TOWARDS A GENERAL SOCIOLOGY OF SCIENCE

Ian Jarvie

Joseph Agassi

Abstract

In this critical and constructive paper we argue that much received sociology of science is skewed by a presupposed idealist philosophy, one that encourages an uncritical attitude towards the supposed facts of the matter. We propose a critical sociology of science that (1) unfuses science, applied science, and technology; (2) emphasizes fallibility in science and the sociology of science; and that (3) deconstructs all claims to authority. Constructively, we sketch the elements of a sociology of science that comprehends the elementary forms of the scientific life as well as contemporary bureaucratized and militarized "normal" science.

Keywords: Merton; sociology of science; strong programme; authority; institutions; Gieryn; language; objective knowledge.

Our Philosophical Caveats

Despite the sweeping title of this paper, which suggests a descriptive, even ethnographic survey of science, its aim is more modest. We offer at most a sketch that is free from three faults, three philosophical assumptions and conclusions that much received sociology of science takes for granted. The first the assumption concerns concerns the special authority of science in the contemporary social order. Its authority is standardly explained as a social construction and so is our obligation to accept that authority. Although we agree that the sociology of science should explain the authority of science, uncritically endorsing that authority is a recipe for stagnation. The second assumption is that there is no social reason to distinguish science from technology. By contrast, we argue that despite their being they are muddled in the public and the official mind, the sociology of science should be critical and present them as differing in aim and in social structure. The third assumption is that socio-economic factors are sufficient to explain the growth of scientific knowledge. Suffice it to say, we concede that research is not as immune to the influence of these factors as the classical thinkers of the Enlightenment Movement assumed but they are scarcely sufficient to capture the workings of the social institutions of science.

Before we examine further each of these assumptions, we need to indicate the nature of our objections, which come from a different philosophical understanding of science as a critical and self-critical enterprise. Before that, we want to link this fault-finding of ours to the title.

If we are to dislodge these (pernicious) philosophical assumptions about science from its sociology making a philosophical case against them is obviously not enough. After all, adherents of the received sociology of science could say, they are describing the social order, however reasonable or perverse the philosophy behind it may be. We accept this in part: we concede that, at least in the case of so-called scientific authority and the mixing of science with technology, confusion occupies the public mind, confusion that reflects features of society that depend on (or at least originate from) the philosophical views of science that we shall challenge here. Yet we also claim that even descriptively the picture is mixed: evidence indicates that the public sometimes allows that science and scientists are fallible, that the public sometimes allows that the latest scientific news is not the last word on its subject matter. Perhaps the public sometimes allows that science and technology are different kettles of fish in the very preference for funding technology and applied science as opposed to pure science.

To make our argument stick, then, we have to sketch the possibilities for a sociology of science that plays up the conjectural side of science and thereby emphasizes its difference from technology. To that end we seek a general sociology of science that makes allowance for social scepticicism about the authority of science together with attributing social authority to science-based technology. As to explaining the progress of science, advocates of the view known as the Strong Programme claim that they can explain it by reference to socio-economic factors. We reject this claim: we find it reductionist and at odds with widely diffused ideas about creativity. That in itself is not a knock-down objection: but it is a good starting point, and we adopt it here.

Let us now expatiate a little on the three philosophical assumptions discussed above that permeate received sociology of science. The problem with the ascription of authority to science is simple: it is a social fact, true, but it nonetheless rests on a cardinal mistake. That mistake is a false description of science. A more critical description of science would reveal that it has no authority and need not command belief. It is an intellectual adventure that throws up some astounding, fertile ideas. These ideas do not thereby carry any authority and if authority is ascribed to them, or, worse, if authority is ascribed to individual scientists (as "experts") then this is due to a serious muddle.[1] The prestigious *status* that the public ascribes to science is, obviously, a social construct. But then the *authority* ascribed to science is similarly a social construct, just like political, religious, bureaucratic, and other authority sub-systems. None need be endorsed by the sociologist. Adherents of the received sociology of science may fear that withholding endorsement undermines the described status.[2] Or they may say that scientific authority is among the social facts with which they deal. (Calling something a fact is tacitly to endorse it.) We would question all this and argue to the contrary that deconstructing the so-called authority of science would be better employment for sociologists of science, and that this way they will increase, not undermine, its social status. Their claim that the authority of science is a social construct, that science is also a social construct, may be transcendentally debunking but, because it brooks no appeal beyond the social it amounts to sleight of hand: debunking that is not debunking at all. The authority claimed for science in society is said to derive from allegedly intellectual (*viz*., "epistemic") authority. We take the view that science has no intellectual authority because there is none to be had. Adherents of the received sociology of science would do well to view the ascription of authority to science as a myth to be explained or as a social institution to be described, or both, but if both then separately ─ without intellectual submissiveness. It is unwise to rule out *a priori* the obvious possibility, not to mention the obvious fact, that it is in the interests of some advocates of science, including some scientists, to pretend to (a non-existent) authority. Particularly on the assumptions that we wish to challenge, but also without them, the very possibility of this kind of pretense suffices to make room for our investigation, and efforts to deny this *a priori* are not serious.

Adherents of the received sociology of science could no doubt respond that they do deconstruct scientific myths and pretensions, particularly in their studies of laboratory life and ongoing research practices. Perhaps; but reducing science to routine practices of work, much like studies of the factory floor, is to miss another philosophical point. We may distinguish between two kinds of scientific work: efforts to refute old ideas and efforts to create and test new ones. Ideas are abstract and intangible products, whose value is only settled by putting them on the intellectual market for others to seek, find, and examine. We can discern three intellectual sub-markets where seeking, finding, and examining take place. There is the *initial market*, in which ideas circulate as brainstorms, research proposals, preprints. (Think of Watson's account of how he and Crick sought the permission of Bragg at the Cavendish to expend some resources pursuing their ideas.) The initial market it quite small and intimate. The second market is the *futures market* where ideas that made a big impact in the initial market may continue to glow, or may fade, those that made little or no impact either continue that way or start to make a larger impact. (Think of the short communication to *Nature* that Crick and Watson used to report their DNA structure, and the tiny hints they offered about the potential ramifications of their discovery for future research. Then think of the exponential growth of their citations.) The third market is that of the *general public*. An example would be the revolution in medical thinking, the rise of germ theory, its refinement into systemic, bacterial, viral, auto-immune, and other variations that entered the public consciousness, became part of the view of disease that we teach schoolchildren, and of the rise as fallout of the popular practices regarding hygiene, nutrition, and immunization. (Think of the award of the Nobel Prize to Watson, Crick, and Wilkins; and the subsequent stellar careers of all three, which included popularizing science.)

None of these three markets turns abstract and intangible products into authority, the way markets in religious or legal ideas turn them into authority by the use of familiar social mechanisms. Indeed, the only authority it makes philosophical sense to claim for science is the view that considered rational inquiry into problems should receive more serious attention than opinions that are not thought-out and therefore untested – but such caution is too timid if it rules out wild ideas just because they are wild. (Think of Niels Bohr's famous disposition to dismiss ideas that were not sufficiently wild.).

Some of the confusion over authority, we suggest, comes from the fusion of science and technology in the public mind – and in the sociological mind as well. We are not puritans: for some purposes of discussion, the difference between science and technology is not important and may be overlooked. But when we talk of authority, the difference is very important. Technology does possess authority – not intellectual but institutional. Society invests technology with its authority, and in doing so society stipulates the standards that technology has to meet to claim authority. Mostly, these standards are legal and quasi-legal, that is, they are enforced by the power of the state and its organs. (Think of local building codes; of best practice in the engineering and design of buildings, bridges, tunnels; think of the régimes of testing that precede the licensing of medicines; of the licensing requirements of the professions; of airworthiness certificates.)

Unlike scientists, technologists are legally and otherwise responsible for their products.[3] Pure science knows no parallel to this. Cosmologists are not in some way socially responsible for the fact that in their standard models human life is a tiny and insignificant episode of the origins and evolution of the universe. One can argue that these ideas, or similar ones, demoralize and have ill effects; even so, there is no one to blame here. Individual scientists can of course warn, be compassionate, try to muffle the implications, and so on. But that is not their scientific responsibility. All scientists are citizens, but scientists claim the freedom to set aside the wishes of their fellow citizens if they think the pursuit of science would be inhibited – but they can do so only within the law. (Think of the Helsinki convention.) Problematic citizens can be unproblematic scientists. (Think of Kurt Gödel and of John Nash.)

The science/technology boundary is worth study. The aims, surrounding social structures, and ethics of technologists are very different from those of the rational community of free inquiry that is science. Confusion of the two may be deliberate, as when the atomic bomb or the cure for cancer are dangled around in order to extract money for research, and when the military authorities dangle money around in order to obtain the services of scientists. Society as a whole should take responsibility for the creation of such an incentive system; adherents of the received sociology of science may of course construct models that foist these incentives on science as though there was no help for it; but social technologists should examine these models before applying them, and seek the best way to test them all, and make attempts to improve upon them in manners that promise success.

The profound cooperation between applied science and pure science explains some of the confusion between them. Applied science comprises efforts to put extant pure science to practical use, or even to develop pure science to serve that end. This last activity is known as basic science. Medicine is an applied science that draws on the pure sciences of biology, biochemistry, chemistry, quantum mechanics, and even classical mechanics (when diagnosing skeletal injuries for example). The technology of medicine is the production of drugs, surgical procedures, and diet regimens, perhaps also the design of clinics and computer-expert-systems, as these put the applied science to work on the patient. It is important to stress – with Gellner – that certain general social forms and certain specific kinds of social arrangements precede both the possibility to apply science and the possibility to devise new technologies. Medicine is first and foremost a social technology: a set of social arrangements that trains specialists in human disorders along with inculcation in the population at large that the way to deal with their suffering is to set up clinical encounters with these specialists and to "present" themselves accordingly. Social technologists devise ways to apply medical technology, and they do so within those social arrangements, including modern bureaucracy and industrialization, that enable them to make available to doctors wide ranges of tested and affordable medical technology. The way all this interacts with the insurance system at large is extremely complex, requires much care and planning, and continuous monitoring and revision.

Classical sociology of knowledge, out of which received social studies of science grew, did not presume to explain the growth of scientific knowledge by invoking socio-economic variables. To see why, apply to itself the theory that says, "scientific knowledge is the result of socio-economic factors". Call it S. We have then the claim that S is the result of some socioeconomic factors. Is this claim true? If yes, then some socio-economic factors lead to the truth! Suppose then that we discover what these factors are. Then, producing them will generate science. This is scarcely credible. Note: this is not to deny that *reception* of scientific ideas in the *futures market* and *general public market* are susceptible to socio-economic examination. Any biologist could see that the Crick-Watson DNA break-through created immense possibilities for further research in several cognate fields. That some financial organizations were receptive and threw money at the projects are matters of the reception of innovations, not to be confused with the innovations themselves.

Innovation, it seems to us, is an intellectual achievement explained best but still only partially by the intellectual pressures that thinkers face, by their problems and their problem-situations. Understanding these problems and problem-situations throws light on ensuing breakthroughs and on breakthroughs that were not seen as such in the day when they became available. Furthermore, this renders it obvious that all purported Strong Programme explanations of changes of scientific ideas explain not the ideas and their emergence but the *reception of these ideas*. Scientific progress is equated with scientific reception. This makes sense, as ideas ignored by the learned public are forgotten. Yet it is erroneous. Counterexamples to it come from both directions. The successful reception of ideas is no proof that they are not duds, and the dismissal of ideas that at first look dud is no proof that they will not eventually gain recognition as explosive. A case of the first kind would be the long-held psychosomatic theory of gastric ulcers. A case of the second kind would be Wegener's theory of continental drift.[4] To these sorts of cases sociologists of science could reply by stressing that consensus changes: it clusters for a while, dissolves, then reclusters elsewhere. This response is unobjectionable. Such clustering, dispersal, and re-clustering, being social processes, are indeed susceptible to socio-economic explanation – at least in some measure. Geographers and geologists explain why Wegener was pooh-poohed: they usually say that there was a big problem with this theory when it first appeared, namely mechanism.[5] What forces could possibly be shifting these massive continental plates? Only after new ideas about the heat and convection currents beneath the earth's surface were devised did it seem possible to account for continental drift and to turn to the independent ecological data to test it, and it turned out to offer a powerful tool for the explanation of the diffusion of plant and animal species of all sorts. This way Wegener's ideas were open to serious discussion, and, indeed, they were then discussed widely and seriously. Socio-economic factors are at play here, of course, especially communication; but the intellectual aspects are the best part of the story, so the response about the vagaries of the consensus, though true, is no reply to criticism: it cannot parry attacks that charge the received sociology of science with a tendency to miss the meat while centering on the sauce.

The meat of the debate is intellectual: deficiencies of a theory explain its neglect, and supplements and modifications of it explain its change over to a positive reception. Adherents to the received sociology of science will respond further. What they observe, what they hear scientists say, what they write, tells a story of uncertainty and dispute, a story of the marshalling of arguments and of evidence, and of the differing suggestions as to the conclusions to draw. This all sounds to adherents to received sociology of science like Debate and Rhetoric 101. Debate and Rhetoric 101 usually teaches that disputes are settled by a vote, of judges in a debating contest, of members present in a debating society like the Oxford Union. Debates in both sorts of places are sometimes framed around issues where there is genuinely something at stake (other times they are tongue-in-cheek). Similarly, adherents to received sociology of science opine, scientists engage in rhetoric and persuasion every time they try to forge a new consensus, and these are social constructs like any other. This leaves too much of the situation needlessly puzzling. Why do scientists not describe the issues in this way? Is it because they are not sociologists and do not know how to frame and describe sociologically? Or is it because, while fully aware of the features that resemble a debating contest, they consider the underlying issue and the content of the arguments to be utterly serious? Even highly sociologically self-conscious scientists such as Michael Polanyi, Thomas S. Kuhn, and Paul K. Feyerabend do not reduce the glories of science to mundane practices of negotiation, rhetoric, and debate and their socio-economic determinants. Those are practices received in the community, to be sure, just as they are received in other social institutions. The question is, what is the scientific bottom line? Is it consensus? Is it public *éclat*? Is it research money? Is it medals and prizes? Is it citations? None of these is sufficient explanation. The consensus is about something or other being the case; the money is given on promise; the medals and prizes are given for achievement; the citations are to ideas. What is the referent of these expressions, “the case”, “promise”, “achievement”, or “ideas” ? If adherents of the received sociology of science say it is self-validating then the argument is circular. The sociologist may be inclined to grant that and then say, but what other measure is there?

The difference between science and magic need not be discussed in order to agree that received sociology of science is pro-science and anti-magic. Yet it can distinguish between genuine and fake magic ─ by reference to it as a social institution, of course. The situation with science is different, as the genuineness of science is much more a matter of objective test than of science being a social institution. To be precise, the testability of science is its major institutional characteristic. We contend that received sociology of science does its best to overlook this, thus pushing itself towards the view of science as effective magic.

The reason for this oversight in the received sociology of science is simple. The question on the agenda of the sociology of science is philosophical, yet the sociologists want to be sceptical towards the philosophers and the scientists who argue that there is a measure that is conjectural and uncertain and not monotonic with the social consensus of scientists (still less the general public). This measure is variously called the facts, the case, the truth, reality, Mother Nature.[6] All attempts to capture and state scientific results begin with joint endeavours and end with assertions framed in language and these are necessarily social, observe the sociologists of science. And, they conclude, scientific results are to be seen as social constructions: necessarily. This inference is invalid: social effort at something does not make that something social. The social efforts have a goal, the goal is socially defined, but whatever the goal is, it need not be a social entity. Democracy, museums, and universities are social entities; citizens and works of art and of science are not.

Ernest Gellner (1973, p. 17 and elsewhere following Russell) notes that the arguments that sustain all forms of metaphysical idealism usually rest on the doctrine of internal relations. This is the doctrine that relations are "internal" to things, namely, that relations [between things] are assimilated to properties [of things]. He also suggests that thinking about society renders this doctrine very tempting. Hence, thinking about society renders idealism very tempting. The idealism of the Strong Programme in the sociology of science has two sources. One is the circular idea that all human activity is social activity and that hence social activity is all there is. Second is the linguistic idealism of the later Wittgenstein, who merely hinted that since language is a social activity anything said in language is shaped, constrained, and explained by language and form of life. (By the term "*Lebensform*", he seems to have meant social form of life, namely, society *simpliciter*: this way he managed to smuggle in much metaphysics with the aid of one obscure technical term.) We are then landed with a special version of the anthropomorphism that takes universal science to be an illusion because all of science, from its fundamental measures to its language, is shaped from the point of view of *Homo sapiens* and cannot possibly map on to non-human observers or even to be intelligible to them.

The question we raise is, then, can we create a general sociology of science that does not take for granted the authority of science but rather deconstructs it, a general sociology of science that unfuses science and technology, and circumscribes an autonomous domain for intellectual achievement as such? We do not wish to hide our value system: we do value ideas and we think they have autonomous existence. This includes sociological ideas by distinguished ancestors: Marx, Durkheim, Simmel, Weber, Evans-Pritchard, Popper, Gellner. Sociologists of science fuse the commonwealth of learning with the diverse institutions that house it. These institutions change faster than the commonwealth. When Popper wrote on objective knowledge as an institution, he surprised and distressed many, as he avoided this erroneous fusion of institutions with the whole they serve and constitute. Objective knowledge is a part of the Commonwealth of learning. It too is not to be fused with the institutions that house it, such as museums, libraries, and the internet.

Our Targets

So far, we have left the specification of the received sociology of science aside. Although we are occasional attentive readers of its literature, neither of us would claim the status of a specialist in it. We can now be a tad more specific. We propose to rely on a survey article by Thomas F. Gieryn, "Science, Sociology of", published in the Elsevier *International Encyclopedia of the Social and Behavioral Sciences,* 2001, that strikes us as comprehensive and to a limited degree critical.[7] Gieryn is a scholar at Indiana University. He divides sociology of science into three main periods (plus a pre-history at the beginning and an uncertain future at the end). The first period is Mertonian; it focuses on science as a form of social organization. The second period focuses on the processes that make science. The third period concerns itself with science as authority, or, more precisely, with scientists as possessors of cognitive authority.

Gieryn presents the sociology of science as value-laden. He iterates the discrepancy between the claims of universalism and communism (Merton) on the one hand and on the other the hierarchical organization of science, the principle of cumulative advantage, the systemic and systematic sexism, and utter lack of democracy. These observations we consider criticisms that we find congenial, although with some reservation as to democracy since criticism has never been suppressed in science to the degree achieved by undemocratic political regimes.[8] Indeed, if we were to sum up our complaint, it would be that the sociology of science gives far too much credence to sham and corrupt claims about science and is itself quite insufficiently critical (and thus not quite scientific). Our aim is a sufficiently critical sociology of science that begins with the sifting of the grain from the chaff. Science is of supreme importance as a human endeavour. It is however in the hands of people who do not always subscribe to that view. Most of the powerful and many of the lesser ranks are normal scientists Kuhnstyle, i.e., plumbers, hacks, and worse, whose primary interests are allowably a living, a career, or, in the less allowable, more ambitious, the accumulation of power, prestige, and the safeguarding of turf. On the most charitable interpretation, all are careerists. Abundant evidence for this is available directly or through the critical reading of the current, uncritical sociology of science. A critical sociology of science would be one that knew the difference between science as the disinterested search for the truth about the world, wherever that is pursued, and the various manifestations and masquerades that parade themselves as institutionalized science or the Voice of Science in the present world. Yet we do find the miracle of petty individuals producing noble harvest, a miracle that interested many an author[9].

Our impression is that Merton's studies of science, and those of his students, Barber, Zuckerman, Ben-David, etc., stemmed from a genuine respect for science as an enterprise and for its importance in the human adventure (sociology being part of it). Merton's work disclosed some interesting social novelties, not all of which were attractive, as, for example, the Matthew effect, namely, the principle of cumulative advantage. The very notion of gaining or taking advantage struck us, on first reading, as the exposure of corruption at the very heart of emerging big science. Merton did not treat it that way, preferring a detached functionalism: possibly, it serves the interests of science that such an institutional deformity persists. True. It does serve certain functions but they may have nothing to do with science as the pursuit of knowledge. After all, the idea that those with strong track records should be showered with advantages is inductivist and philosophers, at least, know that Hume showed induction to be utterly, irremediably invalid. Is this principle then really the best that the invisible hand of society can come up with for science? Is the efficiency that that editors of periodicals enjoy by yielding to the Matthew effect not a public loss? If so, then the function that the effect serves is short-term: in the long run it is a nuisance.[10]

Leaving Gieryn's Mertonian Science section for now, we move to his subsequent coverage of Making Science and Science as Authority. These two succeeding periods or "lines of inquiry" that he isolates raise serious doubts about whether there remains any "genuine respect" for science, both of the natural science under study and of the sociological science in practice. Deeply embedded in these two later lines of inquiry we find the three faulty philosophical assumptions outlined at the beginning of this paper: face-value respect for authority, uncritical mixing of the sociologies of science and of technology, and the claim that intellectual progress (in science) is sociologically explicable (reducible). Lurking behind all three assumptions we find an attitude that is quite different from that of Merton: debunking or demystifying. We have the strong impression that between the lines of studies of this later strain of the sociology of science hides the aim of unmasking science and presenting it as just another form of work, not too dissimilar from factory work or plumbing or engineering, even story-telling ("narratives" is the post-modernist jargon for this). Of course, studies of science might benefit greatly from contrasting it with these sorts of activities. The question is, do sociologists of science keep a sharp eye on where science *differs* from these sorts of practical, technological modes of work? At some level they must, otherwise they cannot claim to be doing sociology of science. What then do they allow is special about science? Not its claims to be pursuing the truth, which they class amongst the persuasive rhetoric to which scientists resort. Its authority, its publicly recognized claims to authority, the public ascription of authority, these qualities make science special to these sociologists. They often do this while pretending to be offering value-free research; yet it makes them power-worshippers. If only they were a little more critical and skeptical.

Let us insert a fragment of sociology here using a different comparison. Art is a form of practice, a form of labour. Writers, musicians, visual artists, all labour at what they do. Most of them must train for long periods, apprentice to masters, and work away with materials as they put together their works. The result may be written works, cut records, and visual art works. They expose their works to peers – publisher's readers, hanging committees, gallery owners, other artists – and, if successful, to the public at large. Their work is subject to the judgement of peers whenever they seek exposure to the public. Their work also enters a popular market which is rough and tumble ─ at least a bit more than the specialist market. They may have very high aspirations, as did Bernard Shaw, Somerset Maugham, and Rudyard Kipling. All three writers were very successful with the general public and sold many copies. Yet the judgement of their peers was mixed. Kipling's sinking reputation with the literati was more or less single-handedly rescued because T. S. Eliot, a high priest of literary modernism, had a change of heart. In the nineteenth century Mendelssohn and Mosheles rehabilitated the relatively neglected J. S. Bach by daringly moving his immense *Mass in B Minor* from the cathedral to the concert hall. In our own lifetime John Constable moved from chocolate box to towering genius. We do not deny these are examples of social processes. Our claim is that the social process has a goal: the recognition of aesthetic merit. And aesthetic merit as such drastically differs from the social recognition of it, and the proof of this is exactly the cases at hand, where the two diverge. (This proof holds even on the false assumption that merit is potential recognition, since the potential and the actual still differ.)

At issue here is the difference between what the ethnographer could find out by being critical as opposed to uncritical. If a meretricious artist is treated with great reverence and showered with honours, then it is incumbent upon a critic to report that. But is it disingenuous to overlook that underlying the struggle for reputation and honour today there is a yearning for artistic standing and appreciation forever? Rhetoric is in use here as everywhere else, of course. That is the way of the world. And some artists are strongly interested in this-worldly success. None of their activities makes much sense, however, without the underlying yearning. This applies to all kinds of success, worldly success as well. There are easier ways to get worldly success than practicing the arts. The double demand for worldly success and being a great artist sometimes incorporates the idea that initial scorn for a work of art is a mark of its ultimate worth that will ultimately find its just recognition. Hence, misunderstanding and suffering are to be welcomed as promises that better is to come. And "better" may mean simply future reputation, an obvious social construct. We say that this is true as far as it goes but it does not go nearly far enough. The artist hopes the reputation derives from ascending Parnassus.

How does art compare to science in this respect? Very closely, in our view. Working in pure science, let us say cosmology, requires arduous training, intense intellectual effort, some degree of competitiveness, and poor prospect of jobs. What drives such actors? What do we lose if we leave out or explain away the basic one, the attempt to get it right? To find the ideas that will survive testing against the facts and move us a little closer to a true picture of the cosmos? Unlike the ability to produce an acknowledged artistic masterpiece, however, there is little market reward. There is no pure science equivalent to the hugely rewarded Picasso. On the average, a plum job in academe for a cosmologist will pay far less than the medical specialty of cosmetic surgery. Although there seems to be a struggle for power and research money, and although oldsters seem to resist new ideas, what are the material stakes? They are all within the narrow band of bourgeois levels of living. To get filthy rich you have to find something you can patent or copyright and in cosmology there is nothing like that. (True, a cosmologist may become an icon and then win fame and fortune. This is as rare as the proverbial unknown youth who manages to climb the entire social ladder to become a Pope or a President of the United States.)

So, to continue with the comparison between art and science, the sociologist can readily view both as simply individuated kinds of work. No quarrel. They can see them as thoroughly social. The media of art are comparable to the media of science, the latter being natural language, scientific terminology, and mathematics. Media and languages are congeries of social institutions. By means of these institutions artifacts are produced and discussed. Both involve work, some carried our in private some in social settings of all sorts. Both have been monopolized by the romantic philosophy that which pictures thinkers as heroic, titanic, alienated, misunderstood, prophets without honour in their own time, and other fancy stories. And both put their products out into markets of sorts. The art market is rather different from the market into which scientific productions enter, be it the *initial market*, the *futures market,* or the *general public market*. Indeed, as soon as the market uses money we might suspect that the discussion has shifted to technology or at best to applied science. One need not be a snob to see the usefulness of this differentiation. The aims of technology, applied science, and science differ; hence, their sociologies need to differ.

Technology is social – from its stipulated problems through to its mandated licensing, safety and quality régimes (Jarvie 1966 ; Agassi 1985). Applied science is not quite the same. Its problems are sometimes technological and sometimes not. Medicine is a prominent example of applied science (as well as of technology). There is little or no pure "medical science". (Theoretical pathology is the paradigm here, yet the very concept of theoretical pathology was introduced only in 1959 or later.[11]) What there is, is biology in various of its branches, especially molecular, biochemistry, and chemistry, and various inputs from engineering, statistics, and so on. The structure of DNA was a discovery in pure (molecular) biology. It answered the question, what is the structure of the replicating molecule? This piece of pure science has consequences of the greatest importance to forensics, to genealogy, to the understanding of certain kinds of disease, and of course to medicine more generally. Thinking up ways to apply pure science to the world is applied science. How could DNA be used as an identifier? How could DNA be modified for various purposes? These are questions of applied science. Technological problems catered to the need for better methods of identifying criminals. The use of identifiers would aid us to link family connections, to link the whole human race as one family, to link all life to Darwin's famous Tree of Life. In the half century since Crick and Watson's breakthrough the technology has been devised and refined to allow work on all these and other lines of inquiry. It remains possible that both the technology and the applied science can throw up results that refute pure science, or basic research.

(Notice: the applications of molecular biology to the theory of evolution belong to pure science, not to technology; the technology seen in research laboratories often belongs to pure science. And so it goes.)

It is the logical relation of contradiction that stands behind all science (Popper 1959, §11),) and the discovery of a contradiction can be a side effect of applied or technological applications as well as of pure research. As we noted, much science engages in refuting earlier, especially pre-scientific, ideas and thereby creating a new objective problem. Any reader of Galileo will know how patiently he tried to show that the evidence goes against received ideas, including those that are strongly intuitive. He marshalled the evidence to put intellectual pressure on the received ideas and compel the audience to see the reasonableness of the alternative. He was a great writer and of course deployed his terrific rhetorical and dramatic skills to put over his case. Such instances led some sociologists of science to claim that science is rhetoric. This is much too vague. That science uses rhetoric is obvious, yet, rhetoric aside, Galileo had a case to put over, namely, that some received ideas are false and better ideas are to hand. By contrast, the rhetoric of Kurt Gödel was so poor that as soon as his classic unreadable paper appeared people better at rhetoric than he was translated his ideas to more accessible idioms. Were science and rhetoric the same his followers not he deserve the credit.

The way we have just framed the matter is of course going to be said by sociological debunkers of science to be itself advocacy and rhetoric, an attempt to negotiate with the audience by manipulating facts, etc. No doubt all such lines of objection are impeccable. However, this applies to those we criticize as much as to us, and then the question is, does the decision of readers to follow our argument or our targets' depend on their ability to reason or on their susceptibility to rhetoric? Or will it depend on their assessment of our relative prestige? The sociologist is treating the intellectualist account of science as the received view and trying to marshall evidence to show that its lack of sociology dooms it. "There is nothing not-social about science" (Gieryn, p. 13696). Is nature social? Yes, nature is a rhetorical construct in our natural languages. True. Better question. Is nature *nothing but* a social construct in our natural languages? This again is much too vague. By "nature" we mean the natural context for society. Yet to represent and discuss nature we have to have society and language. Does that mean nature is "nothing but" its representations and discussions? Some sociologists are strongly tempted by this *non-sequitur*. Nature seems like a hologram to them: transparently created for social purposes. It is, but is or is not the hologram trying to show us something? More technically, in order to assert that X is nothing but Y one needs a larger context that includes both X and Y. That larger context is nature. Were nature nothing but its hologram, natural scientists would lose their task of exploring it. And no research in natural science will save it, no sociology of science, and no rhetoric. At this point we need to begin offering our own suggestions for the skeleton of a critical sociology of science.

Our Aims

By a sociology of science we intend a sociology of scientific institutions – from the Royal Society of London, founded (at least twice) in 1660-63, to the present day. By a general sociology of science we mean one that draws on an available general sociology, its treatment of institutions, and the place it finds for science in society (explicitly or tacitly – as is the general case in the social sciences). Our view is that the current general sociology gets science wrong, and that more specific sociological studies need remedy.

What is the problem? First there are the three philosophical assumptions we have been discussing. Second, can we compensate for the narrowness and partiality of most of the classical work in the sociology of science? Third, our most general question is, to what extent are the institutions of science typical social institutions, to what extent does their being institutions *of science* make them significantly different? (And significant for what?)

We suggest that the main function of social institutions is coordination.[12] Before language could develop there must have been institutions, or what we can call institutions (perhaps by mere extension): they had to grow hand-in-hand. All accounts of how language could have come into being see it as a collective endeavour, a means of improving coordination. The invention of language was undoubtedly a huge step forward in human ability to coordinate and became itself a major and universal social institution. As Goody and Watt argue (1963), the subsequent invention of written language was another quantum leap. It affected human memory, made possible the idea of law, and also the idea of assertions having in some sense an existence of their own. Without written language there is scarcely any science. Otherwise put: the enterprise we know as science is vastly different from those cognitive enterprises that exist in societies without writing.

A general sociology of science should, we agree, begin with language. Although ordinary language is in constant use in the dealings of the natural sciences – in the conduct of research as well as in the transmission of its results, including the teaching of science – it is a commonplace that the esotericism of science is partly due to its having a specialized language, or languages, that usually must be mastered before undertaking efforts to contribute to it. The growth of specialized languages was driven by various considerations, mostly quite legitimate: the more background information serves a discussion, the more terms that allude to items in it are of use; and these are technical terms, the use of which is conditioned on the acquisition of some information. The functional effect of technical terms, however, was exclusionary: those not familiar with the background knowledge assumed in a discussion are excluded from it. C. P. Snow discussed this already in his Rede Lecture of 1959.[13] He saw the culture of science as a specialized subcategory of culture in general, utilizing, for example, ordinary language some of the time but finding the need to invent and use technical languages a lot of the time. Since these technical languages were not taught to every schoolchild, and since they were not easy to master, the effect was that science placed walls around its activities. These abstract, social walls preceded the gated compounds with barbed wire, with security and other clearances that are common on scientific campuses since World War II.

Social anthropologists used to teach their students that mastery of the local language was a *sine qua non* for successful fieldwork. Not so sociologists of science. Although they often pretend to be following anthropological methods, they also declare and valorize the distance that not understanding the science they study allegedly gives them. They look at the work practices and at the discourse as social interactions directed at social ends (persuasion, power, research money, professional prestige) only. The assumption seems to be that in a social interaction all that is at stake are social ends, power, money, status, all achievable by persuasion. What argument persuades? No answer. Hence, neither logic nor language supports their position. Social activities often have non-social ends, such as biological reproduction or building shelter. Reproduction and shelter have social aspects but it does not follow that they are social *tout court*. Or, if someone maintains that they are, then everything is social and we are mired in sociological solipsism. Language, similarly, is used for purposes other than linguistic – to describe, to express, to reason with regard to *something or other*. To equate any and every something or other with another linguistic object is to fall into linguistic idealism. Some philosophers and sociologists find this position obvious and correct. We find it obviously incoherent.

The walls of technical language conceal the first two markets discussed earlier, the *initial* specialized market and the *futures* market where the results move out to peers. The *public market* is different: there scientific ideas are offered with a gesture backward to the other markets. Since these markets are relatively closed to outsiders, the gesture is one of "trust us". When in an argument someone says, "trust me", alarm bells should go off. Why not just explain? It is too technical? Then make it simpler. Is that too hard? Is there any idea, any body of evidence that cannot be explained to people who need to know? Even so, at least one can try. Consider the changes in medical practice in the last generation. Many of us can remember when doctors were trained to behave like gods and to invite us to think of their expertise and technology as a magic wand: the patient should let them wave it and be grateful if the outcome is positive. This attitude was inculcated into medical students, and to some extent it still is. And yet, long ago there was a countervailing institution known as the second opinion. It presumed fallibility and was invoked often when a lot was at stake. It was also resisted.

The atmosphere has improved somewhat, the air is a bit clearer. Today medical plans insurance companies are ready by default to pay for second opinions. Today some medical schools teach that the best expert on a patient's ailments is the patient. The patient monitors minute to minute, the patient directly experiences therapy, the patient is the single factor connecting all the different doctors, nurses, lab technicians and others employed by the newly patient-oriented institutions. It is the patient with whom medical professionals should engage in a careful discussion of the case, what is known about it, what is not known, what research shows as regards various kinds of therapies. This change is coming about in a legally regulated profession, one with much to gain from a hermetic and authoritarian attitude.[14] The relatively clear organization of the profession makes it more tractable to reform. Both in the education of entrants to the profession and in the institutional machinery of practice it is possible to initiate reform and to monitor its success. It is an ongoing question which way and how much the posting of medical information on the internet will impact this process.

Gieryn refers to science as a profession (e.g. Gieryn, p. 13697), also comparing it to plumbing in undertaking "double tasks". His language is illustrative of a tendency to hyperbole, as are references to society's need for "certified knowledge" (Gieryn, p.

13693). These comparisons distort and exaggerate. The exaggeration is in the wrong philosophical direction. Science is better not treated as a profession.[15] The disorganization of science is a feature that functions to promote free inquiry. The amateurs, dabblers, dilettantes, cranks, etc., are not wholly excluded. The undertaking of tasks that are very far from "doable" is just as characteristic of science as the reverse, and closer to its philosophical better self. The problem with factory work and plumbing as analogues for science is that the kind of imagination such work requires bears little or no resemblance to the kind of imagination required to change our views of things. As to whether society has a functional need for certified knowledge: what does society do when it has no such certified knowledge, when certified knowledge fails, or when certified knowledge is declared no longer certified? The answer is, it does without. The reason is, in a deep sense all certificates of knowledge are fakes, no matter how many social endorsements they bear. "Certified knowledge" or "Certified expertise" is a myth; the need for certified knowledge is a myth. (Not so certified competence, namely, certified technical expertise.) We all want sociologists to separate the fact of myth from other social facts. Yet received sociology of science ignores the difference between the two. This wins it the status of a myth.

"Science" is a rather loose expression (dating from the 1850s). Its German cognate "Wissenschaft" is even looser (allowing for such a term as "dogmatic science" to denote theology and the law). Hence the range of institutions and roles that need cataloguing in basic sociology of science is wide and difficult to demarcate. High school and undergraduate science instruction is normally the beginning. People without such background we regrettably no longer consider scientists. The scientists of the Scientific Revolution sometimes had no scientific education; they were self-educated. Later examples would be Michael Faraday and Thomas Alva Edison. Francis Upton, who was Edison's assistant, had a Master's degree. Science is more formal nowadays and a PhD is normally required. Such credentialing is recent, not necessary, and is as much a barrier as a qualification. Polanyi explicitly says barriers are needed to keep out cranks and dabblers. Given that from Kepler to Schrödinger, Pauli, Dirac, and Pauling many scientists of the highest rank have also been cranks or dabblers of one sort or another it is difficult to empathize with Polanyi's fear of the damage they might do.

Elementary forms of the Scientific Life. Let us then take a step backwards. To make things simple, let us date science from the Scientific Revolution. We have then to start with individuals who were making science only in the sense that the outcome ("Science") was an unintended consequence of their activities, which might just as well be described as tinkering, dabbling, observing, speculating, discussing (often by letter), and, particularly and significantly, having great fun. We would say that science was an unintended consequence of those informally organized inquiries and discussions and that it flourished before it was named, it flourished under its first name, "natural philosophy", and later when the preferred name became "science"; the term changed its meaning to allow for the revolution in physics of the early twentieth century. Today's sociologist of science may object to beginning with these elementary forms of the scientific life. After all, we are constantly reminded that, as Derek J. de Solla Price has put it, the majority of all the people who have ever been considered scientists are alive today, and "we should have two scientists for every man, woman, child and dog in the population".[16] Even if this is not an equivocation on the word "scientist", it hardly explains why so many sociologists and historians of science do historical case studies, often in the seventeenth and eighteenth centuries.[17] What does explain this and what trumps the argument from taking the majority of those alive today as a norm is that the sociology of science is primarily concerned with institutions. Just because the earliest institutions are informal, and it is the informal institutions that create the first formal institution – the Royal Society of London and its imitators – does not mean that they are sociologically out of date. To the contrary, the staying power of institutions is well known. Institutions that just grow, being often below the radar, sometimes display greater staying power than designed institutions. In the excellent BBC dramatization of the Crick-Watson discovery the film makers go out of their way to show the great researchers as also tinkerers, eclectics, dabblers, enthusiasts, and so on.[18] To this day one of the characteristic institutions of laboratory science is the group. This may have formal aspects (all those paid to work in a given lab on a given project), but it also has informal aspects of a Simmel-style relatively loose social network, since there is interaction at the edges with non-group members and with other groups, there is the informally developed network with other researchers around the world, there are the water-cooler conversations, the encounters at meetings, the tips handed over, none of which can be easily mapped. They are like the complex and shifting social arrangements among any group of people who have something in common about which they are eager to communicate. Within some groups there is a great deal of tinkering, dabbling, speculating, brainstorming. This is the sociology of science at level zero. One of the favourite representations of the future scientist in the mass media is the kid who tinkers in a kind of workshop-cum-lab young-Edison-style. Are the media so far from the truth?

To put the matter a little more formally, in its pre-history and early history science emerged within civil society and the associations that made possible. When formal institutions emerge the role of civil society does not disappear. Within scientific institutions there are informal civil society associations and these are especially important in leaping over and around the fences of the institutions to formal and informal associations beyond.

The Growth of Institutions. Given institutional patterns of scientific activity producing science, there can emerge formal scientific institutions and scientific education. The earliest scientific education is mathematical. That was Galileo's training. That was Descartes' training. That was where Newton's and Leibniz's innovations lie. Already at Pisa, where Galileo studied, mathematics was part of the university curriculum. The first modern[19] labs were private: at the courts of wealthy dilettantes who wanted to tinker; in the studies of those with the space and the know-how. For over two centuries The Royal Observatory at Greenwich employed one astronomer and no more than five assistants – the only professional scientists in Britain till 1800, when Britain's Royal Institution was founded, which conducted research and communicated to the public, and was and is a stand alone from academe and the Royal Society. The laboratory as institution developed as an unintended consequence of tinkerers wanting to have a place to tinker and to keep their equipment. On the model of the Royal Society, even before it was professionalized, all kinds of institutions grew up to bring together mathematicians, statisticians, physicists, geographers, anthropologists in Britain alone. The first scientific laboratory in an English university was the Cavendish Laboratory in Cambridge. It is sociologically important to stress all this because in the long historical perspective the association of science with universities and big business is recent and anomalous, not natural or predestined. As for the military, we know from Merton's first monograph (1937) that the association is long-standing. This does not mean that the extraordinary intrusion of the military during the Second World War and the Cold War should be taken for granted. Merton was correct in saying that communism was fundamental to the ethos of science. Hence, the large amounts of military money tied to secrecy and isolation were and still are obviously inimical to a communistic community of scientists.

Little modification of this is needed if we shift focus to the growth of labs in industry, especially the chemical industry all over Western Europe in the mid-nineteenth century. Tinkering with the product, studying the issue of scaling up from lab bench to industrial capacity, and so on, were the obvious incentives for in-house labs. That they should gradually add to their repertoire the exploration of possible new products is something that comes from the imperatives of industry even though the pursuit requires science, applied science, and science-based technology.

Final resting place: academe. Last but not least we have academe, which gets onto the science bandwagon only in the mid-nineteenth century, and in some countries very late in the nineteenth century. By that point there is some consensus on science teaching needing laboratory work, there is some promise of scientific careers, there are organs of publication, and there is some agreement on modifying the divisions of knowledge handed down from Aristotle (an example of the longevity of institutions) so as to create Departments tasked with responsibility for particular areas of scientific inquiry. At its simplest that division was physics, chemistry, and biology, as expressed in the founding of technical universities and schools of engineering in older universities in the United States and in Europe. Readers are invited to look at the web site of their own university or some other and count the number of divisions and sub-divisions of these three they can find nowadays. They can also reflect on the variant names and on the different organizational charts of each university.

This departmental fissiparousness is sociologically under-explored. Every department that splits has usually had the activities that will now be split off going on under its aegis. What drives such splits? Autonomy is the most obvious. But then this obviously has its limits, and sociologist of science might profitably explore just what forces were thought to be ranged against autonomy within the previous structure. Is it as simple as being resistance to the new, including new challenges to the boundaries? And why, when a split is engineered, do newly minted departments have a tendency to draw up their own boundaries and make efforts to police them? That is, why do they reproduce the institutional resistance to autonomy the split was designed to overcome? This too is a case of what we mean by persistence of institutional patterns over long periods of time. Why are boundaries and boundary-policing so part and parcel of academic life when they are not inherited from the tinkerers, dilettantes, dabblers, observers, and speculators out of which academic science developed? Why is the curriculum such a battleground in almost every university department ? Why do universities consider it their place to promise that someone with a degree in such and such will have mastered the subject of so and so? Professions often enough administer their own tests for admission and consider it sufficient that the syllabus is made public. It is interesting to read how things worked in German-speaking universities in the nineteenth and the early twentieth century. Once enrolled, a student could wander from course to course in any part of the university and from one university to another, studying whatever took their attention. When they wished to graduate they needed to opt for an area and be sure they were prepared for the examinations in that area. They could emerge with degrees in the most unusual and hybrid combinations. (There were exceptions, for sure, the law and medicine, especially medical specializations, being the most obvious ones.) It is unclear that any harm was done by this to the sciences or to the students.

To answer some of the questions in the previous paragraph sociologists might have to look at the intellectual stakes. More often than not, fissiparousness is promoted by disputes about the world and consequent disputes about how research into it should proceed. There are schools of thought in science over intellectual matters such as which problems are interesting, which lines of inquiry are promising, what certain research approaches show, and so on. These schools sometimes have great longevity and are in constant running battle over the nature of things. Inquiry into the nature of things being a form of friendly-hostile cooperation, to use Popper's apt expression, can naturally lead to collateral disputes about power which can be used to discourage, underfund, underpromote, and generally disparage other parties. To ignore status and power as objects of the exercise is a very naïve utopianism. To see status and power as the sole object of the exercise is worse: it is a very naïve reductionism, the sort of thing that used to be called the viewpoint of the observer from Mars. (Sociology itself is a case in point. What distinguishes the *Journal of Classical Sociology*, say, from all of the other journals?

One answer is that it takes the classics seriously; another is that it encourages sociological research that denies the relativist version of social constructivism.[20])

Finally we come to the modern era, the era of Kuhn, the apologist for the new bureaucratic routinization of science so often taken as the norm in today's received sociology of science. Kuhn divided the life of a science into two cycles, the normal and the exceptional. Normal science consisted of reasonably routinized work on the foothills of an established paradigm. Exceptional science was the situation that developed when the established paradigm began to seem to be in trouble, its anomalies and patches making research difficult or fruitless. A period ensues in which there is a struggle either to renovate it, or to ditch it and seek a new paradigm. This revolutionary period Kuhn viewed as not a period of science at all, but as an interregnum. Research is disorganized hence of unknowable status. (The dabblers and cranks are ascendant!) Science only begins again once a new paradigm is established, generally received, and plays the role of the new base for newly routinized research. We don't know how to resolve the contradiction here between there being "normal science" and "revolutionary science" and revolutionary science not being science after all. What we do know is that much received sociology of science to an astonishing degree swallowed this picture wholesale despite the fact that any observation of any historical detail should lead a follower of Kuhn to the view that the science observed is in an interregnum. It is not mentioned by Gieryn because, we conjecture, he takes it for granted as "deep background". We conjecture that it is buried deep because, notoriously, Kuhn considered the social sciences to be pre-paradigm and hence pre-science or at best proto-science. The counter-claim that they were a new species, multi-paradigm was fruitless. Multi-paradigm means many, which means none in Kuhn's system, where there must be consensus on a single model to make a discipline scientific. Kuhn's admission that his theory is sociological and that the social sciences are multi-paradigm is an admission of defeat. Hence his inspiration is also an embarrassment.

Normal science, Kuhn said, is most efficient. His theory looks attractive as it offers a view of science according to which in science tinkering, dabbling, and speculating in a dilettantish way are all minimized. It has science reasonably routinized so that comparisons can be made with the factory or with plumbing; this allows the assimilation of science to the sociology of work. Yet both factory work and plumbing are technological. The tendency then is to see science as a form of technological work.[21] Scientists are all like Edison, Eastman, Bell, Land working towards some technological end-product. Cosmology is of course the challenge, since it invents nothing tangible, it operates comfortably with several competing paradigms, and is very high-powered. If we look at the strange attempt of the ethnomethodologists Garfinkel *et al.* (1981) to reduce astronomers' activities to routinized work (on the pulsar) we find that the lab notebooks, the recorded lab chat, the discussions of what it is their instruments are registering, are all used to show that they produce (i.e., make) the pulsar on exactly the same model that actors generally produce society by and in their interactions. Hence there is no "Galilean object" called a "pulsar" discovered in the Heavens. There are certain arcane social practices of instrumentation, processing, and discussion that lead to discourse that misleadingly suggests that such is the case. The actors produce a play called science and the play tells a story.[22] The story is not real, only the acting and production are real. Since on this model restaurants produce not food but the story of food, we would say: the sociological solipsist is the one under an illusion. Or perhaps there is no sociology of science and no ethnomethodology; it is just a make-believe that there is.

Ethnomethodology, in all fairness to sociology, is only one school or paradigm for the sociology of science. Like all the other paradigms, it ignores the paper and pencil[23] cosmologists, preferring to study those who can be found "at work" in a "workplace". But this is general in the sociology of science. The farther away they can get from intellectual work the more confident they can be. The more they can focus on discourse and less on ideas the more startling their conclusions. Above all ethnomethodology is nakedly idealist; we have labelled it a version of solipsism, "sociological solipsism" (the world, the facts, the pulsar, are social dreams). They are like the famous solipsist Christine Ladd-Franklin, who wrote to Bertrand Russell that she could not understand why everyone doesn't adopt her position.[24] Not all sociologists of science are idealists veering towards solipsism. But we have said enough to show that there is widespread endorsement – be it serious or not – of poor philosophy and in that sense it is uncritical. This is funny. Above all they want to be critical, to be radical. They think of themselves as debunkers, demystifiers. Instead they take at face value all kinds of myths, illusions, delusions, errors, *non-sequiturs*, and refusals to face the facts. To do that is to endorse, or to re-endorse, the *status quo*. This is certainly a counter-Enlightenment project. As it is unintended, we may invite its practitioners to return to Reason.

REFERENCES

Agassi, Joseph, 1960, Methodological individualism, *British Journal of Sociology*

Agassi, Joseph, 1985, *Technology*

Edwards, Derek, Malcolm Ashmore and Jonathan Potter, 1995, Death and furniture: the rhetoric, politics and theology of bottom line arguments against relativism. *History of the Human Sciences* **8**(2):25-49.

Garfinkel, Harold, Michael Lynch and Eric Livingston, 1981, The work of a discovering science constructed with materials from the optically discovered pulsar. *Philosophy of the Social Sciences* **11**(2):131-58.

Gellner, Ernest, 1973, *Cause and Meaning in the Social Sciences*, London: Routledge.

Gieryn, T. F., 2001, Science, Sociology of, in *International Encyclopedia of the Social & Behavioral Sciences,* pp. 13692-13698.

Goody, Jack & Ian Watt, 1963, The consequences of literacy, *Comparative studies in society and history,* **5**(3):304-45.

Jarvie, I. C., 1966, "The Social Character of Technological Problems",19 , The social character of technological problems. *Technology and Culture*, Vol. 7, July, pp. 384-90. Reprinted in Carl Mitcham and Robert Mackey, eds., *Philosophy and Technology*, New York: Free Press, 1972, pp. 50-53; also in F. Rapp, ed., *Contributions to a Philosophy of Technology: Studies in the Structure of Thinking in the Technological Sciences* (*Theory and Decision Library* 5), Dordrecht: D. Reidel, 1974, pp. 86-92.

Laudan, Rachel, 2000, Review of Oreskes 1999, *Philosophy of Science* **67**(2):343-45.

Merton, Robert K., 1938, *Science, Technology and Society in Seventeenth Century England*, *Osiris* **4**(2).

Oreskes, Naomi, 1999, *The Rejection of Continental Drift. Theory and Method in American Earth Science*, Oxford: Oxford University Press.

Oreskes, Naomi, ed., 2001, *Plate Tectonics. An Insider's History of the Modern Theory of the Earth*, Boulder, CO: Westview.

Popper, K. R., 1959, *The Logic of Scientific Discovery*, London: Hutchinson.

Snow, C. P., 1959, *The Two Cultures and the Scientific Revolution*. Cambridge: Cambridge University Press.

---

[1] In our works we have constantly argued against fusing science with belief, and counselled against "believing science". Belief is a private matter; a free society does not require that one believe this or that. One can endorse the scientific project without believing any of its findings. The focus on belief invites the accusation that science is a religion-substitute. True, some people use it that way. They simply miss the very different sort of cognitive effort science is, including its different sociology. From the days of the scientific revolution, its advocates repeatedly denied that it imposes faith in any doctrine. It is only because of that different sociology that some scientists can both conduct successful research and hold some antiquated religious beliefs.

[2] To the extent that the careers and power of sociologists of science in the academy depends on members of appointment committees who, as professional scientists, view their task as being to maintain the authority of science, to that extent the work of sociologists of science is as inherently suspect as any other public relations exercise.

[3] Sometimes sociologists refer to science as a profession. We take a stricter view: a profession is an organized and legally defined monopoly. Scientists, like any workers, can be spoken of as acting professionally without being members of a profession.

[4] See the interesting study of Naomi Oreskes (1999), and the comments of Rachel Laudan in her review (2000). Oreskes also co-edited an anthology of memoirs by those taking part in the scientific revolution in earth science (Oreskes 2001). In both volumes she resorts to the language of belief (see note 1 above). Our view is that this is unobjectionable and in all cases can be replaced with the language of "ideas" without

loss. For example, her aphorism "*science is not about belief; it is about how belief gets formulated*" translates seamlessly into "science is not about ideas; it is about how ideas get formulated" (Oreskes 1999, p. 6). This translation is helpful and one can see at once that it involves a false dichotomy : science concerns both ideas and their formulations. It is not about beliefs. Ideas can be formulated and promoted without being endorsed, still less believed.

[5] Oreskes (1999) argues most cogently that this was hindsight; several mechanisms had been put forward. Her explanation is different: resistance was due to inductivist dislike of theory-driven science. Oreskes also argues that inductivist American geophysicists operated with multiple "working hypotheses". It is unclear whether she sees that her model of this science is a fascinating counterexample to Kuhn's claim that genuine science requires consensus on a paradigm. The beauty of it is, geophysics is a "hard science".

[6] Oreskes is characteristically clear and blunt: "Scientists are interested in truth. They want to know how the world really is, and they want to use that knowledge to do things in the world." (Oreskes 1999, p. 3.)

[7] We do have some criticism of the piece as scholarship, though: we lament its failure to acknowledge the immense debt that today's received sociology of science owes to Kuhn, and through him to Polanyi; neither of whom is listed in Gieryn's bibliography.

[8] At the time when the USSR suppressed criticism of Lysenkoism and elsewhere it was freely aired the suppression was largely for the general public, not for professionals.

[9] Examples: "Some have greatness thrust upon them", *Twelfth Night*, ii, 5; Rudyard Kipling, "The Man Who Would Be King", 1888; Robert Louis Stevenson, *Father Damien: An Open Letter to the Reverend Doctor Hyde of Honolulu*, 1890.

[10]The generalized Matthew effect is famous in the literature on economic practices as statistical discrimination. See Art. Discrimination, statistical, in *International Encyclopedia of the Social Sciences*, Edition 2, Macmillan Reference USA.

[11] Peter Hucklenbroich in the opening of his "System and disease: On the fundamental problem of theoretical pathology", *Theoretical Medicine and Bioethics*, 1984, 5:307-323, views "Theoretical Pathology as a purely intellectual and theoretical discipline". He gives the date of its birth as 1959 or 1981.

[12] J. Agassi, 'Methodological Individualism', *Brit. J. Soc.*, 11, 1960, 244-70, reprinted in J. Agassi and I. C. Jarvie, *Rationality: The Critical View*, Dordrecht: Kluwer, 1987.

[13] C.P. Snow, "The Two Cultures", 1959, reissued 1993. Snow suggested that the teaching of technical terminology in schools to all pupils would help close the gap between scientists and the lay public and to that end he suggested emulating the Soviet educational system. This showed his unfamiliarity with the Soviet school system and his lack of understanding of what the acquisition of technical terms involves.

[14] This is not to say that cultivating the view of the physician as infallible is in the interest of physicians, even though many of them think so. See Nathaniel Laor and J. Agassi, *Diagnosis: Philosophical and Medical Perspectives,* Dordrecht: Kluwer, 1990.

[15] The scientific revolution established science as amateur and efforts at its professionalization began in 1830 and were completed only after World War II. See below. See also Dorothy Stimson, *Scientists and Amateurs: A History of the Royal Society*. New York: Henry Schuman, 1948. Even Michael Polanyi and Thomas S. Kuhn do not acknowledge this fact although it permeates their sociology.

[16] Derek J. de Solla Price, *Little Science, Big Science*, New York, Columbia University Press, 1965, p. 19.

[17] Of course, quite possibly the sociologists of science are often unable to follow "hard-science" texts of later periods.

[18] We are referring to the 1987 film directed by Mick Jackson, with Jeff Goldblum as Watson, Tim Piggott-Smith as Crick, Alan Howard as Maurice Wilkins and Juliet Stevenson as Rosalind Franklin. It is variously known as "Double Helix", "The Race for the Double Helix", and "Life Story". Another excellent depiction of science in a critical manner was the 1978 BBC series "The Voyage of Charles Darwin" which attended closely to the running dialogue Darwin had with Charles Lyell, Capt. Fitzroy, the geological and fossil evidence, and himself.

[19] Preceding these were the medical, alchemical and mineralogical laboratories that did not possess scientific status by the standards of the Royal Society of London.

[20] One of the most extreme of relativist texts we have come across (Edwards, et al 1995) was published in of all places the journal *History of the Human Sciences*, even though it was not historical but constructivist and philosophical.

[21] Indeed, as sociologists of work care for the quality of working life and so oppose excessive routinization, some of them reached the conclusion that if Kuhn is right then the quality of research life needs improvement too

[22] R.G. Collingwood used this theory theory to explain magic since he was reluctant to take it at its face value, refusing to view its practitioners irrational. The better solution is to take a more flexible view of rationality that allows some rationality to magic and greater rationality to science (Authors Reference 1).

[23] A famous joke ascribes to Einstein's wife the assertion that all a researcher needs is a pencil and a used envelope on the back of which to write something.

[24] "As against solipsism it is to be said, in the first place, that it is psychologically impossible to believe, and is rejected in fact even by those who mean to accept it. I once received a letter from an eminent logician, Mrs. Christine Ladd Franklin, saying that she was a solipsist, and was surprised that there were no others. Coming from a logician and a solipsist, her surprise surprised me." (Bertrand Russell, *Human Knowledge: Its Scope and Limits*, London: George Allen & Unwin, 1948, p. 180).