✦ Counterpoint

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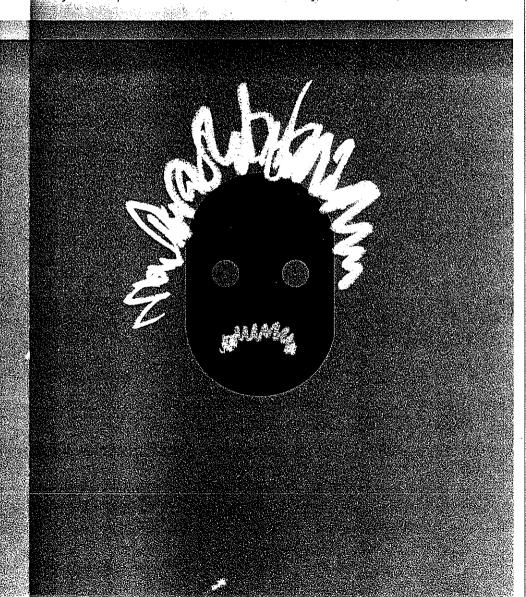cut new channels for dialogue.

Trust me,
I'm a scientist

...thday Counterpoints

Pervez Hoodbhoy, Daniel Glaser, Steven Shapin

Why should we trust scientists? Is there something
about them, or the way their minds are trained to
work, that makes them inherently trustworthy?
What happens to trust when scientists are seduced
by politics, religion, money or glory? Public trust in
scientists may be waning – is this part of a long-
term shift in public consciousness – the growing
distrust of lawyers and journalists and naïve
confidence in television stars? Or does it tell us
something important about our societies and our
world – that the rationalism of the Enlightenment is
on the wane?

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I'm a scientist

Pervez Hoodbhoy, Daniel Glaser, Steven Shapin

With an introduction by
Lloyd Anderson

Series editor
Rosemary Bechler

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The way we trust now: the authority of science and the character of the scientists

Steven Shapin

Many scientists are seriously concerned about what they take to be a crisis in public confidence. For example, Hugh Grant – zoologist and CEO of Monsanto – who told the *Financial Times* this June that we're rapidly moving from a 'trust me' society to a 'show me' society:

> 'A "trust me" society is a paternalistic society that says, "don't worry about it, it'll be fine, I know best, we have your interests at heart, this will work". The "show me" society says, "I might not understand the data – and that's OK if I don't – but I want to know that I can access it at any time, and I want to know that my views are relevant to this debate".'

Monsanto had to take into account both the reality and the consequences of this shift in sentiment, not least because, as Grant said, they'd 'bet the company' on the success of genetically modified foods, and the public wasn't swallowing them as the company, its scientists, and its shareholders would like.

Grant's views are familiar. It is not hard to find any number of similar expressions from leading scientists, especially, but not exclusively, in biomedicine, biotech, molecular biology, nanotechnology, and in energy companies. The public, it is said, has grown sceptical, sour, and unappreciative; scientists are subject to more vigorous surveillance than they have been used to; and their disinterestedness is no longer taken for granted.

Whether the issue is fraud or commercial bias, scientists have grown uneasily accustomed to both internal and governmental instruments checking their work to make sure it is on the level. Grant's conclusion was just that scientists had better get used to these fundamentally new circumstances, do their best to explain themselves, and open up the house of science to public inspection. Such sentiments are linked to a widespread condemnation of the 'public misunderstanding of science': if the public knew more science, it is often asserted, they'd trust scientists more. In any case, and with whatever consequences, Grant's strong formulation of the decline of a trust society surrounding science is widely shared.

Widely, but not universally. Writing a few months before in *The New York Review of Books*, Richard Horton, editor of *The Lancet*, responded to the same public concerns about the 'corruption of science' that were to occupy Grant. But, for Horton, the 'escalating corrosion of values' in biomedical science is a reality. It derives from structural changes that occurred in the 1970s and early 1980s in the commercial relations of academic science and, more fundamentally, in the commercial possibilities of science. The public and its representatives are indeed worried about the effects of these changes on the integrity of science, but scientists themselves, Horton thinks, ought to be more worried, as they have fouled their own nest.

However, Horton differs from Grant in two interesting respects. First, he does not accept that a 'trust society' has been wholly dissolved, or, more interestingly, that it even could be so dissolved. We might say that trust has been compromised, but cannot intelligibly say that a trust society has been replaced by a surveillance society. If there were no trust in scientists, you would not get more reliable scientific knowledge; you would get no knowledge at all. For Horton, this means that the integrity of

scientists is fundamental: it cannot either be improved or replaced by an apparatus of control. Remove grounds for believing that scientists hold themselves to a different moral order and you really will dissolve scientific knowledge and whatever public authority it might have. Even if, Horton writes, 'personal virtue has indeed given way to impersonal expertise, and if moral character has become secondary to institutional prestige, it would be wrong to conclude that the connection between public trust and the integrity of the individual scientist has been wholly erased'. Nor could it be.

Second, Horton does something rare for a scientist or physician in these connections: he seeks to comprehend the way we live now and to appreciate present risks and opportunities by learning something from history. I should immediately declare an interest here, since the history Horton turns to is mine – specifically a book I wrote ten years ago on the relationship between ideas of gentlemanly honour and intellectual authority in 17th century England.[1] Horton uses history to orientate us to long-standing problems in the relations between commercial concerns for property and scientific imperatives towards openness: '[Shapin] argues that our personal knowledge of the world depends to a large degree on what others tell us. Our understanding therefore has a moral character, based as it must be on trust. In constructing a body of reliable individual knowledge, trustworthy people are crucial. Secret scientific knowledge and commercial exploitation of discoveries thus have a long and much-abhorred history within science, whatever scientists might claim in order to justify themselves today.'

I want to repay Horton's constructive use of history by telling him some more history that he, other concerned scientists, policy-makers, and lay people might find interesting. If the imputed integrity of scientists really is central to the authority of scientific knowledge, then how and why did our culture apparently give up the idea that scientists were morally different from anyone else? Get some kind of

grip on the history of this cultural change, and perhaps – as in the case of public trust in science – we can better understand our current predicament, what brought it into being, what sustains it, and what possibilities there might therefore be for change. In particular, I want to suggest that the shift from Grant's 'trust me society' to a 'show me society' is, and can only be, partial. There are matters of principle here, and it is necessary to start by giving some account of them.

The necessity of trust

In what does the public authority of scientists consist? An obvious answer is that authority flows from expertise. Who knows more about heredity than a geneticist, more about rocks than a geologist, more about how your heart works than a cardiologist? Knowing more is associated with power to do more, and, even if the public care little about the scope and depth of knowledge – that's not their business – they surely care quite a lot about technique and the capacity effectively to intervene in the course of nature. Who would trust a car mechanic to fix their heart, a cardiologist to fix their car, or a well-meaning friend ignorant of both hearts and cars to fix either? Power to act is seen to derive from the scope and quality of expert knowledge. So technical expertise is central to questions about the authority of knowledge and 'knowers' in our sort of society. There is no getting around the fact.

Yet there is some difference between expertise as a feature of technical authority and as a sufficient response to questions about authority. I want, first, to be clear about what is meant by the authority of scientific expertise; second, to point out some problems attending distinctions between technical expertise and moral authority; problems associated with Grant's description of ours as a 'show me society'. Finally, I trace some moments in the historical trajectory that have produced present-day thinking about the technical and the moral, and remark on some practical difficulties that flow from that way of thinking.

There are at least three problems involved in the notion that technical expertise offers an adequate basis for public trust. To start with, expertise cannot be known directly. Trust in expertise depends first upon locating that expertise. If you know yourself to be an expert in some domain, then there is no problem for you: you are satisfied on quite direct grounds, even though you may be wrong. But in such cases there is little interest in saying that one 'trusts oneself'. In the nature of things, and for our sort of society, expertise is almost always external: it belongs to someone else and our problem is how to recognise it, access it, and mobilise it. The rationalist, individualist tradition our culture inherits from the 17th-century philosopher John Locke insists that proper knowledge is knowledge whose warrants are wholly within one's own compass: we only really know what we see at first-hand or what we can prove with our own minds.

Yet the practical consequence of embracing such individualism should be the recognition that we have scarcely any proper knowledge at all. Instead, almost all of our stock of knowledge – I would like to say all, but that would take more time to argue – is held by courtesy, through reliance on others, on the basis of authority and trust. I know that *this* particular cardiologist is an expert because I read it several months ago in *Boston Magazine*'s article on 'good doctors', and I know that credentialled cardiologists are the sort of people to go to if I've got heart problems, on the basis of a diffuse range of trusted sources, from school teachers to colleagues to printed and digital media to direct contact with general practitioners. Despite widespread current unease about a climate of mistrust in authenticated experts, the fact of the matter is that no society has ever reposed as much trust as ours in the power of expertise. That is one reason for the widespread rhetorical identification of scientists with priests or magicians. The present-day problem is not mistrust in scientists but, rather, a problem in deciding who the scientific experts really are.

It is important to avoid the tendency to think of the laity as 'other people'. Even experts are lay people with respect to the technical knowledge that lives, so to speak, next door: physicians heal themselves just on the condition that they are the right sort of specialists. Hence, in proper usage, the notion of the laity should not be used to pick out 'them versus us', taken as stable social categories; it is not a discriminator like 'working class'. Recognition of expertise one can safely rely upon depends on a prior ability to trust those that testify to where expertise lives. In our society, people come to radically different conclusions about these things; why else would we have so many people on the Atkins Diet when the American Heart Association and the USDA Food Guide Pyramid counsel so strongly against 'low-carb, high-fat' diets?

The second problem flows from this: in late modern society, technical expertise tends to speak with different voices, increasingly so as expertise extends its reach into more and more areas of political, economic, and personal concern. Psychiatric expert witnesses war with each other in court; geologists are at odds over the long-term safety of storage sites for radioactive waste; biologists disagree in public over how much mercury your body can safely carry; and, of course, you can pick whatever expert-endorsed regime you like when it comes to raising your children, being irresistible to the opposite (or same) sex, or losing weight. These things are not trivial. A very high degree of expert consensus does indeed mark the natural sciences, but only on the condition that you equate science with its textbook and classroom incarnations. The puzzle-solving, precision-seeking, domain-extending phenomena of Thomas Kuhn's 'normal science' are very real.

However, expert disagreement is both endemic and consequential in just those areas where political decisions about what we should do are involved. Whether twice two equals four, or whether DNA is the genetic substance, is not currently in the political domain. But what should be done with radioactive waste, and what is

the correct diet for safe weight loss, very much are. Expert disagreement is to be regarded not as a marginal, trivial, and transient problem – 'the truth will out in the end' – but as constitutive of science when it figures in political decision-making processes, where we always have to decide what to do before 'the truth comes out', before knowledge consolidates enough to put in a textbook and test children on it. You might just as reasonably say that 'the essence of science is disagreement' as stress the defining role of consensus. It depends upon the scientific arena you look at. It follows from expert disagreement that trusting experts necessitates a decision about *which* experts to trust. When the expert community itself hasn't come to any settled view about the identity of genuine expertise, it is asking a lot to expect the public to do so.

Third, there is a point of principle involved in the move from knowing more – which is a technical matter – to doing the right thing – which is a moral and political matter. As the American biologist David Starr Jordan once said: 'Wisdom is knowing what to do next, skill is knowing how to do it, and virtue is doing it.' Technical expertise, that is to say, is a necessary but not a sufficient condition for effective action to achieve the ends you might wish and think to be good. The cardiologist has to care about *your* heart as well as to know a lot about hearts; the geologist has to be acting in the public interest, rather than trying to augment the bottom line of a property developer; the expert nutritionist has got to testify to what he or she believes to be the truth, rather than attending to the profits of the beef-packing industry.

So, in order for trust in experts to have its practical grip, we have to be satisfied not just that certain individuals know more but also that they are well-intentioned, and that, if we trust them, they will try their best to do the right thing, even if – just because they are human – they cannot always bring about the right result. At a practical level, the evaluation of expertise contains within it a moral evaluation. Who are the experts, whom we can trust, as such, to do good?

Is/ought and the authority of science

At one point in our culture, there was a robust response to that question. The scientist, or, in past usage, the 'natural philosopher', was not just someone who knew more, but someone who was better than the common run of humanity or of scholars. There were several reasons why this might be believed, but one derived from the object of scientific study: Nature conceived as God's Second Book, on a hermeneutic par with the Bible, was a very different thing to nature understood as a chance concatenation of atoms. The first had the capacity to uplift those who studied it; the second did not.

In 1775, the English Unitarian chemist Joseph Priestley wrote that: 'A Philosopher ought to be something greater, and better than another man.' If the man of science was not already virtuous when he came to science, then the 'contemplation of the works of God should give sublimity to his virtue, should expand his benevolence, extinguish every thing mean, base, and selfish in [his] nature'. If we find such sentiments ridiculous today, it is in part because our view of natural reality has changed, courtesy of the consensual views of scientists themselves. The secularised object of their studies now contains no moral lessons and has no capacity for moral uplift. In 1898, Leo Tolstoy's essay on modern science described 'a strange misunderstanding' that lay at the heart of modern culture:

'A plain reasonable working man supposes, in the old way which is also the common-sense way, that if there are people who spend their lives in study, whom he feeds and keeps while they think for him – then no doubt these men are engaged in studying things men need to know; and he expects science to solve for him the questions on which his welfare and that of all men depends. He expects science to tell him how he ought to live: how to treat his family, his neighbours and the men of other tribes, how to restrain his passions, what to believe in and what

not to believe in, and much else. But what does our science say to him on these matters? It triumphantly tells him how many million miles it is from the earth to the sun. "But I don't want any of those things", says a plain and reasonable man – "I want to know how to live".'

To which the modern scientist responds, in effect, 'that's not my business'.

This response is entirely reasonable from a modern point of view, and, in the view of many, it is a necessary condition for the objectivity and integrity of science. Science, to *be* science, cannot deal with the intractable and irredeemably subjective 'ought' but exclusively with the potentially consensual and objective 'is'; with fact and not with value. This self-denying ordnance made science powerful, but at the risk of moral irrelevance.

Nevertheless, it was a price that had to be paid. At any rate, this was the opinion of the sociologist Max Weber in his influential 1918 address, *Science as a Vocation*. The world was now, thanks to the scientists, 'disenchanted': 'there are no mysterious incalculable forces that come into play, but rather . . . one can, in principle, master all things by calculation'. Science once promised that it would show 'the path to God', the road to happiness or to virtue, but 'Who – aside from certain big children who are indeed found in the natural sciences – still believes that the findings of astronomy, biology, physics, or chemistry could teach us anything about the meaning of the world?'

During the late 19ᵗʰ-and early 20ᵗʰ century, the technical experts of Western society reflectively and systematically disengaged their work from the moral domain. The object of their study did not invite moralising, and the conditions of internal consensus and external credibility made forays from 'is' to 'ought' fraught with risk. Any slippage from the descriptive to the normative was not just the 'naturalistic fallacy' marked by philosophers from David Hume to G. E.

Moore; it was also ontologically unsustainable and politically inadvisable. If such people as 'moral experts' in modern society existed, they were not to be found in the laboratory or speaking from a scientific podium. This was, for example, Albert Einstein's opinion: 'knowledge of what is does not open the door directly to what *should be*':

> '[All] scientific statements and laws have one characteristic in common: they are "true or false" (adequate or inadequate). Roughly speaking, our reaction to them is "yes" or "no". The scientific way of thinking has a further characteristic. The concepts which it uses to build up its coherent systems are not expressing emotions. For the scientist, there is only "being," but no wishing, no valuing, no good, no evil; no goal. As long as we remain within the realm of science proper, we can never meet with a sentence of the type: "Thou shalt not lie." There is something like a Puritan's restraint in the scientist who seeks truth: he keeps away from everything voluntaristic or emotional.'

From calling to job
By the 1930s, professionalism and secularism had begun to characterise both the way in which scientists presented themselves to the world, and the way their work was understood by academic outsiders. Science was becoming a decently paid and well-regarded, but still ordinary, job, rather than a 'calling'. Scientists working in bureaucratised settings, those engaged in the making of profit and secular power especially, could no longer appear as wholly pure, and might even now worry about maintaining a disengaged image. Although much mid-century academic commentary seems not to have noticed, the vast majority of American scientists were not then employed in institutions of higher education, doing pure research intended to advance knowledge for the sake of knowledge. The 1966 National Register counted 243,000 working scientists, with 37

per cent holding PhDs. Of these latter, only 36 per cent were employed in educational institutions, 34 per cent in industry and business, and 13 per cent by federal, state, and local governments. (In Britain, which was spending a far smaller proportion of its national income on industrial research than the USA, the crystallographer J. D. Bernal estimated in 1939 that 70 per cent of all qualified scientific workers were employed in industry.) Disinterestedness, and its associated virtues, is far easier to ascribe to the powerless than to the powerful, and, through their increasing association with business and the military after the Second World War, scientists were becoming increasingly familiar with the corridors of power.

In 1940, G. H. Hardy's bittersweet *A Mathematician's Apology* warned: 'We must guard against a fallacy common among apologists of science – the fallacy of supposing that the men whose work most benefits humanity are thinking much of that while they do it; that physiologists, for example, have particularly noble souls.' A few years later, insistence on the moral ordinariness of science was a keystone of Robert K. Merton's justification for a sociological approach. 'A passion for knowledge, idle curiosity, altruistic concern with the benefit to humanity, and a host of other special motives,' Merton wrote in 1942, 'have been attributed to the scientist. The quest for distinctive motives appears to have been misdirected.' There is, he said, 'no satisfactory evidence' that scientists are 'recruited from the ranks of those who exhibit an unusual degree of moral integrity' or that the objectivity of scientific knowledge proceeds from 'the personal qualities of scientists.' You could rightly defer to scientists' technical expertise, but there was no reason for deference in any area outside their special expertise, for example, on matters concerning 'what ought to be done'.

By the post-Second World War period, insistence on the moral ordinariness of the scientist had become an American cultural commonplace. In 1950, the point of Anthony Standen's *Science is a*

Sacred Cow was that science should not be treated as a sacred cow, just as Ralph Lapp's 1965 *The New Priesthood* warned against the dangerous mistake of treating scientists as priests. Lapp worried about what later came to be called 'technocracy', while Standen took for granted that the legend of heroic virtuosity persisted in some quarters, only to identify the myth-makers not with practicing scientists but with moralising educationists ('scientific evangelists') and warn the public against their credulity:

> '. . . we are having wool pulled over our eyes if we let ourselves be convinced that scientists, taken as a group, are anything special in the way of brains. They are very ordinary professional men, and all they know is their own trade, just like all other professional men.'

Nor are scientists better than anyone else at predicting an inherently unpredictable future, even when they forecast the future course of science and technology. Knowing more science never means knowing much, if anything, about its future trajectory.

In 1964, Jacques Barzun's *Science, The Glorious Entertainment* pointed to the loss of integrity and freedom associated with the scientist becoming the servant of the state and 'the darling of industry'. The old cosmopolitan loyalties to the free Republic of Science had been replaced by local loyalties to governments or to companies. In the same vein, the sociologist Joseph Schneider bearded the natural scientists in their own lair when he wrote in *The Scientific Monthly*: 'There was a time when men of mind expressed only contempt for the vainglorious show of politics and war. But today the man of science has become a hireling, a willing subject in the service of the nation state; an indefatigable combatant in the righteous cause of a finite warrior god.' And in 1968, Spencer Klaw's *The New Brahmins: Scientific Life in America*, observed, 'the patronage [that scientists now] command, has inevitably changed

the nature of their calling. They have become richer and more caught up in worldly affairs,' as he recorded the older generation's dismay about the linked rise of hack-work and the decline of both genius and virtue.

Many scientists themselves very vigorously insisted on their collective moral ordinariness, warning the public, for that reason, against excessive trust in science. In 1963, the biochemist Erwin Chargaff composed a sour, modern dialogue in which an idealistic 'old chemist' (Chargaff himself) was lectured by a trendily ambitious 'young molecular biologist':

> 'You seem,' the young man sneered, 'to have the romantically foolish idea that only a good man can be a good scientist.' The 'old chemist' conceded the charge but not its foolishness: 'It is always dangerous to use the argument *ad hominem*, and you should not judge from yourself. But is it not a desperate situation when an old proverb must be reversed to read: Wherever the fish stinks there is its head?'

For the biologist Paul Weiss the rise of Big Science and the associated industrialisation of research were the decisive events compromising any notion of distinctive scientific integrity:

> 'Throughout the phase of history, which we have come to survey [the last three centuries], till very recently, to be a scientist was a calling, not a job. The scene, however, is now changing rapidly. The popularity and needs of an expanding science bring in more drifters and followers than pioneers. Shall we let brainpower be overgrown by manpower and mechanical rote performance?'

The appearance of James Watson's smash-hit account of the discovery of *The Double Helix* in 1968 was both an indicator and a further cause of these gathering shifts in sensibility: from the

distance of over 35 years, it is not easy to recall the shock value of Watson's revelation that scientists might be motivated by the same sorts of concerns as anyone else.

Chargaff wrote that the belief that the scientific profession 'was a noble one . . . was certainly shattered in 1945', and, for the bomb-builders of the Manhattan Project, the stipulation of moral ordinariness had special significance. It was a way of laying the spectre of Dr Frankenstein: the idea that these appalling weapons had been brought into the world by bad men, badly motivated. Writing in 1960, C. P. Snow insisted upon moral equivalence as a way of showing the public that there was no reason to be 'frightened' of scientists, that scientists were 'certainly' not morally or temperamentally 'worse than other men.' J. Robert Oppenheimer, whose remark about physicists having come to 'know sin' was emblematic of a supposed collective descent from grace, recognised, but gently rejected, traditional views of the moral superiority of the scientist: 'The study of physics, and I think my colleagues in the other sciences will let me speak for them too, does not make philosopher–kings. It has not, until now, made kings. It almost never makes fit philosophers – so rarely that they must be counted as exceptions.' Just before his death, Oppenheimer drew a measured but sharp distinction between the vast technical knowledge that scientists possessed and the moral and political programmes of action in which scientific knowledge was increasingly enlisted:

> 'Among the things of which we cannot talk without some ambiguity, and in which the objective structure of the sciences will play what is often a very minor part, but sometimes an essential one, are many questions which are not private, which are common questions, and public ones: the arts, the good life, the good society. There is to my view no reason why we [scientists] should come to these with a greater consensus or a greater sense of valid relevant experience than any other

profession. They need reason, and they need a preoccupation with consistency; but only in so far as the scientist's life has analogies with the artist's . . . only insofar as the scientist's life is in some way a good life, and his society a good society, have we any professional credentials to enter these discussions, and not primarily because of the objectivity of our communication and our knowledge.'

In the late 1960s, the physicist Ralph Lapp thought it important to tell the public that: 'Scientists as a group probably have no better sense of human values than any other group. To say that science seeks the truth does not endow scientists as a group with special wisdom of what is good for society. Furthermore, scientists have no single community within which there is a common set of values.' It followed from arguments like these that there would be no reason to pay any special attention to scientists' views on 'what should be done', certainly on matters that did not involve their particular expertise, and even when such decisions implicated technical expertise, which scientists alone possessed.

The advisory role of the scientists to the national security state represented a new form of technical power and authority, but the Oppenheimer security hearings of 1954, and later McCarthyite persecutions, soon offered dramatic lessons in the limits of that power. The state would continue to enlist scientific expertise, and to grant considerable autonomy and vast resources to those experts, but only on the condition that experts left whatever moral and political preferences they might have outside the doors to the corridors of power. Better yet, they should learn that they had no entitlement to expressions of special moral authority or political judgment. Should they insist on such authority, they risked losing access to the enormous material support that was being offered to scientists in the post-war decades. After the Oppenheimer hearings, the Board reminded the American scientific community of the boundaries that democratic society placed around technical expertise:

'A question can properly be raised about advice of specialists relating to moral, military and political issues, under circumstances which lend such advice an undue and in some cases decisive weight. Caution must be expressed with respect to judgments which go beyond areas of special and particular competence.'

Shortly afterwards, a commentator noted: 'They did not care what [Oppenheimer's] moral scruples were. It was the fact that he had any at all which was derogatory.' So the Board's *Final Report* announced: 'We know that scientists, with their unusual talents, are loyal citizens, and, for every pertinent purpose, normal human beings.' It was probably meant as a warning as much as a description. The 'is/ought' distinction, that is to say, was to be institutionalised in the modern scientific role as a condition of its political legitimacy. It was a contract that appealed to both parties: the political powers got expertise on-tap without interference in their prerogatives; the scientists got money and a reconfigured. but still worthwhile, version of autonomy.[2]

Problems of mistrust

If the transition from 'knowing more' to 'knowing what is the right thing to do' is understood as a move between distinct languages, and if our technical experts are considered to be no better than anyone else, then there is no basis for granting them any special moral authority. I have traced the historical development of just such beliefs, the result of which is a bias of technical experts to eschew debates over morality and policy, and of moralists and policy-makers to mistrust those technical experts who may still assert their special authority to make judgments about 'what ought to be done'. This state of affairs is increasingly taken as a matter of course in late modern society: how could it be otherwise?

Yet, for all the mutual convenience of the New Dispensation segregating technical expertise from moral authority, its legacy is a

pervasive awkwardness in contemporary culture. This is particularly the case in debates about 'what ought to be done' in technoscientific matters bearing on public interests and preferences. Who has the right to speak and the authority to be listened to? It would be very foolish not to concede major rights and authority to relevant technical experts, especially insofar as the knowledge implicated in such decisions becomes more complex, arcane, and, ultimately, opaque to all but those few. Nor should such rights be limited by overblown fears of a 'technocratic' hegemony of experts, not least since the possibilities of such technocracy are limited by the varying voices with which expertise speaks in matters of public concern. Moreover, there are some seldom acknowledged problems attending the attributed moral ordinariness of technical experts, and it is to these problems I want finally to turn.

Scepticism is cheap to express, but often expensive in its consequences. If technical experts have no more moral rights to be trusted then anyone else, then it follows that two things must be done. First, we should design instruments of surveillance and control, designed to achieve what personal integrity is now supposed impotent to assure: government bureaus of research integrity, conflict of interest offices, requirements for declarations of financial interest in scientific journals, and, of course, informed consent regulations and institutional review boards. Given The Way We Live Now, it would be foolish not to implement such measures, and they would probably have at least some of the desired effects. But it would also be foolish to think that surveillance can ever be total, and, therefore, that the limitation of trust can ever approach the elimination of trust. Control and integrity must, for that reason, always work together, yet, as sociologists well understand, there is a tendency for people who are mistrusted to live down to expectations.

Second, if our experts are presumed to lack integrity and moral authority, we must put in place people specially trained to evaluate moral options and to advise on or make moral decisions. The rise of

professional 'ethicists' and assorted moral experts is, indeed, a remarkable feature of recent times, and it is at once an expression of conviction in the moral ordinariness – or worse – of technical experts, and the withdrawal of trust in scientists and physicians to 'do the right thing'. But it is a delusion to suppose that morality can be simply and effectively offloaded to a cadre of experts. Even if one accepts the very idea of superior professional moral expertise – and I am not alone in finding that notion something between silly and disturbing – the problems of locating such moral expertise and trusting it are very much the same as those outlined in the case of technical expertise. How do you recognise genuine moral experts?

This objection is not pedantry: many bio- and medical ethics centres now depend massively upon funding from the pharmaceutical industry. In the USA, the further turn of the sceptical wheel began long ago.[3] Quis custodiet ipsos custodes? The regress is potentially infinite, and the economic, intellectual, and moral costs of travelling very far along that road are massive, crippling, and deeply disagreeable. Scepticism tends too easily to spread beyond the bounds within which one might like to keep it.

Late modern society has indeed travelled the sceptical road, but Richard Horton is right to remind Hugh Grant that it has not gone as far as the Monsanto CEO supposes. For an indicator of the extent to which a 'trust society' has not yet been dissolved, and to which technical experts are held to higher-than-ordinary standards of conduct, look no further than the extent to which both the public and many of our experts respond to occasions of scientists' materialism, malfeasance, corruption, and insensitivity with displays of shock and dismay. Again, as sociologists know very well, 'moral outrage' is a pretty good sign that an offence has been committed against what a society holds dear. The 'de-moralisation' of technical expertise is far from complete.

Since I have expressed doubts about the idea of external 'moral expertise', I can at once insist that I have no such credentials and

excuse myself from telling scientists and their masters in any detail what they ought to do about the current malaise. Nevertheless, I am happy to underline what I think this story has to say to those whose concerns are more geared towards questions of 'what ought to be done'.

There is an almost inverse relationship between the ease of taking measures to cope with the de-moralisation of science and the likelihood that these measures will work. The current vogue for professionalising and calling upon ethical expertise is unlikely to be effective. It is relatively cheap; it gives the public an impression of 'doing something'; but solid evidence of its use is lacking. At the other extreme, if indeed the de-moralisation of science proceeds from such large-scale social and cultural changes as secularisation and professionalisation, the possibilities of positive action in these matters are slim to nil.

In pronouncing 'the disenchantment of nature' at the beginning of the 20th century, Max Weber described the conditions in which scientists could no longer be priests, moralists, or even morally uplifted by their work. Few present-day scientists would wish to unwind that bit of history, but no one can now be unconscious of the extent to which announcements of the Death of the Sacred were premature. Both Christian and Islamic fundamentalism have taken care of that. Neither is the 20th century professionalisation of the scientific career, and the transformation of science from a calling to a job, a change that anyone could undo, or that anyone who enjoys the fruits of Big Science-produced technology and medicine would wish to undo.

To return science to an avocation of the priest or the gentleman would be to change our society out of all recognition. Such a thing would be as undemocratic, unwise, and destructive to our current conceptions and expectations of science as it would be impossible to achieve. Late modern science *is* a job, increasingly sharing the characteristics and values of other professional and technical jobs. Our society has too many technoscientific problems in need of

solution to advocate any significant shrinkage of the number of jobs available or the resources committed to the support of science.

But between the ineffectively easy and the impossibly visionary, there lie real opportunities for addressing current crises in scientific integrity and the consequent corrosive growth of public mistrust. One such possibility was pointed out in Horton's essay and is now being canvassed in various forms by many scientists and policy-makers. The commercialisation of science is pervasive, but science without large-scale commercial sponsorship or links is not inconceivable. Many areas of contemporary science continue to do their work totally or largely free of commercial subvention, and, even while academic scientists are continually pressed to make their research 'pay' and to seek out commercial sponsorship, governments -- even now -- accept some responsibility for picking up the tab for basic research when industry can't or won't. The building up of commercial ties delights governments and university administrators, but corporate executives and research managers have *never* uniformly taken the position that academia serves industry best by imitating industry. Since the beginnings of industrial research in the early 20th century, a number of thoughtful executives have urged universities to stick to their last, concentrating on those non-commercial lines of research that industry found it hard to justify and to execute.[4] In short, the scaling back of ties between commerce and academia is not inconceivable, and many scientists may come to join Horton in thinking it practically advisable. If one needs to put it this way, the independence of science has got cash value.

Finally, the obverse of scientists living down to expectations that they will do whatever they can get away with is the possibility that they will live up to a renewed presumption that they are honest, disinterested, and incorruptible. Accordingly, it would be good for scientists themselves to express greater outrage when commerce corrodes disinterestedness, to accept whatever shrinkage in their resources might come from the limitation of compromising

commercial ties, and to speak more openly, and less embarrassedly, in front of their colleagues and students about science as a calling. I'd be happy to trust such scientists to use their expertise disinterestedly, since their moral authority would be everything that is possible for late modern technical experts.

Endnotes

[1] Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England,* Chicago: University of Chicago Press, 1994, ch. 1; see also idem, 'Trust, Honesty, and the Authority of Science', in *Society's Choices: Social and Ethical Decision Making in Biomedicine,* Ruth Ellen Bulger, Elizabeth Meyer Bobby, & Harvey V. Fineberg (eds), for Committee on the Social and Ethical Impacts of Developments in Biomedicine, Institute of Medicine, National Academy of Sciences, Washington, DC: National Academy Press, 1995, pp. 388–408.

[2] See Charles R. Thorpe, 'Disciplining Experts: Scientific Authority and Liberal Democracy in the Oppenheimer Case', *Social Studies of Science* 32, 2002, pp. 525–562.

[3] See, for example, Ruth Shalit, 'When We Were Philosopher Kings: The Rise of the Medical Ethicist', *The New Republic* 216, no. 17, 28 April 1997, pp. 24–28; Carl Elliott, 'Diary', *London Review of Books* 24, no. 23, 28 November 2002: 'Bioethics rests on a thin layer of public trust that can easily be shattered. Full-disclosure policies may help that process along. Once bioethics becomes publicly identified as a tool for industry, its practitioners may face a choice between going back to their old jobs in the philosophy department or signing up for new corporate positions as risk managers and compliance officers.'

[4] See the views of two of the 20th century's most articulate research managers, Ken Mees and John Leermakers of Eastman Kodak, e.g. C. E. Kenneth Mees and John A. Leermakers, *The Organization of Industrial Scientific Research,* 2nd ed., New York: McGraw-Hill, 1950, p. 14; John A. Leermakers, 'Basic Research in Industry', *Industrial Laboratories* 2, no. 3, March 1951, pp. 2–3: 'It is in the universities that true intellectual freedom, so essential to the development of the scientific attitude, is found.' I recall similar opinions about academic research independence expressed during the Thatcher years by Sir John Harvey-Jones of ICI.