

ESSAY REVIEW

When Facts Matter

*By Mordechai Feingold**

Steven Shapin. *A Social History of Truth: Civility and Science in Seventeenth-Century England.* (Science and Its Conceptual Foundations.) xxxii + 483 pp., bibl., index. Chicago/London: University of Chicago Press, 1994. \$29.95, £23.95.

The provocative title of Steven Shapin's new book sets up its even more provocative objective. Shapin sets himself the task of reconstituting the historical understanding of the grounds of our "factual scientific knowledge" and the credentials of its purveyors. Underlying his analysis is the conviction that the crisis that followed the collapse of the traditional authorities of knowledge in the seventeenth century necessitated new forms of assessing evidence and managing trust, for—despite the vehement pronouncements of the propagandists of the new science to the contrary—the solution to the problem of knowledge could not be reached through exclusive reliance on direct experience. What occurred in response to this crisis of authority, according to Shapin, was the specification of a new trustworthy source. And it is here, in the role of purveyor of truth, that the figure of the English gentleman comes into play. Only the gentleman, argues Shapin, was endowed with the necessary characteristics that ensured credibility and, hence, compelled assent. Just as his privileged economic and social circumstances established his integrity and disinterestedness, so, too, the gentleman's place in the social pyramid equipped him with an inviolable "perceptual competence." The ramifications of this perception of the gentleman meant that the truthfulness of his testimony could not be gainsaid without giving him the lie.

While the identity of the gentleman resolved the pressing problem of credibility, the issue of conduct within the community of gentlemen-practitioners was tackled by appropriating into the domain of the new science the conventions of polite culture, complete with its own code of honor. According to Shapin, scientific discourse was refashioned along the model of civil conversation—where too-assertive claims, contradictions, and disputes were scrupulously avoided—in order to safeguard the cohesiveness of the community and protect the knowledge it produced. The immediate result was that the unswerving pursuit of truth was dispensed with as too indecorous, and in its place a temperate probabilism, as well as a markedly lower threshold of verity, was instituted. The architect

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of this new “civil” science was none other than Robert Boyle, and much of the book is devoted to illustrating the manner in which English science after the Restoration was fashioned in Boyle’s image and likeness.

Such is the broad outline of the argument, which is skillfully crafted, forcefully argued, and supported by a wide range of sources (the bibliography cites more than a thousand books and articles by ancient and modern authors). Such an imposing display of erudition may convince even the cognoscenti of the soundness of the historical case—even if they disagree with Shapin’s broader conclusions concerning the role of trust in science. However, in a book where the issue of “truth” is both message and medium, where the scientist’s craft and the historian’s craft form an ongoing dialogue, the stakes for the scholarly community are too high to take anything for granted. I propose to scrutinize Shapin’s scholarship in an attempt to evaluate the evidential basis upon which his provocative study hinges. Such scrutiny, I believe, is imperative not only in light of the considerable influence that earlier versions of Shapin’s argument have already exerted on historians and sociologists of science, but in view of the possible ramifications of his challenging thesis for future scholarship.

I

Surprisingly, in view of the significance Shapin accords the gentleman, the identity of who exactly enjoyed this appellation is left vague (with the important exception of Robert Boyle). Nevertheless, a painstaking reading combined with a process of careful elimination reveals that in essence Shapin equates gentility with the landed aristocracy. Wealth and birth are singled out as the attributes without which no man may be considered a gentleman. The accuracy of this virtual equation of gentility with the landed aristocracy is a matter of legitimate historical debate, one that cannot be treated here at length. Suffice it to point out that, despite his awareness of the fluidity of early modern discussions on the nature of gentility, Shapin tends to be parsimonious in representing the degree to which most commentators embraced a more comprehensive definition. Thus he argues that “the recognized facts of economic circumstance were taken substantially to distinguish the gentle from the nongentle. Most early modern commentators endorsed or adapted Aristotle’s definition of gentility as ancient riches and virtue” (p. 49). But this is not quite what Aristotle said. The relevant passage in the *Politics* reads: “In reality there are three elements which may claim an equal share in the mixed form of constitution—free birth, wealth, and merit. (Nobility of birth, which is sometimes reckoned a fourth, is only a corollary of the two latter, and simply consists in an inherited mixture of wealth and merit).” Indeed, the herald William Segar translated this very passage to denote that Aristotle “maketh foure kindes of Nobility . . . noble by riches, noble by ancestors, noble by vertue, and noble for learning.”¹

Even more problematic, however, and central to his overall thesis, is the affinity Shapin forges between privileged economic and social circumstances and a whole set of values—such as integrity, freedom of action, and disinterestedness—that their possessors uniquely enjoyed. More troublesome still is the claim that these wealthy members of the gentry were accorded distinctive and privileged prerogatives in the realm of knowledge, while those devoid of such means and status necessarily fell into a condition of dependence, with its contingent forfeiture of free action, integrity, and, potentially, credibility. This is the cornerstone of Shapin’s argument. It allows him to argue that Robert Boyle and other propagandists for the new science appropriated resources derived from genteel culture in

¹ Aristotle, *The Politics of Aristotle*, trans. Ernest Barker (Oxford: Clarendon, 1952), p. 176; and William Segar, *Honor Military, and Civill, Contained in Foure Bookes* (London, 1602), p. 226.

order to promote the new science as a body of knowledge unlike traditional learning, as something practiced by gentlemen and managed according to the rituals of polite society. This strategy not only conferred new dignity on science but rendered its gentlemen-practitioners (as well as their allies) credible and trustworthy, in sharp contradistinction to the graduates of the traditional sites of learning—the universities.

In order to substantiate the claim that Restoration science availed itself of a code of conduct unlike that operating in the schools, Shapin finds it imperative to argue that scholars were the most significant group to have been excluded from the ranks of gentlemen. Indeed, he is so emphatic in maintaining the essential incompatibility between the ideal of the gentleman and ideal of the scholar that in an earlier article upon which much of his current interpretation is based, Shapin admitted that his reading was fashioned in the face of “some apparent evidence to the contrary.” Unfortunately, Shapin’s reading of the contemporary documents upon which he bases his generalizations is highly selective and often misleading, and he downplays the nearly *universal* attribution of gentle status to scholars and authors. A case in point is Shapin’s devaluing of the often-cited 1577 representation of William Harrison—repeated almost verbatim by Sir Thomas Smith—that “whoso abideth in the university (giving his mind to his book), or professeth physic and liberal sciences . . . be called master, which is the title that men give to esquires and gentlemen, and reputed for a gentleman ever after.”² For Shapin, such a depiction is representative of an “exaggerated” description of social mobility (pp. 57–58), but the historical record does not support this interpretation.

To illustrate Shapin’s methodology and handling of evidence, we may analyze a passage wherein he attempts to offset the sort of statements made by Harrison and Smith:

In 1602 [William] Segar carefully qualified the alleged rights of the educated to gentility: “knowledge or learning doth not make a Gentleman, unlesse hee be dignified with the title of Doctor, or graced by some office of reputation, and if that be taken away, he shal be reputed a common person.” [in his footnote Shapin adds that “Segar here approvingly quoted Bartolus.”] Insofar as they counted at all, the learned were to be regarded only as *ex officio* gentlemen. The educationalist Richard Mulcaster judged that “the vilest divises be the readiest meanes to become most wealthy, and ought not to looke honour in the face.” [P. 57]

This quotation from Segar was extracted from a section devoted to civil lawyers, where Segar cited (without endorsement) the gloss of Bonus de Curtili on Bartolus to the effect that within that particular community only the title of Doctor of Civil Law or the bestowal of public office confers gentility. That such sentiments were not representative of Segar’s views concerning the affinity between learning and gentility is evident not only from the sentence immediately preceding the cited passage—“men are made Noble for wisdome”—but also from his explicit statement two pages later: “Doctors and Graduates in Schooles, do merit to be ennobled and become Gentlemen.” Indeed, Segar proceeded to cite and endorse that very characterization of a gentle scholar that Shapin dismisses as exaggerated. In another section, entitled “How Gentlemen Are to Take Place,” Segar went so far as to rule that “a Gentleman ennobled, for learning vertue and good manners, is to be preferred before a Gentleman borne, and rich.” Finally, in an earlier work, also used by Shapin, Segar stated that “a Scholler having continued the studie of good learning, & aspired to

² Steven Shapin, “A Scholar and a Gentleman: The Problematic Identity of the Scientific Practitioner in Early Modern England,” *History of Science*, 1991, 29:279–327, on p. 282; William Harrison, *The Description of England* (1577), ed. Georges Edelen (Ithaca, N.Y.: Cornell Univ. Press, 1968), p. 114; and Thomas Smith, *De republica Anglorum*, ed. Mary Dewar (Cambridge: Cambridge Univ. Press, 1982), p. 72.

the degrees of schooles, cannot be denied the title of gentrie”—he is even entitled to issue a challenge to a duel. As for Mulcaster, the insinuation that the educator denied scholars the ability to obtain honor is erroneous. In the passage cited Mulcaster was specifically repudiating “rich men” and not scholars, whom he elsewhere considered fully entitled to gentle status.³

A further misrepresentation of the historical record occurs in Shapin’s discussion of the character of social and scientific discourse. He argues repeatedly that both in polite society and, *mutatis mutandis*, in the Royal Society, disagreements, contradictions, and disputes were forbidden, and that truth itself was often dispensed with in the name of civility. However, contemporaries carefully distinguished between several types of conversation on the basis of audience as well as content; this distinction is lost on Shapin, who invariably treats conversation as if it involved polite (and often mixed) company and, hence, the requirement for “imprecision” and the exclusion of technical content. Obviously, on strictly social occasions and in mixed company polite small talk was the expected norm. But this was neither the forum envisioned for experimental discourse nor the site for the meetings of scientific societies. Pedantry itself was understood to be the failure to distinguish between conversational sites, especially the espousal of specialized learning in the wrong place. And it was precisely in anticipation of such social blunders that authors of courtesy books cautioned young men leaving school to recognize that they were being removed from one society to another.

Shapin’s oversimplified view of the interaction between scientific knowledge and conversation bears directly on his view of the Royal Society. According to him, “conventions and codes of gentlemanly conversation were mobilized as practically effective solutions to problems of scientific evidence, testimony, and assent.” Consequently, conversation was not only “a mark of epistemic efficiency, [but] it was also a civil end in itself. No conception of truth could be legitimate if pursuing and maintaining it put civil conversation at risk.” For evidence Shapin points to “the relative rarity of episodes in the Royal Society setting in which natural-historical or experimental reports were negated.” Likewise, he claims that “factual testimony from gentlemen-philosophers . . . was almost never gainsaid in the public forums of seventeenth-century English science. Gentility powerfully assisted credibility” (pp. 121–124).

The superimposed contrast between the universities and that new incarnation of polite society, the Royal Society, is erroneous, not only in view of the explicit endorsement of “scholarly” pursuits in the courtesy literature but because the proponents of the new science themselves, as well as the actual practices of the Royal Society, contradict such differentiation. True, the published minutes of the society’s meetings, from which disputes and confrontations are absent, seem to license such a reading. Indeed, they perpetrate an image of meetings devoid not only of controversies, but of *any* sort of discussion following either paper or demonstration. But while the significance of this absence in the register is open to interpretation, such reports cannot be accepted as exact records of the meetings or as evidence of extreme etiquette imposed on the assembly. As Thomas Sprat emphasized, the Fellows of the Royal Society never relented “till the whole *Company* ha[d] been fully satisfi’d of the certainty and constancy [of experiments]; or, on the otherside, of the absolute impossibility of the effect. . . . If any shall still think it a just *Philosophical liberty*,

³ Segar, *Honor Military, and Civill* (cit. n. 1), pp. 226–229; William Segar, *The Booke of Honor and Armes* (London, 1590), p. 36; and Richard Mulcaster, *Positions* (London, 1581; rpt., London: Harrison & Sons, 1887), pp. 200–205.

to be jealous of resting on their credit: they are in the right, and their *dissentings* will be most thankfully receiv'd, if they be establish'd on solid works."⁴

II

My contention that the historical record fails to substantiate Shapin's claim concerning the divergence between scholars and gentlemen in the domain of knowledge—or his presumption that the latter were a priori immune from negation or contradiction—can be further corroborated by scrutinizing his principal case study, that of Robert Boyle. A thorough appraisal of the “construction” of Boyle cannot be attempted here; fortunately, however, the recent publication of several new studies of Boyle, particularly *Robert Boyle Reconsidered*, edited by Michael Hunter, goes a long way in establishing the background for a very different representation from Shapin's.⁵ In brief, whereas Shapin depicts Boyle as a poised and purposeful individual, best understood in terms of self-fashioning, Hunter perceives him as a far more complicated human being, troubled by conscience and afflicted by doubts. These personal traits, which prompted Hunter to dub Boyle “dysfunctional,” had important ramifications for his work. Hunter, John Harwood, and Lawrence Principe demonstrate how Boyle's deep-seated ambivalence and complex personality manifested themselves in a convoluted literary style, endless apologies, and overpowering anxiety about criticism. Such writings, then, are unlikely to be the result of a deliberate literary technology, constructed purposely for the defense of the new science. Equally important is the necessity of viewing Boyle first and foremost as a *religious* natural philosopher. Edward Davis, J. J. Macintosh, Timothy Shanahan, and Jan Wojcik, among others, point out Boyle's unmistakable commitment to Christianity, which inspired the sophisticated manner in which he wove together, and defended, science and faith.

Linked to Boyle's profound religiosity was his pursuit of alchemy. The researches of Principe, William Newman, and Antonio Clericuzio, as well as Hunter, highlight both the spirituality that informed Boyle's alchemical studies and the centrality of such studies to his *Weltanschauung*. We learn that Boyle was not as revolutionary as once thought and that, in fact, he shared as much with the medieval tradition as with the iatrochemists of the seventeenth century. More important, Boyle's immersion in alchemy conditioned his actions. Thus, his extensive exposure to alchemical books and practitioners shaped both his experimental practices and the rhetorical strategies he employed in his writings. Finally, the celebrated openness he has been credited with is now believed often to have been wanting. In his own way, and in certain areas, Boyle was not only secretive but also committed to a closed community of a few initiated adepts. Such a rough sketch hardly

⁴ Thomas Sprat, *History of the Royal Society* (1667), ed. Jackson I. Cope and Harold W. Jones (St. Louis/London: Routledge, 1959), p. 99.

⁵ My summary is based on the following sources: Michael Hunter, ed., *Robert Boyle by Himself and His Friends* (London: Pickering & Chatto, 1994); Hunter, ed., *Robert Boyle Reconsidered* (Cambridge: Cambridge Univ. Press, 1994); John T. Harwood, ed., *The Early Essays and Ethics of Robert Boyle* (Carbondale: Southern Illinois Univ. Press, 1991); Hunter, “Alchemy, Magic, and Moralism in the Thought of Robert Boyle,” *British Journal for the History of Science*, 1990, 23:387–410; Hunter, “The Conscience of Robert Boyle: Functionalism, ‘Dysfunctionalism,’ and the Task of Historical Understanding,” in *Renaissance and Revolution: Humanists, Scholars, Craftsmen, and Natural Philosophers in Early Modern Europe*, ed. J. V. Field and F. A. J. L. James (Cambridge: Cambridge Univ. Press, 1993), pp. 147–159; Hunter, “Robert Boyle and the Dilemma of Biography in the Age of the Scientific Revolution,” in *Telling Lives in Science: Studies in Scientific Biography*, ed. Michael Shortland and Richard Yeo (Cambridge: Cambridge Univ. Press, 1995); Lawrence M. Principe, “Robert Boyle's Alchemical Secrecy: Codes, Ciphers, and Concealments,” *Ambix*, 1992, 39:63–74; and Principe, “Style and Thought of the Early Boyle: Discovery of the 1648 Manuscript of *Seraphic Love*,” *Isis*, 1994, 85:247–260.

does justice to the richness and sophistication of these studies, but the scope of this essay necessitates focusing on the domain of science, civility, and truth, and it is to these facets in Boyle's career that we must now turn.

An appreciation of the convergence of scholarly preoccupations and identity should lead us to consider Shapin's conviction that elevated social status automatically conferred credibility in the domain of scientific knowledge. Such a conviction may account for Shapin's inattention to Boyle's conscious and continuous efforts to establish his scholarly authority, especially in domains where he was not an expert. Thus Shapin believes that Boyle's rhetoric was not only part of a strategy to "disengage" himself from his writings, and to avoid imputations of partisanship and questing for fame, but the very means of legitimating his writings. By disavowing professional expertise he was at once deemed proficient to write on, and "credibly to contribute to," any discipline (p. 182). Here Shapin attributes singularly to Boyle the distinguishing mark of the Renaissance "general scholar."⁶

Yet Boyle's protestations did not amount, as Shapin believes, to a *disavowal* of professional expertise as the mark of quality that legitimated his writing: they served, rather, as a request that the *lack* of such expertise not be counted against him. For example, according to Shapin, Boyle insisted that "*not being* a professional philosopher was a condition for 'philosophical freedom'" (Shapin's emphasis). But Boyle made no such claim; he merely reserved for himself the same philosophical freedom that was enjoyed by other naturalists in attempting "a serious inquiry into the vulgarly received notion of nature." Shapin strings together parts of a Boylean apology in order to produce the desired effect: "He persistently protested that he was 'never a professor of philosophy, nor so much as a gown-man,' and, for that reason, could be relied upon to set down experimental findings as they were actually produced, 'without fraudulently concealing any part of them, for fear they should make against [me]'" (p. 181). Once again, upon turning to the citation one quickly discovers that Boyle did not claim credibility *because* he was never a professor, but begs acceptance *despite* it:

the characters, which learned writers, English and foreign . . . have been pleased to give of the intelligence and sincerity employed in setting down the *physico-mechanical experiments*, and those of some other writings of mine, may permit me to hope, that it will be thought, that after having been divers years versed in making trials and experiments, I have made them with some care and wariness, and mentioned them faithfully, where I have not done it amply, upon hopes it may be taken in good part from a person in my present condition, that was never a professor of philosophy, nor so much as a gown-man; to have made shift to make the experiments and observations he communicates, and set them down truly and candidly, without fraudulently concealing any part of them, for fear they should make against him.⁷

Boyle, in other words, never argued that his social status accorded him any privileged prerogative over knowledge. Just the opposite. Time and again he acknowledged his want of proper academic training and asserted his legitimate competency on the basis of his work.

The strong scholarly component determining Boyle's choice of a vocation leads us to challenge another of Shapin's central claims: that Boyle never assumed the identity of the scholar and that he deliberately sought to dispel any identification of himself as an author—

⁶ For a discussion of the ideal of the general scholar see Mordechai Feingold, "The Arts Curriculum," in *History of the University of Oxford*, Vol. 4, ed. Nicholas Tyacke (Oxford: Oxford Univ. Press, forthcoming).

⁷ Robert Boyle, *The Works of the Honourable Robert Boyle*, ed. Thomas Birch, 2nd ed., 6 vols. (London, 1772), Vol. 5, pp. 158, 566. A similar sentiment is expressed at Vol. 3, p. 596.

the characteristic trait of a scholar. Only fairly late in his life, and then only grudgingly, Shapin argues, did Boyle demand “acknowledgement and proprietorship of his own intellectual goods,” as he worried that “his narratives had become *too* transparent for the maintenance of a philosophical identity” (pp. 182–184). Boyle, however, was bent on an authorial career from the start. He was determined to appear in print, and within three decades his output amounted to more than eighty English editions of his work and over a hundred Latin ones. Furthermore, the sheer volume of Boyle’s literary production, orchestrated and managed like a machine, was accompanied from the start by an explicit concern for his status as a writer as well as by the determination to ensure priority, forestall appropriation, and demand unequivocal recognition and credit.⁸

Perhaps by way of anticipating such an objection, Shapin argues that even if in practice Boyle was an “author,” the fact remains that he did not behave like other scholars: Boyle neither sought fame nor staked out priority claims, he loathed disputes, and he habitually refrained from defending himself or criticizing others. Yet such a characterization ignores the fact that it was neither compliance with a gentlemanly code of disinterestedness nor an overt indifference to his discoveries and fame that allowed Boyle to remain disengaged from typical scholarly practices. Boyle was in fact quite protective of his ideas and zealous to ensure proper attribution. Unlike most other authors, however, he was the beneficiary of a partisan network actively committed to the management of his fame, the defense of his ideas, and the disparagement of his rivals. Nor should it be ignored that such an undertaking on Boyle’s behalf would have been inconceivable without his blessing, if not his active orchestration. The full story of this unprecedented endeavor is yet to be written. Here it must suffice to point out the centrality of Henry Oldenburg to this operation. Not only was the secretary of the Royal Society indefatigable in publishing and translating numerous of Boyle’s books and articles, but he effectively manipulated both the *Philosophical Transactions* and his private correspondence to propagate Boyle’s ideas, vigilantly guarding against any trespass on Boylean territory and defending him whenever necessary. Indeed, scarcely an issue of the *Transactions* appeared that did not include an article by Boyle, a review of one of his books, or at the very least an editorial insertion by Oldenburg into someone else’s article, alerting the reader to Boyle’s priority or position vis-à-vis the matter under discussion.

Similarly, Boyle emerges from Shapin’s study as “a master of credibility” whose reports were hardly ever put through a “process of deliberative assessment,” let alone negated. Yet Boyle’s accounts were as likely to be subjected to scrutiny, and even incredulity, as were any reports by other members. Neither his social standing nor respect for his scientific work was expected to shield his experimental results from the process of verification and replication, even among the rank and file of the Royal Society. Thus, for example, on 23 July 1665 Boyle wrote Oldenburg that he and Richard Lower had “been repeating an Exp[erimen]t, to satisfie others rather then ourselves of ye Truth of wt I was relating at Gresham College when I was saying That I had observd yt if ye Thorax were sufficiently layd open ye Lungs though unhurt would not play.” In an unpublished note Boyle also recalled that in order “to satisfie some scrupulous Inquirers,” he was forced to repeat his experiments to substantiate his claim “that the white powder or Calx [he made] out of

⁸ See John T. Harwood, “Science Writings and Writings of Science: Boyle and Rhetorical Theory,” in *Robert Boyle Reconsidered*, ed. Hunter (cit. n. 5), pp. 37–56, esp. p. 38; and Lawrence M. Principe, “Virtuous Romance and Romantic Virtuoso: The Shaping of Robert Boyle’s Literary Style,” *Journal of the History of Ideas*, 1995, 56:377–397.

refin'd Gold by dissolving it in ye menstruum . . . need not be as it might be suspected to be anthing of antimoniall latitant in ye menstruum."⁹

III

It is with the aim of bringing to bear the full weight of the interaction between the matter of "truth" and the truth of the matter that this essay has sought to scrutinize various components of Shapin's thesis as well as the historical record purported to sustain it. To find the thesis wanting on both counts suggests that the problem may be rooted in Shapin's very assumptions. Fundamentally, Shapin's approach is ahistorical. He denies the historian possession of any privileged knowledge of the past. Meanings and intentions in history are forever lost, and all one can do is concentrate on ideals—"publicly voiced attitudes"—which, when viewed through a sociologically informed lens, can provide an insightful understanding of the past. In keeping with accepted norms of historical investigation, however, Shapin claims to have attempted to "triangulate" the idealized representation derived from courtesy books and programmatic statements intended for public consumption with more private sources depicting praxis, such as diaries, letters, and the like (p. xx). But this is very rarely done. Shapin hardly ever considers actual practices or individuals, perhaps believing that too much ambiguity and uncertainty is involved in traditional historical analysis. What too often happens, then, is the transformation of assumptions into conclusions.

An equally persistent problem in Shapin's narrative is his designation of lying as the antithesis of truth. His lengthy discussion of the role of lying in early modern culture and his emphasis on the pernicious effects of "giving the lie" are intended to convince the reader that any deviation from "truth" among practitioners was tantamount to lying; every negation of testimony was indistinguishable from a *mentita*—giving the lie. That Shapin now and then offers a perfunctory nod to the various gradations of untruth does not mitigate the either/or standard he holds his practitioners to. For example, he downplays the indispensability of intention in the making of a lie, and, likewise, his citations of early modern authors gloss over their own cognizance of the issue. Thus Shapin avails himself of the authority of Montaigne in this context but fails to relate Montaigne's unambiguous distinction between "telling a lie" and "lying." The former, Montaigne wrote, "means saying something false but which we have taken for true," while the latter "implies going against our conscience, and thus applies only to those who say what is contrary to what they know." And Montaigne states outright that his discussion is restricted to the latter case.¹⁰

Furthermore, the application of "lying" to the domain of science is fraught with difficulties beyond the binary either/or system of truth. Certainly examples of fraud can be spotted in the early modern period, but surely the vast number of instances that proved to be deviations from "truth" lacked the requisite component of intentionality to be so classified. As Boyle put it, "in matters of fact, which I deliver as having tried or seen them, I am very willing you should think, that I may have had the weakness to be mistaken, but

⁹ Henry Oldenburg, *Correspondence of Henry Oldenburg*, ed. A. Rupert Hall and Marie Boas Hall, 13 vols., Vols. 1–9 (Madison: Univ. Wisconsin Press, 1965–1973), Vols. 10 and 11 (London: Mansell, 1975, 1977), Vols. 12 and 13 (London: Taylor & Francis, 1986), Vol. 2, p. 444; and Boyle Papers, Royal Society of London, Vol. 27, p. 13. For an account of the experiment of opening a dog's thorax see Thomas Birch, *The History of the Royal Society of London*, 4 vols. (1756–1757; rpt., Brussels: Culture et Civilisation, 1967), Vol. 2, pp. 46, 49, 54.

¹⁰ Michel de Montaigne, *The Complete Essays of Montaigne*, trans. Donald M. Frame (Stanford, Calif.: Stanford Univ. Press, 1976), p. 23.

not an intention to deceive you.”¹¹ Analogously, most negations were not interpreted as synonymous with giving the lie. Shapin himself tacitly acknowledges this point, yet he is recalcitrant in his conclusion: “Gentlemanly society well understood the risks of disputing members’ fact-relations. To say that a man’s relation of empirical experience was faulty was to say that he was a liar, perceptually damaged, or incompetent” (pp. 124–125). However, in their programmatic statements practitioners explicitly excluded honor from the domain of science—and they made good such statements in practice.

For Shapin, then, though not for early modern practitioners, lying is a central theme within the domain of science. It is also taken to be an affliction conditioned by sordid social circumstances. We may actually compress several of his statements into a syllogism: Disinterested people don’t lie; gentlemen are disinterested; therefore gentlemen don’t lie. The inherent fallacy of such an argument need not detain us here except to comment on an implied corollary. Throughout his book Shapin imputes incontrovertible perceptual acuity to “gentlemen,” without ever pausing to assess their qualifications. It is one thing to point out that in a court of law an elevated social status privileged the testimony of a gentleman, quite another to conclude that the same privilege was automatically extended to the domain of science. Nowhere does Shapin consider the possibility that *expertise* could underwrite credibility. But, then again, this was not a mistake committed by members of the scientific community.

By way of conclusion, it could be said that though *A Social History of Truth* is often empirically unsubstantiated, its ambitious and iconoclastic character will nonetheless expedite future attempts to confront the undeniably important problem of the role of trust and credibility for the new science.

¹¹ Boyle, *Works*, ed. Birch (cit. n. 7), Vol. 1, p. 351.

LETTERS TO THE EDITOR

TO THE EDITOR:

Mordechai Feingold's essay review of Steven Shapin's *A Social History of Truth* (*Isis*, 1996, 87:131–139) is such as to demand a response. Having until recently been book review editor for *Isis*, I am responsible for its commissioning and appearance. It was no part of my brief to contest the scholarly opinions of contributors, and I did not do so in this case. Now I can respond simply as a member of our Society.

The review carries with it a strongly moralistic charge, in that Feingold sets himself up as the spokesman for Clio. He thus pronounces broadly on what "the historical record" supports or doesn't support, or when it's being "misrepresented," or what it "fails to substantiate" (pp. 133, 134, 135). When he comes to specific cases of evidence and its use, however, such magisterial claims begin to look less convincing. The main problem seems to be a steadfast refusal to consider and engage with the argument of the book itself. Rather than attempt to understand the point of particular arguments, Feingold atomizes particular assertions and then attempts to undermine their documentary foundations. This atomization usually involves failure to consider what is at stake in any particular part of Shapin's argument. Thus page 132 involves the presentation of one remark excised from Shapin's lengthy consideration of what it meant to be called a "gentleman" in seventeenth-century England, so as to be able to quote Aristotle's *Politics* and thereby cast doubt on Shapin's paraphrase of a particular passage. But the offhand remark about Aristotle occurs as a claim about the sorts of things that typically appear in books of the period regarding courtesy and civility, a claim amply illustrated by quotation and citation; Feingold's concern about how we (as opposed to early modern commentators) ought to understand this passage in Aristotle is quite beside the point.

A similarly invidious refusal to engage with Shapin's arguments occurs shortly thereafter, regarding the "scholar/gentleman" distinction (p. 133). Shapin (see esp. *Social History of Truth*, pp. 57–58) gives elaborate consideration to the muddy contemporary meaning of the term *gentleman*; he certainly does *not* represent it as a cut-and-dried matter of definition. He shows an uneasy relationship between the persona of a

"gentleman" and that of a "scholar." Feingold will have none of that, however, and has Shapin claiming unequivocally that scholars were "excluded from the ranks of gentlemen" (p. 133). That statement is then countered by a 1577 quotation explicitly accepting scholars into the ranks of gentlemen, the basis of Feingold's invocation of "the historical record" as the refutation of Shapin's claim. This is despite Shapin's own inclusion of "education" as one of the various practical routes toward gentility (*Social History of Truth*, p. 57).

Feingold's failure to follow the argument is also responsible for his indictment of Shapin's discussions of Boyle and his scholarly persona. Shapin's approach to Boyle's biography is one with which there could well be intelligent disagreements, but again Feingold sidesteps the opportunity to take Shapin seriously and instead portrays the issue as one of simple historical evidence. It is, as anyone who reads the book with attention will see, no part of Shapin's position to hold that "elevated social status automatically conferred credibility in the domain of scientific knowledge" (p. 136). Instead, Shapin argues that it was a *resource* that could be used to such ends. When Feingold criticizes Shapin's characterizations of Boyle as one who distanced himself from the role of "author," on the grounds that many others (like Oldenburg) facilitated this pose with Boyle's connivance, he seems to miss the point of Shapin's argument: that Boyle had help only reinforces Shapin's point. Feingold wants to talk about what Boyle "really believed"; Shapin is after different problems.

Thus when Feingold concludes by saying that "Shapin's approach is ahistorical" (p. 138), he tacitly acknowledges that he has been assessing the book according to inappropriate criteria. The trick is simply to claim that it is Shapin who has made this mistake, rather than the reviewer: what Shapin does is "ahistorical" because Feingold disagrees with some of his analytical approaches.

Feingold's strategy of presenting points of disagreement as technical matters of what "the historical record" shows, rather than as matters of scholarly interpretation, seems to be an attempt at discrediting Shapin as a competent scholar. That one as doggedly determined as Feingold cannot come up with better examples

of scholarly slips (the ones presented are evidently the cream of the crop, culled from the longer assault destined for *History of Science*) is actually sterling endorsement of the care and responsibility with which Shapin has assembled this long, elaborate, and serious work.

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IN REPLY:

Peter Dear has leveled several charges against my review of Steven Shapin's *A Social History of Truth*. They boil down to one claim: namely, that I incorrectly assume there are historical facts that can either sustain or invalidate interpretations built upon them. I plead guilty to holding such a view as well as its corollary: that the scholar who abolishes boundaries between facts and interpretations must be held accountable. This being said, I nevertheless take exception to Dear's charge that my critique was motivated in any way by a personal "assault" on Shapin, intended "at discrediting [him] as a competent scholar." I would not have invested the time and energy necessary to uncover Shapin's errors had I not believed the host of issues he raised to be worthy of a searching and frank engagement. Likewise, I cannot accept Dear's accusation that I relied on "inappropriate criteria" in judging Shapin's book ahistorical and that I should have realized that what I adjudged errors were in fact no more than disagreement "with some of [Shapin's] analytical approaches." Indeed, I fail to see how I can fulfill my duty as a historian unless I assess the evidence presented for the argument in any book I am to review. My judgment of *A Social History of Truth* was reached after a long and sustained attempt to do just that. Dear's allegation that I did not "attempt to understand the point of particular arguments" or that I exhibit an "invidious refusal to engage with Shapin's arguments" is simply incorrect.

Having accused me at the outset of inappropriately "atomizing" particular assertions made by Shapin in order to "undermine their documentary foundations"—a worthless approach, it is suggested, and one that "involves failure to consider what is at stake"—Dear nevertheless appears to concede in his last paragraph that the instances he mentioned are indeed "examples of

scholarly slips." However, while Dear downplays the significance of such slips, and undoubtedly believes they are mere nit-pickings, indicative of an excessively pedantic fixation with "facticity" and an ignorance of the "big picture," I consider details to be at the heart of any historical interpretation. It is on the strength and accuracy of the particulars that all interpretations must rest or fall. In my review I attempted to indicate that one finds an alarming number of "slips" in Shapin's book, that these are invariably advantageous for the author's positions, and that, consequently, some of his most important conclusions are simply untenable.

I will not here enter into a detailed discussion of the two instances mentioned by Dear. I should point out, however, that Shapin's "lengthy consideration of what it meant to be called a 'gentleman'" is matched by an equally lengthy discussion on my part, the upshot of which are two important conclusions: (1) notwithstanding sundry disclaimers, the entire tenor of Shapin's discussion of the gentleman/scholar distinction is based upon his polarization of the two communities, a polarization that supports his exclusion of scholars from the ranks of gentlemen and that has significant repercussions for knowledge claims made by them; and (2) notwithstanding the "elaborate" sources Shapin has gathered, all too often his conclusions are shaped by a confusing and inaccurate discussion of the literature, including citing out of context and the occasional cropping of texts. The examples I chose were, naturally, intended to illustrate this methodology.

As for my "indictment of Shapin's discussions of Boyle and his scholarly persona," I think Dear is unhelpful in characterizing our disagreement simply in terms of cross-purpose argumentation between my preoccupation with what Boyle "really believed" and Shapin's concern with the construction of the public self. It seems to me that a precise understanding of the former is indispensable for our comprehension of Boyle's public stance and, more important still, that the historical record cannot substantiate many of the claims deployed by Shapin.

To his credit, Dear did not attempt to influence the scholarly opinion expressed in my review. He carried out his duty as book review editor, and I thank him for it. However, I would like to clarify the circumstances that resulted in my publishing the review in its present form. Although the longer version Dear refers to was written originally for *Isis*, he insisted that I adhere to the word limit initially agreed upon (though I did suggest to him that the lengthier

version might fit the “Critiques and Contentions” section of *Isis*). In response, I decided to submit the complete text, containing the bulk of the empirical evidence substantiating my claims, to *History of Science*. I respected Dear’s position. He had, after all, commissioned an essay review, and I had agreed to furnish it within the prescribed length. However, I do object to his contention that what I offered is obviously “the cream of the crop” and that I could not “come up with better examples.” Dear had himself read the draft of the projected full-blown essay—four times the length of the *Isis* piece—which contains extensive discussion of several complicated examples. Entire sections, in addition to much documentary evidence, had to be omitted for the purpose of a review in *Isis*. These include a thor-

ough analysis of the scholar/gentleman distinction, a much more detailed probe into Shapin’s depiction of Boyle’s self-fashioning, a rebuttal of Shapin’s depiction of the role of technicians in general and that of Robert Hooke in particular, a scrutiny of the reliability of Shapin’s important case study on the alleged “distribution of truth” concerning the 1664 comet between Hevelius and Auzout, and more. I leave it to the readers of *History of Science* to judge whether the longer text elaborates and, indeed, fully substantiates the argument that I have outlined in *Isis*.

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hand. Given an undated manuscript mainly in an unknown hand, it is especially important that the transcription present the entire document. A comparison of the fourth paragraph of the transcription (p. 56 of *A New Course in Chemistry*) with the same paragraph of the manuscript (Fig. 1) illustrates that the complete text has not been transcribed. Beretta includes the comments that Lavoisier wrote in the margins but has substituted “+” symbols for the passages in the text that Lavoisier crossed out. Words that have been replaced can speak as eloquently as their replacement and should not be omitted.

I have not questioned the authorship of the marginal notes. It is the date of the manuscript that is at issue here. The handwriting of the marginal notes does not resemble Lavoisier’s writing in the many documents that are dated explicitly as 1764; it is the more mature hand of later years.

Nor have I questioned Lavoisier’s retrospective exaltation of La Planche in 1792. In my reading of Lavoisier’s work of 1763–1768—including his notes on chemistry and natural history, his *journal d’expériences*, many short notes and memos, and lists of experiments to do—I have found no reference to La Planche. However, in these works Lavoisier does make repeated specific references to contemporary scientists, including Rouelle, Macquer, Baumé, Pott, Margraff, de Bomare, and de Jussieu.

Beretta has misinterpreted Lavoisier’s use of the hydrometer in the 1765 *Analyse du gypse*. He mistakenly states that by determining specific gravities Lavoisier established that gypsum is a neutral salt, and he then bases his dating of the manuscript on this conclusion (pp. 35–36). There is no question that Lavoisier used a hydrometer. The problem begins with Beretta’s erroneous identification of *voie humide* (wet analysis) with determining specific gravities. *Voie humide* refers to chemical analysis in solution and was a well-established method by 1764. It does not imply the determination of specific gravities or the use of a hydrometer. Furthermore, it is clear from Lavoisier’s *journal d’expériences* and his memoirs on gypsum that long before he brought his hydrometer into the lab he had established that gypsum was a neutral salt and had identified its component parts by rather ordinary chemical methods: dissolution in water, evaporation, crystallization, calcination, tests with acid and alkali, and precipitation.

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TO THE EDITOR:

The extraordinary essay review of my book *A Social History of Truth (SHOT)* in the March 1996 issue (*ISIS*, 1996, 87:131–139) demands response. The reviewer sees little value in the book, while at the same time he worries that unwary readers (and even what he calls “the cognoscenti”) may be taken in by its “imposing”—but ultimately unsound—“display of erudition.” Too many of them had apparently not noticed that the “evidential basis” on which I drew was massively faulty and that the arguments put forward were “often empirically unsubstantiated.” Even before this book appeared, several of my articles had, he says, begun to exert a malign “considerable influence” on scholars, and this new book threatened to spread the infection further (pp. 132, 139). This reviewer to the rescue. He has seen through the only apparently “imposing” scholarship, and he has discerned devastating flaws in my representation of the historical facts and, therefore, in the interpretations that are said to derive from those facts.

All reviewers have a perfect right to their opinions. However, an accusation of systematic “misrepresentation of the historical record” (p. 134)—in plain words, of incompetence or of deceit—cannot be permitted to pass without comment, as, indeed, the reviewer must know if he has understood anything at all about my book.

So let me first make some professions of historical faith which I had naively hoped would never have proved necessary: Historians have an obligation to get their facts right. There are no exceptions to this obligation. It is absolute. It applies to every sort of historian and to every item of factual knowledge, in whatever domain and on whatever scale. That obligation applies to getting right distinctions between commas and semicolons in quotations from documentary sources, to the accurate representation of the precise dates of events, and to ensuring that quotations from sources are faithfully given and honestly interpreted. Sloppiness in such matters is rightly taken as a mark of incompetence, and visibly systematic perversion of the factual record is morally accountable. That is why any historian accused (straightforwardly or by winks and nudges) of incompetence or willful distortion must either publicly acknowledge the fault or reject the accusation as unfounded. So: I reject my reviewer’s charges. They are totally unfounded.

Despite the impression encouraged by the reviewer’s opening and closing flourishes, there is actually only one full-blown charge of factual misrepresentation.¹ I am accused (p. 132) of mis-

representing “what Aristotle said” about the bases of gentility. In fact, this entire section of my book was about how early modern commentators used, construed, and understood Aristotle (and very many other quoted sources)—not about “what Aristotle said.” That is why I referred—in a passage actually quoted by the reviewer—to commentators “adapt[ing]” Aristotle on gentility as “ancient riches and virtue” (*SHOT*, p. 49). Indeed, on the very next page of my book I quote Lord Burghley’s highly influential definition of gentility as “nothing but ancient riches,” and if that is not a free adaptation of Aristotelian sentiments, nothing is.

What, then, is the reviewer’s basis for the grave charge of factual misrepresentation? *His* favored translation of Aristotle’s *Politics* has the phrase “an inherited mixture of wealth and merit,” where *mine* (the equally authoritative *Revised Oxford Translation*) renders it as “only ancient wealth and excellence,” which is faithfully paraphrased by my “ancient riches and virtue,” the Greek *arete* in modern translations being interchangeably given as “virtue” or as “excellence” or as “merit.” If the reviewer thinks I have factually misrepresented Aristotle in using the word “virtue,” then he is just wrong. And in his urgency to accuse me of scholarly shoddiness or immorality, the reviewer has apparently not noticed that the only pertinent *historical* question about sources in this connection concerns not how any twentieth-century translation renders Aristotle but how Aristotelian sentiments were understood and used by sixteenth- and seventeenth-century English gentlemen. Anyone who bothers actually to read chapter 3 of *SHOT* will see that it abundantly documents variation and contestation over the proper definition and justification of gentility. The charge of factual misrepresentation is, therefore, without foundation.

Facts *do* matter, and sources *must* be treated with integrity. One would hope that reviewers setting themselves up as arbiters of scholarly standards showed equal scruples in giving accounts of the character and arguments of the books they review. But the scholarly integrity the reviewer asserts that my book lacks is not notably evident in his representation of it. In the limited space extended to me, I will briefly document a series of misrepresentations and their bearing upon the reviewer’s bill of indictment.

{i} The reviewer takes me to task (p. 138) for the “persistent problem” of saying that “any deviation from ‘truth’ was tantamount to lying.” This too is factually incorrect, and importantly so. An entire section of chapter 3 (“Relative Truth”) argues the *inappropriateness* of equating

untruthful utterances with lying, showing the rich repertoires employed in gentle culture to ensure that no such rigid link was made. I write (*SHOT*, p. 106): “We understand that *not just any departure from truthfulness was recognized as a lie* in early modern culture” [emphases added], and I go on to show how and why lies (taken as intentionally deceptive untrue statements) were identified in the welter of untrue statements. It is fortunate for the reviewer that one of the few early modern gentlemanly norms I embrace and act upon is a reluctance to equate the damagingly untrue with the intentionally deceptive.

{ii} The reviewer claims (p. 131) that my argument is that “[o]nly the gentleman” possessed the characteristics that “ensured credibility and, hence, compelled assent.” Later (p. 132), the language of necessity is associated with my views about the credibility-predicament of those lacking wealth and birth. These claims too are factually wrong.

The entire section of *SHOT* from p. 95 to p. 101 (“Unreliable Gentlemen”) is about those categories of men who might be recognized as possessing gentility but not the credibility otherwise widely ascribed to gentlemen: Catholics, courtiers, foreigners. Hence, any account of *SHOT* which has me arguing either that the credibility of gentlemen was unconditional or that gentle “social status automatically conferred credibility in the domain of scientific knowledge” (review, p. 136), or in any other cultural practice, is a factual misrepresentation of my book.

The whole of chapter 8 of *SHOT* (“Invisible Technicians”)—a chapter about whose contents the reviewer says nothing—documents the centrality of non-gentle support personnel in the making of experimental and natural historical knowledge. Far from claiming that these sorts of people “necessarily” lacked credibility, I show, in great detail, how important the testimony of the non-gentle and the dependent was to making such knowledge. Lest anyone miss the point, I spelled it out repeatedly and as plainly I could. So I say (*SHOT*, p. 392): “It was *not* the case that [dependent and non-gentle] technicians were simply distrusted . . . rather, it was that they *might* be distrusted, costlessly and consequentially.” And I elaborate the point in relation to Robert Hooke (on p. 393), rejecting the propriety of using the language of necessity in these connections: “Royal Society distrust of its curator’s testimony was *not* routine, and one could hardly imagine how the society could have arranged its affairs if Hooke’s experimental testimony had

not been generally accepted. The point is that even Hooke's experimental testimony *might have conditions laid upon it* [emphases added]. My arguments linking credibility to social standing are everywhere, and in every respect, *conditional* in nature: they are about the conditions, costs, and consequences of trusting or distrusting categories of people. And they are about the scenes and circumstances in which these conditional links were played out. The reviewer factually misrepresents those arguments.

{iii} On p. 139 of the review, it is confidently asserted that "Nowhere [do I] consider the possibility that *expertise* could underwrite credibility." This charge too is factually incorrect—and massively so. Chapter 5 of *SHOT* describes the maxims which widely regulated the evaluation of testimony, and in that chapter pp. 218–221, 229–230, and 235 (cf. also p. 266) are explicitly about the maxim counseling assent to the testimony of those who possessed skill, knowledgeable, or *expertise*. Here I show how that maxim functioned as part of a complex cultural system, even noting the limits Boyle placed on honesty if skill were lacking.

On p. 138 the reviewer claims that "Shapin hardly ever [*sic*] considers actual practices or individuals," magisterially diagnosing me as reckoning that the "ambiguity and uncertainty" involved in something called "traditional historical analysis" are beneath me. Just as the reviewer is silent about chapters 5, 7 and 8, so he invites the reader to believe that the whole of chapter 6—containing an elaborately detailed reconstruction of a dispute in cometary astronomy—does not exist. Here, and in two other case studies that make up this 66-page chapter, I deal *painstakingly and at length* with "actual practices" and with the "individuals" involved in them. More than that, I treat the role of expertise in these connections minutely, showing once again how its recognition functioned as part of a cultural system in which the resources of gentlemanly conversation also importantly figured. And if, indeed, the reviewer has read this material at all, I am at a loss to suggest why he considers that "ambiguity and uncertainty" do not feature centrally in my interpretation: it is *all about* uncertainty and its management. It may, after all, have failed to give what the reviewer blandly describes as a "traditional historical analysis," but the charge that my failure proceeds from not treating "actual practices and individuals" is factually incorrect.

At the end of the day, it is not just that the reviewer wants me condemned as a bad histo-

rian; it is handed down from above that I am not a historian at all: "Fundamentally, Shapin's approach is ahistorical" (p. 138). My work is to be dismissed because I "den[y] the historian possession of any privileged knowledge of the past." I am supposed to reject the possibility of recovering "meanings and intentions" in favor of some sort of desiccated sociologism.

I cannot dictate how historians label me and my work, and I now realize that I must have been seriously naive in hoping that my attempt to write a philosophically and sociologically driven account of a past culture might be viewed tolerantly by historians of science. But the naïveté is not all on one side. By what right does my reviewer call my work "ahistorical" because it, occasionally, points to problems with how, and on what conditions, historians secure their knowledge of motivations and meanings?

In the book under review, as in other work extending over more than twenty years, I have pointed out that the ascription of motives to historical actors is irremediably theoretical in nature, and I have intermittently noted that historians should be careful in projecting current motives and meanings onto past cultures whose repertoires may differ from our own. I have also advocated and exemplified a historical project in which motives are taken as elements in a public culture and are, therefore, accessible to historians' ordinary concerns with empirical materials, without necessarily resorting to sometimes dubious psychological theorizing or assumptions. My reasons for making these arguments are, therefore, no different in kind from those sentiments which inform the widely accepted interpretative sensibility in modern cultural history and history of ideas.

As for my denial of historians' "privileged" access to knowledge of the past, I simply do not know what form of privilege the reviewer has in mind to defend from my work. If he means that I deny that historians can have direct contact with past realities, unmediated by the assumptions and the vocabularies of their own culture, I do indeed deny this. So does E. H. Carr; so does Quentin Skinner; and so (I had thought) do the majority of practicing historians dedicated to the retrieval of past meanings but reflectively concerned about the conditions in which they can realize their interpretative goals.

If work of the sort I and many other historians of science are doing is to be dismissed as "fundamentally ahistorical," there should be powerful reasoned arguments for that judgment.

Disciplinary scent-marking is not reasoned argument.

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NOTE

1. In a reply to a previous letter-writer's complaint about the manner and content of this review (*Isis*, 1996, 87:505–506), readers are assured that there are lots and lots more factual misrepresentations to be exposed and that these will be published in another journal. I must be excused from rebutting charges that have not been brought to my attention, but, on this showing, I look forward with pleasure to doing so if and when they are published. I should also acknowledge the reviewer's claims that quotations from William Segar's heraldry are "misleading" (p. 133) and that "the historical record fails to substantiate" (p. 135) my social-biographical account of Robert Boyle. Both these allegations relate more to matters of interpretation than of fact; and addressing both requires elaborate discussion at a length this journal has not been able to allow me. Again, should these or similar charges be published in a journal which will permit me space adequate to reply, the reviewer will receive the response he deserves.

IN REPLY:

Steven Shapin, the noted sociology professor from San Diego, is disturbed that a historian finds his *A Social History of Truth* to lack truth value. This is neither surprising nor bothersome, but the intemperate and humorless tone of his letter indicates that the author was apparently unprepared to have his work subjected to close scrutiny. I will not counter Shapin's unfortunate diction with similar words but will instead concentrate on the facts of the matter.

The issue at hand is what constitutes evidence. I argued that Shapin has important facts—I use the term advisedly—wrong. Nowhere did I accuse him globally of "incompetence," "deceit," or "shoddiness," much less of "immorality." These are his words, not mine. Shapin's errors in this respect assume various forms. These include stringing together sentence or paragraph parts in carefully selected ways to produce a desired implication through form rather than content, the extricating of phrases from their proper context, and partial or highly selective quotation. In addition, thanks to a skillful deployment of rhetoric—copious repetitions intended to drive a message home and the articulation of many key

sentences in a subtle and confusing manner—the reader, who has not infinite time to engage in hermeneutics, can easily mistake the conceivable for the actual. It seems to me that Shapin engages massively in a sort of inversion of what Christopher Hill admitted to some years ago: "I was advancing a thesis, not attempting to sketch the intellectual history of England," wrote Hill, and "therefore picked out evidence which seemed to me to support my case. So, though I hope I have suppressed no facts which make against me, I have often . . . omitted facts which seemed to me 'neutral.'" Where Hill ignored the neutral, Shapin presents it as though it were grist for his mill. Indeed, *SHOT* overflows with "facts." Some are important, germane to the argument; many others are simply neutral or even altogether irrelevant. But all are carefully chosen to support, directly or by nuanced implication, a profoundly controversial assertion. *SHOT* is also replete with precious qualifiers that serve less to modify a claim than to forestall potential charges that the author insists unqualifiedly upon a particularly contentious historical claim. My review was intended as much to expose this technique to the light of day directly, controverting many of the premises upon which it relied, as it was to expose the particular manner in which Shapin forcefully bent facts to the demands of his conclusion.

Rhetorical flourishes aside, the author argues that I have actually leveled "only one full-blown charge of factual misrepresentation" against him, when I accused him of getting Aristotle wrong. My argument hardly rests on so limited a foundation as this, as we shall see, but it certainly deserves its own answer. We must distinguish first of all between the text as written and the text as read (which is, of course, a distinction that Shapin would make little of, since he does not believe that texts carry meaning except when they are read, just as trees falling in lonely forests make no sound). As written, *Politics* 1294a offers an expansive definition of those entitled to the status of gentility: "In reality there are three elements which may claim an equal share in the mixed form of constitution—free birth, wealth, and merit. (Nobility of birth, which is sometimes reckoned a fourth, is only a corollary of the two latter, and simply consists in an inherited mixture of wealth and merit.)" Shapin alters the meaning of this text by suggesting that Aristotle restricted the status of gentility to noblemen. He further altered the original meaning by prefacing his misleading paraphrase of Aristotle with the statement that "the recognized facts of economic circumstance were taken sub-

stantially to distinguish the gentle from the non-gentle.”

Of course, the issue here is not what Aristotle himself meant but, rather, what he was taken to mean, or, at least, how his remarks were used during the period in question. My argument is precisely that there is little difference between the original meaning and how it was read. Indeed, the reason for my commenting on Aristotle's text in the first place was to point out that this was the way *contemporaries* understood it. As William Segar, one of our author's key sources, unambiguously remarked: “*Aristotle* in his 4. booke of *Politikes* maketh foure kindes of Nobility, viz. . . . noble by riches, noble by ancestors, noble by vertue, and noble for learning.”

Indeed, I picked Segar for extended comment in order to illustrate the character of the methods employed by the author of *SHOT*. Shrewdly, however, his letter fails even to mention this sustained critique of misrepresentation—comprising almost one tenth of my review—except to bury in a footnote an “acknowledge[ment]” of my claim that his quotations from Segar are misleading, further excusing himself for not responding to these on the grounds that he was not allowed more space. It seems to me that the additional 750 words he had at his disposal, and which he did not use, would have sufficed for that. [Editor's note: The participants in this exchange were each offered 3,000 words.] Lord Burghley's remark, which Shapin cites as an instance of a “highly influential definition of gentility as ‘nothing but ancient riches’ ” and a supreme example of “a free adaptation of Aristotelian sentiments,” in fact demonstrates nothing of the kind. Burghley, whose career actually provides a superb example of the manner in which merit conferred gentility in early modern England, did not intend in his cryptic remark to define gentility, as Shapin would have it, and nowhere did he mention Aristotle. Instead, he vividly exhorted his son not to squander the family fortune which he'd established.

Shapin's next charge—that I incorrectly assert he does not differentiate between untruthful utterance and a lie—equally misses the mark. My review certainly acknowledges the existence of Shapin's lengthy discussion of lying in early modern culture, about which I remarked that the fact that he “now and then offers a perfunctory nod to the various gradations of untruth does not mitigate the either/or standard he holds his practitioners to.” I am consequently well aware of Shapin's discussion, including the six pages to which he refers me. However, these pages deal primarily with secrecy, simulation, and dissim-

ulation as not always taken to denote untruth, whose applicability to the domain of scientific practice is not inherently obvious. Indeed, this was precisely the point I had made in the sentence that our author “corrected.” I had written that, according to Shapin, “any deviation from ‘truth’ among practitioners was tantamount to lying.” In quoting this remark he simply removed the underlined words, thereby utterly and fatally misconstruing the argument, repeatedly stated in my review, that his general discussion failed to substantiate the parallel he wished to draw between the domain of idealized gentlemanly behavior and the domain of science.

The author further charges me of being “factually wrong” when I assert that he claims only the gentleman to possess “the characteristics that ‘ensured credibility and, hence, compelled assent,’ ” referring me to a section of his book that is supposed to discuss individuals who possess gentility but not credibility. There are two problems with Shapin's claim. First, one would be hard pressed to find in that section (which deals with anti-Catholic polemics, rhetorical flourishes about English sincerity, and courtiers who could be expected to deviate from the truth) any evidence that a gentle Catholic, foreigner, or courtier was denied credibility. But, more importantly, this has nothing whatsoever to do with the point I made, which involved the converse. Where in his book does the author credit anyone but a gentleman with the possession of those characteristics that routinely ensured credibility? And the routine possession of credibility—not its tortuous assignation in the face of discrepant social characteristics—is precisely what is at issue here. I am likewise puzzled by Shapin's claim that my reading of his position to mean that “social status automatically conferred credibility in the domain of scientific knowledge” constitutes “a factual misrepresentation” of his book. On pp. 237–238 of *SHOT* he writes: “In certain sorts of people [he is discussing those ‘characterized by their integrity and disinterestedness,’ i.e., the gentlemen] credibility was *embodied*” [emphasis in the original]. Predictably, in the next paragraph, Shapin adds that “[t]he recognizable integrity of sources remained, of course, a property of a cultural system” and a circumstance that “might be weighed against other properties of the system.” He continues: “The ascribed credibility of a source could not, that is, determine assent by itself. Nevertheless, the integrity of sources was the one inducement to assent which did not generate a counter-maxim, and this integrity of its own was the basis of its justificatory power.” As usual, Shapin is

simply too slippery to pin down. If he intends to argue that his claim concerning the impossibility of a gentleman not being counted as credible—because his very person incarnates credibility—needs to be taken fluidly, then the category becomes useless for historical characterization. For we are left with a sheer tautology: Gentlemen were credible except in those cases when they were not; and other mortals (such as Hooke and the divers) were routinely trusted, except in those few occasions when they were not. And in both instances the withholding of trust had nothing to do with status.

Shapin next charges that I misrepresented him to claim that technicians “‘necessarily’ lacked credibility,” whereas, he asserts, his eighth chapter (which I did not discuss for reasons of space) provides extensive evidence to the contrary. Readers who refer to p. 132 of my review will find that I actually wrote that those devoid of means and status “necessarily fell into a condition of dependence, with its contingent forfeiture of free action, integrity, and, potentially, credibility.” Not quite the same thing, and yet another example of the author’s free way with texts. As for his more general claim that there existed a “conditional” linkage between credibility and social standing in the case of Robert Hooke, I can do no more here than firmly to reject as unfounded the author’s (by now) almost canonical rendition of Hooke’s subordinate status and its implications for his credibility. My forthcoming longer critique will fully document this charge.

Shapin further argues that charging him with the failure to “consider the possibility that expertise could underwrite credibility” is “factually incorrect—and massively so.” He is correct to point out that he offered a short discussion of the *maxim* concerning skills and expertise. But this maxim, like the other maxims he discussed in chapter 5, was shown to be potentially contestable and always subject to countermaxims—except, of course, the maxim “which counseled assent to testimony from people characterized by their integrity and disinterestedness.” Surely, to devote three or four rather perfunctory pages to such an important topic, especially when the entire tenor of the book goes against it, hardly suffices to invalidate my claim that for early modern savants, expertise was much preferred to status in matters scientific.

When the author takes me to task for claiming that he had failed to consider “actual practices or individuals”—pointing to a 66-page chapter that dealt with practices—he again fails to grasp the nature of my criticism. On p. 138 of my review

I specifically referred to his failure to “triangulate,” as he had promised on p. xx of his book, the idealized representation of the gentleman derived from courtesy books with the actions of concrete gentlemen. This is an important issue, for throughout the book we are told, or can surmise, who was not a gentleman: Hooke, Newton, Huygens, Spinoza, Hobbes, Auzout, Hevelius, More, Line, Papin, Locke, Oldenburg, Stubbe, and so on. But apart from Boyle, we are never told explicitly who else was actually considered to be, or considered himself to be, a gentleman/natural philosopher. Viscount Brouncker and Sir Robert Moray perhaps? And if so, do they need to exhibit the same attributes ascribed to Boyle? No “social history of truth” could be based on the study of a single individual—and a study that is totally at odds with other recent studies of Boyle!

Yes, chapter 6 does exist, with its “elaborately detailed reconstruction of a dispute in cometary astronomy.” Unfortunately, the latter is incorrect, while the other two case studies discussed are, at best, debatable. Once again constraints of space prevent me from presenting here the full documentary evidence necessary to refute what by now has come to be a widely accepted claim, namely, that the controversy over the 1664–5 comet was resolved through the “distribution of truth” between Auzout and Hevelius, as representations of nature themselves needed to “be adjusted in order to hold stable the moral order of skillful and sincere colleagues” (*SHOT*, p. 268). I shall do so in my longer critique.

I was asked to explain the reasons for my judging the book to be ahistorical. It was not simply because I believe the author’s rejection of traditional history of ideas/science, as well as that of psychologically informed biography, in favor of the dubious construction of idealized social forms to be intrinsically pernicious. It is rather the insouciant and obdurate manner with which Shapin approaches the evidential record—corroboration of which can be found aplenty in his letter—that most disturbed me. Shapin, it seems to me, regards a text as, in Umberto Eco’s paraphrase of Tzvetan Todorov, “only a picnic where the author brings the words and the reader brings the sense.” I do not. As I emphasized in my previous response to a letter to the editor, the person who abolishes altogether the boundaries between facts and interpretations, and who believes that precise attention to detail is expendable for the sake of preserving an argument, must be held accountable. Shapin’s impassioned “profession of historical faith” talks high of the absolute necessity to get the facts

right and render the quotations accurately, “commas and semicolons” and all. But he is vague at best about the necessity to get the context right as well. I realize that it has now come to pass, for reasons that remain unclear, that some writers of science history believe careful and modest attention to detail to constitute an obstacle to the larger picture being painted. No matter how flawed a book may be, the issues it raises apparently dissolve the significance of the trivial, detail-mongering errors that it contains. I do not share this view. I leave it to the readers to go beyond the heavy rhetoric of Shapin’s letter and to decide for themselves whether my charges are a matter of fact or interpretation. I invite them also to reflect on the disturbing similarity between the historical methodology deployed in his book and the polemical strategy that he adopted in his reply to my review.

The discerning reader will have noticed that Shapin was adamant in not mentioning me by name. It would have been nice to believe that he was motivated to do so by the laudable consideration (often expressed by Boyle) of keeping discussion focused on issues, not people. More likely, however, it was indicative of the author’s passion, which also led him to impute sheer personal spleen to my serious attempt at engagement. His reaction should make him recognize the fragility of yet another of his assumptions—the ability of individuals, both in the seventeenth century and today, to remain unconcerned when contradicted or otherwise challenged.

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