Discussion

The Changing Social Authority of Science

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Writing nearly forty years ago, Robert Merton suggested that 'few sociologists of the twentieth century [can] bring themselves, in their work, to treat science as one of the great social institutions of the time' (1973: 286). I think Merton was broadly correct in this allegation of negligence. The intellectual influences, as well as the more apparent material products of science and technology, are of central importance to the character of the whole century. Yet the institutions of science have attracted relatively little sociological and historical interest. Indeed, special neglect seems to have been reserved for what one might call the big questions - notably, the question of which forces influence the social standing and authority of scientific knowledge in the Western world.

Accordingly, my aim in this paper is to examine the changing social authority of science, particularly in the UK and USA. That its authority is changing can be indicated in many ways. For example, it is now common to hear public dismissals of science. In part, these are motivated by critiques of the scientific world-view emanating from 'political ecology', focusing either on the alleged alienating effects of the way that scientific objectivity obliges us to stand apart from the natural world or on the 'collusion' of science and technology in environmental destruction. There have been sustained criticisms also from sections of the feminist movement, focusing on the supposed cultural bias of 'malestream' science and technologies. Rather more anecdotal evidence suggests to me that the neutrality of science is increasingly questioned. I have been teaching philosophy and sociology of science for thirteen years or so and my experience is that there has been a change in students' assumptions about science. Where once they generally took science to be epitomised by disinterested investigation, they now increasingly view it as typically serving vested interests; their model of the typical sci-

entist seems to have switched from Newton or Galileo to a scientific employee of British Nuclear Fuels. My final piece of evidence for scientific authority having suffered a change comes from the scientific community's own response, as represented for example by activities such as the public understanding of science initiative, which reflects anxiety about this issue. The PUS movement is aimed at promoting public interest in science and reasoned acceptance of scientific authority; such activities are underlain by the assumption that science's well- deserved authority is in decline.

In this paper I aim to draw attention to two sorts of features of science which I believe have been integral to the changing fortunes of science and technology and which have been illuminated by social analysts of science. Some in the audience may find this strange since the social studies of science is presented by certain authors from the natural sciences (notably by Gross and Levitt) as part of the problem. Particularly in the USA, natural scientists have reacted indignantly to seeing colleagues at their universities teach that science is in some sense a 'cultural construct' or that science is gendered. I don't want to enter those troubled waters, though much of what I have to say is inspired by the sociology of scientific knowledge. Instead I want to apply some findings of the sociology of science to understanding science's changing social authority.

In dealing with the perceived decline in the standing of science it is important not to focus on science to the extent that the declining social authority of other social institutions is ignored. Accordingly, it is important to observe that individualistic, consumerist societies encourage a general decline in deference, a growth of irony and a waxing cynicism. With mounting expectations about the 'rights' of the individual and her or his pursuit of personal fulfilment, with social dislocation disturbing established hierarchies, and with the disclosure of 'sleaze' in a variety of institutions which were once accorded automatic respect, modern citizens are less willing to defer to authority in a way which was once commonplace. This is all the more so when the authorities' views conflict with people's favoured beliefs or perceived interests. And there is no doubt that this general process is in part the reason for the decline in the authority of science.

However, the standing of each public institution also follows its own particular trajectory. And in the light of the scientific community's explicit attempts to boost the public understanding of science I think it is helpful to look at two special considerations in the case of science. My suggestion is that we can find part of the answer in the workings of science and part in the 'contract' that science has with society.

The Workings of Science

I am far from alone in suggesting that we focus on the workings of science. It is quite common for spokespersons for science in the PUS movement to find a reason for public misunderstanding in the nature of science itself. However, the key variable they tend to focus on is what one might call the 'provisionality' of science. Spokespersons such as Dawkins insist that one of the characteristics of science is that scientific knowledge is never certain. In essence this is the point, famously associated with Popper,

about the inductive, rather than deductive, nature of empirical science. We can never know for sure that the next instance we see will not behave in some unexpected way, undermining our grand generalisations; hence we can never be certain that scientific beliefs are correct. This point lay at the core of Popper's recommendation to scientists to make bold conjectures and then to try to refute them. The argument which can be developed at this stage is that politicians or policy makers pretend to greater certainty about scientific evidence than is justified, and accordingly over-egg the pudding of public expectation. New, countermanding evidence will accordingly be a big disappointment to the public - fed on talk of certainties - and may breed disillusion with scientific expertise, when - in fact - refutations are exactly what scientists do (from time to time) expect.

Without wanting to dismiss this point about the provisional nature of science entirely, it seems to me less telling than is commonly thought. In part this is because the 'uncertainty' which is admitted is of a special sort. Scientists don't straight away respond to apparent refutations by giving up their cherished theories. They weigh the apparently disconfirming evidence against the supporting evidence, assess the source of the evidence and so on. Hence, Dawkins can express himself with a great deal of certainty against Creationists and not be troubled by the in-principle provisional nature of evolutionary theory. My second reason for down-playing this point is that two other aspects of scientific practice seem to me much more important: trust and judgement.

These two characteristics might not be thought of as the typical features of science and it will be necessary to take a little time to explain what I mean. Taking trust first, my point is that scientific activity depends on trust in people and in assumptions. Despite the business of peer review and the formal methods learnt by scientists during their long training, the scientific life turns on trust. In particular, scientists cannot independently check every detail of every claim made by every other relevant scientist, even during a controversy. Published findings in high-quality journals are generally treated as trustworthy. Neither do scientists personally re-examine the foundations of their discipline, check out atomic theory or the evidence for continental drift. They also 'trust' machines. Scientific observation is more and more performed though the medium of complex machines, yet the design and operating principles of those machines are not exhaustively checked by each scientist who uses them. Even when scientists 'replicate' their colleagues' or their opponents' experiments, they cannot be sure that the replication is precise in every detail (indeed, logically speaking, one might say that a replication can never be fully 'the same' as the original). At the forefront of the creation of new knowledge no-one may fully know what factors are going to be influential: measurements are often being made at the very limits of sensitivity of apparatus or at the very edges of computing capacity so that minor differences in the configuration of equipment may play a big role.

Thus, though it is clearly correct to say that science has a sceptical character - advances in science are usually made by doubting what was believed before - it

is at the same time true that science depends on trust. Further evidence for the importance of trust comes from two sorts of sources.

Firstly, the routine importance of trust can be understood by looking at the special circumstances of controversy. Under normal conditions all of the things mentioned above (the functioning of apparatus, the reasonableness of peer reviewers and so on) are usually taken on trust, but in a controversy each can be open to doubt - long-held assumptions get called into question; the trustworthiness of other scientists and even of the peer review system itself can come to be doubted. Once this process begins, the possibilities for distrust and doubt begin to expand exponentially. Just as philosophers have tended to use the special circumstances of controversy as a laboratory for studying scientific argument, one can say that the study of controversies exposes the deep reserves of trust on which ordinary science is based.

The second source is historical. Shapin (1994) has recently made the case that the founding of key institutions of modern science in seventeenth- and eighteenth-century England was crucially dependent on newly consolidated conventions of trust and civility. The willingness to take as authoritative experimental findings whose production one had not personally witnessed, and to value those findings over learned teachings from traditional sources, depended on the acceptability of the word of the gentleman. Historically speaking, trust is the basis of the scientific community.

The second characteristic of the working of science I want to point to is the role of skilled judgement and interpretation. Although science is methodical, the ordinary life of the scientific community demands that people exercise judgement. Scientists have to decide which readings to ignore because of gremlins in the equipment; they have to decide which papers to read and whose interpretations of findings to take most seriously. Again, the fundamental importance of the operation of this aspect of science can be seen in controversies. Advocates of both sides in a scientific dispute typically see what the options are; all participants are familiar with the principal claims being made. But they evaluate them differently. They may weight the various kinds of evidence being presented differently, they may hold conflicting opinions about the reliability of the types of equipment people are using, they will typically differ in their estimations of the dependability of various scientists' works, or they may feel their own experimental or field-observation skills are under-rated by their opponents. As in any area of skilled work, judgement is indispensable. This point was emphasised some fifteen years ago by Thomas Kuhn when he wrote:

> When scientists must choose between competing theories, two men fully committed to the same list of criteria for choice [such as simplicity or scope] may nevertheless reach different conclusions. ... With respect to divergences of this sort, no set of choice criteria yet proposed is of any use. One can explain, as the historian characteristically does, why particular [scientists] made particular choices at particular times. But for that purpose one must go beyond the list of shared criteria to characteristics of the individuals who make the choice (1977: 324).

I have offered these two points in a neutral tone. The advance of scientific knowledge depends on the exercise of

judgement and on the workings of trust. Expressed in this moderate way the points may seem uncontentious, though I believe they have been overlooked by most commentators. I am not saying that this is all there is to science, or that science boils down to mere judgement and trust - simply that trust and judgement are vital ingredients of the scientific life and that their presence is important to the changes in science's social authority.

Why do Trust and Judgement Matter to the Social Authority of Science?

Having sought to establish that trust and judgements are integral to the internal operation of science, these factors can be now used to help us understand difficulties with the social authority of science. Let us take trust first. Basic science has to proceed on the basis of trust and on procedures which minimise distrust. For example, poorly thought-of articles are seldom ridiculed; rather they are ignored. If trust is important to relations within science, then we can also expect it to matter in relationships between science and its publics. However, the public often encounters science in contexts in which trust is already an issue and where it is quite possibly already endangered. In the majority of cases the science which matters to members of the public matters to them because of the practical context within which the knowledge is to be used.

For instance, people may worry about the hazards of a factory close to them or about the wisdom of disposing of waste by burning it in an incinerator near to their home. They do not encounter science in a contemplative or disinterested manner but in relation to practical projects and to the agendas of various companies, government departments or public bodies. Scientific evidence is presented as part of a campaign to get people to eat meat, drink milk, adopt a specific diet or to use a certain toothpaste. Accordingly, in controversial and contentious matters, members of the public may already be of a mind to distrust the scientists and to ascribe ulterior motives to them (perhaps because scientists are in the pay of the company itself, as happens with the present arrangements for Environmental Impact Assessments). This will make them critical of any background assumptions, specialised jargon, conventional practices or rules of thumb which scientists use. We should not rule out the possibility that, sometimes, citizens may actually have good grounds for distrusting scientists. But even when they don't have especially good reasons, a context of distrust ensures that some reasonable-looking grounds can be found. Furthermore, since science can often be encountered in such adversarial contexts as public inquiries and law suits, there is an interest in picking the other side's arguments to bits, in finding grounds for controversy even when these have subsided by common consent within the scientific community.

These problems with trust are magnified by difficulties over judgement. Particularly if scientists have publicly played up the methodical character of their knowledge, members of the public may not regard it as legitimate when scientists cannot exhaustively say why they regard a specific diagnostic test as adequate or why they accept statistical evi-

dence at the P = 0.0001 level, and so on. If science is methodic then it should be possible to spell it out like an algorithm. But if that can't be done (as in practice it can't, because of the role of judgement), the public may suspect that the judgements used are tendentious. The issue of trust re-enters. Again, issues of judgement can be exacerbated by courtroom procedures as I shall discuss below.

Moreover, policy makers can sometimes allow problematic issues to be lent a spurious scientific character by having them delegated to experts. But the issues on which experts feel confident to comment may not resemble the circumstances experienced by the public. Thus, remarks about the safe limits of individual gases emitted from incinerators do not necessarily apply to the 'cocktails' of gases released from working plant. Claims about the safety of agricultural chemicals sprayed in protective clothing under test conditions may not apply to the actual conditions in which some farm workers operate. Although scientists themselves are usually scrupulous about this, scientific assessments can be a guise under which untested practices are foisted onto the public. That scientific expertise has been invoked on 'dodgy' occasions can then become a reason for routinely distrusting expertise even when it is brought to bear more sensitively.

Lastly on this point there is the question of whether the 'truth will out'. Unlike the circumstances of basic science, with most questions about the practical impact of science and technology the debate cannot be left to run and run. In the case which epitomised the official use of science in the late 1970s and early 1980s, many scientists were interested in acid precipitation. Clearly this was a complex problem and there were differing interpretations of the massive range of sometimes conflicting data. Had the scientific community been allowed to continue to collect evidence then, possibly, an overwhelming consensus would have arisen. But policy decisions had to be made and, in the end, the major European polluters agreed cuts in acid emissions from large plants of between 60 and 70 per cent, figures based on administrative rather than scientific reasoning. The same considerations now apply to expert advice over global warming. In such cases one cannot remain indifferent to the practical consequences of the issues until the truth is finally 'out'. In the case of scientific disputes over the causes of acid rain, environmentalists came to see the demands for better proof as a device for stalling action.

In sum, two central and enduring characteristics of academic, basic science are also the characteristics which make it suspect in practical circumstances. It is tempting for the scientific community to respond to these troubles by stressing the 'hardness' of science and the scientific method. But any such response, however understandable, will tend to be counterproductive since science cannot be automatic and method-based, free of trust and judgement.

Institutional Factors which Exacerbate these Difficulties

In courts and other quasi-judicial contexts the difficulties faced by science are, as a series of case studies has indicated, magnified. This has been of particular significance in the USA where constitu-

tional arrangements mean that official scientific rulings can be tested in court by the independent judiciary. The court-room scrutiny of science has proven to be surprisingly ' successful' - successful not necessarily in the sense that justice has been advanced but that science has succumbed to legal interrogation. The fate of science in court can also be understood in relation to the factors of trust and judgement. Thus, as I mentioned earlier, the circumstances of adversarial court-room interaction commonly lead to the suspension of trust. The opposing sides have an interest in throwing doubt on each others' credibility, even if they cannot replace the queried knowledge with positive answers of their own. Opposing legal teams concentrate on undermining the public credibility of their adversaries. The 'give and take' of basic science, the assumption of reasonableness, is withdrawn. The problems with judgement, too, are often heightened by the adversarial process of legal examination. Scientists are typically called on in court to offer factual determinations. If it turns out that these 'factual' statements hang on judgements and that, therefore, a different scientist might have judged the 'facts' differently, this disclosure can be corrosive of the expertise which allowed the scientists to be present in the first place. The opposing legal team therefore has a vested interest in exaggerating the role of judgement and in counterposing an individual's judgement against the ideal of purely objective, almost machine-like fact recognition.

The fact that scientific knowledge stems from the work - in the everyday sense - of scientists can be developed for the purposes of legal argument as well. The 'disclosure' in court that scientists' testimony depends on judgement can be wheeled out as demonstrating that the scientists don't speak from unquestionable truth but 'merely' from opinion. It is also common to see much play made of the fact that scientific knowledge is founded on work practices. Like everyone else, scientists have to clean their experimental equipment, make adjustments to the detectors and keep records of their actions; laboratory workers can be as slip-shod as any other employees. These incidental features which seldom make it in to formal scientific argument can be invoked in legal settings to undermine the apparent indubitability of science. Additionally, as Jasanoff has noted, the connection between scientific results and ' custom and practice' can be queried (1990: 202). If a case turns on the toxicity of a substance whose dangers have been indicated by animal experiments, it can still be questioned whether the decision to use animal evidence is itself scientifically justified. Of course, in this modern version of an ancient paradox, tests cannot themselves all be independently tested nor can methods all be methodically checked. Finally, legal examiners are not even averse to using the point about science's provisionality to try to inject some uncertainty into their opponents' testimony, though the fact that this argument is potentially available to everyone tends to restrict its usefulness in practice.

The centrality of this legal 'deconstruction' raises a further interesting point. In many cases, critiques of the authority of science in public contexts are perceived to come from the political left. For example, there is a critique of the

elitism of scientific medicine commonly associated with a policy preference for preventative approaches. Equally, there tends to be an elective affinity between the left and an anti-nuclear stance, a connection less visible on the right. One might therefore think - as Gross and Levitt appear to imply - that the critique of scientific authority is associated with the left's critique of political authority. In fact, however, commerce and business have not been shy of using exactly parallel arguments when they have wished to combat policies sanctified by scientific evidence.

This was made particularly clear in the case of the US Environmental Protection Agency which introduced far-reaching reforms in its early years in the 1970s. These were met both by lobbying from the business sector and by successive challenges through the courts from companies questioning toxicological evidence and claims about environmental harm. Whether from the left or from the deregulation-minded right, the arguments have adopted a similar structure. The scientific interpretation in question (whether of the safety of a certain level of exposure to radiation or of the toxicity of an industrial solvent or whatever) has been presented as based on judgement, a judgement tendentiously exercised.

The Response of Scientists may Aggravate the Problem

I want to suggest further that the typical response of scientists accentuates this problem because they have been (and are) encouraged to over-state the role of method. Since the origins of modern science and particularly since the professionalisation of science in the nineteenth century, scientists have pressed for more influence in society. They have presented themselves, not individually but as a body, as in possession of expert knowledge which society needs to run itself better. Scientists advise the military, manufacturing companies, health officials, educationalists, social welfare practitioners and so on. To justify this expanding influence, scientists have stressed the special character of their knowledge. While, as philosophers have lately come to acknowledge, no-one has been able to spell out 'the scientific method' in any detailed way, the idea that there is a scientific method has been widely promulgated. The benefits of this notion are clear: it sets scientists apart from other advisers and professionals who can lay claim only to unmethodical knowledge.

Moreover, the idea that scientists are the best governors of science has been used to establish unparalleled autonomy for the scientific profession: hundreds of millions of pounds are supplied by the government for expenditure on research each year, without - at least until recently - much in the way of public accountability. So, even though there are good reasons to be sceptical about the existence of *a* scientific method, scientists have had reason to promote it as the key to the special superiority of science.

Faced with challenges to the authority of their knowledge claims, it is tempting for the scientific community to respond to these troubles by stressing the 'hardness' of science and the robustness of the scientific method. But any such response, however understandable, will tend to be counterproductive since sci-

ence cannot be method-based, free of trust and judgement. Except under totalitarian circumstances, the application of scientific understanding to matters of public concern will involve some element of public participation and accordingly must depend on a measure of trust. Shutting the public out is likely to increase distrust and thus further corrode the practical authority of science.

The Changing Social Function of Science

In his compelling and popular overview of the 'short twentieth century' Hobsbawm breaks the mould of historians' tendency to overlook science. He notes for example how developments in science have been associated with cultural crises in the last eighty years. But he also draws attention to the extent to which contemporary science - even basic research - cannot avoid accusations of interestedness. Noting the contest for money and the involvement of political and commercial interests in the support of science, he observes that science has not been value-neutral:

> as all scientists knew, scientific research was *not* unlimited and free, if only because it required resources which were in limited supply. The question was not whether anyone should tell researchers what to do or not to do, but who imposed such limits and directions, and by what criteria (1994: 556).

The 'co-opting' of science to economic and political goals is at the heart of his analysis of the precarious situation of the ideal of science at the close of the twentieth century. I consider that his analysis is correct, but only in part. His summary reveals what he overlooks:

> All states therefore supported science, which, unlike the arts and most of the humanities, could not effectively function without such support, while avoiding interference so far as possible. But governments are not concerned with ultimate truth (except those of ideology or religion) but with instrumental truth. At most they may foster 'pure' (i.e. at the moment useless) research because it might one day yield something useful, or for reasons of national prestige, in which the pursuit of Nobel prizes preceded that of Olympic medals and still remains more highly valued. Such were the foundations on which the triumphant structures of scientific research and theory were erected ... (1994: 557).

His observation about the basis of the 'deal' between science and the state is an important point, but I believe he neglects the extent to which the instrumental truths at which science and technology are aimed are themselves undergoing a change.

In the immediate post-war decades, the primary social role of (and justification for) science was to increase productivity and competitive performance, whether economic, military or medical. But, as Roqueplo and Beck among others have noted, there has begun to be a switch away from science being seen as a way of increasing production to a view of it as a means of handling risks and of achieving regulation. Of course, much R&D is still aimed at innovative products and processes. But, to take an extreme example, scientists are now nearly as likely to be advising politicians on the health risks arising from BSE ('mad cow' disease) as they are on ways of increasing agricultural productivity. This growing regulatory role (sometimes referred to as the 'expertisation' of science) places increasing, and increasingly unrealistic,

demands on science since questions are more likely to be publicly raised over trust and judgement in regulatory disputes than they are in relation to the development of innovative products. If designers want racing cars to go faster or aeroplanes to carry more passengers, then there can be legitimate differences in the way that such performance is measured. One car may go faster flat out on the straight and another corner more quickly; one plane may carry lots of people on short hops, another transport large numbers across continents. Both cars can claim to be fast, both planes claim to have a huge capacity. Very little hangs on proving, let alone proving to the public, which car is 'really' fastest, especially since a race-track will have both bends and straights.

However, in the matter of ruling out risks, pressures exist to prove which pesticide is safest, which disposal method for oil platforms is the least environmentally harmful and so on. Moreover, these proofs have to be offered in public forums where various interest groups have a legitimate role. The task now facing science, as well as the context in which that task has to be performed, place new, more exacting demands on scientific knowledge.

Conclusion

Though it is always too easy to suppose that there is something special about the present day, I believe I have given grounds for accepting that the social authority of science is today facing a uniquely difficult challenge. In some sense the points I have raised about the fragility of science in public contexts are timeless. Trust and judgement have always characterised the scientific enterprise. But their insistent appearance nowadays relates to recent changes in the institutional challenges to science (in particular from the US courts) and to alterations in the social role of scientific expertise. On top of this, these corrosive tendencies have a self-perpetuating character. Once public trust in the scientific community is threatened, that very lack of trust jeopardises the future acceptability of scientific expertise. In that sense, if my analysis is correct, there is no easy way to reverse this decline.

Two things do however suggest themselves. The first is simply increased recognition of the new regulatory role of science. The social role played by science has changed in recent decades, and greater public debate about the new demands on science can only be helpful to the public understanding of the role of expertise. Second, it may be worthwhile looking at ways of building public trust in the institutions of science. Over the last seventeen years successive Conservative governments in the UK have accustomed us to the idea that business people should sit on the Research Councils. Though many commentators have expressed concern about this development and the implied commercialisation of British science, these changes could be taken in another light. They could instead be seen as pre-figuring greater public participation in science - with citizen participation not only on the research councils, but for example on university research committees. I don't propose this as an immediate solution, but as an indication of trust-building institutional arrangements which may offer the best prospect for a socially-respected science.

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