· *INTRODUCTION TO THE 2011 EDITION* ·

Up for Air: *Leviathan and the Air-Pump* a Generation On

When Princeton University Press asked us whether we were interested in participating in a new edition of *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*, first published in 1985, we were pleased to have an opportunity to reflect on the circumstances of the book's original composition and some aspects of its early reception. In addition to this substantial new introduction, a decision was made to omit the translation of Thomas Hobbes's *Dialogus physicus*; otherwise, the text is unchanged.

. . .

There are two technologies especially relevant to this new edition of *Leviathan and the Air-Pump*. The first one is obvious: the air-pump. Its physical operation and its role in making seventeenth-century scientific knowledge were this book's stated subjects. It has been said that what distinguished this way of telling a historical story about science is that its "real hero" was not a person but an instrument.[1] The second technology is not so obvious, nor was it obvious to the authors when they wrote the book over a quarter century ago. Just as the air-pump was a device for making scientific knowledge of a certain kind, so this technology was a device for making historical knowledge of a certain kind. That technology was a typewriter.

It's useful to bear this second knowledge-making technology in mind since its workings were transparent to the authors in the mid-1980s when it served their knowledge-making purposes. But it was just about the last piece of work either of them produced using that technology. Within a year or so, like practically every other academic, they entered the digital age. They did not reflect on the relationship between the typewriter's capabilities and limits, on the one hand, and the forms of intellectual and social order it produced, on the other.

[1] Bruno Latour, *We Have Never Been Modern* (Cambridge, Mass.: Harvard University Press, 1993), p. 17; also Ian Hacking, "Artificial Phenomena," *Brit. J. Hist. Sci.* 24 (1991), 235–241, on 235–236.

But they could have done so: the typewriter, the pristine sheet of bond paper, the little jars of Tipp-Ex or Wite-Out (if you came into the world of writing after the late 1980s, you'll want to look up what those things were), the reflective pause before striking a key and confronting the relative permanence of ink newly deposited on cellulose, and the modes of interaction framed by the limits of the telephone and the postal system—its units then not known as "snail mail"—for conveying the physical forms of knowledge—then not known as "hard copy"—from one person to a distant other. Who is to say that the limits, opportunities, and emotional texture of typewriter-based knowledge making are not significantly different from those based on the personal computer and the Internet?

That subject deserves another book, perhaps a book sharing some of the same sensibilities as those in *Leviathan and the Air-Pump.*[2] Drawing attention here to the technologies of writing is mainly a way of reminding ourselves and our readers of how long ago this book was written. The long-ago-ness of the typewriter world has its match in the academic culture from which the book proceeded, into which it was an intervention, and which it apparently—it has been said—did something to change. There were some positive reviews and some outraged ones, but, taken as a whole, the initial reception was flat rather than unfriendly, and there were no academic prizes. That's the fate of most research monographs, and while the authors were mildly disappointed, disappointed authors are not unheard of, and both went on to whatever was next. We cannot recall exactly when it became apparent that the book had eventually found a readership, but it was surely many years after the period usually allocated for reviews and prizes had passed.[3] There are aspects of the book's current celebrity/notoriety that we find at least as worthy of curiosity and of interpretation as its initially bland reception, and we will later suggest some possible explanations.

[2] For examples of such histories, see Hugh Kenner, *The Mechanic Muse* (New York: Oxford University Press, 1987); Friedrich A. Kittler, *Gramophone, Film, Typewriter*, trans. Geoffrey Winthrop-Young and Michael Wutz (Stanford, Calif.: Stanford University Press, 1999; orig. publ. 1986); and Delphine Gardey, *Écrire, calculer, classer: comment une révolution de papier a transformé les sociétés contemporaines, 1800–1940* (Paris: Découverte, 2008); also Marshall McLuhan, *Understanding Media: The Extensions of Man* (London: Routledge, 2001; orig. publ. 1964), pp. 281–288. It was reported in April 2011 that the Indian firm Godrej and Boyce, the last company in the world manufacturing typewriters, had closed its Mumbai production plant.

[3] Jan Golinski, *Making Natural Knowledge: Constructivism and the History of Science, with a New Preface* (Chicago: University of Chicago Press, 2005; orig. publ. 1998), p. viii (for comment on the book's "strangely delayed" reception).

The original blandness did not continue very long. If you don't want to read the whole book, there is now a lengthy Wikipedia summary, and if you are afflicted with an assignment to write an essay on the thing, you can buy one from online sources. On the one hand, the book is widely cited—in all sorts of contexts and disciplines—while, on the other, it has generated a small industry, notably in philosophy of science, and in what is sometimes called "anticonstructivist" theory, devoted to countering what are taken to be its key arguments and methods. We will later speculate about the contexts in which the book has—and has not—attracted notice. But our attention here is principally on early reactions to the book, notably on how its *reviewers* approached the thing and made what sense they could of it. So we take this opportunity, not so much to defend the book, but to place it in the historical setting from which it emerged, and then to speculate about why it seems still to be read and commented upon. Despite the fact that some people apparently now read the book to learn something either about seventeenth-century science or about how one might think about science generally, *Leviathan and the Air-Pump* is a product of its times, a report on historical episodes and itself a historical document.[4] It is a moment in changing scholarly traditions, changing cultural and institutional settings, changing conventions, problems, and purposes.

Historiographic Tradition

There is a passage towards the end of the book that—perhaps more than any other—stands witness to the cultural setting out of which it came and that, at the same time, shows how the authors tried to effect change in that setting:

> The language that transports politics outside of science is precisely what we need to understand and explain. We find ourselves standing against much current sentiment in the history of science that holds that we should have less talk of the "insides" and "outsides" of science, that we have transcended such out-

[4] There are a few comparable attempts by historians of science to situate their work in the historical setting that spawned it; see, for instance, the interview in Thomas S. Kuhn, *The Road since Structure: Philosophical Essays, 1970–1993, with an Autobiographical Interview*, ed. James Conant and John Haugeland (Chicago: University of Chicago Press, 2000), pp. 253–323; Robert M. Young, *Darwin's Metaphor: Nature's Place in Victorian Culture* (Cambridge: Cambridge University Press, 1985; art. orig. publ. 1973), pp. 167–179.

moded categories. Far from it; we have not yet begun to understand the issues involved.[5]

Historians and sociologists of science coming into the profession from, say, the early 1990s may not find it easy to understand what this was all about, and, if so, that's because certain of the categories addressed in that passage now scarcely exist in mainstream academic studies of science and historical change. Few respectable historians or sociologists of science now assess the relationship between what were once routinely referred to as "internal and external factors"; fewer still say that what they are trying to do is to assess the relative significance of these "factors" in scientific change; and hardly any say that a proper story about the "factors" is vital to defending the institution of modern science from political interference and scientific knowledge from distortion. This contest between the "factors," and the notion that historians were supposed to adjudicate their relative importance, now seem locally antique. Advanced thinkers are now even uneasy with the very idea that one can reliably parse cultural and social fields into factors at all, treating the locutions, instead, as the conventions of different modes of academic inquiry.[6] There are indeed some academic historians who still talk unself-consciously about "internal and external factors," assessing their significance in scientific change, but the gesture is sometimes thought to mark them out as not-among-the-knowing, naïve, amateur, disconnected from what counts as mainstream history of science. The discipline's *bien pensants* just don't do that anymore.

But from about the 1930s until around the end of the Cold War, the so-called "externalism-internalism debate" importantly structured the history and sociology of science.[7] At the time this book was written, it was not uncommon to hear historians expressing exasperation at the rigidity of this debate and the constituent categories. Why not, it was said, write history as if an eclectic mixture of factors were operative, sometimes giving attention to internal, intellectual factors, while acknowledging the potential salience of external social and political factors? Why not just concede that all these factors "seamlessly interacted" in making scientific knowledge?

[5] *Leviathan and the Air-Pump*, p. 342.

[6] Michael Lynch, "Pictures of Nothing? Visual Construals in Social Theory," *Soc. Theory* 9 (1991), 1–21.

[7] Thomas S. Kuhn, "The History of Science," in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977; art. orig. publ. 1968), pp. 105–126.

The authors of *Leviathan and the Air-Pump* were also dissatisfied with the "externalism-internalism debate," but they did not see judicious eclecticism about the "factors" as a resolution. The problems, they thought, lay with the identity and coherence of the categories themselves. One incoherence concerned the placement of the boundary between what was deemed internal and what external to science. On what grounds were social and political things accounted not "intellectual"? And how was it that the making and warranting of scientific knowledge was judged not "social"? Did other *intellectual* practices—say religion and natural magic—count as external (since they were not considered to be "scientific"), or was the external-internal boundary meant to mark the divide between what were taken as intellectual practices—presumably including things like religion and natural magic—and what were taken as nonintellectual—things like the production of material goods, the governance of nations, and the practices and patterns of quotidian social life. The authors wondered on what grounds historians allocated items to either side of the boundary. Did historical actors get to say what properly belonged, or did not belong, to their specific practices, or was the "externalism-internalism debate" an expression of the various boundaries between science and nonscience recognized by twentieth-century historians?[8] Sentiments in the historiography of ideas—notably associated with the early work of Quentin Skinner—accounted it simply *ahistorical* to attribute to past actors concepts and categories not available to them or to ascribe to past actors "foreshadowings" of later developments of which they could have no knowledge.[9] Again, in the decades preceding *Leviathan and the Air-Pump*, intellectual historians increasingly identified their goal as something like re-creating past action in past actors' terms, and, from that point of view, the only pertinent categories and boundaries for interpreting past scientific actions were said to be those recognized by those acting in the past.

Shapin and Schaffer suspected that much about the "externalism-internalism debate" flowed not only from twentieth-century descriptive categories and interpretative boundaries but also from twentieth-

[8] Steven Shapin, "Discipline and Bounding: The History and Sociology of Science as Seen through the Externalism-Internalism Debate," *Hist. Sci.* 30 (1992), 333–369.

[9] For example, Quentin Skinner, "Meaning and Understanding in the History of Ideas," *Hist. and Theory* 8 (1969), 3–53; Skinner, "'Social Meaning' and the Explanation of Social Action," in *Philosophy, Politics and Society*, series 4, ed. Peter Laslett, W. G. Runciman, and Quentin Skinner (Oxford: Basil Blackwell, 1972), pp. 136–157; Skinner, "Some Problems in the Analysis of Political Thought and Action," *Pol. Theory* 2 (1974), 277–303.

century *evaluations* and programmes of action. What was deemed internal and external to science was, to a large extent, just our way of saying what we thought *properly belonged* to science and what we considered illegitimate, where we thought *rationality* resided, and what we deemed to be epistemically *virtuous.* What presented itself as description was, they reckoned, thinly veiled prescription. The Cold War did not end until several years after *Leviathan and the Air-Pump* was published, but its authors then had the sense that this relatively esoteric historiographic debate had its roots sunk deep into the great political and ideological cleavages of the century. Was it good, right, and productive to mold and direct scientific inquiry to serve broader social and political goals, or did any such control corrupt the very idea of science? What did the historical record show about the conditions and consequences of "external influence"? Did that record license external control of inquiry, or did it show that whenever the political and commercial orders made their presence felt, scientific objectivity and power were compromised? Put that way, the "externalism-internalism debate" was at once about present-day standards of legitimate historiography and present-day conceptions of legitimate social order. Indeed, the exchanges in the post–World War II period between the Marxist crystallographer and advocate of the state planning of science J. D. Bernal and one of his severest critics, the physical chemist and defender of "freedom in science" Michael Polanyi, crucially mobilized understandings of "what history showed" about the autonomy or the social responsiveness of science.[10]

Seen that way, the intellectual incoherence of the "externalism-internalism debate" was a small price to pay for its cultural and social pertinence, making its terms and framing seem both natural and consequential. Those considerations were still powerful at the time *Leviathan and the Air-Pump* was written. It is accurate to say that the authors

[10] See, for example, J. D. Bernal, *The Social Function of Science* (London: G. Routledge, 1939); Michael Polanyi, *The Planning of Science, Society for Freedom in Science. Occasional Pamphlet, 4* (Oxford: Potter Press, 1946); John R. Baker, *Science and the Planned State* (London: G. Allen & Unwin, 1945). For commentary, see, for example, P. G. Werskey, *The Visible College: A Collective Biography of British Scientists and Socialists in the 1930s* (London: Allen Lane, 1978); Anna-Katherina Mayer, "Setting Up a Discipline: Conflicting Agendas of the Cambridge History of Science Committee, 1936–1950," *Stud. Hist. Phil. Sci.* 31 (2000), 665–689; Mayer, "Setting Up a Discipline, II: British History of Science and 'the End of Ideology,' 1931–1948," *Stud. Hist. Phil. Sci., Part A* 35 (2004), 41–72; Gary Werskey, "The Marxist Critique of Capitalist Science: A History in Three Movements," *Science as Culture* 16 (2007), 397–461; Charles R. Thorpe, "Community and the Market in Michael Polanyi's Philosophy of Science," *Mod. Int. Hist.* 6 (2009), 59–89.

of this book were creatures of their time and were working with, and at times against, its materials and sensibilities. To situate the book firmly in its original political setting is not to say it was meant as a political manifesto. The politics the authors conceived themselves to be doing was the politics of intellectual inquiry. How was it right, interesting, and productive to go about studying science as a historical and social phenomenon? In writing that book, they were not concerned to defend science, to criticize science, or to defend or criticize any version of the Good Society. At the same time, there were other intellectual developments in the 1970s and 1980s which were also opening up a space where alternatives to existing historiography and its categories might be conceived.

Institutional Settings

These other pertinent intellectual developments were (1) the professionalization of the academic history of science and related modes of inquiry; (2) developments in other academic practices engaged with the understanding of science, related forms of culture, and the cognitive practices of everyday life; and (3) changes in the institutional circumstances of the scientific enterprise itself and associated changes in how both laypeople and scientists themselves thought about the nature of science.

The professionalization of academic history during the twentieth century meant that many sorts of historians could, if they wished, reject relations of dependency upon, collegiality with, or of intellectual pertinence to other groups concerned in the practice under study. History, it was said, was written by historians, for historians. The writing of history could and should be governed by standards internal to the community of professional historians and not by standards circulating among the laity or among groups who spoke in the name of the practice—for political history, present-day politicians; for the history of art, present-day artists and aestheticians; and, for the history of science, present-day scientists. These other groups might expect historical stories to celebrate their lineage, to offer up object-lessons of proper conduct, or to find foreshadowings of the bright present in the dark past, but professional historians could establish that they *were* professional through broadly *naturalistic* approaches to their objects of study. History could be description and interpretation; it need not be celebration or criticism. Political history did not have to document progress from servitude to liberty, from absolutism to constitutional democracy. That, indeed, was the thrust of Herbert Butterfield's 1931 *The Whig Interpretation of History*, which criticized "the tendency of many [political] historians . . . to praise revolutions provided they have been successful, to emphasize certain principles of progress in the past and to produce a story which is the ratification if not the glorification of the present."[11]

Yet the strength of the clientage relationship binding academic history to external constituencies is indicated by remarks made by Butterfield himself to an interviewer years later. Butterfield was here criticizing the quasi-Freudian presumptions of the political historian Lewis Namier, who believed that professed ideas were, on the whole, less important in political action than contingent networks of interest and alliance. Butterfield called Namier "a historian's historian, because his research was all-embracing and flawless, his artistry imposing." Yet, speaking

> as a *teacher*, and a *master* of the college [Peterhouse, Cambridge], I have to deplore his method. . . . As far as I am concerned, the point of teaching history to undergraduates is to turn them into future public servants and statesmen, in which case they had better believe in ideals, and not shrink from having ideas and policies and from carrying their policies through. We mustn't cut the ground from under them by teaching that all ideas are rationalizations. In brief, we must take a *statesmanlike* view of the subject.[12]

A certain historical method, and a certain realism about historical objects, were said to be mandated by pedagogical and political necessities.

But the history of science continued to be thought of as a quite special form of historical inquiry. Butterfield's sole foray into the history of science at once insisted on the historicity of science and celebrated the seventeenth-century Scientific Revolution as the very origins of modernity.[13] In the United States, George Sarton, one of the founding figures of the history of science, worked energetically to establish the discipline as a professional academic pursuit while also asserting that the new discipline was not, and could not be, a normal

[11] Herbert Butterfield, *The Whig Interpretation of History* (London: G. Bell, 1931), p. v.

[12] Quoted in Ved Mehta, *Fly and the Fly-Bottle: Encounters with British Intellectuals* (Baltimore, Md.: Penguin, 1965), p. 204. Butterfield's sentiments are worth considering for those who think the teaching of history has been politicized just recently, and by the political Left.

[13] Herbert Butterfield, *The Origins of Modern Science, 1300–1800* (London: G. Bell, 1949).

sort of historical inquiry. Its subject matter was quite special—driving history forward but itself standing outside of history. It was, Sarton said, a "secret history," a history of truth and the appearance of truth in the world, a history of the intimate connections between the scientific spirit and those rare individuals who give it voice.[14] Science, Sarton wrote, was not just uniquely progressive; it was the *sole* source of progress in human civilization. Science was the "only human activity truly cumulative and progressive"; its history was the only sort that could "illustrate the progress of mankind."[15] The progress of science was not, strictly speaking, a progress *towards* truth—scientific truths were fully formed as they were discovered bit by bit; it was, rather, a progress towards the *assemblage* of truths in the complete body of scientific knowledge. The teleological mode of historical narrative was not just pressed on the historian of science by the nature of the subject matter; it was a moral duty, a powerful way of showing people what human progress consisted in, where their hopes for freedom and justice resided, from whence their secular salvation might come. (Note that a conviction about the reality of scientific progress, even its unique progressiveness, does not prohibit the historian from being interested in the historical integrity of the beliefs and practices of the past; what it contingently does is to tell historians that documenting and celebrating the progressiveness of science is by far *the most important* thing they can do.) How could one write about science naturalistically when science itself was considered to be such an extraordinary phenomenon—atypical, drawing on special cognitive abilities, playing by special and coherent rules, standing apart from historical contingency and flux?

Sarton's sensibilities were not, however, the only ones on academic offer at midcentury, not even among those of a broadly "internalist" or "intellectualist" disposition. Where Sarton organized his historical practice around notions like "discoveries," the philosopher-historian Alexandre Koyré meant to shift attention to the structure and arrangement of scientific "concepts." And what made Koyré's work so exciting to a generation of Anglo-American historians encountering it during and just after World War II was its insistence on the historical integrity of past science. One could, if one wanted, chart the progress of science through its historical stations, but that could only be done

[14] George Sarton, *The History of Science and the New Humanism*, ed. Robert K. Merton (New Brunswick, N.J.: Transaction Books, 1987; orig. publ. 1962), pp. 38–43. For the political milieux of these projects in the history of ideas, including those of Sarton, see Simon Schaffer, "Lovejoy's Series," *Hist. Sci.* 48 (2010), 483–494, on 486–487.

[15] George Sarton, *The Study of the History of Science* (Cambridge, Mass.: Harvard University Press, 1936), p. 5.

by stripping away or setting aside the conditions of coherence pertaining to past structures of thought. For example, Aristotelian physics is *false*—there can be no doubt about that—but it is, Koyré insisted, systematic, intelligible, and coherent. That is to say, as a conceptual scheme, it *worked*. "Aristotelian physics," Koyré wrote, "is false, of course; and utterly obsolete. Nevertheless, it is a 'physics.' . . . [It] forms an admirable and perfectly coherent theory."[16] Given its historically specific premises and assumptions, Aristotelian natural philosophy is not only coherent but self-validating. What Koyré did was at once to stipulate the historical integrity of past conceptual schemes and to invite historians to work out the historically specific rules of the game by which they were played.

In the early 1960s Thomas Kuhn acknowledged the influence of Koyré in helping shape his views, showing "what it was like to think scientifically in a period when the canons of scientific thought were very different from those current today."[17] Kuhn's work—and especially his notion of "normal science"—later became decisively important in the development of the sociology of scientific knowledge (even though Kuhn himself was deeply disturbed by the association),[18] but it was the potential for a genuinely historical engagement with science that excited Kuhn and that propelled him, by way of history, and probably unintentionally, into implicit sociology.[19] There were some attempts to appropriate Kuhn's views as a way of displaying the role of "external social factors" in scientific change, but this came to little.[20] The future of the sociology of science lay elsewhere; it was drinking from the same naturalistic springs as the newly professionalized his-

[16] Alexandre Koyré, "Galileo and Plato," *J. Hist. Ideas* 4 (1943), 400–428, on 407, 411.

[17] Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), p. viii (also p. 3). Kuhn here included Koyré with such other historians as Emile Meyerson, Hélène Metzger, and Anneliese Meyer.

[18] Kuhn, "The Trouble with History and Philosophy of Science," in *The Road since Structure*, pp. 105–120. The Edinburgh "Strong Programme," Kuhn declared, was "an example of deconstruction gone mad" (p. 110).

[19] The mobilization of Kuhn's work for the sociology of scientific knowledge is evident in, for example, David Bloor, *Knowledge and Social Imagery* (London: Routledge and Kegan Paul, 1976), e.g., pp. 55–61; and Barry Barnes, *T. S. Kuhn and Social Science* (London: Macmillan, 1982). The challenge to historians of science to maintain a naturalistic attitude towards their subject is evident even among those, like Kuhn, most in favour of a genuinely historical engagement with science. In a 1995 interview Kuhn criticized the authors of *Leviathan and the Air-Pump* because they *failed* to use the modern schoolbook physics of compressible fluids to explain away Boyle's unstable historical vocabulary of pressure and spring: Kuhn, *The Road since Structure*, p. 316.

[20] Kuhn made only brief and marginal remarks about such things in his *Structure* (e.g., pp. xii, 69, 75, 110).

tory of science. The fact that Koyré's work was later used to hammer the Marxists is irrelevant to this development;[21] it was the profound historicity promised by Koyré's sensibilities that moved Kuhn and, through Kuhn, the sociologists of scientific knowledge. Historicity inspired interest in the differing "rules of the game."

Koyré's followers were concerned with the diachronic interpretation of distinct scientific "games," but, from about the 1970s, there were sensibilities and resources available to think naturalistically about synchronic variation and, especially, about contemporary science. During the Cold War, and in the United States specifically following the challenge of Sputnik, practitioners found themselves increasingly accommodated within newly created university departments of the history of science (or, in a largely unsuccessful experiment, the "history and philosophy of science"). They had joined the ranks of the professionals; they now had academic rooms of their own—not many, but enough to support the institutional paraphernalia of a small- to medium-sized modern discipline. They no longer had any special *institutional* reason to be apologists for their colleagues in the science departments. They now had as little reason to see their purpose to be praising modern science as academic historians of art had to act as advocates for the works of David Hockney or Robert Rauschenberg. These developments had little or no connection to any disposition to *criticize* science: the alternative to celebrating science was not denigration but naturalism—about both present-day and past science, the former appealing largely to the sociologists, the latter to historians. Description of past science could be distinct from celebration of present-day science; its interpretation could be distinct from identifying its role as "foreshadowing" modernity.

Placing the Object

Historians now insist that their engagement is with the specificity of the past, not with its foreshadowing of the present. Yet historians' present importantly constitutes the presumptions, conventions, and

[21] See A. Rupert Hall, "Merton Revisited, or Science and Society in the Seventeenth Century," *Hist. Sci.* 2 (1963), 1–16; Hall, "The Scholar and the Craftsman in the Scientific Revolution," in *Critical Problems in the History of Science*, ed. Marshall Clagett (Madison: University of Wisconsin Press, 1959), pp. 3–23; A. C. Crombie, "Commentary [on Hall]," in ibid., pp. 66–78. Remarkably, however, in 1968 the eminent historian of physics, Clifford Truesdell, denounced Koyré's work as an example of modish conceptions of science as "time-conditioned, social and institutional": Clifford Truesdell, *Essays in the History of Mechanics* (Berlin: Springer, 1968), p. 146.

questions that they use to reconstruct past realities. That is the historian's predicament, and there is no evident way around it. It is a predicament recognized—in different idioms—by reflective historians from E. H. Carr to Hans-Georg Gadamer.[22] So one inducement to naturalism in the study of science is, without doubt, the changed circumstances of the scientific enterprise in the West after World War II. And the pertinent features of that change include much tighter institutionalized and culturally recognized links between the once much-distinguished domains of science and technology; a vast increase in government funding for science and the ever-more-intimate enfolding of scientific research in the institutions of the state, and especially of the military; the cultural celebration of science as integral to national security and economic welfare; the scientization of the culture, changing prestige relations between the natural sciences and the humanities, and the increasing role of what were taken to be scientific methods in modelling proper academic inquiry, particularly in the human sciences. A scientific enterprise that was both celebrated and secure seemed in little need of external defense, and a scientific enterprise that was so well integrated into the institutions of government and civic life might, especially after Hiroshima, seem as little immune from criticism as any other civil institution.[23]

Modern historians of science are—usually if not always—members of an academic discipline, and as such their response to the contemporary circumstances affecting science is never direct or immediate. We have now sketched some aspects of the environment from which

[22] Edward Hallett Carr, *What Is History? The George Macaulay Trevelyan Lectures 1961* (New York: Knopf, 1961); Hans-Georg Gadamer, *Truth and Method*, 2nd rev. ed., trans. Joel Weinsheimer and Donald G. Marshall (New York: Continuum, 2004; orig. publ. 1960).

[23] These possible relations between the academic study of science and the post–World War II institutional circumstances of science were later explored in Simon Schaffer, "What Is Science?" *Science in the Twentieth Century*, ed. John Krige and Dominique Pestre (Amsterdam: Harwood, 1997), pp. 27–42; and Steven Shapin, "Lowering the Tone in the History of Science: A Noble Calling," in *Never Pure: Historical Studies of Science as If It Was Produced by People with Bodies, Situated in Time, Space, Culture, and Society, and Struggling for Credibility and Authority* (Baltimore, Md.: Johns Hopkins University Press, 2010), pp. 1–14. For strands of sociology of science, notably including those inscribing "external-internal" vocabulary, as celebration and defense of science, see, for example, David A. Hollinger, "The Defense of Democracy and Robert K. Merton's Formulation of the Scientific Ethos," in *Knowledge and Society*, ed. Robert Alun Jones and Henrika Kuklick (Greenwich, Conn.: JAI Press, 1983), vol. 4, 1–15; Hollinger, "Science as a Weapon in *Kulturkämpfe* in the United States during and after World War II," *Isis* 86 (1995), 440–454; and Everett Mendelsohn, "Robert K. Merton: The Celebration and Defense of Science," *Science in Context* 3 (2008), 269–289.

Leviathan and the Air-Pump emerged in the mid-1980s. It was an environment that bore upon the authors of that book as much as it did on their colleagues. To be sure, there were some circumstances that marked out their institutional place: both authors received their doctoral training in departments of the history and philosophy of science, while many of the historians who trained them had not—a common pattern for the older generation involving a drift into the history of science from careers in science itself.[24] Schaffer moved directly from his Cambridge Ph.D. to academic appointments in the history of science (first at Imperial College, then at Cambridge); Shapin took his doctorate at the University of Pennsylvania (in a department which had just changed its name from "history and philosophy of science" to "history and sociology of science"[25]) but was employed in the "interdisciplinary" Science Studies Unit at Edinburgh University at the time of his work with Schaffer. The interventions manifest in *Leviathan and the Air-Pump* can be interpreted through currents in academic history of science and in the abutting social scientific, and even philosophical, disciplines.

The book was regarded as a peculiar exercise with reference to the disciplines. Shapin's institutional environment—he had been for some time the nominal "historian" in the Edinburgh research and teaching unit that included a similarly nominal sociologist and philosopher of science—is better described as "problem-orientated" than as interdisciplinary. The group's consensually agreed "problem" was how to construct a naturalistic interpretation of science as a social phenomenon, while members' disciplinary affiliations were of little or no concern.[26] Schaffer was, at the time of publication, five years from

[24] To be sure, both authors manifested aspects of that "older" pattern, and it is still common today. Schaffer had received undergraduate training in natural sciences; Shapin's first degree was in biology and he did a year of postgraduate work in genetics. Shapin was the beneficiary of the proliferation of departments of "the history and philosophy of science" in the United States during the Cold War, some of which were founded with funds from the National Defense Education Act passed in 1958 in response to Sputnik.

[25] The name change mainly signalled an early recognition that the marriage between naturalistically and empirically inclined history and normatively disposed philosophy of science was not going well, but there was then next to no formal sociology in the Pennsylvania curriculum.

[26] The philosopher was David Bloor, the sociologist was Barry Barnes, and the group was directed by David Edge, a former radio astronomer and BBC producer with no particular affiliation to a humanistic or social scientific discipline. There were also strong, if not always friction-free, intellectual relations with the sociologist Harry Collins at the University of Bath, a group of sociologists around Michael Mulkay at the University of York, and Bruno Latour at the École des Mines in Paris. The affiliation between

his doctorate; he had studied for a time in Paris, where he attended Foucault's lectures; he met Shapin at a conference in 1980 organized by the sociologist of science Harry Collins; and it was quite common at the time for British historians—much more than their American colleagues—to find selected strands of social science and philosophy pertinent to their historical projects. For many British historians, Marxism was a lingua franca, not necessarily providing a theoretical foundation for political projects but certainly constituting a loosely connected set of concepts and methodological sensibilities with which many historians felt they should engage even while their political affiliations diverged.[27] All sorts of intellectual and cultural historians found interest in the work of Oxford School anthropologists, in Wittgenstein's later philosophy, or in the seams of psychology mined by Warburg School art historians. (That's just to recall some features of the intellectual environment inhabited by many British academics in the 1970s and early 1980s, but looking back at that scene from the perspective of current disciplinary narrowness and self-satisfaction, it's hard to resist a certain nostalgia. The achievement of present-day disciplinary professionalism has been bought at a price.)

If the disciplines are relevant to understanding the book's authors, the same disciplines—their traditions, conventions, and characteristic concerns—also configured the responses of academic readers and especially of the reviewers who were called on to make sense of, and to evaluate, *Leviathan and the Air-Pump*. The reviewers found the book difficult to *place*, or, if they found it easy to place, they collectively displayed marked variation in their sense of what kind of thing it was.

---

different projects in the study of science has prompted some strange attempts to make sense of the book's origins. In an appraisal judging *Leviathan and the Air-Pump* "one of the most important achievements in science studies in the late twentieth century," John Zammito suggested that it was an essay by Michel Callon and Bruno Latour ("Unscrewing the Big Leviathan" [1981]) that "aroused in [Shapin and Schaffer] the consideration that Hobbes was more important for science studies than had appeared hitherto and that *Leviathan* deserved reading as a work in natural philosophy." John H. Zammito, *A Nice Derangement of Epistemes: Post-Positivism in the Study of Science from Quine to Latour* (Chicago: University of Chicago Press, 2004), pp. 177, 339 n. 228; the Callon and Latour paper is "Unscrewing the Big Leviathan: How Actors Macro-structure Reality and How Sociologists Help Them to Do So," in *Advances in Social Theory and Methodology: Toward an Integration of Micro- and Macro-Sociologies*, ed. Karin D. Knorr-Cetina and Aaron V. Cicourel (London: Routledge and Kegan Paul, 1981), pp. 277–303. Zammito's story, however, is not correct. While one of the authors had read the Callon and Latour essay, it is not mentioned in the book, and their joint interest in Hobbes's science developed shortly after their meeting in 1980. Historians of science did not necessarily require the promptings of their philosophical colleagues to find Hobbes's work relevant.

[27] See Young, *Darwin's Metaphor*, pp. 388–406.

The book contained anthropological and philosophical gestures, and there were hints of links between its preoccupations and strands of art and military history. Quite a few readers noted these gestures as nonstandard: some appreciated them; others expressed annoyance. The *Journal of Interdisciplinary History* said that "few readers from any discipline will read through it without . . . being pulled across our artificial boundaries into alien disciplines," and, while this was not evidently considered a bad thing, there was a certain chill in evoking the "alien."[28] One of the most distinguished historians of early modern science bridled at what he saw as "a pervasive sociologizing jargon," while a great historian of Renaissance and seventeenth-century science reckoned that *Leviathan and the Air-Pump* was scarcely professional history at all, possibly appealing most to scientist-amateurs and members of another academic discipline: "this is not primarily a historical work, but by intention a profoundly sociological one," oddly concluding that it must have been directed "more to the scientist-historian than to the historian of science."[29] One of the more radical, "contextualizing" historians of science was exasperated at what she saw as an injunction that historians should abandon their proper tools and go on like anthropologists, using what she took to be anthropologists' "jargon" ("actors," "social spaces"); a then-young member of the small clan of British sociologists of scientific knowledge appreciated the work as a historical instantiation of "social constructivism" (normal practice in his field but a term not to be found in the book). In contrast, a historian expressed relief that the book offered "historical analysis" rather than a demonstration of the worth of "one or another brand of the sociology of knowledge"; while another eminent historian of science applauded a book which, he wrote, "always take[s] philosophy most seriously."[30] A reviewer in a philosophy journal was pleasantly surprised that, while the book's title promised "a purely historical study," there was in fact some philosophical interest in it.[31] Several reviewers noting the sociological bits of *Leviathan and the Air-Pump* judged that the effect was to distort the historical account and to render the style more or less impenetrable. The language of

[28] James G. Traynham, *J. Interdisc. Hist.* 17 (1987), 351–353.

[29] Marie Boas Hall, *Ann. Sci.* 43 (1986), 575–576 (for "the scientist-historian"); Richard S. Westfall, *Phil. Sci.* 54 (1987), 128–130 (for sociological "jargon").

[30] Margaret C. Jacob, *Isis* 77 (1986), 719–720 (for anthropological "jargon"); Trevor J. Pinch, *Sociology* 20 (1986), 653–654 (for "social constructivism"); Robert H. Kargon, *Albion* 18 (1986), 665–666 (for "historical analysis"); Owen Hannaway, *Tech. Cul.* 29 (1988), 291–293 (for "philosophy").

[31] A. P. Martinich, *J. Hist. Phil.* 27 (1989), 308–309.

"spaces"—social, intellectual, and philosophical—seemed especially confusing, if not just silly. Several readers were witty enough to compare what they took to be the book's prolix, didactic, and repetitive tone with that of Boyle himself.[32]

The reviewer for *Journal of Interdisciplinary History* was not a historian or social scientist of any sort: he was an organic chemist working in Louisiana, and it is not clear whether he was aware of the authors' location in the disciplinary scheme of things. Of course, neither author was then senior in the field. Schaffer's 1980 Ph.D. dissertation was indeed about Newtonianism, and by the mid-1980s he had written a handful of articles on the history of astronomy and natural philosophy in the seventeenth and eighteenth centuries. Shapin was twelve years older than his coauthor, but after thirteen years in his job he still had not been put up for promotion from lecturer. Edinburgh was far removed from the discipline's metropolitan centres of gravity, and Shapin distinctly recalls being told by one of his "rivals" for the post, after both had been interviewed, that he didn't think he could accept a position "so far from the action." Shapin had not published "the book of the thesis" he had written in 1971—on institutional aspects of science in the Scottish Enlightenment—and all he had to his name after thirteen years of academic employment were some papers on aspects of the social uses and organization of science in the British Industrial Revolution and a few early attempts at a historical sociology of science whose empirical content was the career of the "pseudo-science" phrenology in early nineteenth-century Edinburgh. He had no formal training in early modern topics, and his first engagement with the history of seventeenth-century science had appeared only recently.[33] Neither author had any significant "form" in the matters contained in their book or in the genre it supposedly represented.

Most historians of science in the mid-1980s were still well able to recognize exercises that belonged respectively to "externalist" and "internalist" genres. This is what some of the book's reviewers were disposed to do. The *Journal of Interdisciplinary History* reviewer observed that history of science was either "inside or outside in perspective or emphasis," while curiously judging that the latter had become "perhaps the major force in the field."[34] That made the authors of

[32] Thomas L. Hankins, *Science* 232 (23 May 1986), 1040–1042; John L. Heilbron, *Med. Hist.* 33 (1989), 256–257; Charles Webster, *Times Lit. Supp.* (13 March 1987), 281; Jacob, *Isis*.

[33] Shapin, "Of Gods and Kings: Natural Philosophy and Politics in the Leibniz-Clarke Disputes," *Isis* 72 (1981), 187–215.

[34] Traynham, *J. Interdisc. Hist.*

Leviathan and the Air-Pump, because of their insistence that such categories were more topic than resource, appear somewhat out of step with developments in the history of science. Other reviewers set aside the authors' explicit disengagement from the external/internal, social/intellectual polarities and were quite confident in condemning this as a particularly pernicious form of externalism. One historian criticized what he saw as an "attempt to define scientific boundaries socially. . . . The differences between geometry and chemistry," he reminded us, "are not entirely social."[35] The "social" was permitted, but it must be tempered by intellectual factors. The historian distressed by "pervasive sociologizing jargon" knew where to position the book in traditional frameworks and therefore why he didn't like it. He took a very different view of the performance from the historian applauding its "historical analysis": "The book is a passionate exposition of a new program in the history of science that is impatient with internal analysis of scientific arguments and insists that science is a social enterprise and can *only* be understood in terms of the socio-political context." Shapin and Schaffer argue, he concluded, that experiment "rests on nothing more substantial than social conventions," though he did not speculate why any historians holding such a view would bother producing such highly detailed accounts of the technical and physical labour involved in actually doing experiments.[36]

There were not many reviewers picking up the other end of the interpretative stick, but one of the major British Marxist political and social historians of the seventeenth century did just that. He was looking for a much more substantial role for social factors in the book but was disappointed to find so little, contrasting *Leviathan and the Air-Pump* negatively on those grounds with work more vividly displaying how seventeenth-century science "justified capitalist world domination, racialism and inequality." Shapin and Schaffer had written, so to speak, the Menshevik version of what had already been achieved by historians making a more full-blooded case for the role of the social as opposed to the intellectual.[37]

[35] Hankins, *Science*.

[36] Westfall, *Phil. Sci.* (emphasis added).

[37] Christopher Hill, "'A New Kind of Clergy': Ideology and the Experimental Method," *Soc. Stud. Sci.* 16 (1986), 726–735, on 728. Historians applauded by Hill, who were reckoned to have made a better job of what he saw as the same task Shapin and Schaffer had taken up, included Brian Easlea, James R. Jacob, Margaret C. Jacob, and Carolyn Merchant.

Modernity Making

For historians of science and ideas, another way of making sense of *Leviathan and the Air-Pump* was to see it addressing linked questions about the "Scientific Revolution" and the "origins of modern science." By the 1980s these questions belonged to recognized traditions in the history of science, even though they had largely been set aside for some time in favour of more particularistic engagements with science and its past: When did "modern science" emerge? What was the mode of its emergence—evolutionary or revolutionary, continuous or discontinuous? What were its essential characteristics? Why did modern science develop when and where it did? What intellectual, cultural, and, perhaps, social and economic forces encouraged its development and what forces inhibited it? And, a corollary question prompting much interest from both Marxist and non-Marxist historians in past generations, why did modern science develop in Europe during the sixteenth and seventeenth centuries and not in one of several non-Western settings, of which China was deemed the most pertinent instance?[38]

At the time the book appeared, almost anyone then charged with teaching courses in history of science was expected somehow to provide an account of the great transformation ushering in scientific modernity, and, maybe, through science, of modernity writ large. In 1983 A. R. Hall's widely read and extensively used *The Revolution in Science, 1500–1750*, first issued three decades earlier, reached its third edition, with a revised title, a slightly constricted chronological scope, and a clear sense that any coherent understanding of the history of science depended on recognizing the massive transformation of knowledge and method that took place in Europe during this period.[39] It was in Hall's book that Shapin and Schaffer found the telling claim that the air-pump was "the cyclotron of its age," a gesture suggesting the interest and legitimacy of framing knowledge making from past and present in similar terms.[40] A few months later, Hall contributed to the George Sarton Centennial Issue of *Isis*, the official journal of the History of Science Society, a moving reminiscence link-

[38] See Joseph Needham, *The Grand Titration: Science and Society in East and West* (London: George Allen & Unwin, 1969).

[39] The original edition was A. Rupert Hall, *The Scientific Revolution, 1500–1800: The Formation of the Modern Scientific Attitude* (London: Longmans Green, 1954). The reissue was titled *The Revolution in Science, 1500–1750*, 3rd ed. (London: Longman, 1983). The book remains in print and widely used in surveys on the history of science.

[40] Hall, *The Revolution in Science*, p. 262 (quoted in *Leviathan and the Air-Pump*, p. 30).

ing the beginning of his career in the field both to the establishment of a Cambridge display of historical scientific instruments and, "far more important," to the delivery there of Butterfield's 1948 lectures that first introduced the notion of a unique, epochal, and decisive Scientific Revolution to Anglophone audiences.[41]

Books like this, and the attendant disciplinary memories, helped sustain the continuing potency within the field of an irreversible, definitive, and foundational early modern revolution, having tantalizing implications for both conceptual schemes and the practical hardware for knowledge making. But even as the Scientific Revolution was being pushed towards the centre of the discipline's sensibilities and historical narratives, trouble was beginning to appear. Arnold Thackray, then editor of *Isis,* had just publicly announced that even if the Scientific Revolution remained the discipline's "central heuristic device," it had lost its conceptual coherence.[42] A collaborative project launched in 1980 to reassess the Scientific Revolution at a moment of growing historiographic self-consciousness confessed pervasive disagreement among historians of science and acknowledged the enormous difficulty of any such coherent revaluation.[43] The historian Roy Porter soon produced an astute essay pointing out the historical contingency of the very idea of the Scientific Revolution. He associated the appearance of the canonical version of this story with mid-twentieth-century contests between "intellectualists" and Marxists, part of the externalism-internalism struggles and assumptions.[44]

So, at the time *Leviathan and the Air-Pump* appeared, some practitioners regarded the notion of the Scientific Revolution as interestingly problematic, while others saw it as the central organizing element in the grand narrative of science and its past—the moment when "modern science" originated, when everything changed, and from which there was no return. In accounts of the Scientific Revolution, Boyle's work with the air-pump, and the institutional place it found within the early Royal Society of London, had been positioned at or somewhere

[41] A. Rupert Hall, "Beginnings in Cambridge," *Isis* 75 (1984), 22–25, on 23. The Butterfield lectures were the basis of his *Origins of Modern Science* (1949).

[42] Arnold Thackray, "History of Science," in *Guide to the Culture of Science, Technology, and Medicine*, ed. Paul Durbin (New York: Free Press, 1980), pp. 3–69, on p. 28.

[43] Robert S. Westman and David C. Lindberg, "Introduction," in *Reappraisals of the Scientific Revolution*, ed. Westman and Lindberg (Cambridge: Cambridge University Press, 1990), pp. xvii–xxvii, on p. xx.

[44] Roy Porter, "The Scientific Revolution: A Spoke in the Wheel?" in *Revolution in History*, ed. Porter and Mikuláš Teich (Cambridge: Cambridge University Press, 1986), pp. 290–316.

near the centre, so the place of this book in that story must be an interesting aspect of its production and reception.

The causes of the discipline's uneasiness with the Scientific Revolution frame are not clear. Was it growing concern with history's "losers" as well as "winners," or even suspicion that "winning" and "losing" were impoverished notions for the interpretation of the past? Was it an increase in the number and range of early modern European practitioners that historians felt it necessary or possible to include in their accounts of the sciences? Was it recognition that many fields of knowledge had undergone no radical transformation in the sixteenth and seventeenth centuries, or even that history actually contained no discrete moments which "made the modern world"? Was it increasing acknowledgment of the extent to which putatively "new" or "revolutionized" practices contained vigorous elements of the "old"? Was it reflection on time scale, doubting that a revolution could be thought of as stable over three centuries, or noting that many features of the twentieth-century "modern" were absent in celebrated seventeenth-century achievements? Was it the already-mentioned discomfort with the historical propriety of mining the past for its "anticipations" or "foreshadowings" of the present, and an embrace of the task of interpreting the past in "its own terms"—a past into which that past's pasts were enfolded but which could not know its future?

Nevertheless, some reviewers expected that a book dealing with these sorts of materials should offer a coherent *explanation* not just of why Boylean experimentation triumphed over Hobbesian deductivism but also of why *science* won and why it continues in cultural dominance. Anything less than that would be incomplete; anything other than that would be a perverse denial of the identity of the Scientific Revolution. A sociologist judged that *Leviathan and the Air-Pump* might have explained the initial success of something called "science," but "the challenge now is to show how the trick has been sustained."[45] A historian of physics noted some of the worthy effects of the book's naturalism and its disinclination to engage in strong causal explanation—yet it was this reluctance that made its characters into "opaque automata," while its localist sociology was incapable of what *had to be* the crucial task—explaining the epochal and enduring success of the Scientific Method and of experimental science: "There has surely been a lasting victory of the experimental approach, of the experimental programme Boyle demanded—and one could hope that this would be correlated with an equally long-lasting reason: perhaps, for example, the formi-

[45] Trevor J. Pinch, *Sociology* 20 (1986), 654.

dable efficacy of this programme in humans' instrumental relation with reality."[46] Similarly, a leading social historian praised the book's avoidance of teleological explanation and its symmetrical approach to all parties in controversy, while identifying as a flaw its lack of any causal explanation of the secular efficacy of experimentation: "Like the experimenters they study, Shapin and Schaffer don't always do what they say they do, or what they should have done."[47]

The discipline's continuing attachment to stories about "the Scientific Revolution" and the "origins of modern science" was sufficiently strong that reviewers of *Leviathan and the Air-Pump* judged that it had identified Boyle as "the 'founder' of modern science";[48] that it had "put [the Scientific Revolution] centre stage," that it had made a "contribution to the historiography of the Scientific Revolution" and to an "understanding of the genesis of the 'new science' of the seventeenth century."[49] And this way of identifying the pertinent frame for understanding *Leviathan and the Air-Pump* is all the more remarkable since *nowhere in the book* is the phrase "Scientific Revolution" (in either upper- or lowercase) actually used.[50] If, indeed, the Scientific Revolution provides any kind of frame for the work, it is through the authors' developing sympathies for historical scholarship from the 1960s to the 1980s that was sceptical of the legitimacy of such a notion and of the accompanying sensibility that this revolution defined the "making the modern world."[51]

Curious Incidents

Leviathan and the Air-Pump directed close attention to a very specific passage of seventeenth-century science. (The passage was, indeed, in-

[46] Dominique Pestre, *Revue d'histoire des sciences* 43 (1990), 109–116, on 110.

[47] Roger Chartier, "De l'importance de la pompe à air," *Le monde des livres*, 28 January 1994, viii.

[48] Westfall, *Phil. Sci.*, 130. (This was slightly naughty, since the purportedly quoted passage from *Leviathan and the Air-Pump* [p. 341] actually attributes the "foundership" view to "modern historians," and the same paragraph specifies that "an unbroken continuum between Boyle's interventions and twentieth-century science is highly unlikely.")

[49] Hacking, "Artificial Phenomena," 235 (for "centre stage"); Westfall, *Phil. Sci.*, 128 (for a "major contribution"); Mordechai Feingold, *Engl. Hist. Rev.* 106 (1991), 187–188, on 188 (for "the 'new science'").

[50] While several of Kuhn's historical essays are cited in the book, his *Structure of Scientific Revolutions* is not.

[51] Shapin later wrote a short book that sought to show the heterogeneity of scientific practices often swept up in the frame of a coherent seventeenth-century revolution: Shapin, *The Scientific Revolution* (Chicago: University of Chicago Press, 1996).

terpretatively framed, yet there was no mistaking the closeness of its engagement not just with scientific beliefs but with the minute and quotidian details of scientific knowledge making.) Most reviewers found the specifics of the air-pump's functioning and of the account of the Restoration polity reasonably well managed in the book—or, at least, they did not see any reason to find fault with these accounts. Some readers accepted these aspects of the book on the basis of their own empirical competencies; others took them "as read" because they sensed that the book's significance lay elsewhere—if not in an explanation of, so to speak, the long-term "success of science," then at least in its apparent invitation to revisit much of the conceptual vocabulary for describing "science and society," and even of "modernity."

In these connections, two assessments of the 1989 paperback edition, by the philosophers Ian Hacking and Bruno Latour, used the book as a platform to discuss that significance. They both did so through a robust assimilation of the book's story about early modern experiment and politics into their own versions of the long-term emergence of modernity.[52] For Hacking, the book provided an origin myth for an enterprise in which scientific truths no longer match states of affairs in an external world, but rather the artificial phenomena produced in a confined laboratory. In a subsequent lecture at Harvard, Hacking summed up this story's lesson: "Hobbes believed in the thesis that phenomena are created, and for his own reasons hated it [viz., the programme of making artificial phenomena]. . . . He lost the match with Boyle, forever."[53]

For Latour, the book described the foundations of a modern constitutional settlement under which political delegation and scientific representation were allotted their appropriately separate and complementary roles. Hacking read *Leviathan and the Air-Pump* as distressingly whiggish: it too evidently attended to the origins of our present concerns by focusing entirely on the ancient heroes of the Scientific Revolution. Latour thought the story was distastefully asymmetric: it was said to explain experiment's career and authority in political terms, while these political terms were never themselves subjected to the scrutiny given to natural terms. The authors were *almost there*, but

[52] Bruno Latour, "Postmodern? No, Simply *A*modern! Steps towards an Anthropology of Science," *Stud. Hist. Phil. Sci.* 21 (1990), 145–171, esp. 147–159, and rewritten in *Nous n'avons jamais été modernes* (Paris: Découverte, 1991), pp. 26–46; Hacking, "Artificial Phenomena."

[53] Ian Hacking, *Historical Ontology* (Cambridge, Mass.: Harvard University Press, 2002), p. 15.

not quite. They did not go "far enough."[54] Yet, flatteringly enough, both Hacking and Latour attributed to the book's modest authors much of the soaring originality of their own models of the nature of science, of society, and of historical change. They saw the authors doing not pretty good historical interpretation but faulty ontology and epistemology. If Shapin and Schaffer were hod-carriers to the building of philosophical theory, nevertheless the bricks delivered in their historical hods could be constructed into a grand edifice. The Restoration disputes so minutely detailed in *Leviathan and the Air-Pump* had apparently become the "fruit flies of the new social theory of science."[55] You could get to a proper metaphysical theory of the nature of science and of the polity through a proper interpretation of the controversies to which Shapin and Schaffer had drawn attention. Both reviewers thus offered magisterial accounts that placed the contests between Boyle and Hobbes at the roots of modern order, whether by way of Hacking's self-authenticating laboratory style or Latour's modern constitution.

These accounts were accompanied by a prediction: in various idioms, reviewers foresaw that *Leviathan and the Air-Pump* was likely to spawn a new generation of studies that followed or extended its patterns. Hacking, for instance, reckoned that the kind of instrumental biography he found in the book was an "art form of which we shall see a good deal in the next few years,"[56] while others pointed to methodological and conceptual achievements that would surely provide concrete models for subsequent historical work—on the Scientific Revolution and on temporally and topically distant materials. Since the book had, Hacking said, so effectively made the air-pump the protagonist of its story, there would certainly be an outpouring of comparable studies of the deeds and sufferings of scientific instruments. True, the study of scientific hardware, especially of early modern optical devices and of the modern equipment of physics, astronomy, and, now increasingly, molecular biology and genomics, has been pursued with great vigour since the mid-1980s. In cases historically close to those we considered, the vagaries of instrumental techniques in Baroque mechanics, the place of the microscope in the seventeenth-century Dutch Republic, the magic lantern as a demonstration device in public displays have all been worked over by historians of science. There have also been some fascinating accounts of the details of air-pump

[54] Duncan Kennedy, "Knowledge and the Political: Bruno Latour's Political Epistemology," *Cultural Critique* 74 (2010), 83–87, on 85–86.

[55] Latour, "Postmodern?," 148.

[56] Hacking, "Artificial Phenomena," 236.

design and its revisions in the later seventeenth and early eighteenth centuries.[57] But it would be an exaggeration to see *Leviathan and the Air-Pump* as a major inspiration for this work,[58] and few, if any, historical studies have sought to follow its example in trying to illuminate knowledge making by linking instruments to both literary representation and modes of social organization.

The historian of nineteenth-century science James Secord called the book "the most influential text in our field since Thomas Kuhn's *Structure of Scientific Revolutions* (1962)," specifically drawing attention to the work's focus on local circumstances in scientific knowledge making and saying that its localism had been immensely influential in the history of science. Yet that very generous assessment seems to attribute too much to one book and too little to a widely distributed interest in local scenes that marked the academic work of the 1970s and 1980s. Secord also applauded *Leviathan and the Air-Pump*'s "brilliant discussion of techniques of literary persuasion," but here too what has been called "the linguistic turn" in the study of all kinds of culture was well under way at the time the book appeared, and, in the event, Secord was disappointed that the history of science, as he said, had been so slow to follow this example.[59]

The historical domain in which one would have expected *Leviathan and the Air-Pump* to have greatest impact is, of course, the study of seventeenth-century science in general and of the work of Hobbes and Boyle in particular. After all, one of the book's distinguishing features was the tightness of its focus on concrete experimental practice in the seventeenth century and its engagement with one of the iconic figures in Scientific Revolution studies. But that impact has not hap-

[57] See, for example, Domenico Bertoloni Meli, *Thinking with Objects: The Transformation of Mechanics in the Seventeenth Century* (Baltimore, Md.: Johns Hopkins University Press, 2006); Edward G. Ruestow, *The Microscope in the Dutch Republic: The Shaping of Discovery* (Cambridge: Cambridge University Press, 1996); Marc Ratcliff, *The Quest for the Invisible: Microscopy in the Enlightenment* (Farnham: Ashgate, 2009); Thomas L. Hankins and Robert Silverman, *Instruments and the Imagination* (Princeton, N.J.: Princeton University Press, 1995). On early modern air-pump designs, see, for instance, Anne van Helden, "Theory and Practice in Air-Pump Construction," *Ann. Sci.* 51 (1994), 477–495; and Terje Brundtland, "From Medicine to Natural Philosophy," *Brit. J. Hist. Sci.* 41 (2008), 209–224.

[58] Major credit for subsequent historical work on scientific instruments and forms of knowledge should go to James A. Bennett, "The Mechanics' Philosophy and the Mechanical Philosophy," *Hist. Sci.* 24 (1986), 1–28; Bennett, "Robert Hooke as Mechanic and Natural Philosopher," *Notes and Rec. Royal Soc.* 35 (1980), 33–48; and, later, Bennett, "Practical Geometry and Operative Knowledge," *Configurations* 6 (1998), 195–222.

[59] James A. Secord, "Knowledge in Transit," *Isis* 95 (2004), 654–672, on 657, 662.

pened. Indeed, *Leviathan and the Air-Pump* seems to have had more influence, and its claims and methods to have been more embraced, outside the history of seventeenth-century science and philosophy than inside it.[60]

Leviathan and the Air-Pump has not been a major resource for the many historians of philosophy working on Hobbes, only partly because Hobbes is still understood to belong to the career of political thought and the interest of these scholars in natural philosophy and related mathematical matters has never been great. Hobbes is a "political philosopher" and Boyle is a "scientist," and nothing that happened in *Leviathan and the Air-Pump* has had any significant impact on those deeply entrenched disciplinary sensibilities. That said, Noel Malcolm, directing the great Clarendon edition of Hobbes's work and correspondence, has given close attention to claims the book made about Hobbes's relations with the Royal Society in general and with Boyle in particular. In impressively argued essays, he has urged that Hobbes did not become a member of the Royal Society because several of its Fellows were too close to his dangerous political position, tactically wishing to dissociate themselves from Hobbes. Malcolm has also concluded that the distinction *Leviathan and the Air-Pump* made between Boyle and Hobbes on explanatory adequacy in natural philosophy, and especially on the issue of evidence for and against a void, was exaggerated.[61] Otherwise, Hobbes scholarship has not found point in giving much detailed attention to questions about experiment and the polity. A monograph devoted to Hobbes's geometry and his long dispute with the Oxford mathematician John Wallis argued that what was called Shapin and Schaffer's "sociological reductionism" can, in principle, have no place in any reliable account of these controversies.[62]

[60] An interesting exercise might be a tabulation of citations to *Leviathan and the Air-Pump* to see what proportion of references citing specific pages are to the framing introduction and epilogue and what proportion to the middle pages detailing the concrete evidence for, and interpretation of, the Hobbes-Boyle controversies. Our impression is that for many readers it is as if chapters 2–7 do not exist. For warning against this tendency, see Zammito, *Nice Derangement of Epistemes*, p. 169. A further exercise would be an assessment of citing practices by *disciplinary affiliation*. We speculate that few citing writers outside the history of science and philosophy direct attention to *any* specific pages at all. This might help make sense of criticisms that, for example, the book ignores "epistemic factors."

[61] Noel Malcolm, *Aspects of Hobbes* (Oxford: Oxford University Press, 2002), pp. 187–191, 330.

[62] Douglas Jesseph, *Squaring the Circle: The War between Hobbes and Wallis* (Chicago: University of Chicago Press, 1999), pp. 343–355. For a summary of some implications

Michael Hunter, the most prolific of modern Boyle scholars, has been consistently hostile to the book. Rarely, if ever, disputing any factual claim, Hunter has repeatedly declared *Leviathan and the Air-Pump* methodologically and even ideologically toxic: Shapin and Schaffer are "simplistically functionalist"; their view of "matters of fact" is "somewhat distorted"; and they wrongly concentrate on "socio-political interests."[63] Their claims and findings about seventeenth-century science should, in any case, be viewed with grave suspicion because they are antiscience relativists, standing rightly accused of "an attempt to undercut the pursuit of truth by presenting all knowledge as relative and socially formed."[64] Hunter has been the Boyle scholar most seriously displeased with *Leviathan and the Air-Pump*, but other Boyle scholars have also been lukewarm or hostile. Rose-Mary Sargent, for example, thinks that Shapin and Schaffer were so concerned to "show the socio-political interests at work" that they "dismissed the epistemic dimensions of Boyle's methodological works"; Lawrence Principe has written that little of what they identify as novel with Boyle's literary programme was genuinely new; Jan Wojcik, remarkably concluding that Shapin and Schaffer had "criticized" Boyle's scientific methods, sought to defend Boyle from what she supposed to be their attacks.[65] A recent guide to matters Hobbesian dismisses the significance of Boyle's "bizarre" disputes with Hobbes as unworthy of

---

of *Leviathan and the Air-Pump* for Hobbes scholarship, see Luc Foisneau, "Beyond the Air-Pump: Hobbes, Boyle and the Omnipotence of God," *Rivista di Storia della Filosofia* 59 (2004), 33–51.

[63] See, for instance, Michael Hunter, "Introduction," in *Robert Boyle Reconsidered*, ed. Hunter (Cambridge: Cambridge University Press, 2003), pp. 1–18, on pp. 4–6; also Hunter, *Robert Boyle: Scrupulosity and Science* (Woodbridge: Boydell & Brewer, 2000), pp. 8, 59 (for the "functionalism" charge). The distinguished historian of seventeenth-century English science and medicine Charles Webster had a rather different assessment of the historical scholarship in *Leviathan and the Air-Pump*: "Shapin and Schaffer have executed their task with meticulous care. Their scholarship is difficult to fault." ("Pneumatic Mission," *Times Lit. Supp.*, 13 March 1987, p. 281.) Thomas Kuhn, who was distressed at the book's interpretative features, nevertheless judged that "the scholarship *is* very good. . . . [I]t's in many ways an extraordinarily interesting and good book. So it's not the scholarship that's bothering me": Kuhn, *The Road since Structure*, pp. 316–317.

[64] Michael Hunter, "Scientific Change: Its Setting and Stimuli," in *A Companion to Stuart Britain*, ed. Barry Coward (Oxford: Blackwell, 2003), pp. 214–230, on p. 221.

[65] Rose-Mary Sargent, "Learning from Experience: Boyle's Construction of an Experimental Philosophy," in *Robert Boyle Reconsidered*, ed. Hunter, pp. 57–78, on p. 66; also Sargent, *The Diffident Naturalist: Robert Boyle and the Philosophy of Experiment* (Chicago: University of Chicago Press, 1995), p. 10; Lawrence Principe, *The Aspiring Adept: Robert Boyle and His Alchemical Quest* (Princeton, N.J.: Princeton University Press, 2000), pp. 107–109; Jan W. Wojcik, *Robert Boyle and the Limits of Reason* (Cambridge: Cambridge University Press, 1997), p. 165.

serious attention: for Hobbes, these belonged to his other personal "oddities," such as his morbid fear of mountains.[66]

It is not our job here to defend *Leviathan and the Air-Pump* from these sorts of criticisms. We have already noted how unwilling or unable these critics have been to engage the evidence of the book on its own terms, while the myopia of these methodological specifications should already be evident from the first part of this introduction.[67] But it is remarkable that the most unambiguous claims for the historical *impact* of *Leviathan and the Air-Pump* come from one of these critics. Michael Hunter has pointed to this book as "one of the most influential historical works to have been published in the last two decades," its authors having purportedly "founded a historiographic school," members of which have produced multiple-prize-winning monographs in the history of science and culture. Yet he finds *Leviathan and the Air-Pump* so devoid of empirical or interpretative merit that he has left himself no way of understanding why it should have had any such "influence."[68] He has a low opinion of the book, and he must therefore have a correspondingly low opinion of his many colleagues so sadly taken in by it.

We cannot help our critic. It is possible, of course, that the book has had the impact he deplores because his evaluations of its empirical and interpretative merits are not generally shared. It is possible that he has misstated its findings, presumptions, purposes, and methods. It is possible that he has assimilated this project to the very social-external vs. intellectual-internal scheme whose adequacy the book meant to criticize—in which case it is possible that the critic did not read the book very carefully, or that he read it with his mind already made up about what sort of thing it was. Our purpose here has just been to show that the "influence" of *Leviathan and the Air-Pump* has been problematic. So far as we can tell, it has had only limited impact on the specific area of the history of early modern science. Some writers in this field have evidently found it sound and resonant; others have not.

Reasons to Be Cheerful

If it has not been our job to account for the criticisms, neither do we find ourselves well placed to interpret the extent of the book's reader-

[66] Glen Newey, *Hobbes and Leviathan* (London: Routledge, 2008), p. 12.

[67] Our only published response to a historian-critic is Shapin and Schaffer, "Response to Pinnick," *Soc. Stud. Sci.* 29 (1999), 249–253, 257–259.

[68] Hunter, "Scientific Change," p. 221.

ship in all sorts of inquiries more than a quarter-century after publication. Nevertheless, the historical background this introduction has described allows us at least to construct a checklist of considerations that might be pertinent here. If one wanted to know why *Leviathan and the Air-Pump* has been found interesting across a spectrum of academic inquiries, these are some considerations that might be borne in mind.

First, the book is indeed about an instrument and about a wide range of collective human practices attending the operation of the instrument, the interpretation and evaluation of its products—the knowledge tracing back to it and the forms of social relations accompanying it. There are several reasons why there might have been a constituency for such an exercise from the mid-1980s. The institutional place of science had changed fundamentally in the post–World War II world, and commentators on science had for some time been trying to come to terms with such changes. From a dominant view of science as pure thought, postwar commentary was moving towards conceiving science as instrumental work, the nature of knowledge flowing importantly from the work routines of producing it. The moment of individual epiphany was giving way to an appreciation of extended chains of collective labour. The role of instruments in the conduct of science had been a feature of Alvin Weinberg's characterization of "Big Science" and of President Dwight Eisenhower's uneasy identification of the "military-industrial complex."[69] Polanyi had insisted upon the work-craft nature of scientific knowledge in the late 1950s, and his views had been assimilated and re-presented in Kuhn's *Structure of Scientific Revolutions* in 1962.[70] In 1971 the historian Jerome Ravetz had attempted to build a picture of the social and political place of science by drawing out the implications of its craft-work status, and much early British sociological writing on scientific knowledge making drew attention to what Polanyi had called "tacit knowledge" and the work-worlds of the contemporary laboratory.[71] At the same time, a few British sociologists of science—working on a wide

[69] Alvin M. Weinberg, "Impact of Large-Scale Science on the United States," *Science* 134 (21 July 1961), 161–164; Dwight D. Eisenhower, "Farewell Address [17 January 1961]," in *The Military-Industrial Complex*, ed. Carroll W. Pursell, Jr. (New York: Harper and Row, 1972), pp. 204–208; see also Derek de Solla Price, *Little Science, Big Science* (New York: Columbia University Press, 1963).

[70] Michael Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy* (Chicago: University of Chicago Press, 1958); Kuhn, *Structure of Scientific Revolutions.*

[71] Jerome R. Ravetz, *Scientific Knowledge and Its Social Problems* (Oxford: Clarendon Press, 1971); see also Steven Shapin, "Signs of the Times," *Soc. Stud. Sci.* 27 (1997), 335–349.

range of materials—were displaying dissatisfaction with what was seen as the glib, facile, and reductive nature of some classic explanatory projects and were suggesting the pertinence and constructiveness of asking "how-questions" about knowledge making.[72] How did you make a fact and warrant its credibility? How were scientific representations made? How were inferences made, proofs offered and established? How did scientific knowledge move from the individual to the collective? How were procedural boundaries between modes of practice made visible and justified? Scientific knowledge making could and should be seen as the product of a collective, as work, as *performance. Leviathan and the Air-Pump* emerged out of that setting; it seems to have mobilized those sentiments in writing about a historical passage of science; it aimed to make a contribution towards the understanding of the quotidian instrumental and social work of making science.[73]

Leviathan and the Air-Pump appeared at a time when many—not all—historians of science had grown weary of the externalism-internalism frame. Even if they did not worry about the categories and framing of the debate, they nevertheless began to feel that its terms were too crude for the historical reconstruction of scientific change. The book also appeared in an ideological setting becoming more and more detached from the grand political cleavages which had charged

[72] See, notably, H. M. Collins, "The TEA Set: Tacit Knowledge and Scientific Networks," *Sci. Stud.* 4 (1974), 165–186; Collins, "The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of an Experiment in Physics," *Sociology* 9 (1975), 205–224; Trevor J. Pinch, "Theoreticians and the Production of Experimental Anomaly: The Case of Solar Neutrinos," in *The Social Process of Scientific Investigation*, Sociology of the Sciences Yearbook, vol. 4, ed. Karin D. Knorr-Cetina, Roger Krohn, and Richard Whitley (Dordrecht: D. Reidel, 1980), pp. 77–106; Pinch, "The Sun-Set: The Presentation of Certainty in Scientific Life," *Soc. Stud. Sci.* 11 (1981), 131–158. Some years later, Collins wrote that understanding science was *only* possible on the condition of real-time ethnographic engagement. Scientific practice destroyed its own history; the historian of science could never witness the process of putting "ships into bottles"—knowledge in the making—and was presented only with the finished product, textbook science. *Historical* understanding was, for that in-principle reason, an impossibility. Both Collins's achievements and his provocation were relevant to the way we framed our project: Collins, "Understanding Science," *Fund. Sci.* 2 (1981), 367–380.

[73] In the early 1980s there was a certain amount of interest among British historians in detailed accounts of scientific knowledge making as performances; see, notably, Martin Rudwick, "*Critical Problems in the History of Science*: Retrospective Review Symposium," *Isis* 72 (1981), 268–271, on 270: "What we urgently need are many more detailed and thorough studies (solve the publishing problems somehow!) of the processes of individual development and social negotiation by which specific pieces of claimed scientific knowledge have been constructed, propagated, maintained, and (sometimes) abandoned."

up those disputes from the prelude to World War II to the height of the Cold War.[74] In academic sociology, the field called the sociology of knowledge increasingly looked played out, generating few new and productive lines of inquiry. For reasons broadly similar to those at work in the history of science, the classic project of showing the operation—or the irrelevance—of social or "existential" factors in the constitution of knowledge was not generating significant new work of interest to practitioners in that or allied fields. In Imre Lakatos's philosophical language, the classic sociology of knowledge was looking very much like a "degenerating research programme."[75] Science—and, within science, observation-statements and deductive inferences—appeared traditionally as "hard cases" for showing the role of "social factors." Nevertheless, from the 1970s, historians of science were exploring whether those tough nuts could be cracked by more and more detailed, and more contextually sensitive, accounts of scientific episodes. The authors themselves had made several attempts at this sort of writing, and it might have been their sense that this line of inquiry had got as far as it was going to go that prompted them to consider whether the "social-factors-influencing-knowledge" frame was either appropriate or constructive.[76] *Leviathan and the Air-Pump* was meant to be, among other things, a large-scale *instantiation* of what the sociology of knowledge might look like if it rejected the "rules of the game" presupposed by traditional exercises. If there was a identifiable methodological slogan in the book, it was this: "Solu-

[74] Shapin, "Discipline and Bounding," esp. pp. 333–345.

[75] Imre Lakatos, *The Methodology of Scientific Research Programmes: Philosophical Papers*, vol. 1 (Cambridge: Cambridge University Press, 1978).

[76] Probably the most celebrated of these exercises was Paul Forman, "Weimar Culture, Causality, and Quantum Theory, 1918–1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Milieu," *Hist. Stud. Phys. Sci.* 3 (1971), 1–115. This paper was one inspiration for Shapin, "Phrenological Knowledge and the Social Structure of Nineteenth-Century Edinburgh," *Ann. Sci.* 32 (1975), 219–243; Shapin, "The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes," in *On the Margins of Science: The Social Construction of Rejected Knowledge*, ed. Roy Wallis, Sociological Review Monographs, vol. 27 (Keele: Keele University Press, 1979), pp. 139–178; Shapin, "Of Gods and Kings"; also Schaffer, "The Political Theology of Seventeenth Century Natural Philosophy," *Ideas and Prod.* 1 (1983), 2–14; Schaffer, Newton at the Crossroads," *Rad. Phil.* 37 (1984), 23–28; Schaffer, "Discovery Stories and the End of Natural Philosophy," *Soc. Stud. Sci.* 16 (1986), 387–420 (orig. publ. 1984). By the early 1980s Shapin was surveying the genres of sociological work on scientific knowledge and finding interest for the history of science in the early writings of such sociologists as Barry Barnes, Harry Collins, Trevor Pinch, and Michael Lynch: Shapin, "History of Science and Its Sociological Reconstructions," *Hist. Sci.* 20 (1982), 157–211.

tions to the problem of knowledge are solutions to the problem of social order."[77] It is a formulation that seems to have travelled quite widely in the academic world.[78]

Leviathan and the Air-Pump was inter alia an instantiation of a research programme in the sociology of scientific knowledge. That is to say, it was a *case study*. It *was* meant to illuminate important features of seventeenth-century natural philosophy, but it was, at the same time, intended to frame an agenda for the sociological and historical study of knowledge generally. It was never just the one thing or the other. By the time the book appeared, the case-study genre had become attractive to the small group of sociologists concerned with the naturalistic study of scientific knowledge making. Points of interest to methodological or theoretical concerns were being made and illustrated, and arguments about the legitimacy and interest of one or another version of the sociology of scientific knowledge were being addressed, by way of a series of close case studies—of modern research on solar neutrinos, gravity waves, latent hidden variable theory, plant taxonomy, etc.[79] The case-study genre and its uses were familiar in cultural anthropology: recall set-piece engagements with "relativism" and "rationalism" by way of the classification of cassowaries and twin-birds, and by way of treatments of the efficacy of poison oracles—but also consider the use of historical case studies in Kuhn's general model of scientific change.[80] And consider, too, the way in which Kuhn urged that normal science *itself* proceeded by reasoning on, and training in, cases.[81] In fastening upon Boyle's air-pump to discuss experimental

[77] *Leviathan and the Air-Pump*, p. 332.

[78] A search for this aphorism in Google Books will reveal some evidence of that circulation.

[79] Many of those cases were surveyed several years before in Shapin, "History of Science and Its Sociological Reconstructions."

[80] For classic anthropological cases, see, for example, E. E. Evans-Pritchard, *Witchcraft, Oracles and Magic among the Azande* (Oxford: Clarendon Press, 1937), esp. pp. 120–163 (for the poison oracle); Evans-Pritchard, *Nuer Religion* (Oxford: Clarendon Press, 1956), pp. 128–133 (for twins and birds); Ralph Bulmer, "Why Is the Cassowary Not a Bird? A Problem of Zoological Taxonomy among the Karam of the New Guinea Highlands," *Man* 2 (1967), 5–25. For the mobilization of such iconic examples in fundamental methodological and conceptual debates, see, for example, Barry Barnes, "The Comparison of Belief Systems: Anomaly versus Falsehood," in *Modes of Thought: Essays on Thinking in Western and Non-Western Societies*, ed. Robin Horton and Ruth Finnegan (London: Faber & Faber, 1973), pp. 182–198; Barry Barnes and David Bloor, "Relativism, Rationalism and the Sociology of Knowledge," in *Rationality and Relativism*, ed. Martin Hollis and Steven Lukes (Oxford: Basil Blackwell, 1982), pp. 21–47.

[81] The antirationalism of Kuhn's crucial *Structure* chapter on "The Priority of Paradigms" (pp. 43–51) drew upon Wittgenstein's scepticism about rational formal method

practice in science, *Leviathan and the Air-Pump* was following in the case-study tradition marked out by J. B. Conant's *Harvard Case Histories in Experimental Science*, a text used in a celebrated Harvard General Education course in which Kuhn himself taught and whose cases made up such an important part of the empirical content of *Structure of Scientific Revolutions*.[82]

So *Leviathan and the Air-Pump* belonged to a casuistical genre well-known in anthropology, and in allied debates over rationalism and relativism. But it can also be seen as a pumped-up contribution to a long-standing tradition of scholarship in the history of science. It is not a tradition very well suited to illustrating, so to speak, the *neighbouring general* by way of the historical particular, and the methods of microhistory offer a substantive reflection on the means through which a "normal-exceptional" case can generate historical understanding.[83] If you are interested in the particularities of eighteenth-century experimental practice in France, you do not necessarily achieve very much through a close study of experimental practice in seventeenth-century England. If specificity is your sole goal, then you will, of course, want to give accounts about your preferred practice and setting that pick out what makes them distinct from all other historical passages. There's nothing wrong about that. But, if suitably framed and qualified, a study of experimental practice in seventeenth-century England may be a pertinent resource in telling stories about, for example, *knowledge making* and how one might profitably go about studying how knowledge is made in a wide range of times and places. The book did not establish or justify its remarks about the present by traversing every temporally intermediate stage. Its authors did not think they were obliged to travel through the eighteenth and nineteenth centuries to license remarks about the present, although, again, for other sorts of historical projects, one would want to do just that.

Leviathan and the Air-Pump juxtaposed these seventeenth-century episodes with the modern contemporary because its authors thought they had described some significant patterns held in common. To

---

statements, as did writers in the Edinburgh Strong Programme of which Kuhn so disapproved.

[82] Joy Harvey, "History of Science, History and Science, and Natural Sciences: Undergraduate Teaching of the History of Science at Harvard, 1938–1970," *Isis* 90 Supplement (1999), S270–S294, on S280–S282.

[83] See Carlo Ginzburg and Carlo Poni, "The Name and the Game: Unequal Exchange and the Historiographic Marketplace," in *Microhistory and the Lost Peoples of Europe*, ed. Edward Muir and Guido Ruggiero, trans. Eren Branch (Baltimore, Md.: Johns Hopkins University Press, 1991), pp. 1–11.

find that sort of thing illegitimate would be to claim that there are no such patterns or, less forcefully, that, if they exist, people called historians must not concern themselves with them. The dense accounts of the Hobbes-Boyle disputes were indeed meant to retrieve many past specificities. At the same time, those dense accounts allowed the display of knowledge- and order-making forms which, the authors suggested, feature in making all sorts of knowledge, in all sorts of settings. That was the kind of thing being gestured at when it was said that solutions to the problem of knowledge are solutions to the problem of social order. The authors could not think of particular passages of knowledge making and order making in which that principle did not apply. You have the historically particular and you have some observations about the transcendental conditions for knowledge and order. It seemed interesting and legitimate to do that. *Leviathan and the Air-Pump* was written in a context where there was a finite but significant audience for that sort of venture.

However one might mine case studies for robust findings, a recognized historical virtue of detailed engagements with cases is, indeed, the capture of particularities. One inescapable feature of the book is its attention to heterogeneity, variation in belief and judgment, controversy. It aimed to treat scientific controversy—about findings and about programmes for producing findings—as natural. It took Hobbes's natural philosophical agenda seriously and showed that many of those who opposed Hobbes took him seriously enough to counter his claims and recommendations.

What were known as "controversy studies" had, by the mid-1980s, become set-pieces in sociological studies of the making of modern scientific knowledge. Controversy was looked for, and focused on, as a sign that one was indeed engaging with "knowledge in the making," and the authors of *Leviathan and the Air-Pump* reckoned that this sensibility had historical grip as well. If there was originality in what they did, it may have been the tightness of their focus on disputes over the "rules of the game" as well as on the rightness of moves within a largely agreed knowledge-game. That said, drawing attention to controversy attending one of the iconic programmes of seventeenth-century science and, still more, extending "charitable interpretation" to one of its adversaries were not compatible with traditional talk about "the essence of the Scientific Revolution" or of "what seventeenth-century scientists believed." The book sought to report on what is so often found when the impulse to retrieve historical specificity is extended beyond its normal bounds—and that is texture, variation, and contest.

Leviathan and the Air-Pump was, in one sense, quite a traditional historical exercise: it took historical particularism seriously, perhaps more seriously than was then thought necessary. Its authors were not made uneasy by the heterogeneity they found; they saw that it is a normal, and arguably a pervasive, feature of science-in-the-making. But they insisted on the historical salience of engaging with heterogeneity, and "the dog that didn't bark" in their book was the absence of reference to a coherent "essence" of seventeenth-century science, of what it meant to be "modern," of "the Scientific Revolution." Such sensibilities about variation and contest were not very common among historians of science at the time the book was published, but they were increasingly influential among a range of social historians concerned, for example, to retrieve the submerged perspectives of groups traditionally neglected by academic history, including those picked out by class, race, ethnicity, gender, or other modes of disempowerment. Struggle, including struggle to secure cultural credibility and legitimacy, was seen as normal, and documenting and interpreting struggle was increasingly seen to be worth historians' most serious attention.[84] *Leviathan and the Air-Pump* therefore appeared at a time when many sorts of academic historians were seeking to document heterogeneity and struggle. What might have been news to some of them was that one might do the same when one's subject was science.

Crossing the Bar

One last consideration bears on the spread of interest in the book, though here the situation is more ambiguous. Interdisciplinarity has been much talked-up in recent decades. Little is said about its drawbacks, while the virtues of interdisciplinarity are more often asserted than argued. Despite Kuhn's powerful account of the change-inducing effects of paradigmatic "narrowing of perception," close observance of the disciplines' conventions, procedures, boundaries, and schemes of value is commonly equated with restricted imagination or an impoverished sense of intellectual adventure. The celebration of "open-mindedness" is one basis for the widespread, but wrong-headed, reading of Kuhn's *Structure* as a *criticism* of "normal science"

[84] E. P. Thompson, *The Making of the English Working Class* (Harmondsworth: Penguin, 1968; orig. publ. 1963), p. 13: "I am seeking to rescue the poor stockinger, the Luddite cropper, the 'obsolete' hand-loom weaver, the 'utopian' artisan, and even the deluded follower of Joanna Southcott, from the enormous condescension of posterity."

and a *celebration* of "revolutionary science." The disciplines rule academic life, but, curiously enough, few publicly applaud any benefits they bring. For all that, one can wonder whether the recent commendation of interdisciplinarity—especially in the humanities and social sciences—is little more than mouth-music. Academic administrators increasingly attach themselves to interdisciplinary rhetoric, sometimes just as a way to curb the power of the departments, and the professors find ways to pay lip service to the idea of interdisciplinarity while perpetuating institutional and career-reward structures which penalize its practice. It's the disciplines which continue to train, appoint, publish, promote, and reward. There can be no doubt about their continuing power, and a case can be made that the disciplines' dominance, while ebbing in the natural sciences and the professional schools, has been increasing in the humanities and social sciences. Meantime, interdisciplinarity is sometimes given a bad name by practitioners seeming to equate it with the playful rejection of any sort of discipline rather than serious submission to more than one.

In the discipline called the history of science, a measure of interdisciplinarity was more a momentary feature of its institutional circumstances than a well-rooted preference. Many practitioners were housed in departments of the "history and philosophy of science," whose shop windows advertised an in-principle radical interdisciplinarity. Others found themselves in history departments, despite Sarton's insistence that science was no ordinary historical object, and despite "mainstream" historians' tendency implicitly to agree.[85] *Leviathan and the Air-Pump* has been called an interdisciplinary exercise, and at least some of the reaction to it seems to have been informed by its visibility as an interdisciplinary accomplishment. But it arrived in an academic setting which was at least ambivalent about interdisciplinarity, in practice if not in public rhetoric. In the United States, undergraduate programmes labelled as interdisciplinary (also variously "transdisciplinary," "multidisciplinary," or "cross-disciplinary") were growing in popularity from the 1970s, students, for a range of reasons, seeming to welcome their "flexibility" or just the freedom to carry on their education for as long as possible without being obliged to cast their lot with a single discipline. In other moods, interdisciplinarity was approved as a way of providing the adaptable skill sets that would be called upon by the nature of late modern social and political

[85] Mayer, "Setting Up a Discipline, II." She points out the nice irony that the more externalist histories of science were those produced by immediately postwar scientists like Bernal and Needham, and the more internalist accounts were those produced by immediately postwar historians like Rupert Hall.

problems, which obstinately refused to sort themselves out into "scientific," "social scientific," and "humanistic" species. Similarly ambiguous normative themes operated in the United Kingdom during the economic and political crises of the 1970s and early 1980s. Interdisciplinary studies had long been touted as means of compensating for technocratic overspecialization. In part, this move was announced as a response to legitimate student concern with the narrowness of traditional British degree courses; in part, it was justified through energetic arguments around the integrity of the university as a site of disinterested inquiry. The tone shifted somewhat in the context of mid-1970s political and economic conflict and its aftermath. Interdisciplinary strategies, with significant backing from major research councils and from educational charities, were now judged worthy methods that might better orientate British university programmes to strongly utilitarian ends.

Both the rhetoric and the institutional reality of academic interdisciplinarity were experienced in especially coherent ways in and around the divide between the sciences, on the one hand, and the humanities and social sciences, on the other. Sometime in the 1960s, responding to these developments, new institutional configurations arose which housed a small number of historians of science under the same roof as social scientists and policy students concerned with science and technology. In the mid-1960s the Edinburgh Science Studies Unit was established partly as a response to C. P. Snow–inspired concern with the "Two Cultures." The biologist C. H. Waddington initially intended it to supply a missing "liberal arts" component to undergraduate science education.[86] (Waddington is reported to have told the Unit's founding director, David Edge: "We'll teach 'em the science—you teach 'em the rest.")[87] A recognized problem was healing the conflicts of the faculties, and a remedy was offering "bridges"—that term was pervasive—across which students could perhaps safely and surely travel from one field to another.

It was sometimes thought that the faculty too would benefit from "science and society" interdisciplinarity. The journal *Science Studies*—later retitled *Social Studies of Science*—was coedited by David Edge from 1971, and, during the 1970s and 1980s, it often published the work of historians as well as social scientists and philosophers. The professional organization called the Society for the Social Studies of Science

[86] John Henry, "Historical and Other Studies of Science, Technology and Medicine in the University of Edinburgh," *Notes and Rec. Royal Soc.* 62 (2008), 223–235.

[87] Reported in David Bloor, "David Owen Edge: Obituary," *Soc. Stud. Sci.* 33 (2003), 171–176.

(4S), founded in 1975, was once widely attended by historians of science and, from 1978, published its own journal, *Science, Technology & Human Values*.[88] From the 1960s other frankly interdisciplinary "science studies" or "science and technology studies" teaching and research units came onto British and American academic scenes, some responding to "common context" intellectual currents and "Two Cultures" educational sensibilities, others to different sorts of activist sentiments—for example, how to make science and technology socially useful; how to identify and cope with the "social problems" accompanying rapidly advancing modern science and technology; and how to encourage "public understanding of science."[89]

The pertinence of these circumstances to the appearance and evaluation of *Leviathan and the Air-Pump* seems clear. The book concluded with remarks on the modern condition; one of the authors was employed in an "interdisciplinary" Science Studies Unit; and the authors first met in 1980 at one of the disciplinarily promiscuous meetings on sociology, history, and philosophy that were a notable feature of the British academic scene in the 1970s and 1980s.[90] It was not considered odd that historians, sociologists, and philosophers might find mutual interest in each others' work, even if that interest often expressed itself in focused disagreement. Both shared interests and sharp arguments helped create and sustain a bibliography that was itself, to a degree, held in common. People in different disciplines often read the same books; they often had overlapping senses of pertinent problems and relevant resources for thinking about those problems.

Understood that way, *Leviathan and the Air-Pump* was, of course, an interdisciplinary exercise. It was just the sort of book one might ex-

[88] The European Association for the Study of Science and Technology (EASST) was founded in 1981, with precursors going back to the early 1970s.

[89] Some early programmes include the Department of Science and Technology Studies at Cornell, the Liberal Studies in Science programme at the University of Manchester, the Science Policy Research Unit of the University of Sussex, and the Program in Science, Technology and Society at MIT. For reflections on the relation between the institutionalization of science studies and forms of activism in the 1960s, see Jon Agar, "What Happened in the Sixties?," *Brit. J. Hist. Sci.* 41 (2008), 567–600, on 592–595.

[90] The meeting at the University of Bath on 27–29 March 1980 was attended by philosophers (including Mary Hesse, Bruno Latour, and David Bloor), sociologists (including Barry Barnes, H. M. Collins, Trevor Pinch, Andy Pickering, Donald MacKenzie, and John Law), and historians of science (including Martin Rudwick, Maureen McNeil, Chris Lawrence, Roger Smith, L. S. Jacyna, John Pickstone, and David Gooding). Billed as "New Perspectives in the History and Sociology of Science," it was nominally a joint meeting of the British Society for the History of Science and the British Sociological Association Sociology of Science Study Group.

pect to emerge from the intellectual and institutional setting of mid-1980s Britain, and what might need explaining is why there were not more books quite like it and why it did not, as some commentators expected it would, "set a trend."[91] And yet the interdisciplinarity of the book was different in tone, texture, and purpose from some recommendations of interdisciplinarity that one found at the time. *Leviathan and the Air-Pump* did not aim to celebrate the virtues of mixing the disciplines or rubbing them together. Rather, its authors thought of themselves as engaged with a set of *problems*, and the resources and methods they used were conceived pertinent to addressing these problems. One problem could be called the *problem of knowledge* as it presented itself to historians of early modern natural knowledge. How did seventeenth-century scientific practitioners go about making natural knowledge and advertising its status and worth? What variants were then available that might count as natural knowledge, and how was the contest between these variants conducted? What was thought to recommend different knowledge-making programmes, and how did their agendas, conventions, and procedures stand with respect to other social and cultural practices? Under one description, one could say that the problem of knowledge belonged to philosophers, even if they tended to approach it normatively, with a view to sorting out genuine knowledge from what merely counted as knowledge. The authors' preference was to engage with knowledge making in a naturalistic mood: What did people actually do when they were making what they considered to be knowledge? How did they warrant what they produced, and how did they secure credibility and authority for it?

[91] Martin Rudwick's magisterial and universally applauded *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists* (Chicago: University of Chicago Press, 1985) appeared the same year as *Leviathan and the Air-Pump*, and while it avoided the sorts of explicit transhistorical gestures in Shapin and Schaffer's book, it clearly shared some of their concerns. Rudwick had spent some time in the early 1980s at the Edinburgh Science Studies Unit as a visiting fellow and later was a prime mover in establishing the interdisciplinary Science Studies Program at the University of California, San Diego. British sociologists of scientific knowledge treated these two historical works as natural pairs: H. M. Collins, "Pumps, Rock and Reality [extended review of *The Great Devonian Controversy* and *Leviathan and the Air-Pump*]," *Soc. Rev.* 35 (1987), 819–828; Trevor J. Pinch, "Strata Various [essay review of *The Great Devonian Controversy*]," *Soc. Stud. Sci.* 16 (1986), 705–713. Rudwick had for some time announced his vigorous support for the study of the "social dimension" of science, while leaving more or less intact the "social-intellectual" polarity and worrying about "the blatantly politicized forms" taken by some "science and society" programmes. In 1981 he expressed anxiety that the history of science was being sacrificed to politically orientated interdisciplinary forms, and that "unless we fight hard, there may be no historians of science left in the year 2001": Rudwick, "*Critical Problems*," p. 271.

Another problem the book addressed has been called the *problem of order*. How is social order possible? How does it happen that groups of people act as if they more or less agree about the quotidian forms of collective life; to sustain institutions in which forms of collective life may be carried out; to coordinate their activities—not just to achieve collective ends but to frame the grounds of their internal conflicts? This problem has traditionally belonged to the sociologists, who had sorted two major possible solutions into coercive forms—which they associated historically with the thought of Thomas Hobbes—and those stressing the role of shared senses of what is right and legitimate—which were linked to Max Weber's social thought.[92]

Leviathan and the Air-Pump was an attempt to see the problem of knowledge and the problem of order as *the same problem.* Wherever and whenever groups of people come to agree about what knowledge is, they have practically and provisionally solved the problem of how to array and order themselves. To have knowledge is to belong to some sort of ordered life; to have some sort of ordered life is to have shared knowledge. The "social" and the "intellectual" were two contextually intelligible ways of parsing configurations that were always and everywhere neither unintellectual chunks of interactive life nor unordered free-floating and disembodied ideas. There was no need to ban talk of the social and the intellectual—as Bruno Latour famously proposed in 1987—because these kinds of speech were, and continue to be, among the resources people used (and use) to make sense of their world and to identify proper and improper conduct.[93] If the authors of *Leviathan and the Air-Pump* had been metaphysicians, they might have insisted that only speech of something like "knowledge-order conjunctures" was permissible, but they were historians, and their enterprise was interpretative at its core. They allowed themselves to be curious about what historical actors meant, and what these actors were doing, when they invoked categories like the "social" and the "intellectual"—and related usages. This degree and texture of curiosity about the categories belonged to the authors, not to the historical actors. The authors meant to describe the grounds of historical meaning and usage, not to criticize historical actors for bad metaphysics.

[92] See, e.g., Dennis H. Wrong, *The Problem of Order: What Unites and Divides Society* (New York: Free Press, 1994); Wrong, *Skeptical Sociology* (New York: Columbia University Press, 1976), esp. chs. 2–3.

[93] For advocacy of a moratorium on such speech, see Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Milton Keynes: Open University Press, 1987), p. 247, and, for a counter, see Shapin, "Discipline and Bounding," pp. 355–356.

Leviathan and the Air-Pump belongs to the past. When the book was written, interdisciplinary science studies was relevant to the project, even if advocating interdisciplinarity was not its goal. We are gratified that there is apparently still a readership for the book more than a quarter-century after its original publication, but one reason we accepted the awkward task of preparing this introduction was that it allowed us to situate the book as a historical object. This was an opportunity to write a bit of history in which we ourselves were historical actors. But as we thought about the introduction, we were unavoidably aware of how fragile the conditions were for anything like this book to be written, published, and found intelligible. For some time, the relevant humanistic and social scientific disciplines have been rounding up their wagons, expelling intruders from their midst, and ever more powerfully controlling their members' bibliographies, their senses of legitimate problems, pertinent resources, and approved modes of writing.[94] *Leviathan and the Air-Pump* was the product of an intellectual and institutional environment which has not been easy to perpetuate and which, indeed, some of our academic colleagues have worked to undermine. The republication of the book is in one sense welcome, but in another sense it would be wonderful to inhabit an academic world in which there would be no call for a new edition of a work of empirical history produced by members of a previous generation. We look forward to the day when *Leviathan and the Air-Pump* is, in every sense of the word, history.

[94] For a remarkable recommendation that the now properly professionalized academic history of science separate itself from interdisciplinary science studies, see Lorraine J. Daston, "Science Studies and the History of Science," *Crit. Inq.* 35 (2009), 798–813; and a response by Peter Dear and Sheila Jasanoff, "Dismantling Boundaries in Science and Technology Studies," *Isis* 101 (2010), 759–774. Note that Daston, making only a few references to *Leviathan and the Air-Pump*, seems to dissociate it from "science studies," while, for Dear and Jasanoff, it is one of the major evidences of the vitality of links between history and "science and technology studies."

· NOTES ON SOURCES AND CONVENTIONS ·

For citations of sources in footnotes we have adopted an economical convention similar to that employed in Elizabeth Eisenstein's *The Printing Press as an Agent of Change.* Bibliographic information is kept to a minimum in the notes, apart from the occasional addition of date of publication where that information is not given in the text and is germane. Full titles and publication details are provided in the Bibliography. Complete details of unpublished manuscript sources, seventeenth-century periodical articles, and items in state and parliamentary papers are, however, given in the notes and not repeated in the Bibliography.

We have made liberal use of correspondence and other material not published in the seventeenth century. Our major concerns have been with knowledge that was public or designed to be so, and this has affected the extent of our use of such sources. Where we are interested in material that was incompletely public or, possibly, intended to be restricted (as in chapter 6), our use of manuscript material is correspondingly greater.

During the period with which this book is concerned, the British Isles employed a calendar different from that used in most Continental countries, especially Catholic ones. The former used the Julian (old style) calendar, which was ten days behind the Gregorian (new style) calendar employed on the Continent. In addition, the British new year was reckoned to begin on 25 March. Because we deal in some detail with exchanges between England and Continental countries, we give all dates in both old and new style form, but we adjust years to correspond with a new year commencing 1 January. Thus, the English 6 March 1661 is given as 6/16 March 1662; the Dutch (who used the Gregorian calendar even though Protestant) 24 July 1664 is given as 14/24 July 1664; and so forth.

We have endeavoured, within reason, to preserve seventeenth-century orthography, punctuation, and emphases, and have dispensed with *sic* indications, save where absolutely necessary.

In our usage, "Hobbesian" refers to the beliefs and practices of Hobbes as an individual; "Hobbist" to the beliefs and practices of his real or alleged followers. We distinguish between religious Dissent (upper case) and intellectual and political dissent (lower case).