

BOOK REVIEW FORUM

Objectivity and Its Critics

LORRAINE DASTON AND PETER GALISON

We are grateful to Amanda Anderson, Theodore Porter, and Jennifer Tucker for their thoughtful and thought-provoking reflections on our book and to the editors of *Victorian Studies* for the opportunity to respond to them. Given the constraints of space, we have chosen to concentrate on those points that allow us to widen the discussion about the kind of history we have attempted in the book and to clarify how that history stands in relation to more familiar alternatives.

Why Scientific Atlases?

No one science has monopolized scientific objectivity: whatever objectivity is and wherever and whenever it came from, its career cross-cuts many scientific disciplines. Moreover, if we are right about the central claim of the book, namely, that objectivity has a history, then there must have been a time *before* objectivity. Finally, objectivity and the other epistemic virtues tracked in our book are not just the stuff of prefaces, popularization, and philosophy but also of specific and significant scientific practices. We therefore needed a source of sufficient longevity, breadth, and importance to practice in order to capture the phenomena in which we were interested—otherwise we would perpetually find ourselves comparing apples and oranges. As we stress (17-19), atlases are by no means the sole possible source for a history of scientific objectivity and its alternatives, but they do have several signal advantages: (1) a long baseline as a key scientific genre, dating back to at least the eighteenth century (or earlier, depending on discipline); (2) a broad span across almost all the empirical sciences (and even some branches of mathematics); and (3) a central place in the teaching and sustaining of key scientific prac-

tices. Because atlases are so expensive along every dimension (time, money, skill) to make and remake, they are also sensitive barometers of *deep* shifts in epistemic values. No discipline supplants its vade mecum to vision without urgent cause: a new atlas demands that the community of practitioners learn to see anew.

Given the prominence of atlases in our book, both Tucker and Porter ask why we did not pay more attention to the details of atlases as a genre. Our project is, however, not a history of scientific atlases per se. We are, instead, *using* atlases to track changes in normative right depiction. For our purposes, scientific atlases are an index, not the primary subject matter. There is ample evidence for the wide dissemination of these costly volumes, which generally went through many editions, and for their status as constantly consulted reference works, often showing vivid physical evidence of use: sufficient proof of their impact in teaching scientists how to see.

Tucker raises the key issue of how atlases are read:

Atlases, indeed, serve as descriptive guides to idealized scientific identities or personae; a bit like conduct books for scientists, they offer moral lessons about how proper knowledge is to be secured. However, a crucial historical question about both atlases *and* conduct books concerns their reception and use, not their production alone. We do not really accept that nineteenth-century women lived according to prescriptive literature. (XXX)

The atlases are normative but not, as Tucker suggests, in the sense of conduct books: scientific atlases exemplify rather than merely prescribe a way of seeing. To use an atlas to diagnose a disease, identify a plant, or classify an elementary particle track is willy-nilly to absorb its visual conventions. An apt analogy would be reading a map constructed according to a certain projection, scale, and legend: the more one uses this particular kind of map, the more second-nature its conventions become—as evidenced by the initial disorientation experienced in using a map constructed according to a different projection and scale. Unlike conduct books, maps don't prescribe rules on how to see; they embody a set of practices. To read a map is to master a way of seeing by doing; atlases function in the same way. To use an atlas is to see, over and over again, and thereby to learn how to see in a specific way: it is more like practicing a musical instrument than reading a treatise on how to play. This is an area in which historians of science, historians of art, and historians of reading might fruitfully make common cause,

combining the brilliant insights of recent works on the history of seeing and the history of reading with equally innovative studies on the history of scientific visualization.

In the context of atlases in the service of mechanical objectivity, there is a still deeper connection between reading and doing—one might even say, between reading and being. The act of repeatedly distinguishing between objective image and subjective interpretation for image after image created the phenomenon it was meant to enforce: the sharp boundary between objectivity and subjectivity. In the era of mechanical objectivity, that boundary isolated what was experienced as the subjective, uncontrolled will of the mid-nineteenth-century scientist from the scientific, objective image that was defined precisely by its degree of independence from the scientist's will. Mechanical objectivity and willful intervention specified each other in contradistinction. To read such atlases in the mechanically objective spirit in which they were conceived was to practice drawing and redrawing that boundary, simultaneously reinforcing it visually and existentially.

Objectivity is a history of learning how to see scientifically: how to see essences behind appearances, how to restrain the hand and leave imperfect things imperfect, how to see artifact and pry it from right depiction. These are hard lessons that must be learned over and over again in painstaking detail. There are techniques of preparation, techniques of image-making, techniques of image reproduction, techniques of seeing—a myriad of devilishly hard-to-master steps. But as the disciples of objectivity mastered the microphotography of snowflakes, they transformed not only the snowflake image but also themselves—from the sage whose techniques of depiction and vision extracted geometric perfection behind appearances to the self-restrained observer who could depict the flawed individual flake in all its particularity. In such cases, reading, seeing, and being all merge.

Epistemic Fear

In *Objectivity* we argue that epistemology and ethos fuse in epistemic virtues such as truth-to-nature, objectivity, and trained judgment. This claim conjoins knower with knowledge in ways that are orthogonal to two more familiar ways of understanding that relationship: the psychological and the sociological. Broadly speaking, the psychological conception of how knower and knowledge interact treats

the knower as an individual, with an emotional and intellectual complexion conditioned by his or her biography; this distinctive complexion in turn explains insights and blind spots, predilections and aversions, hopes and fears peculiar to the person. In contrast, the sociological conception analyzes the investigator as a member of a group—a profession, a social class, a scientific school—and explains scientific affinities and allegiances collectively, on the basis of group membership. Although psychological and sociological approaches have dueled with one another since Comte, the history of science has been greatly enriched by both. But viewed from the perspective of either, our analysis must look distinctly odd, and both Anderson and Porter find it so, especially when we write about epistemic fear.

Porter agrees that mechanical objectivity is an expression of “timidity” (XXX) but finds it implausible that stridently self-confident scientists of the stamp of Claude Bernard or Thomas Henry Huxley should cower before anyone or anything: “For me, it is more than a little strange to see the period from 1850 to 1920—the high-water mark of public science—construed as an era of mechanical objectivity” (XXX). In his excellent study of the self-presentation of science in the public, political world, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life*, Porter has shown how professional groups such as actuaries and engineers display the strategies of mechanical objectivity only with the utmost reluctance and then from a position of weakness (e.g. when their autonomy is threatened by political meddling). Strong, established elites, Porter argues, prefer the exercise of judgment and personal discretion to the corseted protocols of mechanical objectivity. From this sociological standpoint, the evidence we present (especially in Chapter Three of *Objectivity*) for the avid and voluntary embrace of mechanical objectivity in the mid-nineteenth century across a spectrum of disciplines, from mineralogy to astronomy, anatomy to ballistics, must seem jarring. It is this explosion of mechanically objective atlases of the most diverse phenomena that is our focus—not just the public pronouncements of celebrities like Huxley and Bernard (whom we described in Chapter Four in connection with the scientists’ use of Kant).

Anderson objects to what she regards as the gratuitous and destabilizing intrusion of the psychological element of fear into epistemology. Taking up our claim that all epistemology is driven by fear (372), she queries: “But once ethos is reinterpreted as pathos, or as reactive fear

or anxiety, it seems far less easy to cast it as an aspiration toward an ideal that the practitioners know cannot be reached—now the psychological dimension becomes mere symptom, and the ideal collapses into a form of defensive illusion” (XXX). Instead of epistemology and its attendant ethos being self-contained pursuits of regulative ideals, they now, in her reading, seem reduced to a mere emotion of “excessive dread” (XXX), and a rather shabby emotion at that. In her reading, fear, that most unphilosophical of the passions, pulls the rug out from under what might otherwise be a legitimate, even noble aspiration to rid knowledge of errors. Moreover, any mention of fear or other emotions undermines our attempts to avoid reductionist explanations: “a certain form of psychological theory reintroduces the causality that Daston and Galison had seemed to eschew, and it sits uneasily next to their avowal of a stringently superficial approach” (XXX).

What is epistemic fear, and what does it have to do with epistemology and ethos? The answers require an effort to stand outside of precisely the conceptual framework of objectivity and subjectivity our book attempts to historicize. Epistemic fear straddles the boundary between the epistemological and the psychological, as well as that between the psychological and the sociological—and therefore appears within that either/or framework to be chimerical, to be a hybrid monster. But there are other ways of dividing up the world. The historical evidence in this case speaks strongly in favor of those alternatives.

If mechanical objectivity is provoked by fear, why should self-assured elites pursue it, especially if the fear in question is collective, and therefore a *prima facie* candidate for the usual sorts of sociological explanations? We would note in passing that there is nothing inconsistent in the use of self-denying asceticism as a means of self-aggrandizement by aspiring elites, as Nietzsche notoriously observed (just how well-established an elite scientists were in the late nineteenth century is a matter of controversy among historians, but we will bracket that issue here). However, in the historical cases we treat there is a thornier problem for the kind of explanation Porter prefers: the professions and practices of mechanical objectivity in the atlases we cite in Chapter Three were not for public consumption—many of the atlases were technical, difficult texts on arcane subjects such as hemoglobin crystals or echinoderms. They were used in the work of medicine or science, not Sunday sermons for patrons, politicians, and popularizers. Mechanical objectivity in these cases was not a response to the peering

eyes of parliamentary regulators or congressional pork-barrel rollers but to perceived imperatives of inquiry by working specialists. Conversely, the eighteenth-century savants described in Chapter Two, whose standing as a culturally recognized elite was precarious at best, nonetheless idealized, perfected, and generalized boldly. Porter does not question this evidence; he simply finds such conduct inconsistent with a certain view of elites. We contend that the evidence suggests at very least that there is no necessary connection between elite status and epistemic practice.

We further argue in favor of a deep connection between epistemology and ethos—so deep that they cannot usefully be pried apart. It is just this link that Anderson believes to be threatened by the invocation of epistemic fear. Where Porter immediately reads fear as sociological, because it is collective, Anderson infers it must be psychological, because it is an emotion. We must insist that it is genuinely epistemic, a reasonable (if neither logically necessary nor historically inevitable) response to the genuine and multiple obstacles to the acquisition of knowledge. Although mechanical objectivity is the most dramatic expression of epistemic fear, because the fear in question is of oneself, from whom there is no escape, truth-to-nature and trained judgment are also rooted in epistemic fears—respectively, the fear of being overwhelmed by the sheer complexity and variability of the world or the fear of missing significant patterns by myopically fixating on details. These are certainly affective states, and powerful ones at that; are they thereby incompatible with epistemology? A prevalent view of epistemology, broadly Kantian in its outlines, would answer firmly in the affirmative: epistemology (and for that matter, ethics as well) belongs to the realm of objective validity and therefore stands opposed to subjectivity in all its forms, most particularly to the psychological. In neither ethics nor epistemology is there, according to this (roughly neo-Kantian) view, any need for an ethos, a cultivated self sustained by collective practices: the individual exercise of reason and will suffice. We disagree.

Our book seeks not only to historicize this view but also to propose an alternative. Epistemology is rooted in an ethos, which is at once normative and affective—or affective *because* normative. To school the senses and the intellect over decades in the service of collective empiricism creates not only collective habits but also collective values and affects. In this sense, the practices of the scientific self resemble the spiritual exercises of the ancient Stoics or Christian ascetics, which

systematically modified the emotional complexions of initiates in the interests of religious, ethical, and epistemological goals. The indignation triggered by the violation of an epistemic virtue is real and vehement, as our book repeatedly documents. So is epistemic fear. These affects are part and parcel of ethos and epistemology; they are not underlying explanations or contaminations of either.

There is nothing irrational (or reductive) about epistemic fear—quite the contrary. This claim sounds paradoxical only within a framework that drives a wedge between the epistemological and the psychological, assigning one to the realm of objectivity and the other to that of subjectivity. Nor need the psychological be opposed to the collective, except within a framework that assumes that the psychological is ipso facto the individual and therefore in contradistinction to the sociological. It is precisely the ineluctability of this framework that our book questions by intertwining epistemology and ethos—and by insisting that epistemic fear can be simultaneously reasonable, psychological, and collective. This is a book about central territory in the work of science writ large—the historical evolution of collective empiricism.

What Kind of History Is This?

Historians too know epistemic fear, and for the last few generations, their greatest nightmare has been a *Zeitgeist*-haunted intellectual history with ideas leaping, as if magically, from thin air to science, with no mechanisms, no concreteness, no flesh and blood in sight. One response to this ethereal conjuring act has been to narrow the gaze and turn up the magnification: thickly textured case studies and microhistories would stand where ghostly connections had been. Another was to proffer depth-sociological mechanisms that would explain the conduct of scientists not so much through the molecular details of a miniature situation but rather through the external modification of the molar ensemble. *Objectivity* attempts a kind of history that is neither micro-situationist nor macro-externalist, and it is worth saying a few words about what this mesoscopic vision is. But first, a few words about microhistory and case studies, which have played a far more prominent role in recent history of science than the macroscopic alternatives.

In general history, the microhistory has inspired a generation of cultural historians. The trailblazers of this approach—Giovanni Levi, Carlo Ginzburg, Natalie Davis, Robert Darnton, and others—

plumbed a well-chosen and tightly circumscribed historical episode to evoke worlds of cultural-historical specificity; from a different discipline, Clifford Geertz joined them in offering a way of reading history anthropologically, and in turn Geertz joined others in historicizing anthropology. Through the richly detailed and imaginatively narrated accounts of a heterodox sixteenth-century miller or a case of disputed identity in a French village, these scholars discerned alternative universes (other times, other cultures) in a grain of sand.

Science studies also has a long, deep attachment to its own version of the micro-situationist approach, the case study, albeit for somewhat different reasons. Already before World War II, the case study had emerged as a way of instantiating methodological principles so that they would be more easily grasped: case studies in business, psychology, and law, for example, promised to capture general ideas with enough specificity for students. The 1957 *Harvard Case Histories in Experimental Science* were explicitly enlisted in this movement to reform university teaching: studying Robert Boyle or John Dalton would illustrate *the* experimental method in science and thereby illuminate (or so was its goal) the nature of experimentation in general, everywhere and always. James Bryant Conant—chemist, president of Harvard, senior science administrator, and a prime advocate of the case studies approach—was consumed with worry in the late 1940s about atomic physics and nuclear apocalypse. One of his responses to it was to create a curriculum that would teach the nature of science. But if his real worries centered on top-secret contemporary science, he thought the underlying nature of scientific reason could be taught, openly and more simply—using seventeenth-century examples. Science, he believed, was science: case studies could do the job. Although the contrast between the emphases of case studies in science studies and the microhistories of cultural history is stark—cultural and historical variability versus the putatively enduring nature of science—both approaches shared a commitment to the microscale and the single episode, variously interpreted as a parable, exemplum, illustration, or case in point of some larger category (individual identity, experiment) or phenomenon (political revolution, scientific consensus).

Over the last half century, case studies have become more sophisticated, cross-fertilized by sociology, anthropology, philosophy, and history, and historians of science have used them in ways unimagined in the 1950s. There are now case studies of science that show the

difficulty of replication, case studies of gender and science, case studies of large groups in high-energy physics. And the list goes on. The advantages of the case study are many, their virtues by no means exhausted. In the finely woven fabric of case studies and biographical inquiries, unsuspected connections between domains come to light—as in M. Norton Wise and Crosbie Smith’s portrait of Lord Kelvin in *Energy and Empire: A Biographical Study of Lord Kelvin* as the living nexus of religion, telegraphy, geology, and industry in the heyday of the British empire.

Somewhat inconsistently, the case study of science studies has often borrowed, sometimes uneasily, from the cultural-historical strategy of microhistory, playing both sides at once: the case study is putatively culturally specific *and* it aims to tell us about science-in-general. Be that as it may, case studies are indisputably essential to historical, sociological, and philosophical work these days. For though case studies were created at a time when scientific method seemed as simple as confirmation or falsification, the best sharply focused studies have shown again and again how much more complex, interesting, and embedded science can be in the particularities of the world.

Why stop? What’s lacking in case studies and microhistory—something both of us authors of *Objectivity* have been engaged with throughout our careers? Why not, as Porter would prefer, home in on “possible explanatory factors that would be specific to atlases and images” (XXX)? Doesn’t microhistory in some sense exhaust historical research, and in the limit, with enough such studies piled one on top of the other, don’t case studies in their aggregation complete the science studies project of explaining science with no residue?

The question arises because *Objectivity* is not a case, biographical, or microhistorical study, at least not in any usual sense of those words. As Tucker notes, our “analysis overflows the usual boundaries (of discipline, geography, time period) that organize the writing of the history of science” (XXX); in the process, “finer points of the topography disappear; the specificity, disorder, and unpredictability of things on street level often seem remote; and so forth” (XXX). *Objectivity*’s story spreads out over countries, languages, and centuries—and consequently lacks the ultra-tight focus of studies of a particular laboratory or hospital. Instead, it ranges over physics, meteorology, biology, chemistry, astronomy, mathematics, and medicine; it is by no means a circumscribed disciplinary case study. But precisely in the measure that it follows the surface ramification of atlas “runners,” ramifying

into myriad national scientific communities and disciplines, *Objectivity* gets at a regularity that would otherwise be completely hidden. We could, for example, look to the specific reasoning of individual physicists or naturalists or pathologists as they debated, in Geneva or Jena, London or Vienna, whether to use drawings or photographs. But the debate is fundamentally misconstrued if it is thought to be taking place at this local, laboratory-specific level only. It leads up blind alleys to seek a local explanation, or worse a local, causal explanation, for what is, in fact, a global phenomenon.

In making their huge collective atlas of nuclear emulsion images, for example, physicists built on the tradition of medical atlases—not via some abstract *Zeitgeist*, but through specialized, high profile publishers, such as Lehmann or Springer, which ran atlas series. Nor were such atlases the pontificating ruminations of a famous scientist in his dotage—rather, they were collectively assembled, sometimes with dozens of contributors to a single volume. Atlases and allied genres served *in the work* of dissection, classification, discovery—they built on one another, borrowed images, traded image-reproduction techniques. They were, in many different ways, the achievement of the scientific world on a scale larger than the laboratory, larger than the journal, larger even than national scientific societies.

An analogy from general history may help make the point of the advantages of shifting scale. One learns a great deal about, say, the rise of Nazism by microhistories of this or that town in 1930s Germany. But thick descriptions of the actions of the teacher, butcher, lawyer, pastor, and mayor can also occlude the coordinated, nation-wide campaigns of nationalist and anti-semitic broadsheets, of newspapers, radios, films, and other mass media, of a charismatic national leader. Even a huge compendium of hermetically-sealed town studies, each exquisitely detailed, would not obviate the need to grasp the distributed, mass phenomenon of Nazism. Similar problems crop up in characterizing scientific work, at least in recent centuries. The choice of scale should fit the problem; there are larger-scale transformations that are utterly obscured by focusing too small. Not everything is local.

Because microhistories and case studies countered the abstraction and generality of *Zeitgeist* history and the history of ideas, there is a temptation to assume that all history that is not written in the local, circumscribed mode suffers from such nebulousness. Our argument is that *Objectivity*, the bulk of which is devoted to concrete, specific details

of scientific practices, shows that widely disseminated practices need not be nebulous practices. Some of the techniques described, such as those pertaining to image-making, are easily recognized as specific and concrete. Other techniques, intertwined with them, such as those that mold and sustain the scientific self, are less obviously so. But both image-making and self-making are techniques, as concrete as an observation notebook and as specific as the broken arm of a snowflake.

Techniques produced *both* the scientific self and the scientific image. This too is central to this kind of history: a history of technique, where practices shaped both seen and seer. On this view, objectivity and subjectivity entered together—the history of the scientific self is not a separate layer with its own, supposedly more fundamental gear-work, driving the mere phenomenon of scientific objectivity. We argue that the ethos of the scientific self is not a distinct factor that causes alterations at the level of epistemology; ethos *is inseparable* from epistemology. Nor can the scientific self be reduced to an epiphenomenon derived from a base of bureaucracies, factories, or universal psyches.

We realize that this form of explanation may puzzle many readers accustomed to seeking underlying causes, sharing Anderson's judgment that the declared aim of placing explanans and explanandum on the same level "constitutes a moment of opacity in an otherwise lucid book" (XXX). But our central claim is not, we hope, mysterious: namely, that the practices of truth-to-nature, mechanical objectivity, and trained judgment were one and the same as the practices that produced corresponding scientific selves. This is what we mean by deliberately superficial history: no underlying gear-work; no overarching, transhistorical principles, be they sociological or philosophical.

For historians of science at least, patterns of historical discontinuity—scientific revolutions that immolate all that came before them—are at least as ingrained as the stratigraphic metaphors of "underlying" or "bedrock" causes. As Porter writes, "the persistence of antique forms of objectivity is hard to square with the discontinuities on which so much of the book's argument is based" (XXX). Why hard to square? Lots of things start disjunctively and persist—humans, horses, and horseshoe crabs can be found milling around today's East Coast shores. But they have very different points of origin. Analogously, recent botanical atlases with idealized images still circulate widely; we provide examples of contemporary atlases that ardently defend mechanical procedures and unretouched photographs and others that

proudly defend the author-artists' interventions in images not to idealize but to remove artifacts. As in the case of ecosystems, the older forms of sight that endure are modified by new forms. Botanists idealize today to make identification easier and to sharpen distinctions; this is not the ontological claim for the *Urpflanze* made by Goethe two hundred years ago.

So, in the end, what kind of history is *Objectivity*? It is a history that is mesoscopic, superficial, and ethico-epistemic. It is mesoscopic in grappling with scales that reveal the spread of techniques across disciplinary and geographical lines and communities of technique bound together by the use of working objects that could circulate through atlases and related publications even where more detailed strategies stopped at disciplinary or national lines. It is superficial in not searching for hidden gears endowed with the privileged ontological status of being unmoved prime movers; superficial too in treating philosophical frameworks of analysis—like those of Ludwig Wittgenstein, to cite one example from recent science studies—as living among the practices, not as the royalty of science ruling, trans-historically, from above. And ethico-epistemic in fusing epistemic virtues like objectivity with a certain kind of self—and embedding both in a history of specific practices like image-making. It is a history that aims to be relentlessly historical—a simple enough precept, but one that forced us to rethink a great deal of what we had taken for granted about the history of science and the doing of history. We thank the three reviewers for helping us to rethink yet again.

*Max Planck Institute for the History of Science
and Harvard University*

WORKS CITED

Daston, Lorraine, and Peter Galison. *Objectivity*. Brooklyn: Zone Books, 2007.

