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 sociol-
ogist 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 hydrostatic 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 to 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, won-
der-mongering, pedantic, or otherwise indecorous
manner (221) or if it came from the mouth of
someone whose personal vices, interest in a par-
ticular 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 im-
plausible, e.g., that an out of shape sixty year old
should high jump over 7 feet, or that Santa Moni-
ca Bay should freeze over. It can depend upon on
psychological or sociological factors; it's implausi-
ble that a British two year old should master an-
cient 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 influ-
enced 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 ob-
servations 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 ob-
served later on was not the same one Hevelius
saw seems plausible for reasons which don't
sound particularly sociological. The Royal Soci-
ety's concern for its continuing relations with
Hevelius and Auzout must have influenced its
treatment of the dispute. But was this as impor-
tant a factor as the astronomical and mathemati-
cal considerations which friends of the received
view of science would emphasize?

Many features of the science Shapin studied
are idiosyncratic to 17th century scientific prac-
tice. But a tantalizing epilogue suggests how the
morals of this book might apply to current scien-
tific practice. Shapin speculates that even if
present day "outsiders" treat scientists with un-
genteel skepticism of the kind which Merton
thought was essential to scientific practice, small
"communities" of scientists might nevertheless be-
have in something like the manner of the manner
of the Royal Society (409-417). This is a fascinating
idea. Its implementation would require an ac-
count 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 to-
gether 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 in-
tellectual history should be done.
If there is additional discussion of this review, you may access it through the network, at
https://networks.h-net.org/h-world


URL: https://www.h-net.org/reviews/showrev.php?id=82

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