

LAUNCH PERSPECTIVE

Why the public ought to understand science-in-the-making

Steven Shapin

In early modern society people tended to live where they worked. The miller lived at the mill, and when you paid a call on the miller you visited the place where water-power turned the wheels that transformed corn into flour. One of the distinguishing marks of modern society is the radical disjunction of residences and workplaces. The place of work is set aside from home: people leave where they live to go to where they work. Typically, corn is now ground in large mills, and when we visit the people who work in the mills we no longer see the process in action. Workplaces thus take on a double specialization: they are set apart in space and in knowledge. Many people who go to work in mills, but few who do not, now know how flour is made.

The analogy with science-making is both appropriate and pertinent to thinking about the public understanding of science. In seventeenth-century England, for example, such practitioners as Robert Boyle lived where their laboratory was. When you paid a call on Boyle, you visited the place where technicians cranked the piston of the air-pump up and down and where knowledge of the physical properties of the air was produced. And, although for practical reasons the seventeenth-century laboratory might be at the back of the house, discussions about scientific findings and theories typically took place in the public rooms of the residences occupied by public persons. In modern society, however, despite various characterizations of science as 'public knowledge', it gets made and evaluated in some of our most private places. If you want to pay a call on a friend or neighbour who happens to be a high-energy physicist, you do *not* wander into CERN.¹

These circumstances create as well as express a quite fundamental problem for the place of science in a society with democratic pretensions or aspirations. Scientific workplaces are modern power-houses. The public is repeatedly told that by trustworthy sources; and on that basis the public is asked or told to pay up. Whereas consumers of bread can, by buying this or that sort of loaf in the market, affect the working of the flour-mill, there really is no public market for most varieties of scientific knowledge. Decisions to pay for this or that type of science are taken at many arms' lengths from the—anyway impoverished—forums of public debate. We, and even our elected political representatives, have to take scientific goods on trust. (And before we too smugly assume that 'the public' are 'the poorly educated' we should remember that we are all—particle physicists and bio-engineers included—'the public' with respect to the knowledge produced in the laboratory across the street.)

There are great public benefits in these arrangements. The division of labour and the segregation of workplaces enable expertise to accumulate and to work with minimum hindrances. The removal of sites of science-making from public spaces is a price we pay for the smooth functioning of those places and the proliferation of goods we enjoy through their efficient working. Only a fool would want to tear down the walls comfortably housing a goose laying so many golden eggs.

But the same arrangements also present great potential drawbacks for a democratic polity and for the scientific community. I want to suggest the importance of enterprises that seek to make the walls of scientific workplaces a little less impenetrable and opaque. I think that traditional exercises which seek to inform the public about *what scientists know* are undoubtedly important. Yet, *in so doing*, I think that another aspect of public education tends to be neglected, and this is a commitment to tell the public *how, with what confidence, and on what bases, scientists come to know what they do*. This can be achieved by telling people what science is like *in the making*.

I do not have in mind the fables about 'the scientific method' so beloved of textbook writers. I mean that we should seek to find ways of introducing the citizens of democratic societies to the work-world of science-making. We should find ways of showing and explaining to them such things as: the collective basis of science, which implies that no single scientist knows all of the knowledge that belongs to his or her field; the ineradicable role of trust in scientific work, and the consequent vulnerability of good science to bad practices; the contingency and revisability of scientific judgment, and thus the likelihood that what is pronounced true today may, without culpability, be judged wrong tomorrow; the interpretative flexibility of scientific evidence, and the normalcy of situations in which different good-faith and competent practitioners may come to different assessments of the same evidence.

No doubt, some scientists will react to these suggestions with horror: the house of science simply *must be* spruced up before visitors arrive. And yet I suspect that it isn't so much scientists as their self-appointed academic defenders, basking in the reflected aura of scientific prestige, who display shock at such ideas. The scientific community has little to fear from a disinterested inspection of its premises. However its intellectual products are made, these are some of our culture's most valued goods. Science does not need methodological fairy-tales to defend it, since whatever means it uses to produce those goods will naturally come to be highly esteemed. Science has little to hide, and, if there are things that cannot stand up to informed public scrutiny, then so much the worse for those who insist they be hidden.

There are also positive reasons why the public understanding of science ought to include 'warts-and-all' pictures of the process of knowledge-making. Since both the support of scientific research and the credibility of scientific pronouncements may be in the public's hands, it is not to anyone's benefit that the public believe what is untrue about how science is made and about the status of current knowledge. Take, for example, current debates over the greenhouse effect. This is an entirely typical episode of modern complex science in which public assent is vital to practical interventions. Yet scientists publicly *disagree* in their assessments of what the evidence means and, indeed, of what the evidence is.

What the public understands about such episodes will depend upon what appreciation they have of the processes by which science is made. Some fairy-tales would have the public believe there is a universal efficacious scientific method which sorts out good from bad data and confirms or disconfirms scientific theories. To the extent that the public labour under such apprehensions they will have little choice but to pick

among these conclusions: (a) that one lot or another of scientists is incompetent, or lying, or in the pay of special interest groups like the nuclear-power industry or the Confederation of British Windmill Manufacturers; or (b) that the area concerned is not science at all. Having been told that scientists possess a magic wand, the public may well react with cynicism or disillusionment to such entirely normal displays of contingency and uncertainty. In whose interest is that? I do not think that modern scientists want to be understood as magicians or priests or gods. People can get very angry when their gods turn out to be human. In democratic societies it is always a sound instinct to trust the people with the truth—even if some work has also to be done to overcome institutionalized idealizations. And in this case the truth is that scientists are neither more nor less than our best current experts in their domains. Their workplaces have got all the clutter of your favourite mechanic's garage, and he is the man you trust with your car.

Over the past fifteen years or so sociologists of scientific knowledge have produced a body of theoretical and empirical work which is dedicated to understanding science in action, and, therefore, highly suitable to the public task of opening up the house of science. In my opinion, this research has yielded some of our most detailed and reliable understandings of how scientists interrogate the natural world, relate to each other, and seek to secure credibility for their claims. One hopes that any enterprise committed to telling the public what science-in-the-making is like would familiarize itself with work by such sociologists as Barry Barnes, David Bloor, Harry Collins, Bruno Latour, John Law, Donald MacKenzie, Andrew Pickering, Trevor Pinch, Leigh Star, *et al.*²

I have sometimes heard it said (and, more often, implied) that work of this sort is 'anti-science'. I suspect that whoever says this either has not really read much work in the area or is attempting tactically to identify 'science' with some locally-cherished, but ultimately irrelevant, academic 'apology for science'. Sociology of science is too diverse a discipline to permit facile generalization. Perhaps I ought to speak for myself. I am an historian employed in a sociology department. I try to understand science-making largely because, like my friends in the natural scientific community, I am curious about how things work. How is scientific knowledge made? How do scientific claims come to be authoritative? I do not have a brief to defend science, just as geneticists do not have a brief to praise DNA. Nor is it possible for me to imagine how I *could be* against science. On the one hand, the products of science are so important in providing me with my sense of the real and the true; on the other, I cannot be against a process, whatever it is, that yields these goods and whose nature is anyway so hard to characterize.

It is, of course, possible that the moderate opening-up of the house of science which I propose might cause scientists some inconvenience, though I would think only a very slight one. Scientists aren't the only practitioners who want privacy while their public performances are being prepared: any restaurant kitchen would yield exactly parallel attitudes. Inconvenience to scientists would be regretted. But public ignorance, and public idealizations, of science-in-action also have costs. And these costs, I suggest, will ultimately be paid by the scientists whose support and credibility in democratic societies depends upon public understanding.

References

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- 2 Barnes, B., 1974, *Scientific Knowledge and Sociological Theory* (London: Routledge & Kegan Paul); Bloor, D., 1976, *Knowledge and Social Imagery* (London: Routledge & Kegan Paul); Collins, H. M., 1985, *Changing Order: Replication and Induction in Scientific Practice* (London: Sage); Latour, B., and Woolgar, S., 1979, *Laboratory Life: The Social Construction of Scientific Facts* (Beverly Hills, CA: Sage); Law, J., and Lodge, P., 1984, *Science for Social Scientists* (London: Macmillan); MacKenzie, D., 1990, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (Cambridge, MA: MIT Press); Pickering, A., 1984, *Constructing Quarks: A Sociological History of Particle Physics* (Edinburgh University Press, University of Chicago Press); Pinch, T. J., 1986, *Confronting Nature: The Sociology of Solar-Neutrino Detection* (Dordrecht: Reidel); Star, S. L., 1985, Scientific work and uncertainty. *Social Studies of Science*, **15**, 391–427.

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