CAN SCIENCE STUDIES BE SPOKEN IN A CIVIL TONGUE?

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Abstract

The emerging field of "science studies" has finally reached a field of public visibility. Indeed, it is perceived as a threat to the future of science. Two prominent works of science popularization – Steven Weinberg's *Dreams of a final theory* and Lewis Wolpert's *The Unnatural nature of science* – devote entire chapters to describing and criticizing science studies. While neither Weinberg nor Wolpert believe that science studies will warp the minds of scientists, they do believe that it can have an unsavory influence on science policymakers who are looking for excuses to trim down expensive science. I examine the arguments that Weinberg and Wolpert make on behalf of science and against science studies, with an eye toward turning their charges into an opportunity for public debate about the future of science. I especially focus on how Weinberg and Wolpert mobilize the history of science for their purposes, and their implicit notions of the "scientific mind" and what constitutes a "rational" attitude toward science. One notable feature of their critiques is that they put positivist philosophy of science and relativist sociology of science – normally at loggerheads with one another – in the same boat as opponents to the idea that scientists should set the course of their own inquiries.

These two prominent works of science popularization demonstrate that something called "science studies" has now acquired a recognizable voice in the general intellectual discourse that helps shape public opinion and policy. It should come as no surprise that the field is portrayed in these two books as a kind of organized opposition within the academy to right-minded thinking about science. In that case, science studies practitioners now have an opportunity, or at least an excuse, to enter the larger public discourse. At first, this may be with an eye toward correcting the misperceptions that the likes of Weinberg and Wolpert purvey about our field, but in the process we may shed some productive light on the appropriate ways of talking about – and talking for – science in civil society. For a striking point of convergence between Weinberg's and Wolpert's accounts of science studies is that inspite of their belief that science studies is unlikely to affect the thinking of practicing scientists, they worry that our field may well have a dangerous effect on the thinking of nonscientists who make science policy. Indeed, this concern seems to have partly motivated the writing of both books.

Weinberg and Wolpert are accomplished science popularizers, though they represent two quite different routes to the genre. Weinberg, a front-line American theoretical physicist, shared a Nobel Prize in Physics in 1979 for unifying two fundamental forces of nature, electromagnetism and the so-called weak nuclear force. Since moving from Harvard to the University of Texas, Weinberg has been among the most distinguished and persistent supporters of the Superconducting Supercollider, that 53-mile underground particle accelerator, whose 10 billion dollars (and growing) pricetag has been the focus of many recent fights on the floor of the United States Congress. The timing of Dreams of a Final Theory is not lost on its author, who devotes the final chapter of the book, "Down in Ellis County," to a meditation on the possible benefits of the Supercollider to the decidedly unscientific citizens of Waxahachie, Texas, in whose backyard this massive scientific instrument would be built.

In contrast to Weinberg, Wolpert has had a broader-based, more applied, and less distinguished career in science. He is currently "Professor of Biology as Applied to Medicine" at University College, London, after having worked for several years as a civil engineer in South Africa. Although Wolpert explicitly retains the idea that physics is the clearest case of science, his most worked-out examples of scientific research come from the annals of cell biology and embryology – though rarely from any work that he himself has done.

Whereas Weinberg writes as a scientific native – so much so that it sometimes seems that all of recent physics has crossed his blackboard at some point – Wolpert conveys more of the air of a cicerone who is clearly knowledgeable about the artifacts on display but, equally clearly, not their designer. Consequently, unlike Weinberg's literary gravitas, which may strike the reader as bordering on the self-important, Wolpert's prose is sprightly, always with clear targets in sight. Yet, despite their different points of departure, Weinberg and Wolpert find the thing called "science studies" sufficiently threatening to the public reception of science that each devotes his most polemical chapter to putting down its claims.

1 When Positivism Is As Big a Threat to Science As Relativism

The most initially striking feature of the attack mounted by our two authors is not its actual portrayal of science studies, which could have been worse than it is, but the cast of characters who are taken to be playing on "our" side. To hear Weinberg and Wolpert talk about "science studies," positivist philosophy of science is just as much a threat to the conduct of inquiry as relativist sociology of science. In fact, a reader who knew little of either philosophy or sociology would come away with the impression that they were simply alternative strategies for undermining the credibility of science – or, more precisely, scientists. For even though Weinberg and Wolpert pay lip service to the conceptual distinction between scientists and the science they produce, their instinctive response to any perceived attack on scientists is to appeal to the peculiar nature of scientific knowledge as demanding behaviors and skills not normally exhibited by the rest of society.

Of course, philosophers and sociologists have quite different ways of challenging the peculiarity of scientific knowledge. Indeed, at least in the pages of this journal, they spend most of their time reminding each other of that fact. But to appreciate the source of our two authors' fears, we must examine the sinister similarities they sense. Philosophers have pointed to the possibility – if not the actuality – of a fully articulated method that would allow anyone to test a given scientific hypothesis. The attempts to computerize scientific reasoning championed by Herbert Simon and his followers take this tendency to its logical conclusion – and, not surprisingly, Wolpert is quick to deny the validity of their efforts. Concomitant with the philosophical stress on method is a devaluation of the scientist's creative powers. These are relegated to the "serendipitous," "irrational," or "random" character of the so-called context of discovery, which lacks cognitive significance for the larger scientific community until such creative urges are translated into a publicly accessible language of "testing" and "justification." On this account of the philosophy of science, sociologists would seem to be quibbling only over details: Does the scientist need to justify her claim to some unspecified universe of perfectly rational agents or simply to specific communities of imperfectly rational agents? Take your pick: The autonomy of the creative intellect is denied in either case.

Wolpert and Weinberg are extravagant in their attempts to demonstrate the distinctiveness of the scientific mind. But Wolpert is probably the more extravagant because he claims to be capturing a generic distinction between scientists and nonscientists, whereas Weinberg typically waxes only about the thought processes of elite scientists such as himself and his friends. However, the reader can judge a couple of examples for herself. Here is Wolpert:

> Research into how people reason about complex issues of genuine importance such as crime and unemployment again emphasizes the difference between common-sense thinking and more formal scientific thinking. At the extremes there are two very different attitudes toward knowledge. One pole is the comfortable ignorance of never having considered that things could be otherwise; the other is continual self-aware evaluation of the evidence and subsequent modificiation of views.... Only a minority (about 15 percent) to appear to have the latter capacity but scientists – even though they may not like to – have to adopt this approach.

Two points tell against this outrageous comparison. First, in what he would no doubt consider a regression to common-sense modes of thought, Wolpert manages to cite much of the cognitive psychology literature that speaks to the limitations and biases of ordinary reasoners, while he conveniently fails to mention that the same shortcomings have been identified in "experts" whose training was supposed to have remedied them. Second, what Wolpert takes to be the mark of the typical scientific mind – the ability to consider that things could have been otherwise – is the exact opposite of what Weinberg finds attractive about the most elite forms of scientific thinking, namely, the prospect of producing a maximally rigid theory, one whose parts could not have been otherwise. Such a theory Weinberg deems "beautiful":

> In listening to a piece of music or hearing a sonnet one sometimes feels an intense aesthetic pleasure at the sense that nothing in the work could be changed, that there is not one note or one word that you want to have different....The same is partly true (it is never more than partly true) of general relativity. Once you know the general physical principles adopted by Einstein, you understand that there is no other significantly different theory of gravitation to which Einstein could have been led. As Einstein said of general relativity, "The chief attraction of the theory lies in its logical completeness. If a single one of the conclusions drawn from it proves wrong, it must be given up; to modify it without destroying the whole structure seems to be impossible."

Weinberg goes on to observe that Newtonian mechanics lacks this sense of "logical inevitability," which partly explains why it was eventually superseded. Of course, more mundane, sociologically inspired accounts could be given for why the physics of the very big (general relativity) and the very small (quantum mechanics) seem to be pointing in the same direction these days – and I shall consider one of these below.

2 The Harder the Science, the Easier the Argument

Lest I leave the impression that Wolpert and Weinberg hold views of the scientific mind that are completely at odds with one another, it is worth noting that Wolpert comes around to Weinberg's position – at least temporarily – when he staves off relativist claims that science could have developed in some other, perhaps preferable fashion from the way it has. Although the individual scientific mind may be a great juggler of alternative hypotheses, the collective effort of science appears pointed in a single direction. Consider Wolpert's argument:

> In my view science, despite blips and errors, more and more provides an understanding of the world. There is one argument that may be persuasive – the role of mathematics. The quantitative aspect of science is fundamental. Probably even the most ardent relativists do not believe that mathematics is a social construct. Yet some parts of mathematics – often from unexpected areas – provide essential tools for describing particular phenomena. One cannot imagine a science of motion, a successful science, that does not rely on the calculus.

> If the relativists wish to persuade us of social constructs, they will have to provide, at the least, major counterexamples.

Putting aside Wolpert's apparent ignorance of what made the Strong Programme in the Sociology of Scientific Knowledge seem so "strong" to its followers, he fails to appreciate what the relativists take to be compelling about their case for the non-uniqueness of contemporary science. It is not the mere logical possibility of an alternative science radically disjoint from our own; rather, what compels is that contemporary science is merely the product of following one of several paths that had been equally open at an earlier point in our own history. These "paths not taken" can be recovered either by returning to the relevant turning point in history (as in Shapin and Schaffer's Leviathan and the Air Pump) or by exploiting ambiguities in contemporary scientific practice that point to alternative interpretations of what it means to do science (as in Collins and Pinch's studies of the "scientific" status of parapsychology experiments).

This sense of alternative science applies no less to the history of mathematics. Unless Wolpert wishes simply to beg the question about the necessity of mathematics to science – by building a certain conception of mathematics into the very definition of science – he would have to admit that the relevance of mathematics has been contested at several different levels throughout the history of science. Consider these two sites of contestation:

> (1) How exactly does one demarcate mathematical from non- mathematical forms of reasoning? Plato and Pythagoras are traditionally credited with the idea that mathematics is no mere calculational convenience, but inherent in the nature of things. However, most of what they called "mathematics" was in fact reasoning in ratios, which to today's eyes look more like such literary modes of expression as analogy and metaphor. In that case, when was the break with the literary made – if at all? This question becomes especially vexed, once we introduce the claims of Hobbes, Bentham, and most contemporary economists and cognitive scientists, all of whom believe not only in calculability of things human, but also in our ability to calculate as itself the essence of humanity.
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> (2) Even granting the autonomy of mathematical from non- mathematical forms of reasoning, what form of mathematics is necessary for the conduct of scientific inquiry? Do absolute magnitudes expressed by real numbers need to be pinned to the objects of science, as Platonists have traditionally claimed? Or is the Platonic preoccupation with numbers just so much notational fetishism that obscures the underlying set-theoretic relations of inclusion, exclusion, and exchange that are, in turn, rooted in mundane material practices?

By tying the stability of science too closely with the formality of mathematics, Wolpert and Weinberg overlook that mathematical reasoning has historically accrued several divergent interpretations, each of which has had profound implications for what counts as permissible mathematical developments or applications. In particular, our two authors fail to see that such "interpretive flexibility" actually increases with a greater formalization in thought, as formalized thought acquires the character of an abstract technology adaptable to multiple ends – a point that Wolpert himself subliminally acknowledges when he calls parts of mathematics "essential tools" for science.

Once mathematics is admitted as an abstract technology, one of Wolpert's intutively strongest arguments for the unnaturalness of science falls by the wayside. Wolpert latches on to the Popperian doctrine (as developed by Ernest Gellner) that knowledge gained for purely instrumental purposes tends to preempt further inquiry, as the community that constructs a successful technology will tend to jealously guard its uses, which, in turn, limits the possibilities for the critical transformation of that technology. According to Popper, science arose when people began to think about the significance of technologies not always producing the desired results, especially as they were extended to new domains. Presumably, the technologies in question had to work sufficiently well to yield the surplus needed to support the leisure of these early scientists – but not so well that the question of fit between technique and nature would never have occurred as a topic for inquiry. Thus history happened upon what has been called "the Miracle of the Greeks," or "Thales's leap," as Wolpert dubs it. Indeed, for all their other disagreements, a major point of convergence among Wolpert, the Popperians, and the few Marxists who respect the science-technology distinction is that science emerged as a spinoff from the factors of production and has been sustained only insofar as it has not had to contribute directly to the production of wealth. Of course, the Marxists who adhere to this history do not draw the same positive conclusions about the social character of science that Wolpert and the Popperians do. After all, one ideology's image of autonomous inquirers can be easily turned into another's image of parasitic elites.

But how would our attitudes toward science change, were we to learn that, as a matter of fact, so-called pure scientific inquiry did not emerge by becoming detached from the material conditions of life, but rather was itself an opportunistic way of coping with those very conditions? After all, the early Greek scientists – the pre-Socratic philosophers – typically did not enjoy the stipends of Egyptian, Persian, and Indian bureaucrats, for whom "technology" was coextensive with the state apparatus itself. In these instances of what Marx called the "Oriental Mode of Production," the bureaucrats had no motivation to articulate abstract physical principles that enabled, say, elaborate irrigation systems to work, because they had no interest in selling those principles so that other states could erect similar systems of their own. Rather, the bureaucrats could only envisage extending their own system to neighboring lands, making local adjustments to the technology where needed – with no clear line drawn between the physical and political dimensions of these adjustments.

For their part, the freelancing Greeks appeared attractive as outside consultants by offering the hope that lesser states could improve their political situation by following their advice. Thus, they had to persuade clients that there was a form of knowledge, access to which did not depend on investing enormous amounts of time and labor in a particular technology. In other words, they had to conjure up Latourian "immutable mobiles," namely, concepts that travelled well as the Greeks themselves moved from one venue to the next. Elementary principles of geometry, astronomy, hydraulics, and mechanics fit the bill well. From these entrepreneurial instincts arose, then, the prototype for the abstract physical principles that are so distinctive of science. The beauty of this account, of course, is that it rejects a view of the early history of science promoted by Wolpert and so many others – namely, that the Sophists, and perhaps even Socrates and Plato, marked a turn away from an interest in science to an interest in politics. On the contrary, the key role played by entrepreneurship marks Thales and Protagoras as kindred spirits who merely differed in the markets where they plied their wares.

Diagnoses Rampant, or Will the Real Relativist Please Stand Up! Among the more amusing yet instructive moments in the two books under review are when their authors speculate on why science studies practitioners fail to respect the epistemic uniqueness of science. First Wolpert:

> In a sense, all science aspires to be like physics, and physics aspires to be like mathematics. But too great an aspiration can lead to frustration....what hope is there for sociology acquiring a physics-like lustre? ...It is thus not surprising that, as Howard Newby, chairman of the Economic and Social Research Council, put it, because of their 'massive inferiority complex' social scientists have 'descended with glee on those who have successfully demystified the official credo of science and who have sought to demonstrate that science is but one means of creating knowledge'. For them it then becomes quite unncessary to have to try to emulate traditional science.

The problem with this diagnosis is that it misreads the recent history of social sciences and, as a result, overestimates the popularity of science studies among social scientists. In particular, Wolpert overlooks the clearest legacy of Kuhn's Structure of Scientific Revolutions to the social sciences: namely, to inform its practitioners that they too could constitute themselves as sciences by rallying around a "paradigm" and proceeding along the path of "normal science." Having finally discovered the secret of true science, most mainstream social scientists have remained ill-disposed to any subversive conclusions that science studies practitioners might draw from Kuhn's work. In particular, they have resisted the conclusion that by making more disciplines eligible to become sciences, Kuhn unwittingly diminished the sociocognitive advantage of being a science. Had social scientists (and even some humanists) not been successful in using Kuhn's theory to enhance their academic legitimacy, they would probably be more receptive to science studies. But as it stands now, one witnesses the spectacle of the most distinguished and erudite sociological theorist in the English-speaking world (Anthony Giddens) feigning both ignorance and disinterest in the epistemological issues raised by science studies – as if to raise such "reflexive" issues would be to undermine the legitimacy of his own project.

For his part, Weinberg countenances a broader and more interesting range of diagnoses, which together show that he has given some serious thought to the nature of science studies. First, Weinberg proposes that science studies is positivism's own reductio ad absurdum: It is hard to appreciate the special relationship that high energy physicists enjoy with ultimate reality, if one focuses exclusively on what is visible to the naked eye in the physics lab. If Percy Bridgman's operationalism failed to explain the practice of quantum mechanics in the 1930s, there is even less reason to believe that Latour and Woolgar's anthropologism would be able to do so in the 1990s. Although, as we have seen, Weinberg is content to be a reductionist with regard to ontological commitments, he clearly will not allow the spirit of reductionism to infect his methodological commitments. Once again we see the ease with which philosophy and sociology of science can be collapsed into a single disrespectful parody of the complexity of scientific practice.

Next Weinberg tackles the anthropologism of science studies head on. His argument here is worth quoting in detail, especially since it turns Wolpert's diagnosis of an inferiority complex on its head:

> Imagine if you will an anthropologist who studies the cargo cult on a Pacific island. The islanders believe that they can bring back the cargo aircraft that made them prosperous during World War II by building wooden structures that imitate radar and radio antennas. It is only human nature that this anthropologist and other sociologists and anthropologists in similar circumstances would feel a frisson of superiority, because they know as their subjects do not that there is no objective reality to these beliefs.... Would it be surprising if, when anthropologists and sociologists turned their attention to studying the work of scientists, they tried to recapture that delicious sense of superiority by denying the objective reality of the scientists' discoveries?

The only problem here is that Weinberg has gotten his schools of anthropology mixed up. The classic debates over of the "rationality" of cargo cults were conducted largely from two standpoints, neither of which has served as a model for science studies inquiries. On the one hand, Popper-inspired anthropologists denied outright the rationality of the cargo cults because the natives did not appropriately alter their practices in light of negative experience. On the other hand, functionalist anthropologists denied only the "manifest" rationality of the cults but admitted their "latent" rationality as the cults served to shore up group solidarity in tough times. While either of these responses would, indeed, fit Weinberg's charge of superiority, neither captures what ethnographers typically do in scientific workplaces, which is to confront the natives with their own accounts of their activities, bringing to their attention divergences of word and deed. And while the natives can usually make up the differences to their own satisfaction, the reader of the science ethnography is meant to be faintly scandalized that there should be any divergence whatsoever. After all, the natives under study are scientists!

Interestingly, despite the ill-chosen analogy from anthropology, both Weinberg and Wolpert periodically indicate that they half-realize how science studies works. The half they fail to figure out is why anyone would become disturbed by the prospect that much of the philosophically inspired talk of "truth" and "rationality" spouted by scientists does not capture science in action. Our two authors wonder: Aren't both positivists and relativists fixated on loose language, when they should be focusing on durable practices? Needless to say, some members of our own field – especially those who spend much of their time studying experimental practices – are not above asking such a question. And such a question is perfectly reasonable to ask, if you also happen to believe that science is nothing but a certain set of social practices, in which case you might advise we set our gaze on the details of the lab and set aside the philosophical public relations talk. Unfortunately, our two authors want to present science as too extraordinary to be captured in such sociological terms. In that case, they are themselves forced to draw selectively from the philosophical rhetoric and hope that it sticks to their practices when it comes time to justify spending billions of dollars on the next high-tech scientific instrument. For it is unlikely that the mere sight of an ensemble of scientists competently practicing their trade will be enough to move Congress.

Weinberg's last diagnosis, one he claims to draw from Gerald Holton, purports that science studies is only the latest expression of hostility to modern Western civilization, the roots of which reach back to Oswald Spengler's (1919) The Decline of the West. Thus, science studies researchers are said to be motivated by self-loathing for having to participate in a culture that has been responsible for the atomic bomb and other scientifically induced forms of destruction. We are portrayed as longing for a less alienated time, when inquirers were committed to preserving the objects of inquiry. Before turning to Weinberg's own curious response to this charge, a few words should be said about his attempt to draw an invidious comparison between Spengler's Weimar Germany and post-1960s Euro-American counterculturalism, of which science studies is taken to be the academic wing.

A common feature of Weimar thinking was the belief that Germany had lost World War I because of the collusion of scientists and capitalists, whose abstract ("calculative") detachment prevented them from dealing effectively with both the concrete and spiritual dimensions of war. The negative lesson learned was so great that Weimar engineers altered their professional identity from that of "applied scientists" to "self-conscious folk practitioners." Some even tried to justify engineering's inclusion among the Geisteswissenschaften. Moreover, it was generally held that the natural sciences, especially physics and chemistry, would not have been so influential under Kaiser Wilhelm, had their research not been so generously funded by a few major industries and banks with close ties to the state. These firms – like the scientists themselves – typically also had substantial international connections, which could be used to cast aspersions on the extent of their commitment to the German cause. The much-vaunted "cosmopolitanism" of scientists was thus publicly reinterpreted as an indifference to local interests, which, in this case, meant the nation. Even precision-made laboratory instruments, whose use required specialized training, were made to seem inimical to folk craft sensibilities. These points are worth bearing in mind when Weinberg blithely claims that science is "the shared possession of mankind." For one of the most important conclusions of science studies is that such a claim cannot be taken literally: It is one thing for a form of knowledge to announce universality for its claims; it is quite another for that form of knowledge to be equally accessible (both usable and contestable) to all who would would be governed by its claims.

This brings us to Weinberg's response to the allegedly antiscientific sentiments of science studies. In light of our brief glimpse into Weimar culture, the response is clearly open to the pejorative interpretation of "cosmopolitanism," whereby the author's privileged position prevents him from recognizing the power relations in which his form of knowledge participates:

> Even the most frightening Western applications of science such as nuclear weapons represent just one more example of mankind's timeless efforts to destroy itself with whatever weapons it can devise. Balancing this against the benign applications of science and its role in liberating the human spirit, I thinking that modern science, along with democracy and contrapuntal music, is something that the West has given the world in which we should take special pride.

Weinberg seems to find the destructive potential of modern science ultimately discountable primarily on grounds of banality. The novelty of science's benefits would seem to outweigh its contribution to evils that have occurred time and again. Such is the moral scorekeeping one would expect after several rounds of playing Hermann Hesse's "Glass Bead Game," but it is completely inappropriate for evaluating the overall impact of science. Problems that refuse to go away – or only get worse over time – may frustrate the mind that constantly craves new sources of intellectual stimulation, but that is not a normatively acceptable position for a scientist in today's world – certainly as long as words like "mankind," "the human spirit," "the West," "the world," and "we" remain euphemisms for overlapping sets of elites.

Here we arrive at a crucial point that trumps any charge that science studies is antiscientific. Science studies practitioners have acquired a healthy skepticism about the ex cathedra pronouncements that scientists make about their trade because scientists are not necessarily the most reliable informants about what science is. Instead of taking scientists at their word – which would amount to a very prescientific reliance on authoritative testimony – science studies practitioners "see for themselves," typically by going on site where the science is purportedly done. In a sense, I am merely reiterating the first sentence of the first course in our field, but it suggests why our two authors fail to see science studies as a natural extension of their own practices: to wit, Weinberg and Wolpert do not sufficiently distance "science" from their own circle of scientific friends, heroes, and the communal bonds these forge. Indeed, Weinberg is especially prone to lapse into a Le physique, c'est moi! manner of address worthy of the Sun King himself. But more than mere megalomania, this blurring of the personal and the professional threatens to undermine what has classically set science apart from more authoritarian forms of knowledge and their reliance on the collusion of elites.

Weinberg and Wolpert periodically admit that science has a "sociological" dimension. But by this they mean the Polanyiesque point that scientists constitute themselves as an autonomous community held together by bonds of mutual respect. These bonds cannot be grasped without prolonged contact with other scientists and learning to model one's own judgment on theirs. This process of perspective-taking and mutual modelling issues in common intuitions about acceptable and unacceptable paths of inquiry. Yes, sometimes heated controversies erupt, but typically they involve only a few elite members of the community who are keen in finding a quick resolution so as to cause as little reputational damage as possible – both to the participants and to the field. The fate of new recruits to the community is, of course, determined by the current crop of scientists. In short, science appears in our two authors' writings as a thoroughly gemeinschaftlich operation that fosters what I will dub bad relativism, namely, judgments that tend to reinforce – rather than compensate for – standing prejudices. If that was all there is to the sociological dimension of science, then, ironically, that should arouse, rather than dampen, public suspicion about the disposition of a community that would seem to have all the institutional trappings of a special interest group.

However, if we wish to take seriously Weinberg's claim that science is the collective possession of humanity, then we must relinquish the vision of science as Gemeinschaft. In the alternative vision, science belongs to more than those who are directly involved in producing it. Indeed, the purported universality of science's knowledge claims should make them accessible to all knowers. Of course, this is not realized in practice. To that extent, science has yet to earn its reputation for producing a form of knowledge that transcends the existence of its producers. But how can the science studies practitioner endorse the idea of a "transcendent" form of knowledge with a straight face? Here it becomes crucial to appreciate the good relativism that science studies has been promoting since the advent of the Strong Programme.

As Bloor and his followers employ the term, "relativism" is not an unconditional epistemological doctrine on par with, say, rationalism or realism. Rather, it is a methodological heuristic designed to counteract the science studies practitioner's own standing prejudices by revealing their limits – namely, their relativity to his or her own culture. Thus, one of the original four tenets of the Strong Programme, the controversial "symmetry" principle, compels the science studies practitioner to bracket any beliefs that he or she might have about the truth or falsity of the science under investigation. Thus, relativism in this sense offers the inquirer the opportunity for a kind of self-transcendence that is in line with one of science's most distinctive normative ideals.

Ironically, then, the cognitive "unnaturalness" that our authors detect in science does not seem to translate into a sociological unnaturalness. Instead, one finds a rather ordinary sociology of the close-knit community rooted in its peculiar prejudices. Where is the sociological correlate of "openness to change" in such an account? Particularly telling in this regard is the lack of attention that both Wolpert and Weinberg pay to cross-disciplinary and cross-national migration, patterns common throughout the history of science whereby scientists trained in one field or country are able to initiate a radical transformation of another field's or country's line of inquiry.

3 The Supercollider: A Big Bang for the Buck or the Heat Death of Physics?

Weinberg certainly cannot be faulted for a lack of candor in his defense of the Supercollider. The construction of this largest of all particle accelerators would enable physicists to recreate the energy levels that are thought to have obtained in the earliest history of the universe, just before the fundamental forces of nature became differentiated, thereby losing their primal symmetry. (Physics after the Fall?) Avoiding spurious, but perennially popular, appeals to economic and military spinoffs, Weinberg unabashedly rests his case for granting the Supercollider funding priority on the "fundamental" character of high energy physics research – that is, fundamental to all scientific inquiry: "...it is closer to the point of convergence of all our arrows of explanation." The argument has not been received well, even from within the scientific community. Reflecting on his own professional association's lukewarm support of the Supercollider, Weinberg can only conclude that the American Council of Physics has let concern about the livelihoods of scientists working outside high energy physics outweigh strictly cognitive considerations of what is in the best interest of science. Indeed, much of Weinberg's metaphysical commitment to "reductionism" boils down to a nonnegotiable opinion about the scientific significance of his own research.

In fairness to Weinberg, it must also be said that reductionism in contemporary physics is hardly the either-or doctrine that it was in the nineteenth century, when mechanists and holists argued about whether the universe was greater than the sum of its atomic parts. In fact, today's reductionist tries to reach both ends of the cosmic scale – the very large and the very small – at the same time. The key is the Big Bang, which postulates that the universe began much like the highly dense and energized masses that physicists now study in particle accelerator experiments. Thus, among the elite physicists who work on foundational problems, it is not uncommon for the same person – Weinberg himself is one of them – to author textbooks on subatomic particles and general cosmology. Given the centrality of the Big Bang, today's "final theory" aspires to much more than earlier attempts by Newton and Laplace. Whereas eighteenth century physicists would have been satisfied simply with knowing the rules whereby they could predict any event in the universe, followers of Einstein and Hawking also want to know how the rules came into being.

However, few seem to share Weinberg's optimism that building the Supercollider will bring physicists significantly closer to the final theory. With the benefit of historical hindsight, we can say that few – if any – research programs in science have ever voluntarily closed shop as a result of having declared their most significant problems solved. Research programs are typically terminated under one of two circumstances: Either problems accumulate more rapidly than can be solved, leading to a Kuhnian "crisis" in the research program, or other problems associated with other research programs appear more attractive to the new generation of scientists. Weinberg predictably hedges his bets by arguing that if the findings made on the Supercollider do not answer solve all the fundamental problems of physics, it will be because it has turned up some new particles and forces that will provide a new generation of physicists food for thought.

This last claim raises an interesting point of divergence between how Weinberg and science studies practitioners understand the relationship between theory and experiment. To be sure, there are common foes, namely, those whom Lakatos dubbed "naive falsificationists," who believe that one good crucial experiment can topple a well-confirmed theoretical edifice. But there the agreement ends. Science studies practitioners have been persuaded by the work of Hacking, Pickering, and Galison that traditions of experiment – and the phenomena that experiments generate – are largely autonomous from traditions of theory, and, indeed, may be decisive in explaining the long-term stability and cumulativeness of the scientific enterprise. No such sensibility is to be found in Weinberg, who devotes an entire chapter to showing how experiments are conducted for the express purpose of providing theoretically suggestive data, which are then often massaged or rejected, if they threaten to render a pet theory "ugly."

Because most empirical research in science studies has been so far concerned with the experimental sciences, it is easy to forget the peculiar sensibility that informs theoretical work, especially in that most elite of sciences, physics. Even a non-physicist such as Wolpert marvels at the ability of theoretical physicists to know how to intuit the perfect curve from otherwise unruly data points. Why can't theorists in, say, psychology enjoy such a harmonious relationship with their experimental counterparts? Instead, one finds a bunch of theories, each of which captures only a portion of the data, much in the way that each of the proverbial blindmen gropes only a part of the elephant. One answer might be – as Weinberg and Wolpert maintain – that physicists do, indeed, have that special capacity for intellectual intuition, nous, which Plato and Leibnitz so greatly admired and which Kant adamantly denied.

Another answer, one more in the spirit of science studies, is to observe that physicists are not as scrupulous as psychologists in accounting for the data generated by a body of experimental work. Since psychologists presume that their subject matter is quite complex and elusive, they do not pretend to have many clear intuitions about which data are worth keeping or throwing out when drawing some theoretically relevant inference. Consequently, psychologists have pioneered the development of sophisticated "meta-analytic" statistical techniques capable of integrating all the data from all the experiments. Needless to say, these techniques produce a rather complicated overall empirical picture of psychological phenomena, one that resists simple theoretical formulations. Physicists, by contrast, presume that they have a relatively clear understanding of their experimental situations, which, in turn, instills greater self-confidence in their discretionary judgments of data. This difference in methodological attitudes between psychologists and physicists draws on divergent folk images of their competence as scientists, images that have themselves been rarely subject to empirical scrutiny. Thus, while physicists are commonly seen as superior scientists, the most thoughtful and rigorous works on scientific methodology have been written by psychologists.

This last point raises the larger historical question of accountability in physics. One subtle but pervasive legacy of reductionism is that scientific theories are immediately discounted if they are incompatible with current physical theories. But do physical theories need to be compatible with theories in the other sciences? If the history of science is our guide, the answer would seem to be, for the most part, no. A vivid reminder of this point is Lord Kelvin's declaration that Darwinian evolution must be false because thermodynamics did not allow the earth to be as old as evolution required. After this pronouncement, many scientifically minded biologists started to rethink their commitment to Darwin's "uniformitarian" view of biological change (i.e., slow steady changes of the kind still seen today). Thus, the last quarter of the last century witnessed a physics-induced revival of the periodic geological cataclysms postulated by the rival "catastrophist" approach, which had been traditionally associated with Creationist attempts to read the Noachian Flood into the fossil record of the earth. Luckily, for the fate of Darwinism, physicists soon managed to stumble upon a refutation of Kelvin's chronology in their own bailiwick, once radioactivity was discovered as a source of heat that could have fueled the earth's core for the hundreds of millions of years needed for evolution.

As we reach the end of the twentieth century, accountability in physics is limited mostly to achieving synchrony between the various branches of the discipline. Because today's fundamental physical theories postulate that the world is radically different at the extremes (the very big, the very small, the very old, etc.) from the way it appears in the meso-reality canvassed by the other sciences, few non-physicists have had the temerity to argue that physics is incompatible or somehow out of step with current trends in the rest of the sciences. However, if one wanted to make such an argument, evidence could be sought by noticing the increased amounts of money being spent on scientific instruments, such as the Supercollider, which generate findings of little obvious cross- disciplinary relevance. Indeed, what would seem to be a liability according to some enlightened theories of science policy is made to appear a virtue here. Specifically, Weinberg argues that the integrity of particle physics is ensured by the fact that no other field can directly benefit from the billions of dollars that are sequestered exclusively for its purposes.

Given that these funds are drawn from the public coffers, Weinberg's argument turns out to be a profoundly anti-democratic one. For, in a democracy, one would suppose that a principle of fungibility operates in matters of public policy. In the case of funding high energy physics, the principle implies that physicists should not be granted billions of dollars for their work unless they can produce a scheme to benefit, or somehow compensate, those who will be deprived of those billions for their own work. For example, if sequestering billions for the Supercollider forced a decrease in funding for the social sciences, then part of the deal for constructing the Supercollider would include allowing social scientists to play an active role in designing the community that will be built to support the facility and, then, to do field work once the community has been built, perhaps ultimately advising the physicists on how they might alter their patterns of social interaction in order to improve their work. Were the physicists to balk at such intrusions on their autonomy, then they have the option either to seek funding in the private sector or to scale down their funding request so that they do not pose a direct threat to progress in other areas of inquiry.

The principle of fungibility is difficult to put into practice, especially when the policy concern is science and the country is the United States. The main reason is that no federal forum exists for comparing the merits of alternative research programs in different disciplines. Thus, the policymaker is typically never in a position to decide to pursue research in, say, biology instead of, say, physics. This is especially true in the case of "megaprojects" such as the Supercollider, whose funding comes directly from Congress. The unprecedented price and promise of megaprojects make Congress reluctant to judge them on anything other than "their own terms." Scientific advocates can often use this to their rhetorical advantage. To budget-conscious members of Congress, scientists will warn that to cut the Supercollider does not necessarily mean that some other worthy scientific project will be funded instead. In fact, it may not even mean that the money will be saved. The specter of a myriad of special interest groups waiting for the first opportunity to pounce on the money "saved" by not funding the Supercollider – in order to convert it into "pork" – is usually enough to shame Congress into continuing its support for a palpably noble (if not necessary) scientific cause. Given the support that fundamental physics research has enjoyed so far in the American policy forums, it should surprise no one that Weinberg does not complain much about this remarkably unscientific way of funding research.

4 Conclusion: Striking the Right Pose

Weinberg and Wolpert are the first of what is bound to become a trend in science popularizers acknowledging and answering the challenges posed by science studies to conventional accounts of science. But how should we respond to this newfound publicity? Clearly, there is no one strategy that will sit well with everyone who considers themselves part of the science studies community. However, let me close by proposing some general considerations that will help enable us to move coherently into the public forum.

First, we should be clear about the exact sense in which Weinberg and Wolpert find science studies "dangerous," "offensive," or simply "wrong-headed." What mainly riles our two authors is the suggestion that we claim to show that science is, in some deep sense, "just another social practice." Such a view undermines the attitude that they wish to instill in readers: namely, to make them feel that they understand enough about science to identify with its continued success, but not so much that they feel competent to participate in decisions concerning science policy. Cultivating this kind of respectful enthusiasm is no mean rhetorical feat. However, science studies makes it difficult for one to be a mere "fan" of science. Rather, one develops the impulse to jump onto the playing field and redo the rules a bit. Specifically, even a little science studies makes one wonder why one group of scientists manages to frequent the best labs rather than some other.

But can scientists and science studies practitioners go beyond mutual fear and suspicion – and toward public-spirited debate? As I have hoped to demonstrate in my own writing and in the writings of Weinberg and Wolpert, many important points can be made and issues raised, even in a less than ideal speech situation. However, to make the most of this situation, we must realize that not every theoretical commitment of science studies will travel well from the academic to the public sphere. In fact, some of these commitments may be used against us in debate. For example, we saw that one of Wolpert's grounds for dismissing science studies was that it seemed to make all science look like its parent discipline, sociology, which is, indeed, a highly contested, flexibly interpretable, interest-laden field. But can the same really be said of physics or chemistry – once an "adequate" account of their activities – especially their track records – is given? Needless to say, few science studies practitioners are likely to be moved by Wolpert's intuitions here. Yet, he has stumbled on a reflexive difficulty for our side. After all, to think that every science can be analyzed in the same way is to violate science studies's own commitment to the "heterogeneity of fields" and the "disunity of science."

Speaking for myself, I believe that science studies must be sure to couple its commitment to disciplinary diversity with an imperative to rearrange disciplinary boundaries (or "hybridize," to wax Harawayan). Otherwise, we run the risk of being interpreted as supporting – or being logically compelled to support – the "good fences make good neighbors" model of epistemic diversity that our two authors promote. Luckily, even they do not advance this line consistently. Both are concerned with what they call the "social responsibility" of science, which demands substantial public input. Wolpert is the more direct on this point:

> It is not for scientists to take moral or ethical decisions on their own: they have neither the right nor any special skills in this area. There is, in fact, a grave danger of asking scientists to be more socially responsible – the history of eugenics alone illustrates at least some of the dangers. Asking scientists to be socially responsible, other than by being cautious in areas where there are social implications, would be implicitly to give power to a group who are neither trained nor competent to exert it.

The reader, having gotten this far in Wolpert's book (near the end), may be surprised by these sentences. Apparently, the very "unnaturalness" that makes science our premier form of knowledge also makes scientists professionally ill-disposed to issue moral judgments. What should a science studies practitioner make of this claim? First, it reveals the absurdity of drawing a hard, virtually essentialist distinction between scientists and non-scientists. Just because scientists should not unilaterally dictate the ends for which their skills are used, it does not follow that they should remain mute in public deliberations. On the contrary, scientists must acquire a competence in the consummate democratic art of negotiation – especially with a public who will bear the finanical costs and sustain the eventual impacts of whatever research is commissioned. But perhaps more important, scientists must realize that the value dimensions of their activities extend not only to the capacity of their research to do good or harm but also to the opportunity costs that are incurred by deciding to fund one sort of research over another – or, for that matter, over a nonscientific yet worthy public works project. In short, part of the social responsibility of science is to welcome the public's participation in setting the priorities of the research agenda itself.

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