

What is a moral economy good for? Quantification (see p. 8): A mock version of Leibniz's "universal characteristic" was allegedly used by the Academy of Laputa to "write Books in Philosophy, Politicks, Law, Mathematicks, or Theology, without the least Assistance from Genius or Study"—the cranks were turned to create innumerable sentences. From Jonathan Swift, Gulliver's Travels, Part 3: A Voyage to Laputa (London, 1726).

The Moral Economy of Science

By Lorraine Daston*

I. WHAT IS A MORAL ECONOMY?

The mind lives on the heart
Like any parasite.
—Emily Dickinson, “The Mind Lives
on the Heart” (c. 1876)

WE ARE HEIRS to an ancient tradition that opposes the life of the mind to the life of the heart, and to a more recent one that opposes facts to values. Because science in our culture has come to exemplify rationality and facticity, to suggest that science depends in essential ways upon highly specific constellations of emotions and values has the air of proposing a paradox. Emotions may fuel scientific work by supplying motivation, values may infiltrate scientific products as ideology or sustain them as institutionalized norms, but neither emotions nor values intrude upon the core of science—such are the boundaries that these habitual oppositions would seem to dictate. The ideal of scientific objectivity, as currently avowed, insists upon the existence and impenetrability of these boundaries. I will nonetheless claim that not only does science have what I will call a moral economy (indeed, several); these moral economies are moreover constitutive of those features conventionally (and, to my mind, correctly) deemed most characteristic of science as a way of knowing. Put more sharply and specifically: certain forms of empiricism, quantification, and objectivity itself are not simply compatible with moral economies; they require moral economies.¹

What exactly is a moral economy? Although several recent studies in the history of science testify to the existence and significance of moral economies, such studies have yet to crystallize around a common rubric, much less to rally around a common standard.² Part of my work here will be to extrapolate implications and tendencies

* Department of History, University of Chicago, 1126 E. 59th Street, Chicago, Illinois 60637.

¹ I am grateful to John Carson and Nathan Reingold for pointing out that my use of the term *moral economy* diverges significantly from E. P. Thompson's in “The Moral Economy of the English Crowd in the Eighteenth Century,” *Past and Present*, Feb. 1971, No. 50, pp. 76–136, reprinted in Thompson, *Customs in Common: Studies in Traditional Popular Culture* (New York: New Press, 1991), pp. 185–258, along with replies to critics and later reflections, “The Moral Economy Reviewed,” pp. 259–351. My appeal here to “economies” of affects and values has little to do with Thompson's accounts of corn markets and the tradition of “setting the price” by persuasion or riot, although it does appeal to a broader sense of “legitimizing notion.”

² See, e.g. (this list is by no means exhaustive), Owen Hannaway, “Laboratory Design and the Aim of Science: Andreas Libavius versus Tycho Brahe,” *Isis*, 1986, 77:585–610; Theodore M. Porter, “Objectivity as Standardization: The Rhetoric of Impersonality in Measurement, Statistics, and Cost-Benefit Analysis,” *Annals of Scholarship*, 1992, 9:19–60; Porter, “Quantification and the Accounting

that seem to me to unite these scattered studies, and to clarify their contributions to a nascent investigation of moral economies in science.³ What I mean by a moral economy is a web of affect-saturated values that stand and function in well-defined relationship to one another. In this usage, “moral” carries its full complement of eighteenth- and nineteenth-century resonances: it refers at once to the psychological and to the normative. As Gaston Bachelard decades ago remarked, to imbue objects or actions with emotion is almost always thereby to valorize them, and vice versa.⁴ Here *economy* also has a deliberately old-fashioned ring: it refers not to money, markets, labor, production, and distribution of material resources, but rather to an organized system that displays certain regularities, regularities that are explicable but not always predictable in their details. A moral economy is a balanced system of emotional forces, with equilibrium points and constraints. Although it is a contingent, malleable thing of no necessity, a moral economy has a certain logic to its composition and operations. Not all conceivable combinations of affects and values are in fact possible. Much of the stability and integrity of a moral economy derives from its ties to activities, such as precision measurement or collaborative empiricism, which anchor and entrench but do not determine it.

It may help to etch the outlines of the notion of moral economy more crisply to point out what it is *not*. It is not a matter of individual psychology. Whatever and however vehement their other confessional differences, historians, sociologists, and philosophers of science share a certain horror of the psychological, properly so-called, and I confess I am no exception to this general hostility. The historians glare in distrust at the purported eternal verities of the mind, just because they are alleged to be eternal; the sociologists, recalling the warnings of Auguste Comte and Emile Durkheim, bare their teeth at the isolated individualism of much current psychology, including that labeled “social psychology”; the philosophers, post-Frege, take the word “psychological” into their mouths only as an epithet, as *ipso facto* proof that the problem or explanation at hand has nothing to do with genuine philosophy.⁵ Although moral economies are about mental states, these are the mental states of

Ideal in Science,” *Social Studies of Science*, 1992, 22:633–652; Simon Schaffer, “Astronomers Mark Time: Discipline and the Personal Equation,” *Science in Context*, 1988, 2:115–145; Schaffer, “A Manufactory of Ohms: The Integrity of Victorian Values,” in *Invisible Connections: Instruments, Institutions, and Science*, ed. Robert Bud and Susan Cozzens (Bellingham, Wash.: SPIE Press, 1992), pp. 23–56; Steven Shapin, “The House of Experiment in Seventeenth-Century England,” *Isis*, 1988, 79:373–404; Shapin, “‘The Mind in Its Own Place’: Science and Solitude in Seventeenth-Century England,” *Sci. Context*, 1991, 4:191–217; Lorraine Daston and Peter Galison, “The Image of Objectivity,” *Representations*, 1992, 40:81–128; and Daston, “Objectivity and the Escape from Perspective,” *Soc. Stud. Sci.*, 1992, 22:597–618. I am grateful to authors who made republication versions of this recent work available to me when I was preparing this essay.

³ I undertake this task with considerable diffidence, given the obvious risks of misinterpretation and misappropriation. The studies I shall treat here have served as inspiration for my analysis, but their authors are wholly innocent of any responsibility for that analysis.

⁴ See, e.g., Gaston Bachelard, “Libido et connaissance objective,” *La Formation de l’esprit scientifique* (1938), 14th ed. (Paris: Vrin, 1989), pp. 183–209.

⁵ Among the notable exceptions to this general disdain of the psychological within the history of science is Ryan D. Tweney, “Faraday’s Discovery of Induction: A Cognitive Approach,” in *Faraday Rediscovered: Essays on the Life and Work of Michael Faraday, 1791–1867*, ed. David Gooding and Frank James (London: Macmillan, 1985), pp. 189–209. The recent surge of interest among philosophers of science in approaches imported from cognitive science does not contradict their general dislike of psychology. Philosophers were repelled earlier not only because psychology smacked of the “irrational,” but also because the psychological perspective seemed to them ooily invertebrate,

collectives, in this case collectives of scientists, not of lone individuals. To extend Ludwik Fleck's terminology, what is meant here is a *Gefühls-* as well as a *Denkkollektiv*.⁶ Apprenticeship into a science schools the neophyte into ways of feeling as well as into ways of seeing, manipulating, and understanding. This is a psychology at the level of whole cultures, or at least subcultures, one that takes root within and is shaped by quite particular historical circumstances.⁷ I hope that the collectivity and particularity of mental economies will go some way in assuaging the suspicions of the sociologists and historians, respectively; I shall return to the worries of the philosophers in the conclusion.

Nor is a moral economy confined to the level of motivation, whether in spurring individuals into scientific careers or in persuading society that science is worthy of encouragement and support. Since motivations of both sorts have been one of the principal loci for the discussion of values in science studies, it may be tempting to assimilate moral economies to them. However, this temptation should be resisted. The classical studies of how values, predominantly religious values, motivate both individuals and societies to pursue science grant such values at best a neutral and at worst a negative role with respect to the forms and content of scientific methods and assertions. Robert Merton's pioneering study *Science, Technology and Society in Seventeenth-Century England*, as well as his subsequent work in the sociology of science, advances the neutral alternative: the fervent desire of a seventeenth-century Englishman to glorify God through the investigation of His works might steer him toward a career in natural philosophy; the equally fervent desire of his twentieth-century counterpart to win the good opinion of a select circle of peers might propel him toward a scientific career. At the macrosocial level, utilitarianism, piety, or other cultural values may bolster the prestige and funding of science and even elevate some kinds of science above others. But none of these values impinges on scientific ways of knowing. As Steven Shapin points out in a lucid recent essay on the reception of the Merton thesis: "For Merton, the explanandum was emphatically not scientific method or scientific knowledge: it was the dynamics and social standing of a scientific enterprise that was itself conceived of as a black box."⁸ Moral economies belong to the interior of Merton's black box. The outstanding example of the negative alternative also treats religion and science in seventeenth-century England, and it provides what are still some of the most exquisitely sensitive readings of how the Christian virtuoso's frame of mind and soul inclined him toward natural philosophy. R. S. Westfall was concerned, however, not only with the shading of religious

lacking all coherent structure. Although cognitive science has done little to rehabilitate the mind's rationality, it offers structures aplenty.

⁶ Fleck in fact emphasized that emotions as well as concepts were shared by members of scientific thought collectives, and suggested that it was just this unanimity of feeling that created the illusion of freedom from emotions: Ludwik Fleck, *Genesis and Development of a Scientific Fact* (1935); ed. Thaddeus Trenn and Robert K. Merton; trans. Fred Bradley and Thaddeus Trenn (Chicago/London: Univ. Chicago Press, 1979), p. 49.

⁷ I have in mind a gradual shaping of a collective personality akin (and, as will be seen below, sometimes identical) to Norbert Elias's "civilizing process": see Elias, "Synopsis: Towards a Theory of Civilizing Processes," in *Power and Civility*, Vol. II of *The Civilizing Process* (1939), trans. Edmund Jephcott (New York: Pantheon, 1982), pp. 229–333.

⁸ Steven Shapin, "Understanding the Merton Thesis," *Isis*, 1988, 79:594–605, on p. 595. Merton's work was originally published in *Osiris*, 1938, 4:360–632; rpt. (New York: Harper Torchbooks, 1970).

reverence into scientific dedication, but also with the interaction of theological with natural philosophical doctrines. When these doctrines clashed, as Westfall believed they did on the topics of miracles and providences, the only role that values could play was to veil contradictions and foment inconsistencies.⁹ Values could mix with scientific knowledge, but only as a contaminant. Moral economies, in contrast, are integral to science: to its sources of inspiration, its choice of subject matter and procedures, its sifting of evidence, and its standards of explanation.

Much the same might be said apropos of the relationship between moral economies and ideology in science. This is the other classical locus of how and why values enter science, this time opening the black box of scientific assumptions and assertions, and treating it very much as Pandora's box.¹⁰ Whereas moral economies moralize scientists, ideologies moralize nature in the service of social interests. The numerous case studies in this genre run the gamut from piecemeal attempts to unmask this or that scientific claim as a piece of political interest tricked out as neutral fact, to more systematic exposés of all of science as a "social construction," laboriously if clandestinely built up out of interests, resources, and negotiations. Because it in principle encompasses all science, not just this or that ideologically tainted claim, the social-constructionist program comes closest to acknowledging the integral role of values in scientific work and its products: values do not distort science; they are science. This is why the annals of this program supply some of the most intriguing insights for the study of the moral economy of science.

However, because social constructionism focuses primarily on interests, be they political, social, or economic, and on (hidden) labor, it retains some of the muckraking character of more conventional revelations about ideology in science.¹¹ In contrast, to examine a moral economy of science may render familiar scientific procedures such as quantification strange, but seldom devious. Insofar as the study of moral economies in science is about power, it is power of the microscopic, internalized Foucauldian sort, rather than of the political (or martial), externalized kind.¹² In other words, the moral economy of science is more about self-discipline than coercion. Moreover, because social constructionists generally understand values as veiled interests, they are seldom concerned to explore the links between values and affects, unless these affects have an overtly societal character or influence. The stressed "social" in the social constructionist program refers not only to the dis-

⁹ Richard S. Westfall, *Science and Religion in Seventeenth-Century England* (New Haven: Yale Univ. Press, 1958), pp. 90 et passim; for the sensitive readings see, e.g., pp. 27–28.

¹⁰ Perhaps the most challenging of the current wave of ideology-and-science studies are those which address scientific accounts of gender: see, e.g., G.E.R. Lloyd, *Science, Folklore, and Ideology* (Cambridge: Cambridge Univ. Press, 1983); Ludmilla Jordanova, *Sexual Visions: Images of Gender in Science and Medicine between the Eighteenth and Twentieth Centuries* (Madison: Univ. Wisconsin Press, 1989); Londa Schiebinger, *The Mind Has No Sex? Women in the Origins of Modern Science* (Cambridge, Mass.: Harvard Univ. Press, 1989); and Cynthia Eagle Russett, *Sexual Science: The Victorian Construction of Womanhood* (Cambridge, Mass.: Harvard Univ. Press, 1989). [See also the articles by Evelyn Fox Keller (especially) and Sally Gregory Kohlstedt in this volume, with the relevant citations.—Eds.]

¹¹ For a more sanguine view of how ideology-laden or socially constructed science can sometimes count as an intellectual achievement rather than as a distortion, see M. Norton Wise, "Mediating Machines," *Sci. Context*, 1988, 2:77–113.

¹² See Michel Foucault, *Discipline and Punish: The Birth of the Prison* (1975), trans. A. Sheridan (New York: Pantheon, 1977); cf. Elias, "Civilizing Processes" (cit. n. 7), pp. 240–242. For the coercive, indeed bellicose view of power in science see Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge, Mass.: Harvard Univ. Press, 1987).

guised social components of which science is purportedly assembled, but also to the social uses to which science is put. Traffic flows in both directions across the science-society divide. Moral economies, however, tend to be one-way affairs. Although moral economies in science draw routinely and liberally upon the values and affects of ambient culture, the reworking that results usually becomes the peculiar property of scientists. Traces of the original cultural models—for example, the simplicity, dedication, and humility of Christian saints or the unworldly innocence of the pastoral idyll—lie ready to hand, and can be evoked by the spokesmen of science to win public approval and support. But the ultimate forms that moral economies assume within science, and the functions that they serve, are science's own.¹³

Finally, moral economies are not Mertonian norms, although here again there is a certain fleeting resemblance. Merton defines the “ethos of science” as “that affectively toned complex of values and norms which is held to be binding on the man of science. . . . These imperatives, transmitted by precept and example and reenforced by sanctions, are in varying degrees internalized by the scientist, thus fashioning his scientific conscience or, if one prefers the latter-day phrase, his super-ego.”¹⁴ However, the well-known norms of universalism, communism, disinterestedness, and organized skepticism, although “procedurally efficient,” represent for Merton “one limited aspect of science as an institution,” as carefully cordoned off from the “characteristic methods” and the “stock of accumulated knowledge” of science as motivations had been in his historical work. Moreover, these norms were, once established, immune to the vagaries of history and the pressures of context, for they were ultimately enforced not by human conscience but by nature. Scientists might violate the norms of universalism or communism, but only at their peril, for they were underwritten not simply by human sanctions but also by uniform, inexorable natural laws. Despite this alleged metaphysical grounding, a handful of scientific exposés, followed by a generation's worth of contextual studies in the history of science, apparently presented empirical refutation of Merton's norms, for here was candid testimony that violations could produce science of the first magnitude.¹⁵ In contrast to Mertonian norms, moral economies are historically created, modified, and destroyed; enforced by culture rather than nature and therefore both mutable and violable; and integral to scientific ways of knowing.

To define an entity either directly or by contradistinction as I have tried to do above offers little proof that such entities exist, much less of their significance. Do

¹³ On the evocation of these models in French academic éloges see Dorinda Outram, “The Language of Natural Power: The Eloges of Georges Cuvier and the Public Language of Nineteenth-Century Science,” *History of Science*, 1978, 16: 153–178; see also Suzanne Delorme, “La vie scientifique à l'époque de Fontenelle après les éloges des savants,” *Archeion*, 1937, 19: 217–235; and Charles B. Paul, *Science and Immortality: The Eloges of the Paris Academy of Sciences (1699–1791)* (Berkeley: Univ. California Press, 1980), esp. pp. 90–94.

¹⁴ Robert K. Merton, “The Normative Structure of Science,” rpt. in *The Sociology of Science: Theoretical and Empirical Investigations*, ed. Norman Storer (Chicago/London: Univ. Chicago Press, 1973), pp. 267–278, on pp. 268–269. Originally published as “Science and Technology in a Democratic Order,” *Journal of Legal and Political Sociology*, 1942, 1: 115–126.

¹⁵ The most spectacular of these exposés was James D. Watson, *The Double Helix: A Personal Account of the Discovery of the Structure of DNA* (New York: New American Library, 1968); for reactions see Watson, *The Double Helix*, including text, commentary, reviews, original papers, ed. Gunther Stent (New York: W. W. Norton, 1980), pp. 161–234. See also Nico Stehr, “The Ethos of Science Revisited: Social and Cognitive Norms,” in *The Sociology of Science: Problems, Approaches, and Research*; ed. Jerry Gaston (San Francisco: Jossey-Bass, 1978), pp. 172–196.

moral economies of science really exist, and if so, what are they good for? These are challenges that can be met only by instantiation, not definition. In the next section I examine three examples of how moral economies have structured key aspects of how scientists come to know: quantification, empiricism, and objectivity.

II. WHAT IS A MORAL ECONOMY GOOD FOR? QUANTIFICATION

L'excès de précision dans le règne de la quantité, correspond très exactement à l'excès du pittoresque dans le règne de la qualité.
—Gaston Bachelard, *La formation de l'esprit scientifique* (1938)

Quantification is a portmanteau term that holds a multitude of meanings. It is part of our number fetishism that we seldom distinguish among them. Historians of science routinely use it to refer to abstract mathematical models that may or may not be tethered to measurements or even observations (e.g., Nicole Oresme's doctrine of the latitude of forms, or Jakob Bernoulli's probabilities of legal evidence); measurements that may or may not connect to a mathematical model of the phenomena under scrutiny (e.g., the physiological researches of Stephen Hales); straightforward counting (e.g., almost all of descriptive statistics); estimates grounded neither in measurement nor theory (e.g., many of William Petty's figures in his political arithmetic); methods of data representation and analysis (e.g., graphs and tables or the method of least squares); and the creation of new entities (e.g., index numbers such as the gross national product). The common denominator (so to speak) of all of these usages is not even numbers, for many historical instances of quantification in the sciences have been purely geometrical: when Galileo claimed in *Il Saggiatore* (1623) that the book of nature was written in the characters of "triangles, circles, and other geometric figures," he probably meant it quite narrowly.

Amidst this plurality of forms that scientific quantification has assumed, only some have aspired to accuracy, that is, to a close fit between mathematics and a select set of phenomena, although this is the virtue most heeded and praised by historians. Other mathematical virtues touted by quantifiers of various stripes have included precision, communicability, and impartiality, all of which can be cleanly detached from accuracy. For example, when in 1699 the English mathematician John Craig calculated the date of the millennium (A.D. 3150, when the credibility of the New Testament decays completely) on the basis of assumptions about the probability of human testimony, or when G. W. Leibniz proclaimed (with breathtaking optimism) that it would take a team of scholars less than five years to construct a Universal Characteristic by matching numbers to ideas and arithmetic operations to thought processes, they aimed primarily at precise knowledge, and only secondarily at accurate knowledge.¹⁶ Accuracy concerns the fit of numbers or geometrical magnitudes to some part of the world and presupposes that a mathematical model can be anchored in measurement; precision concerns the clarity, distinctness, and intelligibility of concepts, and, by itself, stipulates nothing about whether and how those concepts match the world. Although striving for precision as a goal in and of itself is distinctive of much early modern quantification, in part because of a largely psycho-

¹⁶ John Craig, *Theologiae christianae principia mathematica* (London, 1699); and Gottfried Wilhelm Leibniz, "Towards a Universal Characteristic" (1677), in *Leibniz Selections*, ed. Philip Wiener (New York: Scribners, 1951), pp. 22–23.

logical account of the grounds for mathematical certainty, it is by no means extinct among latter-day quantifiers.¹⁷

The cults of communicability and impartiality—again, with or without accuracy—also have an almost unbroken history in the sciences as well as in public life from the seventeenth century to the present. These quantifying virtues have often worked in tandem, usually to the end of damping controversy and compelling consensus. Even when neither measurements nor statistics were available, quantifiers of, say, the productivity of Holland or of the efficacy of smallpox inoculation pleaded for the superior clarity and communicability of numbers, favorably contrasted to “only comparative and superlative Words, and intellectual Arguments.”¹⁸ Leibniz contended that lack of clarity was at the root of almost all controversy and could therefore be cured by a goodly dose of numbers: “We need not be surprised then that most disputes arise from the lack of clarity in things, that is, from the failure to reduce them to numbers.”¹⁹ Although these attempts to silence dissent through quantification were (and still are) occasionally parasitic upon the vaunted certainty of mathematical demonstrations and operations, their dominant appeal was to consensus achieved through communication and thereby shared understanding, rather than through the necessity of demonstration. Even when the truth of the matter was not to be had, numbers could be invented, dispersed to correspondents at home and abroad, and, above all, mentally shared: you and I may disagree about the accuracy and the implications of a set of numbers, but we understand the same thing by them.

The moral economy of this form of quantification is sociable but intolerant of deviations, and it is not surprising that it flourishes under conditions of weak or confused authority—for example, the contested intellectual authority of sixteenth- and seventeenth-century natural philosophy, or, as Theodore Porter has recently argued, the contested political authority of twentieth-century pluralistic democracies.²⁰ In both cases the aim of quantification is not to secure individual conviction, but rather to secure the acquiescence of a diverse and scattered constituency. That is, the scientific polity that cherishes quantification is not only a collective, but also one whose members may differ from one another in nationality, skill, training, assumptions, or material resources such as laboratory equipment or statistical bureaus. It is quite possible to imagine, and to instantiate historically, scientific ideals and practices that preferred the solitary sage to the collective, or a more local and homogeneous collective that need not resort to the minimalist, information-losing techniques of quantification in order to communicate and persuade. For quantification, no matter how thorough and detailed, is necessarily a sieve: if it did not filter out local knowledge such as individual skill and experience, and local conditions such as this brand of instrument or that degree of humidity, it would lose its portability.²¹ The moral commitment to a certain form of sociability among colleagues who may

¹⁷ See, e.g., René Thom, “Mathématique et théorisation scientifique,” in *Penser les mathématiques*, ed. François Guénard and Gilbert Lelièvre (Paris: Editions de Seuil, 1982), pp. 252–273.

¹⁸ William Petty, *Political Arithmetick* (London, 1690), preface.

¹⁹ Leibniz, “Universal Characteristic” (cit. n. 16), p. 24; cf. his plans for a language with “no equivocations or amphibolies,” “Preface to the General Science” (1677), in *Leibniz Selections*, ed. Wiener (cit. n. 16), p. 16.

²⁰ Porter, “Objectivity as Standardization” (cit. n. 2).

²¹ *Ibid.* The same is true, *mutatis mutandis*, of the conditions for replicating empirical results: on the discord that ensues when aspects of local knowledge (e.g., a certain kind of glass prism) are not

never meet face to face must be strong in order to countenance the loss of so much hard-won detail.²² It is in part the systematic erasure of these details in the service of extended sociability that creates the impression of the uniformity of nature: to turn Merton on his head, the uniformity of nature presupposes universalism among scientists, rather than the reverse.

Among the preconditions for this far-flung sociability are the oft-remarked impartiality and impersonality of quantified results and procedures. These qualities may flourish even in the absence of accuracy, and are indeed all the more highly valued when accuracy seems unattainable. Impartiality is first and foremost a judicial rather than a scientific virtue, and at most a prerequisite for rather than a guarantee of the truth of a verdict. Similarly, there is no a priori reason to believe that the elimination of all that is idiosyncratic will clear a path to the “really real”: if the idiosyncrasy in question is skill, one might expect just the opposite. The point here is that impersonality and impartiality are cultivated by quantifiers as much for moral as for functional reasons. It is proverbial that both require dutiful self-abnegation so as to repress individuality and interest, and neither accrues automatically to quantified procedures and results. “Faceless numbers” fairly radiate personality in the hands of numerologists and cabalists; the chicaneries practiced with statistics are all too familiar. Abstraction alone never eliminates all traces of individuality and interest, and the history of applied mathematics, particularly social mathematics, is strewn with examples of partial impartiality.²³ Impersonality and impartiality in quantification might be better conceived as a continuum, more or less achieved by an effort of self-imposed restraint, rather than as properties inherent in the numbers themselves. To practice the form of quantification that breaches the boundaries of language, confession, nationality, and theoretical allegiance demands that the quantifiers voluntarily restrict their sphere of discretion. They must also sacrifice some of the meanings attached to numbers and techniques: Johannes Kepler’s successors stripped his “laws” of their Pythagorean halo; Adolphe Quetelet’s successors jettisoned his normative understanding of the normal curve. In other words, the choice of an extended form of scientific sociability incurs certain forms of moral obligation and discipline: the reining in of judgment, the submission to rules, the reduction of meanings—what Bachelard once called “that asceticism that is abstract thought.”²⁴

omitted see Simon Schaffer, “Glass Works: Newton’s Prism and the Uses of Experiment,” in *The Uses of Experiment: Studies in the Natural Sciences*, ed. David Gooding, Trevor Pinch, and Simon Schaffer (Cambridge: Cambridge Univ. Press, 1989), pp. 67–104. On the ideal of the solitary intellectual see Steven Shapin, “Mind in Its Own Place” (cit. n. 2); see also Martin Warnke, “Das Bild des Gelehrten im 17. Jahrhundert,” in *Res publica litteraria: Die Institutionen der Gelehrsamkeit in der frühen Neuzeit*, ed. Sebastian Neumeister and Conrad Wiedemann (Wolfenbüttler Arbeiten zur Barockforschung, 14) (Wiesbaden: Otto Harrasowitz, 1987), Part I, pp. 1–34. For one collective that valued details over replicability or communicability see Lorraine Daston, “The Cold Light of Facts and the Facts of Cold Light: Luminescence and the Transformation of the Scientific Fact, 1600–1750,” *Early Modern France*, in press; see also Steven Shapin, “Robert Boyle and Mathematics: Reality, Representation, and Experimental Practice,” *Sci. Context*, 1988, 2:23–58.

²² On the origins of this form of sociability among European intellectuals see Lorraine Daston, “The Ideal and Reality of the Republic of Letters in the Enlightenment,” *Sci. Context*, 1991, 4: 367–386.

²³ See, e.g., Donald A. Mackenzie, *Statistics in Britain, 1865–1930: The Social Construction of Scientific Knowledge* (Edinburgh: Edinburgh Univ. Press, 1981).

²⁴ Gaston Bachelard, “Les obstacles de la connaissance quantitative,” *La formation de l’esprit scientifique* (cit. n. 4), pp. 211–238, on p. 237.

The affinities and arguably the origins of this ethos are bureaucratic, appealing to the rigid rationality of rules, conscientiously blind to variations of person or situation.²⁵ This is one moral economy of the several varieties of quantification.

When concerns for precision and accuracy combine in the enterprise of precision measurement, the moral economy takes another form. Whereas the quantification of precision alone aims at impersonality in the service of a collectivity, the quantification of precision measurement aims at integrity, sometimes in defiance of the collectivity. The more precise the measurement, the more it stands as a solitary achievement of the measurer, rather than as the replicable common property of the group. Not all scientific measurement aspires to precision: Robert Hooke, for example, recommended mathematics to the natural philosopher because it “accustoms the Mind to a more strict way of Reasoning, to a more nice and exact way of examining, and to a much more accurate way of inquiring into the Nature of things.” But he did not require “Mathematical Exactness” of his measurements, “for we find that Nature it self does not so exactly determine its operations, but allows a Latitude almost to all its Workings, though . . . it seems to be restrain’d within certain Limits.” The belief in the sharp-edged determinacy of nature grew slowly, and the scientific cult of precision measurement, with its rites of instrument making and error analysis, emerged only in the nineteenth century.²⁶

With precision measurement emerged a quite different moral economy of quantification, one just as stern in its call for self-discipline, but self-discipline channeled to different ends. This is the self-discipline of caution and fastidious attention to detail, the painstaking prudence of the account ledger. In her fine recent study of Franz Neumann’s physics seminar (established 1834) at Königsberg, Kathryn Olesko shows how the “ethos of exactitude” evolved in German astronomy, geodesy, and experimental physics, and how it was inculcated by the practices, particularly that of error analysis, taught in Neumann’s seminar. The initiates of Königsberg scrupled to graph their measurements, for they distrusted the unobserved interpolated values. They warily sifted the results of colleagues, according to the known diligence and care of the experimenter. They balked at theoretical generalizations, unpersuaded that the data had been sufficiently purged of errors. In contrast to the moral economy of precision *tout court*, that of precision measurement cultivated certain personal idiosyncrasies, namely those of skill and, especially, the character traits of diligence, fastidiousness, thoroughness, and caution. Nor did scientific sociability figure prominently in their creed. Although the devotees of precision measurement never meant to withdraw from the scientific community, the rigor of their faith effectively isolated them even from other experimentalists, not to mention theorists, for all measurements were in principle subject to revision, correction,

²⁵ Gerd Gigerenzer *et al.*, *The Empire of Chance: How Probability Changed Science and Everyday Life* (Cambridge: Cambridge Univ. Press, 1989), pp. 236–237; see also Porter, “Objectivity as Standardization” (cit. n. 2).

²⁶ Robert Hooke, “A General Scheme of the Present State of Natural Philosophy, and How its Defects may be Remedied By a Methodical Proceeding in the making Experiments and collecting Observations,” in *The Posthumous Works of Robert Hooke* (1705), ed. Richard Waller, with an introduction by Richard S. Westfall (New York/London: Johnson Reprint, 1969), pp. 19, 38. On the instrumental preconditions for, and relative indifference to, precision measurement in eighteenth-century science see Maurice Daumas, “Precision of Measurement and Physical and Chemical Research in the Eighteenth Century,” in *Scientific Change*, ed. A. C. Crombie (London: Heinemann, 1963), pp. 418–430.

improvement. To pursue the “duty” of perfecting precision led to the perceived incommensurability of experimental results.²⁷

This is perhaps a pathological expression of the moral economy of precision measurement, but like so many pathologies, simply an exaggeration of the same values and affects that sustained precision measurement under more normal conditions. Olesko correctly identifies integrity as the cardinal virtue of precision measurement, simultaneously applied to the character of the measurers and to the quality of the measurements.²⁸ Yet paradoxically integrity sometimes teetered on the edge of disintegration: the disintegration of a smooth curve into discrete data points, the disintegration of a set of apparently uniform measurements, the disintegration of the bonds between experiment and theory, the disintegration of the scientific collectivity.

III. WHAT IS A MORAL ECONOMY GOOD FOR? EMPIRICISM

“Next vnto *Arui* there are two riuers *Atoica* and *Caora*, and on that braunch which is called *Caora* are a nation of people, whose heades appeare not aboue their shoulders, which though it may be thought a meere fable, yet for mine owne parte I am resolued it is true, because euery child in the prouinces of *Arromaia* and *Canuri* affirme the same.”

—Sir Walter Raleigh, *The Discoverie of the large and bewtiful Empire of Guiana* (1596)

“As it happened to a Dutch ambassador, who entertaining the king of Siam with the particularities of Holland, which he was inquisitive after, amongst other things told him, that the water in his country would sometimes, in cold weather, be so hard, that men walked upon it, and that it would bear an elephant, if he were there. To which the King replied, *Hitherto I have believed the strange things you have told me, because I took you for a sober fair man, but now I am sure you lie.*”

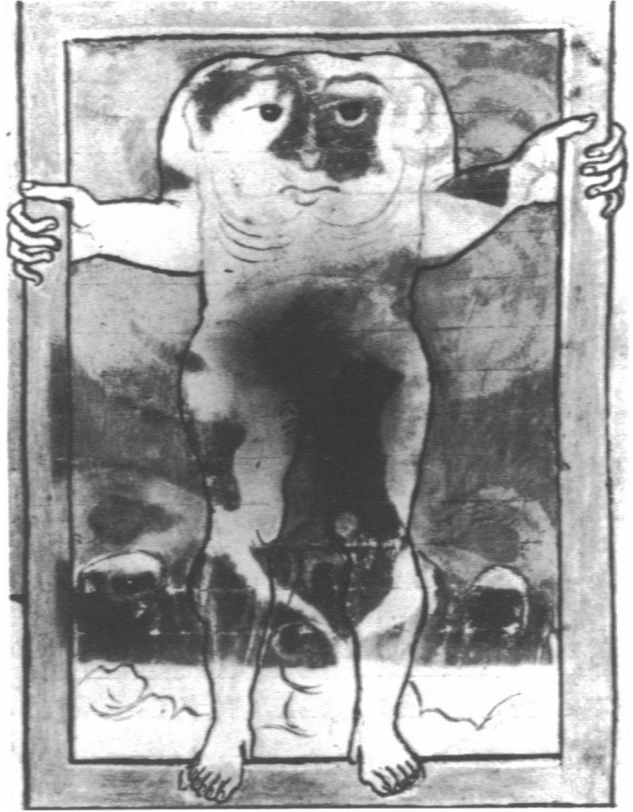
—John Locke, *Essay Concerning Human Understanding* (1690), Book IV, Ch. 15, Sect. 5.

Empiricism is at least as multifarious as quantification, and correspondingly fertile in moral economies. Here I shall restrict myself to three distinctive aspects of the empiricism of seventeenth-century natural philosophy: testimony, facticity, and novelty. Each relied crucially upon intertwined values and affects: testimony upon trust, selectively extended; facticity upon academic civility; novelty upon the rehabilitation and transformation of curiosity. Critical for all three was the emergence of a new understanding of experience in natural philosophy in the middle decades of the seventeenth century.

Aristotelian natural philosophy aspired to causal knowledge, formulated in demonstrations about universals. It is history that deals with particulars, and this is why history is inferior not only to philosophy but also to poetry. As Aristotle explains in

²⁷ Kathryn M. Olesko, *Physics as a Calling: Discipline and Practice in the Königsberg Seminar for Physics* (Ithaca, N.Y./London: Cornell Univ. Press, 1991), pp. 250–252, 287, 392–393, 378–386. On precision measurement as a matter of character see Schaffer, “Astronomers Mark Time” (cit. n. 2). On the constraints practical and economic considerations could place upon the atomizing tendencies of precision measurement see Crosbie Smith and M. Norton Wise, *Energy and Empire: A Biographical Study of Lord Kelvin* (Cambridge: Cambridge Univ. Press, 1989), pp. 684–722.

²⁸ On the integrity of values, in both senses of both words, see Schaffer, “Manufactory of Ohms” (cit. n. 2).



Empiricism: This blemmye from a medieval manuscript on the monstrous races of the East closely resembles Sir Walter Raleigh's secondhand description of the people of Caora. From MS Cotton Tiberius B.v., folio 82a; courtesy of the British Library, London.

the introduction to *On the Parts of Animals*, particulars occupy the philosopher only as stepping stones to generalizations and the discovery of causes. It is a happy fact that our minds are so constructed as to synthesize universals out of particulars: "Sense perception must be concerned with particulars, whereas knowledge depends on the recognition of the universal."²⁹ This does not imply that Aristotle's natural philosophy was not empirical, for his natural philosophical treatises reveal him to have been a sharp-eyed and indefatigable observer of an astonishing variety of phenomena. However, Aristotle's experience was common experience, "that which is always or that which is for the most part."³⁰ Medieval scholasticism was too long-lived and varied a set of doctrines to admit of any monolithic generalization concerning the meaning of experience in natural philosophy; it can nevertheless be cautiously asserted that most observations cited were indeed about what happened always or for the most part, and that these universals of experience served as the axioms for scholastic demonstrations.

Peter Dear has argued that in the course of the seventeenth century natural philosophers gradually abandoned universals in favor of particulars as the primary form of

²⁹ Aristotle, *Posterior Analytics*, 100a10–15, 87b37–39, translation from *The Complete Works of Aristotle*, ed. Jonathan Barnes, 2 vols. (Princeton: Princeton Univ. Press, 1984). See also *Poetics*, 1451b1–7; and *On the Parts of Animals*, 639a13–640a10.

³⁰ Aristotle, *Metaphysics*, 1027a20–27.

scientific experience.³¹ Specific events, described in first-person singular historical narratives, with details of who saw what when and where, replaced universal generalizations. These particulars were increasingly published as short, semiepistolary articles in fledgling scientific journals such as the *Journal des Savants*, *Philosophical Transactions of the Royal Society of London*, the *Histoires et Mémoires de l'Académie des Sciences de Paris*, and the *Acta Eruditorum* rather than as long, systematic treatises. Just because natural philosophical experience had been transformed into discrete, specific events, previously deemed the stuff of history, their credibility was to be assessed by the traditional historical (and legal) means: that is, by a cloud of witnesses, each testimony carefully weighted by degree of credibility according to familiar criteria, including sex, age, character, and social standing.³²

The new-style natural philosophical experience had at least one striking disadvantage vis-à-vis the old-fashioned scholastic sort: whereas universals and commonplaces are by definition accessible to all, specific events, particularly those produced by experiment with finicky, expensive equipment, were not. As the large medieval and early modern literature on “secrets” testifies, some kinds of knowledge about nature, particularly alchemy and artisanal techniques, had been at least partially shrouded from public view.³³ But as Owen Hannaway and Steven Shapin show, this retreat to the privacy of the monastic cell or the alchemist’s den clashed with the reformed natural philosophy’s pointed critique of both forms of intellectual aloofness.³⁴ For reasons we still do not fully understand, seventeenth-century natural philosophers envisioned themselves as members of an international collective (“the ingenious of Europe,” as their title pages had it), and empiricism as a collaborative enterprise. In order to honor simultaneously the ideals of particulate experience and publicity, natural philosophers had necessary recourse to witnessing, both actual and “virtual,” and to testimony.³⁵

Yet not all testimony was trustworthy: how to assess both the credibility of the witness and the plausibility of the event became a central epistemological problem in the latter half of the seventeenth century.³⁶ As the dilemmas of Sir Walter Raleigh and John Locke’s king of Siam show, it was all too easy to err on the side of either excessive credulity or excessive skepticism. The dilemma was sharpened by the dis-

³¹ Peter Dear, “*Totius in verba*: Rhetoric and Authority in the Early Royal Society,” *Isis*, 1985, 76: 145–161; and Dear, “Jesuit Mathematical Science and the Reconstitution of Experience in the Early Seventeenth Century,” *Studies in History and Philosophy of Science*, 1987, 18:133–175.

³² See, e.g., Francis Bacon’s lawyerly instructions for sifting evidence for a “Natural and Experimental History”: Bacon, *Description of a Natural and Experimental History* (1620), in *The Works of Francis Bacon*, ed. J. Spedding, D. Heath, and R. L. Ellis, 15 vols. (London, 1870), Vol. I, p. 401, Aphorism VIII.

³³ William Eamon, “Arcana Disclosed: The Advent of Printing, the Books of Secrets Tradition, and the Development of Experimental Science in the Sixteenth Century,” *Hist. Sci.*, 1984, 22:111–150; and B. J. T. Dobbs, “From the Secrecy of Alchemy to the Openness of Chemistry,” in *Solomon’s House Revisited: The Organization and Institutionalization of Science*, ed. Tore Frängsmyr (Canton, Mass.: Science History Publications, 1990), pp. 75–94.

³⁴ Hannaway, “Laboratory Design”; and Shapin, “House of Experiment” (both cit. n. 2).

³⁵ On virtual witnessing and its relationship to the establishment of “matters of fact” in Robert Boyle’s experimental philosophy see the seminal study by Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton Univ. Press, 1985).

³⁶ On philosophical and mathematical treatments of the credibility of testimony see Lorraine Daston, *Classical Probability in the Enlightenment* (Princeton: Princeton Univ. Press, 1988), pp. 306–342. For the English legal background see Barbara J. Shapiro, *Probability and Certainty in Seventeenth-Century England* (Princeton: Princeton Univ. Press, 1983), pp. 163–180.

tinctly odd character of many of the particulars retailed in the annals of the natural philosophers, a point to which I shall return shortly. Shapin suggests that trust among natural philosophers, as well as access to the places where experimental particulars were produced, was extended to gentlemen, following codes of honor and courtesy that sanctified a gentleman's word, however implausible his report, and opened his home (where most experiments took place) to other gentlemen, however inconvenient their visits.³⁷ There is considerable evidence that these conventions applied to natural philosophers of established reputation, as well as to the titled and well-born. When, for example, the Paris Académie des Sciences abandoned in frustration its attempts to replicate Johann Bernoulli's glowing barometers, Perpetual Secretary Bernard de Fontenelle reassured Bernoulli that "the confidence one has in his [Bernoulli's] word" made the academicians more willing to believe in protean nature than to doubt his unverifiable account.³⁸ Trust, rather than replicability, made the collaborative empiricism of particulars possible among natural philosophers. Belief in natural regularities wavered before belief in the testimony of trusted witnesses.

This trust was all the more sorely tried by the kind of particulars that often exercised early modern natural philosophers. Raleigh's tribe with "their eyes in their shoulders, and their mouths in the middle of their breasts"³⁹ could have taken their place alongside the numerous reports of anatomical anomalies such as monstrous births, celestial apparitions such as armies battling in the clouds, odd weather such as cyclones or rains of blood, and other strange phenomena in the *Journal des Savants*, *Philosophical Transactions*, and *Histoire et Mémoires*. The experience that replaced Aristotelian universals with particulars also replaced Aristotelian commonplaces with rarities and singularities. Many heeded Francis Bacon's charge that the axioms of Aristotelian natural philosophy were abstracted from too scanty a collection of particulars, and that experience of nature's ordinary course alone was inadequate to reveal the rules and species of nature. Scholastic commonplaces must be supplemented and corrected by a collection of "Deviating Instances, that is, errors, vagaries, and prodigies of nature, wherein nature deviates and turns aside from her ordinary course."⁴⁰ This "history of pretergenerations," or of "nature out of course," was meant to stand as warning and reproach to premature generalization and theorizing in natural philosophy, a collection of exceptions to all rules. The impact of these instances of "nature erring" was indeed chastening: amongst the numerous reports of strange phenomena published in the natural philosophical journals, very few offered an explanation or attempted to subsume the anomaly under a theory.

These strange facts had several obvious disadvantages. Rare unto marvelous, they strained even the ample trust of the new-style empiricism nearly to the breaking point. Observing them was a matter of luck, and they were even less amenable to public witnessing and sustained study than the most capricious of experimental phenomena. Moreover, the ban on premature theorizing could be used to cover a mute

³⁷ Shapin, "House of Experiment" (cit. n. 2). Shapin points out that the taboo against doubting a gentleman's word was so strong that skepticism concerning testimony was all but unknown in natural philosophy (p. 398); see also Shapin, "O Henry" (essay review), *Isis*, 1987, 78:417-424.

³⁸ Bernard de Fontenelle, "Sur le phosphore," *Histoire de l'Académie Royale des Sciences*, Année 1701 (Paris, 1743), pp. 1-8.

³⁹ Sir Walter Raleigh, *The Discoverie of the large and bewtiful Empire of Guiana* (1596), ed. V. T. Harlow (London: Argonaut Press, 1928), p. 56.

⁴⁰ Francis Bacon, *Novum organum* (1620), Aphorisms 1.25, 2.29, translation from *Works*, ed. Spedding, Heath, and Ellis (cit. n. 32).

and uninquisitive wonder, which hindered the causal explanations of uncommon as well as of common experience that Bacon had sought. Yet strange phenomena had the virtues of their vices, at least within the context of the seventeenth-century scientific academies that avidly pursued them. These academies self-consciously distinguished themselves from what they perceived to be the pedantry and pugnacity of university scholastics by insisting upon civility in their discussions.⁴¹ The rivalries that proved most divisive were theoretical ones, and the most explosive of these conflicts pitted one member's pet theory against another's. Hence the pronounced preference among academicians for strange phenomena, which baffled theories on all sides. As Thomas Sprat commented apropos of discussions of experiments at the Royal Society, "There was no room left, for any to attempt, to heat their own, or others minds, beyond a due temper; where they were not allow'd to expatiate, or amplify, or connect specious arguments together." This applied doubly to strange phenomena, the immediate effect of which was to paralyze speculation, and therefore, it was hoped, to pacify discussion.⁴²

Therein lay the otherwise obscure attraction of strange phenomena, which became the archetypes of the first scientific facts. Facticity in science has a history, and these early facts resemble those honored by later generations only in part. There is considerable historical variability in what kind of phenomena can, in principle, become facts. Whole domains of experience—dreams, electrophosphorescence, musical harmonies—have drifted in and out of facticity since the seventeenth century. There is also historical variability in the virtues becoming to a fact. The facts of strange phenomena were neither reliable nor robust—they could not be produced at will, much less against one's will. These facts were stubborn not because they would not go away (the problem was to make them stay), but rather because they resisted explanation by any and all available theories. In contrast to the inductive and statistical facts of the eighteenth and nineteenth centuries, seventeenth-century matters of fact were neither mundane, repetitive, homogeneous, nor countable. Rather, they were rare, heteroclitic, and singular. They qualify as facts because they were the first form of empiricism within natural philosophy to pulverize the continuum of experience into discrete particulars and to sever radically the link between a datum of experience and the inferences and conjectures founded upon it. Part and parcel of the moral economy of scientific civility, strange phenomena shaped seventeenth-century empiricism—its standards of evidence, its (very) peculiar objects, its model of facticity.⁴³

For those familiar with the empiricism of scholastic natural philosophy, seventeenth-century natural philosophy presents yet another striking novelty—namely, novelty itself. While scholastic natural philosophy was far from static, its appetite

⁴¹ On the "moral conventions" used to regulate dispute see Shapin and Schaffer, *Leviathan and the Air-Pump* (cit. n. 35), pp. 72–76; and Shapin, "House of Experiment" (cit. n. 2). Although Shapin and Schaffer situate the irenicism of Boyle and other early Fellows of the Royal Society within the context of the Restoration and Clarendon Code, the academic call for civility was a pan-European phenomenon. See, e.g., the prohibition against name-calling in *Histoire de l'Académie Royale des Sciences*, Année 1699 (Paris, 1718), p. 7.

⁴² Thomas Sprat, *The History of the Royal Society of London* (London, 1667), p. 91. On academic civility and the facts of strange phenomena see Lorraine Daston, "Baconian Facts, Academic Civility, and the Prehistory of Objectivity," *Annals of Scholarship*, 1992, 8:337–363.

⁴³ On the nature and origins of this brand of facticity see Lorraine Daston, "The Factual Sensibility" (essay review), *Isis*, 1988, 79:452–470.

for novelties seldom embraced whole new vistas of experience. Instead, scholastic natural philosophers—like philosophers to this day—worked at the ever more subtle and penetrating analysis of a largely fixed stock of examples. In contrast, the appetite of their seventeenth-century counterparts for empirical novelties (it is not clear that they were so enamored of theoretical novelties) was gluttonous. Their journals and books were crammed with observations of new objects—the moons of Jupiter, a hurricane in Bermuda, a brilliant artificial phosphor—and old objects seen in a new way—a flea enlarged under the microscope, a lark suffocated in a bell jar by the air pump. The performing of experiments was in the first instance a method of manufacturing novelties of experience, which were served up with much the same breathless terseness in natural philosophical articles as the latest tidings of battles or heinous crimes were in the broadsides of the day. We are so accustomed to the pell-mell, headlong pace of scientific novelties that we are hard put to recognize its oddity: why crave new experiences before the old ones have been duly digested?

The answer lies in part in the sensibility and the epistemology of curiosity that distinguish much early modern science. In the course of the sixteenth and seventeenth centuries curiosity was not only elevated from grave vice to peccadillo to outright virtue. It was also transformed through a realignment in the field of vices and virtues, passions and interests: briefly, curiosity wandered from the pole of lust and pride to that of greed and avarice.⁴⁴ Whereas Augustine and a host of medieval commentators had criticized curiosity as a form of incontinence and passivity, early modern writers associated it with self-disciplined activity, all faculties marshaled and bent to the quest. Newly distinguished from the desires of the body, which could be lulled and sated, curiosity was insatiable, pure *conatus* or endeavor, and marked, as Thomas Hobbes said, “by a perseverance of delight in the continuall and indefatigable generation of Knowledge, [which] exceedeth the short vehemence of any carnall Pleasure.” Marin Mersenne also mused upon the restless acquisitiveness of curiosity: “And thus we always desire to go beyond, such that acquired truths only serve as means to arrive at others: this is why we take no more stock of those we have than a miser does of the treasures in his coffers.”⁴⁵

Early modern curiosity had become a subspecies of consumerism, and its dynamics mirrored those of the trade in luxuries. Both curiosity and the luxury market thrived on novelty, for today’s luxuries—tea, shoes, white bread—were tomorrow’s necessities, and today’s knowledge staled just as quickly for voracious curiosity. Like the market in luxuries, curiosity had become open-ended and insatiable, and this structural affinity was decisive for the chosen objects of curiosity in early modern science. All that was small, intricate, and, especially, hidden exerted a particular fascination for early modern investigations of nature, for the most suitable objects

⁴⁴ On the elevation see Hans Blumenberg, *Der Prozeß der theoretischen Neugierde* (Frankfurt am Main: Suhrkamp, 1988); Jean Céard, ed., *La curiosité à la Renaissance* (Paris: Société d’Edition d’Enseignement Supérieur, 1986); and Carlo Ginzburg, “High and Low: The Theme of Forbidden Knowledge in the Sixteenth and Seventeenth Centuries,” *Past and Present*, Nov. 1976, No. 73, pp. 28–41. On the transformation and its impact on early modern science see Lorraine Daston, “Neugierde als Empfindung und Epistemologie in der frühmodernen Wissenschaft,” in *Macrocosmos im Microcosmos: Die Welt in der Stube: Zur Geschichte des Sammelns, 1450–1800*, ed. Andreas Grote (Opladen: Leske & Budrich, 1994), pp. 35–60.

⁴⁵ Thomas Hobbes, *Leviathan* (1651), Book I, Ch. 6; and Marin Mersenne, *Les questions theologiques, physiques, morales et mathematiques* (1634), reprinted in Mersenne, *Questions inouyees* . . . , ed. André Pessel (Paris: Fayard, 1985), Quest. 23, p. 302 (my translation).

of curiosity were those that matched insatiable desire of the Hobbesian sort to inexhaustible detail, allowing the eye of the body or the eye of the mind to wander from one minute part or labyrinthine convolution to another, never at rest yet still fixed upon the same object in all its multiplicity. This power to awaken, hold, and even deepen attention made curiosity an indispensable part of the militant empiricism of seventeenth-century natural philosophy, with its abiding distrust of Aristotelian generalizations and natural kinds. Curiosity was most easily ignited by “Things strange and rare,” but by means of a habit of estrangement it could also be marshaled to study more prosaic objects: “In the making of all kinds of Observations or Experiments there ought to be a huge deal of Circumspection, to take notice of every least perceivable Circumstance . . . And an Observer should endeavour to look upon such Experiments and Observations that are more common, and to which he has been more accustom’d, as if they were the greatest Rarity, and to imagine himself a Person of some other Country or Calling, that he had never heard of, or seen the like before.”⁴⁶ The curious sensibility singled out objects, subjects, and stance: strange objects (or common ones estranged) studied with every-nerve-strained attention by people often united only in their taste for such objects and their cultivation of that stance. It also impressed the prestissimo pace of novelty tumbling after novelty upon seventeenth-century scientific empiricism.

Trust, civility, and curiosity were thus three moments of the moral economy of seventeenth-century empiricism. It was a moral economy that set evidentiary standards, stipulated the forms of facticity, selected certain objects as worthy of inquiry, and accelerated the rate of that inquiry. Its models lay in gentlemanly codes of honor, civic humanism, and an emotional mutation that mimicked the dynamic of consumerism. However, the whole, once assimilated by the natural philosophers, was more than the sum of these parts, for trust, civility, and curiosity meshed into an economy heretofore unknown within early modern culture. Civility privileged the facts of strange phenomena; trust in testimony expanded to correspondingly generous proportions; curiosity initially excited by rarities and oddities could also be schooled to examine more ordinary objects. This is only one possible moral economy of empiricism, and a rather short-lived one at that, but it was of great significance for the reformed natural philosophy of the seventeenth century.

IV. WHAT IS A MORAL ECONOMY GOOD FOR? OBJECTIVITY

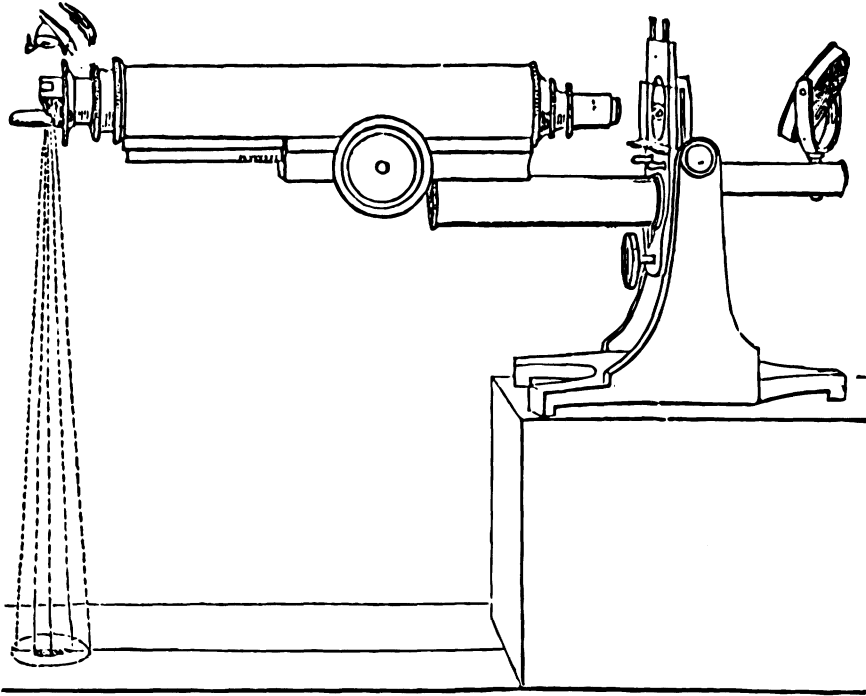
For that part of the scientific world whose opinion is of most weight, is generally so unreasonable, as to neglect altogether the observations of those in whom they have, on any occasion, discovered traces of the artist. In fact, the character of an observer, as of a woman, if doubted is destroyed.

—Charles Babbage, *Reflections on the Decline of Science in England and on Some of its Causes* (1830)

The various forms of quantification and empiricism have their distinctive moral economies; objectivity however *is* a moral economy. The philosopher Thomas Nagel writes: “Objectivity is a method of understanding. It is beliefs and attitudes that are objective in the primary sense. Only derivatively do we call objective the truths that can be arrived at in this way.”⁴⁷ As in the case of quantification and empiricism, it

⁴⁶ Hooke, “General Scheme” (cit. n. 26), pp. 61–62.

⁴⁷ Thomas Nagel, *The View from Nowhere* (Oxford: Oxford Univ. Press, 1986), p. 4.



*Objectivity: The camera lucida, invented in 1807 by W. H. Wollaston as a mechanical means of reproducing objects in perspective, is here adapted for use with a microscope. From William B. Carpenter, *The Microscope and Its Revelations* (London, 1868).*

would be more accurate to speak of objectivities, in the plural, for because scientific objectivity has a history, it too displays diversity. Here I can only sketch two of its most important variants, both of nineteenth-century vintage: mechanical objectivity and aperspectival objectivity.

Mechanical objectivity is objectivity in the postlapsarian mode.⁴⁸ It is grounded simultaneously in an epistemology of authenticity—in a yearning for “data” in its root sense of “givens,” bestowed with the effortlessness of grace—and also in the guilty conviction that fallen humans, left unsupervised, can only meddle with the givens of nature. This is the form of objectivity that strives to eliminate all forms of human intervention in the observation of nature, either by using machines, such as self-inscription devices or the camera, or by mechanizing scientific procedures, as in deploying statistical techniques to choose the best of a set of observations.⁴⁹ Most forms of objectivity share an enmity towards the personal, but which aspect of the personal—judgment, inarticulable skill, an intense aesthetic response to nature, partiality towards one’s own pet ideas—depends on the particular kind of objectivity. In contrast to aperspectival objectivity, which combats idiosyncrasies of individuals,

⁴⁸ On the nature and history of mechanical objectivity see Daston and Galison, “Image of Objectivity” (cit. n. 2).

⁴⁹ Zeno G. Swijtink, “The Objectification of Observation: Measurement and Statistical Methods in the Nineteenth Century,” in *The Probabilistic Revolution*, ed. Lorenz Krüger *et al.*, 2 vols. (Cambridge, Mass./London: MIT Press, 1987), Vol. I: *Ideas in History*, pp. 261–286.

mechanical objectivity battles the general, all-too-human tendencies to aestheticize, anthropomorphize, judge, interpret, or in any other way “tamper” with the givens of nature. Goethe gave voice to the worries that impel mechanical objectivity when he preached caution in interpreting experimental results: “For here at this pass, this transition from empirical evidence to judgment, cognition to application, all the inner enemies of man lie in wait: imagination, which sweeps him away on its wings before he knows his feet have left the ground; impatience; haste; self-satisfaction; rigidity; formalistic thought; prejudice; ease; frivolity; fickleness—this whole throng and its retinue. Here they lie in ambush and surprise not only the active observer but also the contemplative one who appears safe from all passion.”⁵⁰

Mechanical objectivity found its characteristic expression in successive waves of first enthusiasm for and then disappointment in devices that seemed to promise an escape from this inner ambush, from the automatic registration of observations to the photograph. The latter became emblematic of mechanical objectivity and reveals its essentially moral, rather than epistemological core: the advantages of the photograph for art as well as science were not necessarily those of verisimilitude—naturalistic drawings in color often excelled photographs in creating a copy of what could be seen—but rather those of authenticity. By its very automatism the photograph created the illusion of an unmediated image, free of human intervention, if not visually faithful to its original.⁵¹ It is this craving for authenticity that also explains why some scientists toyed with the idea of employing uneducated assistants. Not only was their labor cheap;⁵² they were, it was thought, nearly as much *tabula rasa* as machines were, and therefore more fit to record observations than the all-too-well-informed and expectant scientist. For this reason Claude Bernard divided the labor of the experiment between the design, to be entrusted to the scientist’s mind, fully equipped with hypotheses, and the execution, carried out by the senses “that observe and note,” or even by an “uneducated man . . . knowing nothing of theory” and therefore able to see “new facts unperceived by a man preoccupied with an exclusive theory.” Whereas Bernard was hesitant “to raise ignorance to a principle,” the Astronomer Royal John Pond was blunter: “But to carry on such investigations, I want indefatigable, hard-working, and above all obedient drudges (for so I must call them, although they are drudges of a superior order), men who will be contented to pass half their day in using their eyes and hands in the mechanical act of observing, and the remainder of it in the dull process of calculation.”⁵³

These attempts to divide and thereby mechanize at least a part of the scientific labor of observation are redolent of the attitudes of contemporary manufacturers, for whom the workers and the machines in their factories were interchangeable, and

⁵⁰ Johann Wolfgang Goethe, “The Experiment as Mediator between Object and Subject” (1792, publ. 1823), in *Goethe: Scientific Studies*, ed. and trans. Douglas Miller (New York: Suhrkamp, 1988), p. 14.

⁵¹ Charles Rosen and Henri Zerner, *Romanticism and Realism: The Mythology of Nineteenth-Century Art* (New York: Viking Press, 1984), pp. 107–108.

⁵² Charles Babbage, *On the Economy of Machinery and Manufacturers*, 4th ed. (London: Charles Knight, 1835), p. 195.

⁵³ Claude Bernard, *An Introduction to the Study of Experimental Medicine* (1865), trans. Henry Copley Greene (New York: Dover, 1957), pp. 23, 38; and Pond in minutes of the Council of the Royal Society, 6 April 1826, quoted in Charles Babbage, *Reflections on the Decline of Science in England and on Some of its Causes* (1830), *The Works of Charles Babbage*, ed. Martin Campbell-Kelly (London: William Pickering, 1989), Vol. VII, p. 64n.

for whom the word *mechanical* still carried the derogatory lower-class associations of “rude mechanical.” Although scientists often policed the character of their observer-drudges, the more striking moral overtones in mechanical objectivity were those of self-discipline, confronting Goethe’s “inner enemies” on their own territory. Here scientists attempted to hold their own predilection to judge, interpret, aestheticize, and anthropomorphize in check, and their language of self-restraint sometimes echoes that of Christian asceticism.⁵⁴ Charles Gillispie caught this note of manful self-denial, of speculation crushed and beguiling illusions willfully destroyed, in his evocations of the “cruel edge of objectivity.”⁵⁵ It is in the nineteenth century that stubborn facts also turn “nasty,” “ugly,” and “contrary.” By the turn of the twentieth century, to “face the facts” always implies some unpleasantness, and therefore objectivity—some measure of resolve and self-command.⁵⁶

If the emblem of mechanical objectivity is the photograph, the motto of aperspectival objectivity could be “the view from nowhere,” in Nagel’s brilliant oxymoron. Whereas mechanical objectivity is about suppressing the universal human propensity to judge, interpret, and aestheticize, aperspectival objectivity is about eliminating the idiosyncrasies of particular observers or research groups. Although all idiosyncrasies came to be tarred with the brush of subjectivity in the nineteenth century, they are by no means always handicaps: The ability to split a double star with the naked eye is as much an idiosyncrasy as a sluggish reaction time. Charles Babbage thus spoke for the ideal of aperspectival objectivity when he insisted that “genius marks its tract, not by the observation of quantities inappreciable to any but the acutest senses, but by placing nature in such circumstances, that she is forced to record her minutest variations on so magnified a scale, that an observer, possessing ordinary faculties, shall find them legibly written.”⁵⁷

Aperspectival objectivity serves scientific sociability and therefore enlists the various techniques of standardization, both quantitative and experimental. Transcendence of individual viewpoints seemed a precondition for a coherent scientific community to many nineteenth-century scientists and philosophers. The existence of such a community, stretching over time and space, in turn seemed to some a precondition—or even an eventual guarantee—for reaching scientific truth. Charles Sanders Peirce conceived of this necessarily communal form of truth-seeking as proceeding by a kind of symmetric cancellation of individual errors: “The individual may not live to reach the truth; there is a residuum of error in every individual’s opinions. No matter, it remains that there is a definite opinion to which the mind of man is, on the whole and in the long run, tending. . . . This final opinion, then, is independent, not indeed of thought in general, but of all that is arbitrary or individual in

⁵⁴ See Daston and Galison, “Image of Objectivity” (cit. n. 2). On policing drudges see Schaffer, “Astronomers Mark Time” (cit. n. 2).

⁵⁵ Charles C. Gillispie, *The Edge of Objectivity: An Essay in the History of Scientific Ideas* (Princeton: Princeton Univ. Press, 1960), pp. 44–45; cf. pp. 73, 107, 156, 241.

⁵⁶ See, e.g., Max Weber’s typical reflections on the link between “wissenschaftliche Objektivität” and “unbequeme Tatsachen”: Weber, “Die ‘Objektivität’ sozialwissenschaftlicher und sozialpolitischer Erkenntnis” (1904), *Gesammelte Aufsätze zur Wissenschaftslehre*, ed. Johannes Winckelmann, 3rd ed. (Tübingen: J. C. B. Mohr, 1968), pp. 154–155.

⁵⁷ Babbage, *Reflections* (cit. n. 53), p. 86. On aperspectival objectivity and its origins see Daston, “Objectivity and the Escape” (cit. n. 2). On Weber’s views on the prospects for objectivity in the social sciences see Robert N. Proctor, *Value-Free Science? Purity and Power in Modern Knowledge* (Cambridge, Mass.: Harvard Univ. Press, 1991), pp. 134–154.

thought; it is quite independent of how you, or I, or any number of men think.”⁵⁸ Peirce here gives philosophical voice to changing scientific ideals, ideals rooted in changing scientific practices in the middle decades of the nineteenth century. International scientific congresses and collaborations in demography, astronomy, geodesy, and meteorology multiplied, creating networks of coordinated observers intent on capturing phenomena so vast that they were invisible to the individual observer.⁵⁹ It is no accident that Peirce himself contributed to several of these international efforts during his stint at the United States Coast and Geodetic Survey.

Within these global collaborations aperspectival objectivity became a scientific creed, the ideal that corresponded to the practices of well-nigh constant, coordinated communication: articles were circulated across oceans and continents, measurements exchanged, observations tallied, instruments calibrated, units and categories standardized. Aperspectival objectivity was the ethos of the interchangeable and therefore featureless observer—unmarked by nationality, by sensory dullness or acuity, by training or tradition, by quirky (even if superior) apparatus, or by any other idiosyncrasy that might interfere with communication, comparison, and accumulation of results. It should come as no surprise that proponents of this ideal frowned upon carrying precision measurement to hair-splitting extremes: “The extreme accuracy required in some of our modern enquiries has, in some respects, had an unfortunate influence, by favouring the opinion, that no experiments are valuable, unless measures are most minute, and the accordance among them most perfect.”⁶⁰ Scientists paid homage to the ideal of aperspectival objectivity by contrasting the individualism of the artist with the self-effacing cooperation of scientists, who no longer came in the singular—“L’art, c’est moi; la science, c’est nous,” as Claude Bernard neatly put it. For those like Bernard who celebrated the ideal of aperspectival objectivity, there was a certain nobility in the abandonment of the personal, a sacrifice of the self for the collective—if not for the collective good, then at least for the collective comprehension. Norbert Elias argued that all complex coordinations of human activity partake of the “civilizing process,” that is, the “permanent self-control” demanded by “the lengthening of the chains of social action and interdependence,” and science is no exception here. The self-control and detachment required of scientists by aperspectival objectivity was strenuous: scientists must not only wait to be recognized for their efforts; they must give up recognition altogether. Bernard exhorted scientists to bury their pride and vanity in order “to unite our efforts, instead of dividing them or nullifying them by personal disputes,” for all scientists are ultimately equal in their anonymity: “In this fusion [of particular truths into general truths], the names of promoters of science disappear little by little, and the further science advances, the more it takes an impersonal form and detaches itself from the past.”⁶¹

No doubt the manifestos of both mechanical and aperspectival objectivity

⁵⁸ Charles Sanders Peirce, “A Critical View of Berkeley’s Idealism” (1871), *Values in a Universe of Chance: Selected Writings of C. S. Peirce (1839–1914)*, ed. Philip Wiener (New York: Doubleday, 1958), pp. 81–83.

⁵⁹ See Susan Faye Cannon, *Science in Culture: The Early Victorian Period* (Folkstone: Dawson; New York: Science History Publications, 1978), Ch. 3, on “Humboldtian science.”

⁶⁰ Babbage, *Reflections* (cit. n. 53), p. 85.

⁶¹ Bernard, *Introduction* (cit. n. 53), pp. 43, 39, 42. See Elias, *Power and Civility* (cit. n. 7), pp. 230–248, 273–274.

reflected a high-minded ideal rather than a sociological reality. Yet even values honored erratically are nonetheless genuine values, reflecting choices and revealing attitudes. Moreover, the values and affects of both mechanical and aperspectival objectivity left visible traces in the conduct of scientists, in their ever stronger preference for mechanized observation and methods, in their ever more refined division of scientific labor, in their preference for the authentic as opposed to the verisimilar or clear-cut image, and in their ever more exclusive focus on the communicable. The self-restraining and self-effacing counsels of mechanical and aperspectival objectivity reverberate with the stern voice of moral duty: the self-command required in both cases to suppress the merely personal is indeed the very essence of the moral. This does not mean we must admire these ideals, for the annals of cultural anthropology are full of moral conventions enforced by the most stringent self-command which we nonetheless abhor. But we cannot ignore them, particularly since they are interwoven into those key aspects of science we somewhat grossly term quantification, empiricism, and objectivity.

V. CONCLUSION: DISTINCTIONS WITHOUT PRIVILEGE

The few facets of quantification, empiricism, and objectivity that I have so briefly explored here by no means exhaust those aspects of science that are in whole or in part constituted by a moral economy. Many other practices and commitments might, I believe, be profitably so studied. One thinks, for example, of the kinship of form (short, private dated entries) and coincidence of timing (sixteenth and seventeenth centuries) of the diary and the notebook of experimental and field observations.⁶² The *Verinnerlichung* and construction of self by the more expansive diary writers seem to parallel the construction of what F. L. Holmes has described as the construction of "larger units of meaning" in the reports experimentalists write up from their lab notebooks.⁶³ The checkered history of scientific attitudes toward secrecy also cries out for investigation *qua* moral economy.

However, I do not mean to suggest that moral economies in science are the Universal Key to all Mythologies. As I hope will be clear from the above examples, they tell us very little about the detailed contents of scientific articles and treatises, and equally little about the institutional framework of science. Rather, they answer old questions and pose new ones about how scientists at a given time and place dignify some objects of study at the expense of a great many others, trust some kinds of evidence and reject other sorts, and cultivate certain mental habits, methods of investigation, and even characters of a distinctive stamp. Above all, they focus our attention on the distinctions between and the history of the heterogeneous pursuits we are accustomed to lump together under the headings of "quantification" or "empiricism" or "objectivity." And because moral economies are part of what one might

⁶² On the history of the diary see Elisabeth Bourcier, *Les journaux privés en Angleterre de 1600 à 1660* (Paris: Publications de la Sorbonne/Imprimerie Nationale, 1976); and Albert Gräser, *Das literarische Tagebuch* (Saarbrücken: West-Ost Verlag, 1955), pp. 19–38. Literature on the origins of the practice of keeping a laboratory notebook is harder to come by, but see W. E. Knowles Middleton, *The Experimenters: A Study of the Accademia del Cimento* (Baltimore/London: Johns Hopkins Univ. Press, 1971), pp. 359–382; and M. J. van Lieburg, "Isaac Beeckman and His Diary-Notes on William Harvey's Theory of Blood Circulation (1633–1634)," *Janus*, 1982, 69:161–183, for some suggestive early examples.

⁶³ F. L. Holmes, "Scientific Writing and Scientific Discovery," *Isis*, 1987, 78:220–235, on p. 235.

call historical epistemology—a history of the categories of facticity, evidence, objectivity, and so forth—they should interest the philosopher as well as the historian.

As for the sociologist, an investigation of how intellectual work is saturated with moral, emotional, and aesthetic elements at a collective, not just a biographical, level opens perspectives into psychology that go beyond the calculus of self-interest, strategically deployed to the ends of discipline- or career-building. Here there is ample room for a revival of Weberian-style historical sociology that carries the study of the cultural origins and character of scientific rationality beyond a few mournful sighs over the disenchantment of the world. There is excellent evidence that the moral economies of science derive both their forms and their emotional force from the culture in which they are embedded—gentlemanly honor, Protestant introspection, bourgeois punctiliousness—and here are promising grounds for an alliance of science studies with the new cultural history and its anthropological affiliates. However, there is also evidence that once these cultural forms have been uprooted and combined within a moral economy of science, they become naturalized to that milieu. Honor among scientists is not quite what it was among gentlemen, asceticism among scientists is not quite what it was among the devout. Swimming against the stream of contextual studies of science, moral economies reassert rather than dissolve the boundaries that separate the mentalities and sensibilities of scientists from those of ambient society. Science is not thereby privileged—an analysis of moral economies sometimes has the power to render the practices of the tribe of scientists as bizarre as those of any other tribe—but it is distinct. By examining in a new light just those ways of knowing once thought to exempt science from the realm of emotions and values, a study of moral economies may illuminate the nature of the rationality that seemed to exclude them.