientists and scholars alike: boldness as thinkers and a recognition of the scientific practices (and the faith in them) that bind us together.

[2] John Harley Warner, "The History of Science and the Sciences of Medicine," *Osiris*, 1995, N.S., *10*:164–193.