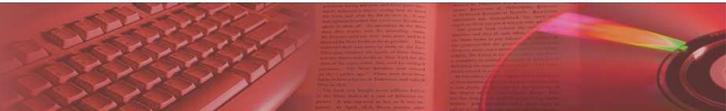


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in the Humanities & Social Sciences



Steven Shapin. *A Social History of Truth: Civility and Science in Seventeenth-Century England (Science and Its Conceptual Foundations series)*. Chicago: University of Chicago Press, 1995. \$23.00 (paper), ISBN 978-0-226-75019-4.

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Published on H-World (June, 1995)

This fascinating book is far too rich for adequate review in a discussion of this length. I'll gesture at some of its leading ideas and ask a few questions in hopes of tempting you to read it.

By "truth" Shapin means claims which are accepted. Since different groups accept different claims at different times, truth, so conceived, is various and changeable. By "social history of truth" Shapin means a history of micro-social factors which influence the generation and acceptance of claims by the groups (during times) chosen for study. Shapin's primary focus is on scientific claims developed by Robert Boyle and other 17th century members and correspondents of the Royal Society.

He orients his study in opposition to some received views about science and scientific discourse. One is epistemological individualism—the official position of the British empiricists—according to which the "legitimate springs of empirical knowledge" are located, not in the discourse of groups of people, but "in the individual's sensory confrontation with the world" (202). On this view, the ideal scientist bases his evaluations of empirical claims as much as possible on his own observations and reasonings, instead of depending on the testimony and theorizing of others. Shapin argues, to the contrary, that scientists rely so heavily on the testimony of others whom they trust that we cannot understand the production of scientific truth unless we understand how trust is established and maintained, weakened and lost.

Shapin also opposes the idea that scientific discourse is radically different from what Luhman, Oakeshott, and Rorty call "conversation". The aim of "conversation" is to perpetuate itself along with the social practices and institutions which support and are furthered by it. By contrast, says the received view, science is dedicated to the discovery of facts, rigorous refutations of false claims about

facts, and rigorous demonstrations of true claims. Thus scientific discourse is marked by skeptical examinations and harsh criticisms which would be non-conducive, if not fatal, to conversational discourse. Shapin believes, to the contrary, that even if this contrast between conversation and scientific discourse is "...a significant actor's distinction...", the historian or sociologist can "...illuminat[e] the nature of scientific practice..." analyzing it on the model of conversation (352). Social and political communities cannot thrive, and their members cannot fruitfully pursue their goals without mutual trust and the sociability and cooperation it fosters (10-13) and Shapin argues that the same holds for "communities" of working scientists. The conventions of non-scientific, genteel conversation provided ways to avoid (or where they could not be avoided, to manage) disputes which could otherwise weaken the trust and sociability required for community life. His leading idea is that conversational conventions deriving from those of genteel discourse were incorporated into and performed the same function in the scientific discourse of the Royal Society.

In a fascinating and authoritative survey of material from 17th century "courtesy literature" on what it is to be, to act, and to speak like a gentleman, from political philosophy and from Bacon, Locke, and other scientifically minded philosophers of the period, from the scientific correspondence of the Royal Society, and from the life and writings of Robert Boyle, Shapin extracts seven maxims (212ff) which the prudent 17th century gentleman should use to assess the credibility of empirical claims. So evaluated, the acceptability of a claim would depend to a great extent, upon how closely the person who made it conformed to the picture of a gentleman set out in the courtesy literature and related ethical and political writings. And it would also depend upon whether the claim was made in a way which met the requirements of

genteel conversation.

Shapin's development of his thesis features a number of meticulously described and engrossing case studies. In one, Boyle resolved an apparent incompatibility between travelers' descriptions of icebergs and received hydrostatical principles by devising a story which allowed both to be accepted without contradiction. Shapin thinks an important reason for resolving the dispute this way instead of by rejecting the travelers' accounts as hydrostatically implausible was the gentlemanly authority of the travelers (247-258): "Factual testimony from gentleman-philosophers...was almost never gainsaid in the public forums of seventeenth-century English science" (124).

In a second case, the testimony of some deep sea divers who claimed not to have felt crushing pressure underwater conflicted with the principle that parcels of a body of water should press upon one another no matter where the body they belong to is located. Shapin claims that instead of trying to reconcile principle with testimony as in the first case, Boyle and others rejected the divers' testimony because, unlike the travelers who described the icebergs, the divers were too ungentle to merit trust (258-266).

Shapin uses a third case study—the Royal Society's handling of a controversy between the astronomers, Hevelius and Auzout, over the position and trajectory of a comet observed in the mid 1660s—to argue for the similarity between scientific discourse and conversation. Neither disputant met all of the conditions for gentility. But they belonged to and had powerful friends in the Royal Society. Because of this, and because it wanted to maintain its access to the data Hevelius and Auzout could provide, the Royal Society had a strong interest in managing the dispute so as to minimize enmity among their partisans, and to preserve their reputations as trustworthy observers. Shapin thinks the explanation of complicated social and theoretical maneuvers (including the treatment of a proposal that the most troublesome of the observations were sightings of different comets) by which the controversy was diffused (266-291) lies in the relocation to the scientific setting of conversational practices historically "...adapted to protect the reliability of testimony and the integrity of the moral order...in gentlemanly society..." (125). Here and elsewhere the task of preserving scientific conversation itself not only took precedence over "...the attainment of more, more exact, or more powerful truth" but shaped the

conversationalists' standards for factually acceptable empirical claims (309).

Shapin's book makes it hard to doubt that a sociological account of trust can be of great value to philosophers, and historians, as well as sociologists of science. Its value derives from the important fact—well illustrated by Shapin's case studies—that isolated individuals typically cannot find, establish, or promulgate answers to important scientific questions and that the development of scientific knowledge is the work of groups of people. (On pain of trivializing Shapin's project, we must avoid confusing this version of the claim that scientific knowledge is a group product with the wholly uninteresting version which can be established on the cheap just by thinking of truths as claims which groups accept and appealing to the banality that it takes the work of groups of people to get claims accepted by groups of people.) But although the sociology of trust can be quite important to the study of science, it's worth asking whether Shapin hasn't overemphasized the importance of "connections between the identity of individuals...and the credibility of what they claim" (126) and the influence of trust in people on assessments of the reliability of their empirical claims. Here are some questions about this.

First, Shapin's theme leads him to ignore how important confidence in experimental and observational equipment and in techniques for their employment can be to assessments of the reliability of claims they are used to establish. Consider the discredited divers. Shapin observes that the results of experiments in which bottles were lowered into the sea (e.g., implosions of glass bottles at 40 fathoms) counted more heavily for the claim that water presses upon itself than the testimony of the divers who said they felt no pressure counted against it (264). Was the non-gentility of the divers an important reason for this? Quite apart from any questions concerning the reliability of the divers or the truth of their testimony, it's plausible that scientists believed that what happens to submerged bottles was a clearer and more reliable indication of whether water pressed in upon itself than the experiences (no matter how accurately reported) of submerged people. Why in general should confidence in empirical claims derived from the employment of reliable instruments, experimental designs, etc. have to be explained mainly in terms of the "identities of individuals"? (For more on this in connection with recent science, see Peter Galison, **How Experiments End**, and Alan Franklin, **Neglect of*

Experiment*, and *Experiment, Right or Wrong*.)

Secondly, it's conceivable that people relied upon some of the empirical reports Shapin considers because of external constraints which encouraged truth telling and discouraged speaking falsely rather than because of the character of the reporter. The constraints built into the work situations of Shapin's non-genteel "invisible technicians" are an example (Ch. Eight). Other reports might have been relied upon not so much because of the identities or situations of the reporters as because of perceptual and cognitive psychological factors which made mistakes unlikely. Thus Boyle's assistants describe some things which cognitively and perceptually normal human beings typically don't get wrong (385). Here it's worth considering Annette Baier's discussion of reasons other than trust for relying on others (see: "Trust and Anti-trust" in Baier, *Moral Prejudices*).

Thirdly, Shapin documents the importance of the plausibility of what is reported to the assessment of the reliability of the report. There are obvious connections between plausibility and trustworthiness; someone who wishes to establish a reputation for genteel trustworthiness is well advised to try to avoid making implausible claims. More interestingly, there is a matter of weighting. A prima facie implausible claim delivered in a manner which "...inspires a just confidence" by someone with the genteel virtues of "integrity and disinterestedness" (212) will get a better hearing than the same claim delivered in a boastful, wonder-mongering, pedantic, or otherwise indecorous manner (221) or if it came from the mouth of someone whose personal vices, interest in a particular outcome, or susceptibility to the influence of others argue against his veracity (223 ff.).

But plausibility has to do with the likelihood of what is reported, and likelihood can depend upon biological or physical probabilities; it's implausible, e.g., that an out of shape sixty year old should high jump over 7 feet, or that Santa Monica Bay should freeze over. It can depend upon on psychological or sociological factors; it's implausible that a British two year

old should master ancient Greek, or that a woman should be elected president of the Royal Society during the 17th Century. And so on. This makes it natural to ask to what extent assessments of plausibility are influenced by facts of nature and other factors Shapin tends to downplay. Consider the astronomical controversy. What caused trouble was not Hevelius's observations, but the implausibility of a comet's trajectory which he calculated from them. This implausibility derived from later observations which—if they were observations of the same comet—would require Hevelius's trajectory to continue in a physically improbable way. In light of this, the suggestion that the comet observed later on was not the same one Hevelius saw seems plausible for reasons which don't sound particularly sociological. The Royal Society's concern for its continuing relations with Hevelius and Auzout must have influenced its treatment of the dispute. But was this as important a factor as the astronomical and mathematical considerations which friends of the received view of science would emphasize?

Many features of the science Shapin studied are idiosyncratic to 17th century scientific practice. But a tantalizing epilogue suggests how the morals of this book might apply to current scientific practice. Shapin speculates that even if present day "outsiders" treat scientists with ungentle skepticism of the kind which Merton thought was essential to scientific practice, small "communities" of scientists might nevertheless behave in something like the manner of the Royal Society (409-417). This is a fascinating idea. Its implementation would require an account of what sort of social unit a "community" is, and an investigation of the epistemic significance of interactions between scientists working on common problems in competing laboratories or research groups as well as scientists working together in the same groups. I hope Shapin will write something about this. But whether or not he does his *Social History of Truth* is one of the finest of science studies books, and a model of how intellectual history should be done.

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Citation: James Bogen. Review of Shapin, Steven, *A Social History of Truth: Civility and Science in Seventeenth-Century England (Science and Its Conceptual Foundations series)*. H-World, H-Net Reviews. June, 1995.

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