

Knowledge Policy: Where's the Playing Field?

As that archdeconstructionist Jacques Derrida might say, science policy is captive to the “metaphysics of presence.” In other words, science policy is treated as something that occurs only when traces of intervention are left (e.g. added funding or regulation), but not when such traces are lacking (e.g. allowing science to continue as is). Yet policy is always being made even when nothing is changed (Bachrach and Baratz 1962). Refusing to steer the course of science policy is a very potent form of science policy. One reason why this axiom of policy science is rarely given its due in science policy is that both the public and its policymaking representatives regard science as something that proceeds in a relatively autonomous fashion. Science policy is, therefore, something that intrudes, for better or worse, on this ongoing enterprise. In much of my earlier work (Fuller 1989: esp. Chap. 1, Coda), directed at the *internal history of science*, I wanted to deconstruct a bad pun that had been masquerading as a sound argument, to wit: If the trajectory of scientific research is subject to *inertial motion*, then the trajectory of science policy should be subject to *institutional inertia*. Even if the antecedent of this conditional were true, which it is *not*, only an inductivist of the naivest sort (or, in political terms, a traditionalist of the most conservative cast) would accept its consequent.

My original deconstruction had two immediate targets that will surface again in this chapter. The first target is the tendency of scientists (often under the influence of philosophers of science) to calibrate desires to match expectations so as to appear to be able to get what they want. This strategy usually involves an *adaptive preference formation* (Elster 1983). In this instance, scientists end up defining anything outside their sphere of control, such as funding and research prioritization, as “external” to the scientific enterprise and, hence, a drag on the scientific spirit. This strategy serves no one in the long run. Scientists look like what Marxists have traditionally seen them as—namely, benighted slaves for whom “freedom” is little more than the awareness that their masters can exploit “only” their bodies, not their souls.

The second target is the more general tendency to neglect the material consequences of satisfying intellectual needs. A way of trenchantly making this point is to observe that the maintenance of “free inquiry” normally entails the ability to pursue false leads with impunity. This capability materially involves the freedom to waste resources, which, in an age of increasingly expensive science, means channeling more funds away from other public and private interests. Here, too, we see what Marxists would call alienation of the scientist from both herself and her fellows. After all,

what joins scientists to other human beings is the space and time they take in the material world as expressed in the media of social relations. Moreover, an increasing portion of a scientist's energies is spent on activities that look more like the work of entrepreneurs and managers than that of "scientific professionals." Nevertheless, the scientist continues to believe that she is really in her own element only during the vanishingly small period in which she works with test tubes and formulae.

In what follows, I use a locution of my own coinage, *knowledge policy*, where one would expect to find "science policy." Part of the reason is to remind the reader that, even when the examples are taken from the natural sciences, the range of fields included for policy scrutiny include all the *Wissenschaften*. Ultimately, I argue that claims to funding and attention made by the natural sciences need to be evaluated alongside those by the social sciences and humanities in contemporary democracies. But, in addition, using the phrase "knowledge policy" sustains the point that once cognitive needs are taken in conjunction with their material realizations, the standard policy decisions associated with funding and accounting become *de facto* epistemological ones.

SCIENCE POLICY: THE VERY IDEA

The refusal of policymakers to steer science policy is nicely captured in one of the many tacit maxims codified by Harvey Averch (1985), former staff officer at the U.S. National Science Foundation. In contrast to other social programs, scientific research is held not to experience diminishing marginal returns on investment: *Any* research funded for *any* length of time will yield *some* benefit. This maxim could easily be regarded as a call to institutional inertia; the tendency to continue a policy, regardless of opportunity costs and rate of return, unless it has obviously negative effects that impinge on a politically sensitive constituency. As an instrument of knowledge policy, social epistemology is designed to address the sorts of issues that would otherwise be decided by institutional inertia.

At the outset, the social epistemologist needs to persuade policymakers that they do not already know enough about the production and distribution of knowledge to make intelligent decisions. This task, especially in the United States, is easier said than done. The bulk of funded research appears as line items on the budgets of agencies that are officially devoted to addressing the public's medical, environmental, energy, or defense needs. Such an occluded accounting procedure reinforces the idea that scientists are sufficiently self-regulating to be inserted comfortably into any politically sensitive environment. Moreover, this procedure impedes collecting the evidence needed to reveal the dysfunctional character of this distribution of scientific effort.

Not surprisingly, then, the American *science policy advisor* is defined as a conduit between science and the government. These two institutions are

presumed to work reasonably well by themselves, but can do more for the public at large by extended periods of cooperation (Guston 2000). The actual job of the advisor is to communicate the range of public needs to the scientists and the state of scientific research to the politicians. Furthermore, the information required for this two-way exchange is presumed to be fairly accessible if one is an “insider” in the relevant scientific and political circles (D. K. Price 1965). Thus, science policy has been institutionalized to rely almost exclusively on scientists’ folk understanding of how knowledge production works. These intuitions rely more on a few anecdotes than on systematic study, let alone sustained criticism or experimentation with a course of action that goes beyond a mere extrapolation of “current trends.”

The institutional inertia currently gripping science policy reflects the policymaker’s relative satisfaction with both our current knowledge of how science works and the policy ends toward which that knowledge is put. This coupling of factual and normative satisfaction is, in turn, indicative of what Daniel Bell (1973) characterized as our “knowledge society” (Stehr 1994). Presuming that the workings of science are substantially understood, the knowledge society takes the uses to which science ought to be put as dictated largely by the very nature of science. Thus, among the foremost items on the science policymaker’s agenda is the conversion of the amorphous problems that emerge in the public sphere to ones that are tractable by scientific means. Whatever escapes the categories of science is then relegated to a residual irrationalism, pejoratively called (in Bell 1960) “ideology” and euphemistically called “politics.”

Yet for all their interest in scientizing the public sphere, policymakers in the knowledge society still operate with what is properly seen as a “folk theory” of how science works. “Folk theory” means something like common sense: a set of beliefs that reinforces the “normal” or “natural” character of some phenomenon in the course of explaining it. Thus, strictly speaking, a folk theory is “ideological” in that ideas about how science works—well founded or not—are constitutive of science’s identity (cf. Fuller 1988a: Chap. 2). Policymakers typically despair of identifying any principled (as opposed to “merely political”) grounds for shifting science funding priorities because they believe that scientific research never exhibits diminishing marginal returns. As to be expected of a folk theory, this belief is subject to considerable anecdotal support. It comes mainly from cases in which a line of inquiry led to many long-term beneficial products that had little to do with the original conception of the inquiry. But there are no attempts to submit this belief to rigorous tests. The “naturalness” of the policymaker’s understanding of science is traceable to a metaphysical presupposition of folk theories. That is, one does not explicitly court challenges to their beliefs because whatever errors they contain will be revealed in the normal course of events.

But even when suspecting their own folk wisdom, science policy practitioners take the sheer pursuit of science, regardless of its palpable

consequences, to be an activity that morally elevates society. Of course so-called free, nonutilitarian inquiry has traditionally promised some major long-term cultural benefits. A short-term indicator that this promise is being met has been the spread of higher education to larger segments of the population. Even if most college students never directly contribute to the production of scientific knowledge, they are nevertheless exposed in the classroom to exemplary lives in action. In scientists, students meet people regarded as apt replacements for the religious and aesthetic icons of more superstitious and elitist times. (It is a short step from this political sensibility to one historically tied to the German university system, which uses science as a rallying point for cultural identity and national unity.) For this reason, the increasingly obvious disparity between the value that the academy and the public places on teaching has engendered a public relations crisis in higher education that is unprecedented even by America's traditionally skeptical lights. By severing teaching from research, academics are now in the process of undercutting the most persuasive case for free inquiry in a democracy.

Investing cultural significance in the pursuit of “pure research” or “basic science” says nothing about how many people, of which sort, should be doing what, where, or when. Indeed the arguments surrounding such pursuits make clear that the “freedom” of free inquiry lies largely in its alleged spontaneity or *unmanageability*. This sense of freedom has profoundly affected the American conception of scientific inquiry. Indeed the expression “scientific community” does not enter American English until the early 1960s, with the roughly simultaneous publication of works by Thomas Kuhn, Warren Hagstrom, and Don K. Price (Hollinger 1990). Before that point, little discussion occurred regarding the decision-making process by which science could function as a self-governing—let alone externally governed—enterprise. Even the need to establish internal accounting mechanisms had been obviated by the allegedly spontaneous fair-mindedness of scientists. Like Rousseau's “noble savage,” each scientist freely follows wherever the path of inquiry leads, using up as many resources as she needs, but never so much as to deprive her colleagues of a similar luxury. Even the four principles that Robert Merton advanced in the 1940s under the rubric of “the normative structure of science” failed to specify any mechanisms for their institutionalization.

Nevertheless, imagining that flesh-and-blood “free inquirers” would behave like noble savages is difficult. Using capital expansion in the marketplace as our benchmark, progress is measured by the supersession of past products and processes. Still progress can be artificially accelerated by manufacturing goods with “planned obsolescence.” This idea applies no less to transitory fields of inquiry whose sole purpose seems to be to maintain the visibility of researchers until (if ever) something intellectually more substantive comes along. In that case, fully realizing the ideal of free inquiry would produce a system modeled on the convenience foods

industry, aptly called *Fast Science*, which would maximize waste by ever quickening cycles of resource use and disposal. (See De Mey 1982: Chap. 9. The economics of this phenomenon has been analyzed from Marxist [Agger 1989] and neoclassical [McDowell 1982] standpoints.)

Once researchers are rewarded for this mentality, one can easily see how they would start to loathe teaching. Teaching has always seemed attractive to *teachers* because they have regarded the stuff taught as worth *preserving*. Once preservation is no longer valued in the knowledge system, then teaching seems to offer little more than partial, transitory snapshots from the frontiers of research. Thus, if Fast Science continues uninterrupted, we should expect the continued devaluation of teaching.

The question of values gets to the heart of science policy's inertial character. Perhaps the two most important issues normally resolved by institutional inertia are the relative value of the research produced by academic disciplines and the means by which a discipline may produce more of value. Does molecular biology, for example, "pack more bang for the buck" than high-energy physics? What may be done to address whatever discrepancies exist? The vast disparity in the costs and benefits that disciplines have to offer would, one might think, be an area for systematic knowledge policy research. However, the contrary is often the case. The suggestion that we might be spending too much money on, say, high-energy physics is typically treated as exemplifying a "know-nothing" attitude toward science. Yet the underlying motivation for this suggestion may be a desire to apply science to science itself, specifically, to determine the best projects in which to invest given certain short- or long-term goals (cf. D. de S. Price 1986; Irvine and Martin 1984). In this regard, the STS practitioner is the soulmate of the "philistine" government economist who fails to see why science cannot be subject to cost-benefit analysis, just like every other federally funded social service (cf. Chubin and Hackett 1990: Chap. 6). Perhaps some of the philistinism may be removed by looking at science funding through the eyes of a historical counterfactual.

Suppose it were 1870, and I were a knowledge policymaker interested in promoting an atomic view of reality. Up to this point, scientists had been reluctant to think of the quest for "ultimate reality" in terms of getting at the smallest unit of matter because no techniques existed for isolating and analyzing such units. This impasse was (and still is) discussed as "logico-conceptual" in nature. But why not regard it instead as "techno-economic"? Thus, the impasse could have been discussed in terms of a lack of relevant mechanical devices—something on the order of a dynamo or a digital computer—to stimulate the experimental imagination into proposing testable hypotheses about such micro-units. By calling the impasse "logico-conceptual," the would-be knowledge policymaker is left to the whims of scientific creativity with no clear sense of how to focus funding. However, by calling the impasse "techno-economic," the policymaker can call for the manufacture of certain gadgets that will enable scientists to hang their

abstractions on something concrete. The scientist would need to visualize the analogical implications of mapping properties of a theoretical construct onto those of a material object. If a “technological determination” of thought exists, it more likely vindicates McLuhan than Marx: Technology determines less the content than the form that thought takes. A striking piece of technology may even determine that the thought takes a form at all, and not simply criss-cross various levels of analysis. A historical case in point is the focus that the introduction of first the mechanical clock and then the self-regulating steam engine gave to 17th- and 18th-century discussions of governance in the natural and human worlds (Mayr 1986).

Science policy research, however, tends to be problem-centered. Consequently, this research often deliberately avoids recourse to the more systematic cognitive interests fostered by social epistemology. Interestingly, this problem-centeredness has been justified from opposing ideological directions. On the Right, science policy researchers try to solve the problems of their clients in government or industry who are usually interested in manipulating their access to knowledge to serve their own ends. For example, funding for research into the health of factory workers has rarely been done to advance the frontiers of medicine—although it sometimes has had this effect. The more immediate goal has been to prevent worker illnesses from slowing down production schedules. On the Left, science policy research has often been prompted by problems that have reached mass media visibility as instances of science “impacting” on the public.

AN ASIDE ON SCIENCE JOURNALISM

Science policy research plays hardly any role in *discovering* or *constructing* the problems it tries to solve. Unfortunately, this situation also applies to *journalists* who rarely track down stories about science with the same investigative zeal that they would a story concerning a politician. (Greenberg 1967 is the locus classicus for this complaint, which was revisited in Dickson 1984.) Except in cases of scientific misbehavior sufficiently grave to threaten public health or coffers, journalists tend to print watered-down or mystified versions of scientists' own press releases. This practice only ends up increasing the public's confidence in science without increasing its comprehension (Chubin and Chu 1989: Chap. 3). This state of affairs is an especially curious turn for the “in use” epistemology of journalism to take because the modern journalistic commitment to “objectivity” has much the same constructivist bent as STS research. Both aim to present as many sides of a story as possible, so as to let the reader decide for herself (Stephens 1988: Chap. 13; cf. Mulkay 1985). Just as the public rarely trusts a politician to give the last word on a topic in which they have a vested interest, why shouldn't a similar skepticism (politely put: “open-mindedness,” “neutrality”) be instilled in the public's

understanding of scientific pronouncements? Short of generating alternative facts and theories journalists have done little to raise the public's consciousness about science.

Moreover, journalistic objectivity becomes complicated once besieged scientists openly court the press in quest of a "fair hearing." Here journalists have often brought larger political and economic angles into the disputes that start to give science a public face comparable to that of other institutions. Sometimes (e.g. in the sociobiology controversy) this strategy ultimately benefited the besieged scientists, whereas in others (e.g. the "cold fusion" controversy) it did not. To some extent, this process is a step in the right direction, although in these episodes the press rarely operates with a sophisticated sense of the methodology of science (Nelkin 1987).

In particular, journalists often presume that theory choices are winner-take-all contests that turn on some crucial fact or event (i.e. a news item) that will be decided within a limited time frame (i.e. before boredom sets in). Generally, the more provocative the disputed theory the more likely journalists will champion it. As a result, the burden of proof shifts onto the opponents (typically, the scientific establishment) to design the relevant "crucial experiment." In the case of cold fusion, such experiments were designed and the underdogs lost. But in the case of sociobiology, its distinguished opponents (e.g. Stephen Jay Gould and Richard Lewontin) could offer only more talk to E. O. Wilson's original talk. In that case, boredom soon set in, and the press declared Wilson the winner by default. Some think that, given their role in shaping public opinion, science journalists should be more scientifically literate. Perhaps, however, sociobiology's opponents should take a few lessons in democratic rhetoric (Seegerstrale 2000).

Independent science journalism also contributes to a subtler phenomenon—an increased public impatience with the pace of scientific progress. Two images are worth keeping in mind here. The first is the supermarket tabloid, the public's primary source of information about the latest developments in science. The second is the growing pressure on government agencies from both industry and the public to limit the period of testing on scientific products before making them generally available. Clearly, we are ready consumers of science. But we would be wrong to believe that each dominant knowledge system excels by the standards set by its own culture. It is certainly *not* true of our own scientific culture. The problem here is that philosophers—not only relativists—fail to register the effects that publicity for scientists' initial expectations have on the standards used to evaluate subsequent scientific achievements. Promises of impending breakthroughs, strategically made to muster funds from Congress, may come back to haunt the scientists if, on delivery, the goods are late or somewhat less than promised. Discoveries that would have counted as clear cases of progress by an earlier standard now come to appear as disappointments because they fall short of current expectations. Moreover,

one scientist's ill-fated boast may unintentionally set the pace for subsequent researchers, who are then forced to contribute to inflated standards of achievement (Klapp 1991).

I do not bemoan the fate of science journalism once it decides to pursue an independent course of investigation. Journalists' instincts are often good. Scientists may complain that journalists take their arguments and announcements "out of context," but often that simply means that they are being taken *literally*. After all, scientists claim universality for their message. What difference *should* public eavesdropping make on promises made originally for the ears of Congress? By simply taking the scientists at their word, the press believes their word is uttered for all to hear. If, however, the press did not so often believe the specific promises of scientists, it might be able to help scientists realize the situated, and hence rhetorical, character of their utterances. However, journalists exude a certain vulgarized positivist sensibility, which sees science as theoretical debate punctuated by crucial experiments. When experiments fail to be forthcoming or crucial, the press simply gets bored, whereas the positivist declares a lack of "cognitive significance" to the proceedings. Yet both attitudes partake of the mythos of "the spectacle"—that combination of "put-up-or-shut-up" and "seeing-is-believing" which dominates both political *and* scientific imagery in a democracy (Ezrahi 1990). Since, historically, a free press has been democracy's most characteristic medium of expression, the pursuit of the spectacular moment should be seen as an attempt less at debasing scientific thought than at reaffirming democratic values. Here then begins the tension between science and democracy that will figure increasingly in this and the next chapter.

MANAGING THE UNMANAGEABLE

In January 1991, the American Association for the Advancement of Science filed a petition. It called on the U.S. federal government to double the science budget over the next decade. However, from a supporting piece that appeared in *The Atlantic* around that time, the call clearly was for more *unmanaged* money to be put into science (Crease and Samios 1991). Perhaps even the current amount of money will do, but with a smaller portion eaten away by such "costly" accounting procedures as grant renewals and program evaluations. Taken at face value, this subtext is much less persuasive: Is that most scientific of standards, efficiency, abhorrent to the conduct of science itself? More pointedly, we might ask: What exactly is supposed to be the difference between administering science "for its own sake" and running it as a profit-making venture judged according to business values? The issue runs deeper than a facile contrast between long-term, market-insensitive investment in "basic research" and short-term, market-sensitive investment in something called "applied research."

Philosophers in the Popperian tradition recognize the similarities between scientific innovation and the best features of entrepreneurial capitalism. For example, Imre Lakatos (1979) distinguished his own position from the “naïve falsificationist” who would immediately disown a program that was subject to many refutations. Lakatos advised holding onto a currently unsuccessful research program until it either attracts enough people to capitalize on its strengths or the needs of the scientific marketplace are restructured so as to favor the program over its competitors. Here Lakatos echoes the entrepreneurial strategy of jumping in early and staying the distance with a new product. He eschews the kind of short-run thinking typically used to sell goods that represent only a slight improvement on those already on the market. In this latter, naïve falsificationist case, once the market for the product dries up, one simply tries to latch onto the next fad. However, if all business enterprises ran on this principle of moderate gains at low risk, no major innovations would ever be made (Brenner 1987).

Historically, the basic-applied distinction was an artifact of U.S. federal government accounting procedures. They were designed to prevent as much science as possible from being implicated in the manufacture of instruments of destruction (Hollinger 1990). Currently, the distinction conjures up differences in both the motivation and the content of science. Put simply, the presumption is that “basic” research has more impact on the conduct of academic science than “applied” work. Nevertheless, “basic” research may be rendered “applied” under two conditions: (i) once the sphere of accountability is extended to include consumers who are themselves not producers of science (e.g. bureaucrats); and (ii) once the frequency with which scientists need to give accounts is increased.

Conversely, if the frame of reference for evaluating the outputs of putatively applied research was made solely by other applied researchers and such accounts were rarely required, the outputs would start to look like basic research. In principle, then, writing up any research project as either basic or applied should not be difficult. Indeed bold defenders of basic research have tried to use this convertibility to their rhetorical advantage:

The suggestion is that large scientific projects unfairly monopolize scientific capital, squeezing out the little guy who might make valuable innovations if given a chance. But the analogy is false. Knowledge generated by large scientific projects, unlike the profits of large corporations, becomes the property of the entire community and restructures the scientific background against which research teams large and small execute new ventures. (Crease and Samios 1991: 83)

The prior argument trades on what philosophers of language, after Quine (1960), call *referential opacity*: The same thing identified in two mutually exclusive ways (e.g. the expressions “Morning Star” and “Evening Star” refer to Venus but not to each other). In the case of business bigness

is condemned, whereas in the case of science bigness is praised. Big Business monopolizes capital, and is therefore bad. But what does monopolizing capital mean other than to have enough clout in the market to force all potential competitors to orient their activities towards one's own? Yet this very consequence of large scientific research projects is then praised! The fact that reference to Big Science as Big Business remains opaque testifies to another missed opportunity in the journalistic portrayal of science. Instead of reporting science like the serious side of the entertainment industry (fluctuating between its own kind of dazzle and scandal), the press should accustom people to follow the short- and long-term trends in the public investment of their tax dollars. After all research is the largest expenditure of federal agencies, and education is premier among local and state authorities. Why not, then, be concerned with performance records? An itemization of projects funded, the proportion of the budget they consume, and their track records at various points would dissipate some of the mystique of unmanageability that Crease and Samios continue to promote. (This idea would appear more attractive if citizens could reinvest their taxes in other public projects, as they see fit, just as they can with their untaxed moneys.)

Philosophers of language generally cast referential opacity as demonstrating that the same reality can be *described* in multiple terms. However, idle description is hardly the only reason why one might want to identify or refer to something. Rhetorically, seeing the multiple identifications of an object as alternative ways of *prescribing* for the future of the object suggests different ways to treat it. These choices, depending on how they are made, could subsequently change the character of the object. (The politically correct term for this process is *performativity*.) Thus, Big Science is untouched if its practices resist the predicate "monopolistic," but they are likely to change if the predicate sticks because of the different evaluative standards invoked by calling an activity monopolistic. Sociologists will recognize this point as following from W. I. Thomas' concept of *definition of the situation*: "If men [*sic*] define situations as real, they are real in their consequences" (Thomas and Thomas 1928: 572).

Referential opacity is often compounded by ambiguous connotations. Generally valued above applied research, basic research more explicitly engages the creative intellect of the scientist. She goes beyond merely enhancing what is already known to make a genuine discovery, the significance of which may remain unknown for some time. However, creativity does not always carry a univocal rhetorical advantage. Sometimes less creativity pays off if the scientist wants to claim credit for something she has done. Patents are a good case in point. The basic researcher may ideally want to distance her work from applied research by claiming that her equipment enabled the manifestation of a phenomenon that would have existed, albeit undiscovered, even without the introduction of any special equipment. After all, the phenomenon's ontological independence

—“realism” in the philosophical sense—makes for a genuine discovery, an insight into nature that merits the engagement of the basic researcher's intellect. Unfortunately, a scientist who fails to stress the necessity of her equipment will be unable to acquire the legal rights and economic power that accrue to patents. Indeed the task of securing a patent may require a role reversal between the ends and means of research. The scientist, for example, may portray her discovery as a demonstration of the equipment's ability to work according to set instructions (Miller and Davis 1983).

The ease with which scientists switch between regarding their work as discovering new things and as extending old ones indicates the rhetorical convertibility of the basic-applied distinction to meet specific needs. This convertibility serves social epistemology's assignment to democratize the intellect. As the flexibility of legal rhetoric suggests, the first step involves acknowledging that all attributions of “creativity” and “genius” are dependent on the reception given to a piece of work and are necessarily made in retrospect (Brannigan 1981). Such works are, in the first instance, anomalies and, as such, may be ultimately diagnosed as the product of either creative genius or foolish effort. The determination depends on whether the anomaly manages to change the disciplinary norms or falls victim to them. So Einstein's 1905 papers marked their author as a revolutionary physicist rather than a harmless crank because of the network of people who came to support, or otherwise rely on, the Special Theory of Relativity. The strength of the network caused the norms of physics to bend to the theory. To claim otherwise is to be faced with the embarrassing question of why a scientist's genius varies directly with the extent of her impact, over which she exerts relatively little direct control.

Notice that this social analysis of genius does not actually involve proposing that someone other than Einstein could have come up with Special Relativity. While perhaps true, relying on a counterfactual makes something about the theory—its “content” perhaps—appear marked as a work of genius no matter who came up with it first. A counterfactual question truer to this analysis is whether mobilizing some comparably extensive network, at roughly the same time, would lead to the revolutionary overthrow of Newtonian mechanics. The idea that scientific creativity can be fruitfully subjected to this kind of network analysis is hardly new to social science (Rogers 1962, updated in Latour 1987).

The point raised here may be cast in the vocabulary of evolutionary epistemology (cf. Campbell 1974). Darwinian evolution requires two sorts of mechanisms, one for genetic variation and one for environmental selection. The traditional epistemological fixation on creativity, genius, and the generation of theories—a focus retained in science policy thinking—stresses variation at the expense of selection. Consequently, policymakers attempt to construct environments that foster creativity before clearly understanding the selective aspects retained in the history of science. For example, although physics was clearly revolutionized by the

Special Theory of Relativity, we still do not know what activities associated with the theory's introduction led to its success (a.k.a. "selection"). The answer would lie in professional gatekeeping practices (especially Max Planck's), prior expectations and interests of potential allies, and competing research agendas. This alternative counterfactual focuses thinking on selection mechanisms that are relatively independent of the context of Einstein's discovery. Nevertheless, these mechanisms determined the theory's survival in the scientific community.

A variant of referential opacity—*strategic opacity*—is increasingly important in philosophical contributions to the public understanding of science. The idea is based on the classical trope of *catachresis*, or the misuse of names. If a situation can be described in alternative ways so as to motivate alternative courses of action, then surely one of those ways could be a literal misdescription that is nevertheless necessary for the audience to act in a normatively desirable manner. At first glance, this simply sounds like manipulation. But "manipulation" presupposes that the audience is being made to act against its own interests or beliefs. Yet in this case the audience has yet to form any clear views on an issue that requires prompt action. If strategic opacity succeeds, then in the long term the world comes to resemble more closely the strategic misdescription.

My example is the role of philosophers of science as expert witnesses on the nature of science. In a trial involving the teaching of Creationism in public school biology courses, philosopher Michael Ruse was asked to demarcate science from nonscience (La Follette 1983). He responded by giving Popper's falsifiability criterion, knowing full well that his answer failed to do justice to the serious objections that philosophers and others have raised to the criterion's plausibility (although it could be turned easily to exclude Creationists). However, had Ruse attempted to represent the complex battles that are waged over even the intelligibility of the demarcation problem, he probably would have undermined the philosophers' credibility as authorities on the normative character of knowledge production. Thus, Ruse's charge was to represent both his opinions and the opinions of others who may be called to testify on similar matters in the future. Even if these future witnesses would oppose Ruse's theory of science, they would probably object even more to being preempted from offering an opinion. Rhetorically speaking, even if philosophers of science have abandoned Popper, Ruse may still be right that it would be to the advantage of both our knowledge enterprises and the public at large—at least in this case—to act as if falsifiability were true.

The social function of strategically opaque accounts of science has been long familiar to philosophers. For example, John Herschel's *Preliminary Discourse* presented the scientific method to the lay Victorian audience as systematically applied common sense. Herschel wanted to normalize science's relations with a public that marveled at the spectacle of experimental demonstration, but remained skeptical of its relevance to the

“humane” knowledge mastered in the British liberal arts curriculum. Herschel's strategy was to transfer the “technical” character of science from the construction of apparatus to the design of nomenclature: a conversion of experiment to rhetoric. He deployed oversharpe distinctions in the stages of scientific reasoning—such as the contexts of discovery and justification—that were illustrated by homely examples. These stock cases are used when philosophers of science argue about the nature of science. This last point is important because, in the 20th century, philosophy of science came to be practiced more by professionally trained philosophers than by scientists. Thus, Herschel's rhetoric was crucial for philosophers to convince *themselves* that they could opine significantly on the nature of science after having mastered some scientific vocabulary and syntax but without laboratory training. Although this “shallow,” “merely philosophical” view of science has received criticism, it nevertheless kept alive a publicly accountable image of science throughout this period of increased disciplinary specialization.

Referential opacity is just one tactic by which public attention is diverted from the more encumbering social consequences of Big Science. The most effective tactic involves the biggest ruse: It is to believe that the only consequences of research are the officially intended ones. For example, social science research, given its focus on the human and on the applied, likely has more socially dislocating consequences than, say, basic research exclusively designed to study the abstractions of microphysical reality. This particular myopia follows from overlooking the material character of intellectual needs. Specifically, *that even unintended consequences need not be unexpected*. A careful empirical study into the social effects of different lines of research might enable the prediction of outcomes that researchers did not intend. This result would be a valuable tool for prying open scientists' *ex cathedra* pronouncements on what their research can or cannot do. Policymakers evaluate any practice, including an intentionally scientific one, in terms of the groups most likely affected by that practice's consequences in an appropriate expanse of space and time. For instance, although a series of high-energy physics experiments is intended to affect the community of high-energy physicists, presumably in a positive manner, the experiments' biggest impact may turn out to be on another disciplinary community even more impressed by the results. More to the point, lay people conceptually unconnected to science may find themselves the indirect recipients of subsequent experimental applications.

The typical high-energy physics experiment offers an especially vivid example of the strategic conflation of intention and expectation. What is tested in such an experiment? The intended answer, of course, is some range of hypotheses about the nature of microphysical reality. But given the material conditions needed for realizing this intention, we should come to expect that other hypotheses will also be tested at the same time—not in physics, however, but in political economy. These social experiments, no

less than their “natural” counterparts, involve the enforcement of *ceteris paribus* clauses. That is, the experiments are designed to exclude all factors from the test site other than the ones that are thought to bear some responsibility for the phenomena under investigation. In this way, scientific research appears subject to its own kind of inertial motion. For example, current high-energy physics experiments commonly pool the financial and human resources of several countries based on an international agreement. The agreement’s wording constitutes instructions for converting the physics experiment into a test of a certain theory of international relations. The experiment also tests a certain scheme for redistributing income and personnel. After all the physicists’ freedom to manipulate variables as they see fit rests on the ability of governments, universities, and other scientific support agencies to coordinate labor and capital over vast spaces for long periods that might otherwise move in disparate directions. Indeed large-scale natural science experiments are both the most powerful testing ground for hypotheses about social interaction and potentially the biggest source of large-scale social dislocation during peacetime.

The tendency to conflate intention and expectation is ultimately a Platonic conceit. Having one’s mind in harmony—or in “reflective equilibrium” as students of John Rawls (1972) like to say—is a matter of knowing what one wants and wanting what one knows. However, the Jesuit moral casuists foresaw the hazards of this conflation four centuries ago and tried to reestablish a distinction between the *epistemic* (i.e. the expected) and the *ethical* (i.e. the intended) sides of action with *The Doctrine of Double Effect* (Harman 1983). The Jesuits, unfortunately, formulated the doctrine accordingly: You can expect things you didn’t intend, and, therefore, you can knowingly do something without being culpable—a convenient moral psychology for the religious warrior! In contrast, one can invoke the doctrine to demystify the idea that, say a physics experiment is only—or even primarily—about physics. Consequently, it may empower nonscientists (including policymakers), rather than excuse scientists. In short, then, scientists should be held accountable for what can be expected to follow from their hypotheses, regardless of their intentions.

Regrettably, the distinction between the expected and the intended remains clouded regarding the context in which unintended consequences are most often discussed—economic prediction. Economists postulate an idealized rational agent who, although not a Platonist, always seems to intend in proportion to her expectations. When she does not, the consequences are generally beneficial as in invisible hand accounts of economic order. If the economic agent is not omniscient, she at least remains *blissfully* ignorant. Yet the evaluative asymmetry between basic and applied research creeps into how even this conflation is handled by the defenders of pure inquiry. Only basic research is portrayed as having positive unintended consequences (usually in opening up new lines of inquiry, but often in the applied realm as well). Applied research is seen as

having primarily negative consequences especially in terms of foreclosing opportunities for pursuing basic research, but also in its unwitting production of instruments of mass destruction. The positive unintended consequences of basic research supposedly flow “serendipitously” from the unconstrained pursuit of inquiry. However, the negative unintended consequences of applied research appear to be opportunities that ideologically inspired ministers of science are all too eager to exploit.

From a strictly scientific viewpoint (the viewpoint from which one might think scientific rhetoric should be judged), all the anecdotes that may be cited as evidence for the beneficial by-products of basic research, and the destructive capabilities of applied research, are the stuff of which superstitions, rather than careful policy, are made. As the cognitive psychologists say, the privileged anecdotes contribute to a *confirmation bias*. To claim that basic research unwittingly courts good and avoids evil better than applied research consider the following possible questions:

1. If a large enough expanse of space and time is examined, might not the effects of basic research turn out to be just as deleterious as the consequences of applied research?
2. Or, rather, might not the effects of applied research turn out to be just as beneficial?
3. Even granting the serendipitous consequences of basic research, might not applied research have reached the same conclusions sooner and more efficiently?
4. Even granting that serendipity reaches those conclusions more efficiently, might not more desirable conclusions have been reached by replacing a particular line of basic with applied research?

Questions (1) and (2) ask the historian to manipulate the parameters within which she examines the actual consequences of applied and basic research. Questions (3) and (4) call for counterfactual historiography. This practice, having established its credentials in economic and social history, has yet to take root in intellectual history. The general strategy would be to go back to the latest point in time when the alternative trajectory in question could have been pursued. Then one would estimate the probable consequences of pursuing that trajectory, instead of the one actually pursued, assuming that little else of the actual subsequent history would have changed. The goal would be to show that, given the chance, applied research could perform at least as well as basic research without disturbing too many historical background assumptions.

A burgeoning sphere of litigation exists where the manipulation of possible pasts and futures makes a major practical difference. The cases

turn on the liability of scientific research for unwanted environmental change. The battle between Big Science and the Ecologists is often portrayed as a disagreement over matters of fact and levels of risk, but behind it all is a dispute over one's sense of history. Ecologists typically suppose that the trajectory of scientific research will not veer enough off its current course to preempt or resolve any long-term environmental disasters. Yet Big Scientists presume that most of the potential for disaster will be contained or addressed by potential research breakthroughs. Given this contrast in historical vision, Big Scientists, on the one hand, have a fairly short-term conception of liability (since significantly new factors may intervene in the future to confound any current tendencies). Ecologists, on the other hand, project their legal concern on the long term, wanting to hold scientists responsible for the remote consequences of their actions. In these cases, the courts adopt a third-party standpoint—the involuntary stakeholders, if you will—who are the potential beneficiaries or victims of the scientists' actions. In a fully democratized knowledge enterprise, the effects of unsuspecting third parties might serve as the sociological surrogate for the check of an "independent reality" or "external validity." Thus, the judge in an environmental damage case would revert to the Greek origins of her office, *kritos*, the "tester" of alternative causal accounts (Kelsen 1943).

The basic-applied distinction is truly clear only in government accounts of science funding. However, the philosophical history of the distinction has aimed to keep basic research beyond accountability. Consider the *pragmatist* vision of science, especially as articulated by John Dewey, vis-à-vis the *positivist* vision articulated by the members of the Vienna Circle. Pragmatists saw the epistemic authority of science as resting in its ability to transform nature in the interests of humanity (Procter 1991: Chap. 3).

Dewey saw no sharp distinction between basic and applied research and had no desire to make value neutrality a virtue of science. In contrast, the Vienna Circle traced the epistemic authority of science to the logically valid and empirically testable terms of its theories. Whereas for Dewey "instrumentalism" indifferently referred to a position in epistemology and ethics, for Vienna Circle eavesdropper A. J. Ayer such indifference was the height of philosophical folly. What lay between the pragmatist and the positivist was World War I. The German scientific community—generally regarded without peer on the world scene—openly accepted responsibility for the military hardware that led to the most devastating war in history, culminating in a humiliating defeat for Germany. This admission unleashed an antiscientific irrationalism in the 1920s. Intellectuals supporting science adapted. They promoted a science that the public could see either as irrationalist or as conceptually independent of its destructive technological capability. The indeterminacy thesis in quantum mechanics—which denies causal determinism for microphysical reality—is an outgrowth of the irrationalist tendency (Forman 1971), logical positivism an outgrowth of the

independence tendency. To put science beyond reproach, positive value connotations were reintroduced by emphasizing that some of the best consequences of basic research may come in unexpected quarters. The much publicized service of basic physics researchers in the Allied cause in World War II performed the function effectively for the popular imagination. These events were celebrated in Vannevar Bush's *Science: The Endless Frontier*, the ideological statement behind the founding of the U.S. National Science Foundation.

But why should scientists, and their favorite epistemologists, resort to these backhanded rhetorical maneuvers to avoid accountability? What have scientists to fear from subjecting themselves to greater public scrutiny? Nothing, except a stereotype of what being accountable means. That stereotype reaches back to the primal moment of accountability, the academic exam, in which an individual's merit is judged on the basis of an externally driven standard. For example, the inquisitorial style of courtroom accounting procedure that characterizes Continental European legal systems arose from the practice of university examinations in the Middle Ages (Hoskin and Macve 1986). As Foucault suggests, both society and the individual are “co-produced” in the process of inquisition. But accounting need not be a process for measuring the fit of individual cases to general rules. It can, rather, be a diagnostic procedure that treats cases as symptomatic of the overall state of the rules. Thus, wayward scientists need not fear having their PhD's revoked if their deeds fail to match up to their words. Instead the scientists' incentive structure may be altered so as to get them to work in a different way or in a different field. Moreover, accounting for science may act to award compensation to affected third parties, especially when the consequences of research stray significantly outside the academy. A polluting laboratory, for example, might be required to devote substantial research to cleaning up after its messes or, perhaps, developing technologies that improve the well-being of the affected parties.

THE SOCIAL CONSTRUCTION OF SOCIETY

Revering each of these distinctions—basic-applied, invention-discovery, genius-error—stands the folk wisdom of science policy on its head. Doing so is central to the doctrine of *the social construction of facts and values*, the philosophical cornerstone of most STS research. This doctrine maintains a sharp separation between determining *what the norm is* and *when the norm applies* and, in turn, distinguishes the *script* from the *scene* of action. A norm is any pattern of social action that is scripted. Consequently, a distinction can be drawn between right and wrong ways to perform the action. So far social constructivists and philosophers of science agree regarding how scientists justify their research to each other and to policymakers. These justifications are taken generally to work and, hence, continue to keep the scientists in

business. All would cite chapter and verse of the hypothetico-deductive method and other positivist scripts as examples of scientific norms.

But now ask the constructivist and the philosopher *why* the script works. Whereas the philosopher will focus on properties of the script (e.g. its logic), the constructivist will turn to the scenes where the script is typically enacted. These scenes provide access to the specific mechanisms that enable the verbal performance to elicit the desired effects. Moreover, the constructivist need not presume that these mechanisms will be the same across situations. Instead she may simply believe that the script must be performed somewhere at some point. Indeed I am such a *script transcendentalist*—someone who believes that arguments and claims concerning the valued form of knowledge or “science” are necessary for the possibility of society. But I leave open to empirical investigation (of the past and present) and normative negotiation (in the future) the exact backdrop against which such arguments and claims can be successfully made (Fuller 1988a: Chap. 7).

Philosophers of science are no strangers to the study of scenery. However, this study is shrouded in Latin, the *ceteris paribus* clause, and shoved into the background of philosophical analyses (Fuller 1988a: Chap. 4). Philosophers suffer from the physicist's prejudice of undervaluing in concrete what can be so easily done in abstract, as in the case of deriving the laws of motion from a world of frictionless planes. For their part, constructivists realize that heavy transaction costs are incurred in moving from the abstract to the concrete. Human and material resources, for instance, need to be strategically situated (including things that were prevented from getting in the way) for “all other things to be equal.” The folk wisdom of science policy is symptomatic of a metalevel version of the same prejudice. Just as the physicist regularly forgets to consider exactly how one would materially construct a frictionless plane, likewise the policymaker forgets to consider what it would take to construct environments to enable future physicists to arrive at their abstractions. The script is thus fallaciously made to do the work of the scenery.

The more locally one considers the construction of scenery needed for enacting a script—say, one laboratory that agrees that certain evidence supports a certain hypothesis—the more social constructivism appears to be a species of dramaturgy. Indeed the followers of Erving Goffman and Harold Garfinkel who have introduced a microsociological perspective in STS have conveyed this impression. Consequently, STS practitioners espouse a bias toward *localism*, or the ontological privileging of the “here-and-now” over the “there-and-then.” Sometimes localism is little more than a politically correct way to talk about what positivist philosophers of science have called “the observable.” Other times localism is simply a nominalist (i.e. a negative) stance toward the reality of such macrosocial entities as institutions and classes. In either case, the STS practitioner takes herself to be showing how various localities interlock to produce the dispersal of

effects that characterizes today's technoscience (Ophir and Shapin 1991; Shapin 1991). Such a research agenda presumes that the places where scientific work is done are “indexes” for various sorts of knowledge. In a more rhetorical vein, these places serve as reminders of the skills that are called for on particular occasions.

Indexes also trigger what art historians call *iconographic* associations. These associations supply the observable foreground with an affectively charged conceptual background. For 20th-century art historians like Erwin Panofsky and Ernst Gombrich, iconography effectively documented the *Weltanschauung* or collective memory of a culture as a set of ubiquitously cueable and applicable symbols (cf. De Mey 1982: Chaps. 10-11). These associations are verbally elicited when people are asked to explain or excuse their behavior. Thus, engaging in a routine lab technique involves a certain attitude toward the activity that the participant–observer tries to access conversationally. In effect, this attitude toward one's place—one's “station” as it were—is the manner in which the scientist embodies her community's ethos (Polanyi 1957). It suggests a more expansive sense of indexicality. What then is the appropriate binary contrast to “local”? After all, the very existence of iconographic memory concedes that some of the most important things that happen and matter locally come from the outside. In short, *the nonlocal is always already inscribed in the local*.

This last point finally allows us to talk about knowledge policy from a constructivist standpoint. Can interlocking enough locales together ever produce the sort of “global” picture of knowledge production that would enable a policymaker to set priorities, anticipate outcomes, and adapt to changes in “the system”? Both positivists and Marxists, bureaucrats and activists, are skeptical of the constructivist attempt to eliminate such macrostructures as “power” and “objectivity.” These macrostructures often slip between the cracks of locales, yet give scientific knowledge its distinct sense of independence from much else that happens in the social world (Fuller 1988a: Chaps. 2, 10). Here is a strategy for explaining knowledge production that attempts to respect both local and global sensibilities:

1. The translocal uniformity of a piece of knowledge is largely an artifact of the restricted channels—the standardization of words and objects—in which knowledge must be officially communicated.
2. Nevertheless, one wonders why a wide range of independently and diversely managed laboratories find themselves communicating roughly similar, if not downright identical, messages.
3. The answer is that each such message should be treated as the predictable outcome of a decision procedure that, although differing across labs, has precedent as the decision procedure used in other sectors of society, with which lab members would have had some contact.

4. Thus, the apparent independence of the knowledge that emerges from multiple labs is due to a concatenation of individually predictable events that are then rendered uniform by the restricted channels mentioned in (1).

Consider the convergence of the physics community on the existence of neutral currents. Pickering (1984) has shown that the different labs involved behaved in ways that could have been predicted based solely on their particular social arrangements, even if the existence of neutral currents were not at issue. Just because the labs *end up* agreeing on the existence of a particular entity, it does not follow that their agreement is *due to* the existence of that entity. For example, one lab may come to believe in neutral currents because it always follows whatever the research director thinks. Another lab may come to the very same belief as a result of a weighted average of what the entire research team thinks. If operating in customary fashion, each lab's convergence in beliefs could have been predicted simply on the basis of knowing its decision-making procedures, without knowing anything about the content of the belief on which they converged.

The natural conclusion to this line of thought is that the convergent belief in neutral currents is an epiphenomenon of the diverse social processes that issued in assertions of that belief. This view runs contrary to such official communications as journal articles, which give one the impression that the various labs reached the same conclusions for largely the same reasons. The decision-making procedures that distinguished the labs earlier—deference to a superior and the weighted averaging of peers—are found in other, nonscientific sectors of society. Indeed these procedures go to the very heart of how modern society is organized and maintained.

THE CONSTRUCTIVE RHETORIC OF KNOWLEDGE POLICY

We have stressed the *social* over the *construction* side of social construction. However, the construction side brings us into the heart of the rhetoric of knowledge policy. The rhetoric of knowledge policy covers the construction of individual rationality out of beliefs and desires and the construction of collective rationality out of facts and values—the rationality of the *researcher* and of the *research* as it were. I call these the rhetoric of *rationality attributions* and *fact-value discriminations*. Let us consider each in turn.

The Rhetoric of Rationality Attributions

Rationality is the rhetorical balance sheet for our budget of *beliefs* and *desires*. Be it ordinary common sense or rational choice theory, beliefs and desires tend to be emphasized at the expense of each other. Desires and beliefs are

the mind's metaphorical movers and moved. Only a small imaginative leap is required from here to David Hume's aspirations for a "mental mechanics" to parallel Newton's physical mechanics. Thus, a picture of the mind containing mobiles and mobilizers, passive reflectors (beliefs), and active resisters (desires) of nature would support some epistemologically sharp distinctions. For beliefs and desires are usually held to be irreducible to anything else, including each other. For example, beliefs are tempered by evidence, whereas desires are often strengthened by evidence to the contrary. But more fundamentally, can we clearly say when something should count as a belief rather than as a desire?

At stake here are the criteria used to evaluate the rationality of someone's actions. We normally explain behavior by appealing to a configuration of beliefs and desires. However, we tend to lean more heavily on beliefs when evaluating actions by criteria in the agent's immediate vicinity that are not necessarily of her own creation. In contrast, desires bear a greater burden when the evaluative frame of reference is expanded to cover criteria of the agent's own creation although often not in her immediate vicinity. Thus, to answer why Mary walked out into the rain without an umbrella, we can say either that she did not think it was going to rain or that she wanted to get to the office quickly. In the former, belief-driven account, Mary appears to have simply erred. In the latter, desire-driven rendition, Mary is portrayed as having taken a calculated risk. Clearly, the two accounts are compatible, yet the first Mary is a victim of misinformation, whereas the second Mary deliberately suffers short-term losses to achieve long-term goals. Our evaluation would not change had Mary gone out into the rain *with* the umbrella. On the one hand, her correct belief would correspond to a reality (i.e. that it was raining) that did not require that belief for its existence. On the other hand, her risk would have appeared still more calculated, thereby enabling her to minimize even short-term losses.

Generalizing from the prior example, we see that an agent's rationality can be rhetorically enhanced by giving desires the upper hand over beliefs in the explanation of her actions. Desire-driven accounts can mitigate the surface irrationality of an isolated episode by making it comply to a more extended life plan. This point has been invoked by behaviorists in reinterpreting the many cognitive psychological studies that make people out to be incompetent calculators of expected utility. Rachlin (1989) observed that these studies focus exclusively on the performance of subjects in the experimental task. Accordingly, these studies portrayed the subjects as possessing false beliefs about their immediate situation, rather than relating their response to some long-term goal not directly represented in the situation. To avoid such "instant rationality" judgments—without having to postulate a "deep structure" to the mind—behaviorists assessed the rationality, or "efficiency," of animal response on the basis of a *series* of trials and usually not until a stable pattern of performance was detected.

Ironically, then, the rigid protocols of cognitive psychology have compelled behaviorists to recover some of the phenomena associated with the very mentalism whose existence they have traditionally denied. These mental phenomena include foresight, hindsight, and any other form of inference that forces the organism to adopt a historical perspective toward its own behavior.

The Rhetoric of Fact-Value Distinctions

When knowledge policymakers argue for either maintaining or changing a line of research, a strong distinction between *facts* and *values* can play a strategic role in the argument. If the policymaker wants to stick to a research trajectory despite resistance from the environment, she can appeal to the “value” of pushing onward. But if she is looking for an excuse to abandon the trajectory, an appeal to the countervailing “facts” of experience will typically figure in a winning strategy. The strategy outlined next is largely an elaboration of this point.

The practice of replicating experiments is central to science's self-image, especially to its image of having a firm database. Thus, Harry Collins' multi-pronged challenge to the feasibility of the norm was bound to prove controversial. According to Collins (1985), professional disincentives exist to performing replications (i.e. they were rarely published). Still even in cases where replication was crucial for continuing a line of research, important details of the original experiment could be gleaned only by personally contacting the experimenter since the published text turned out to be singularly uninformative. Collins' empirical finding calls enough of the policymaker's natural understanding of science into question to force her to take a stand on whether replication is part of the “is” or the “ought” of science. These recalcitrant cases may be seen either as refuting replication as a fact about science or as violating replication as a norm governing science: One interpreter's falsification may be another interpreter's infraction. It all depends on how one manages the anomalies.

The foregoing line of reasoning builds on David Bloor's (1979) attempt to use Mary Douglas' anthropology of cultural boundary maintenance to make sense of Lakatos' (1978) theory of anomaly management. Lakatos identified four strategies for handling counterexamples to mathematical arguments: monster barring, monster adjustment, exception barring, and Popperian falsification. Drawing on Douglas, Bloor argued that a society's preference for one or another of these strategies will depend on so-called *grid-group factors* (i.e. the society's internal stratification [grid] and its external differentiation from other societies [group]). I propose that *episodes of anomaly management bring into existence the occasions that warrant making the fact-value distinction*.

Initially, I present a grid-group analysis of possible policy reactions to Collins' counterinstances to replication as a norm of science. Then I offer a

grid-group analysis of a science policy issue. Running through the Collins case helps show that grid-group analysis speaks not only to the management of “dangerous objects”—Douglas' original concern and subsequent direction (e.g. Douglas and Wildavsky 1982)—but also to more abstract threats, namely, to one's implicit theory of how the world (or some part of it) works (cf. Thompson et al. 1990). With that in mind, grid-group analysis suggests that Collins' counterinstances can be examined along two dimensions as epitomized in the following questions:

(X) Is replication judged against the counterinstances (i.e. descriptively [X-]) or is it the standard against which the counterinstances are judged (i.e. prescriptively [X+])?

(Y) Are the counterinstances representative of a more general tendency (Y-) or restricted to just those cases (Y+)?

The (X)-axis captures the “group” character of the judgment. Here the policymaker must decide whether the practice of replication will be opened to correction from the counterinstances (“low group”) or whether the counterinstances will be banned to uphold the integrity of replication as a norm (“high group”). The (Y)-axis captures the “grid” character of the judgment. The policymaker, in this instance, must decide whether there is likely to be a difference between the instances that Collins reports and those that have yet to be observed in the relevant population. “Low grid” suggests no substantial difference between the seen and unseen cases; “high grid” suggests more heterogeneity. Thus, combining the possible answers to the previous two questions, the following interpretive possibilities emerge:

(X-,Y-) *Falsification*: Replication is an empirical hypothesis about how science works. The hypothesis may be rejected in toto in light of counterinstances.

(X-,Y+) *Exception Barring*: Replication is a principle whose empirical breadth may be adjusted in light of the counterinstances. These counterinstances are thus rendered irrelevant to a proper test of the principle.

(X+,Y-) *Monster Barring*: Replication is a normative standard that may be used to discount all counterinstances as cases of scientific malpractice.

(X+,Y+) *Monster Adjustment*: Replication is a principle whose normative depth may enable a charitable reinterpretation of the counterinstances. These counterinstances are thus rendered less contrary than they first seem.

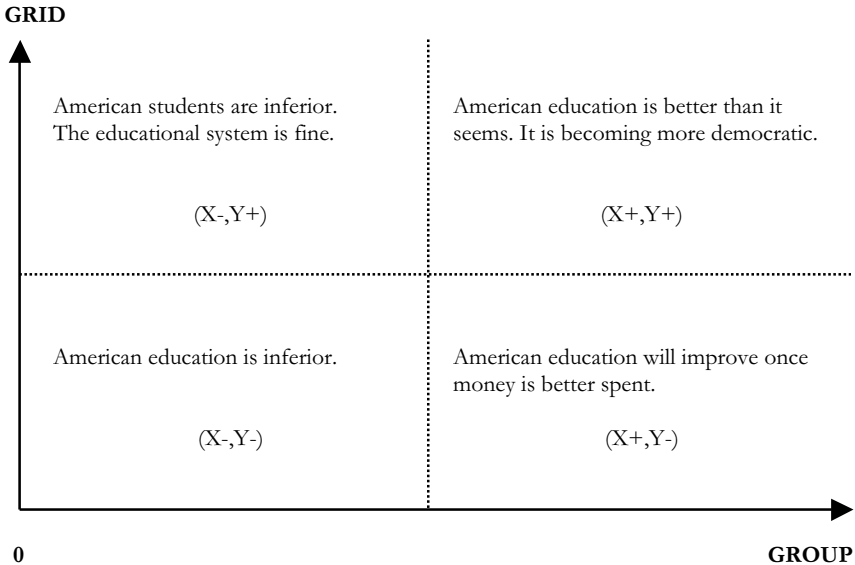


FIG. 7.1 A grid-group analysis of responses to the crisis in american education.

We can easily apply this anomaly management scheme to a major knowledge policy issue: the long-term decline in the academic performance of American students when compared with their counterparts in other countries (see Fig. 7.1). This decline comes in spite of the large and increasing funding for education. Moreover, the decline *prima facie* challenges a piece of policy folk wisdom expressed by the following maxim: “Academic performance will improve in proportion to the amount of money spent on education” (cf. Averch 1985: Chap. 4).

Commentators on this anomalous state of affairs have occupied every position on Bloor's scheme. Corresponding to (X-,Y-) is a frank admission that American education is inferior, and that clearly current funding patterns are not improving matters. These critics take the trend as symptomatic of a need to radically rethink our educational policy. Being low on both group and grid, these commentators are receptive to the educational initiatives taken in Europe and Japan. Representing (X+,Y-) are those who find the trend relatively superficial, suggesting simply a problem with the accounting procedure used to evaluate education funding. Perhaps moneys are being used to renovate buildings, for instance, when they would be better spent on raising the salaries of the best teachers. Tighter scrutiny would presumably remedy such poor managerial judgment. The strategy is to locate “the enemy within” who can be scapegoated and ultimately exorcised. As a result, balance is apparently restored to what is essentially a sound educational policy. Critics occupying position (X-,Y+)

might alter the terms of the argument by pointing out that, although American nationals continue to decline academically, a larger number of foreigners are matriculating in the United States, where they form an ever-increasing percentage of the excellent students. This account suggests that the problem is more contained than first appearances indicate. America, then, is becoming more of a world educational mecca, thereby vindicating the folk wisdom. Unforeseen, however, was that relatively few Americans would thrive in this competitive environment. Thus, these commentators advise a continuation of the same policy, but with revised expectations about the policy's exact beneficiaries. Finally, the rosier picture is painted by (X+,Y+). Here the decline in test scores is symptomatic of the relative democratization of education in this country vis-à-vis other parts of the world. People from all walks of life now go to school in this country, for a variety of reasons, few of which can be satisfactorily evaluated by standardized test scores. Accordingly, an apparent sign of failure is reinterpreted as a success in disguise.

Philosophers who insist on a “real” fact-value distinction would interpret our grid-group analysis as suggesting that the “is” and the “ought” pull in opposite directions. For example, the maintenance of replication as a norm of science rests on marginalizing new information about scientific practice. Giving that information its empirical due, however, would undermine replication's normative status. Yet if we focus on policymakers' natural understanding of how science works, then its specifically “empirical” and “normative” features are revealed only in cases where its naturalness is challenged. As long as replication is regarded outside the context of problematic cases, the policymaker will unlikely feel any need to decide whether it is descriptive or prescriptive of science. But once the counterinstances are conjured up, policymakers are forced to take a stand in terms of the four options outlined earlier. By the logic of this argument, then, a strong sense of the fact-value distinction should arise in periods of severe challenges to a long-standing natural understanding of things, or what a positivist might regard as genuine tests of a set of beliefs.

ARMED FOR POLICY: FACT-LADEN VALUES AND HYPOTHETICAL IMPERATIVES

The sociologist Max Weber has been most closely associated with modern concerns about separating “is” from “ought” or distinguishing “fact” from “value.” Weber's position is normally caricatured as wanting to protect factual inquiries from being tainted by value commitments (i.e. the *value-freedom* thesis). But Weber was more inclined to the opposite thesis: that values needed protection from facts (Proctor 1991: Chap. 10). He did not want to make our value aspirations hostage to the fallible and partial forms of knowledge represented by the latest scientific trends (i.e. the *fact-freedom* thesis). Weber's training in economics can explain his existentialism.

Economists believe that we are saddled with too many possibilities for action and a scarcity of the knowledge needed to eliminate all but the best of them. Personal commitments and social conventions must therefore compensate for the uncertainty of this situation. A third reading of Weber suggests that we value certain social practices and their products—scientific ones, in this case—only because we presume that certain things are true about their role in society as a whole. But if these presumptive truths were shown to be false, then the value of the practices and products would be thrown into question. In short, I claim that the *fact-laden* character of value commitments is more rhetorically revealing than the *value-laden* character of facts. This additional Weberian thesis is often held responsible for stalemating rational discourse (Fuller 1988a: Chap. 12; Fuller 1989: Chap. 3).

My view may be usefully contrasted with the pragmatist analysis of the fact-value distinction classically presented by John Dewey (1958, 1960) and, more recently, and specifically in the context of science, by Larry Laudan (1990, 1996). The pragmatist analysis also emphasizes the fact-ladenness of values, but only *after* the fact-value distinction has been already made. It does not explain how the distinction first gets constructed. From the social epistemologist's standpoint, this prior move is crucial for pragmatism's much-vaunted "instrumentalism" remaining a tool for critical, and not merely technocratic, rationality (Fuller 1994). The pragmatist argues that norms are really hypothetical imperatives for reaching a certain end by the most efficient means. The imperatives are experimentally derived regularities for which any ordinary human action is potentially a test case. The primary role of the social sciences is to discover and codify these regularities, evidence for which has been accumulating since the dawn of civilization.

But how does one decide on which end to pursue? According to the pragmatist, each end can be regarded as a means to some other end. Each end may then be factually judged by the extent to which it enables the higher end to be achieved. For example, a typical hypothetical imperative would be (assuming that it is true), "If you want to expedite the growth of knowledge, then pick theories that explain the most data by the fewest principles." But why might we want to expedite the growth of knowledge? Is this an end that we must embrace or reject unconditionally? No, says the pragmatist. We may regard expediting the growth of knowledge as a means toward improving the quality of human life. Whether it actually does so is an empirical question. Distributing currently existing knowledge more widely may turn out to be a more efficient means for improving the quality of human life than encouraging the production of new knowledge that only elites can use. In that case, if we want to expedite the growth of knowledge mainly because we thought that it would best promote the quality of human life, then we should stop expediting and start redistributing instead.

The pragmatist analysis starts by treating as an open question which of several courses of action one ought to pursue. Thus, the normative inertia that ordinarily engulfs the policymaker has been interrupted by the time the pragmatist enters the picture. Given Dewey's definition of intelligence as one's ability to "react to things as problematic" (Dewey 1960: 224), the pragmatist is understandably reluctant to admit the robustness of normative inertia among intelligent beings like policymakers. In contrast, my own analysis addresses how the policymaker's inertia might come to be interrupted. One can show that even an unproblematic course of action presupposes an account of how the world works that makes the action appear natural. However, once these presumptive facts are challenged, then the policymaker is forced to sort out explicitly facts from values. Consequently, she must choose from among a variety of means and ends in the manner that the pragmatist suggests.

But that is not the end of the story. My analysis can be applied to the pragmatist's, leading to the following question: What does the pragmatist's very strategy of constructing hypothetical imperatives presuppose about how the world works, and what if those factual presuppositions turn out to be false? Even avowedly pragmatist accounts of knowledge may contain empirically dubious premises that need to be ferreted out if the accounts are to prove truly practicable.

Unfortunately, this story offers an additional wrinkle because policymakers often implicitly rely on pragmatist principles to frame their own inquiries. Ironically, policymakers may have been misled into thinking that pragmatism is more practicable than it really is!

The idea I have in mind is that the track record of a hypothetical imperative consists of multiple cases of single individuals or groups (and pragmatists are crucially indifferent between these two possibilities) who have tried to achieve their ends by using a stipulated means. This seemingly innocent assumption is built into the form that a hypothetical imperative typically takes. The form is a statistical correlation between indefinitely many *independent* events of two types, one type covering those who pursue a given end and another type covering those who use a given means. Accordingly, certain features of human pursuits are not represented in this analysis: How many people are attempting to pursue a given end or use a given means *at the same time*? With what *other ends and means* are these people pursuing the end and means stipulated in a particular hypothetical imperative? These two questions remind us of the commonplace that no one follows a hypothetical imperative in isolation from other people and other imperatives. A good example of this shortcoming in pragmatist thinking was discussed previously regarding Laudan's attempt to test 300+ philosophical norms of scientific change against a set of historical case studies.

The pragmatist misses here what has traditionally been regarded as the source of the normative dimension of such imperatives. Following from the

Scottish Enlightenment tradition, I take it that the feature distinguishing norms from ordinary statistical regularities is that norms enable many agents to pursue diverse projects at roughly the same time by drawing on a common pool of resources (Hayek 1973). According to this tradition, norms emerge out of a concern that agents may unwittingly interfere with one another's pursuits, thereby leading to counterproductive results for all involved. A norm, then, is rarely the most efficient means by which any given agent could pursue her ends. Rather, the norm offers a relatively efficient means by which a diverse group of agents can pursue their ends with a reasonable chance of success. Therefore, to assess the normative range of the pragmatist's hypothetical imperatives, we need to know the social environments in which these imperatives were operative. Here the force of pragmatism as an "experimental" approach to knowledge and value may be felt, but in a way that goes beyond the pragmatist's own analysis.

In a laboratory, the experimenter can control the interactive effects of competing subjects or competing ends and means to whatever degree she deems appropriate. In so doing, she approximates the social conditions presupposed in the construction of the pragmatist's hypothetical imperatives (Fuller 1989: Chaps. 2-3). But the same cannot be said of the historical track records on which the pragmatists actually wish to rely. Yet it is methodologically naïve to think that the fate of a given means to achieve a given end is unrelated to other means and ends pursued at roughly the same time. Recall our original example of a hypothetical imperative: "If you want to expedite the growth of knowledge, then pick theories that explain the most data by the fewest principles." In each supporting historical case, few competing principles may have explained a range of disparate but relatively well-defined data. Therefore, if too many scientists follow the announcement of this hypothetical imperative, then the resulting proliferation of principles and data domains might undermine it as an efficient means to expedite the growth of knowledge (Ackerman 1985). Natural science would start to look like sociology, literary criticism, or even pre-Socratic philosophy.

The pragmatist's failure to see these consequences of her position is revealed in Dewey's easy recommendation that the hypothetical imperatives be made available to the public at large. Dewey (1946) presumed that human welfare would be best promoted by involving as many informed people as possible in the knowledge enterprise. Yet the success of many, if not most, of the hypothetical imperatives that can be inferred from the history of science has crucially depended on restricting access to the knowledge enterprise. Whether these imperatives would work in environments more democratic than the ones in which science has been normally conducted is unclear. This argument is not against democratizing science, but a cautionary note about the complexities in using history as a basis for making science policy. In our own day, feminists are probably the most alive to this point, especially in their deliberations over whether, in the

long term, the influx of women into science will change how and why research is done (Harding 1986: Chap. 3; Harding 1991: Chap. 3). In this regard, pragmatist intuitions match those of “liberal feminists” who do not envision that a massive change in personnel will radically alter the character of the enterprise.

While drawing lessons from history is tricky, we cannot solve *all* our problems by laboratory experiments on groups of scientists working under various conditions. After all my critique of pragmatism ultimately rests on pragmatism's insensitivity to the *frequency* and *distribution* of a given norm across society. In short, pragmatism lacks a theory of *power*. Seen in this light, the standard methodologies for studying science have some striking shortcomings. On the one hand, histories (and ethnographies) tend to overestimate the pervasiveness and, hence, the constancy and even the “naturalness” of a readily observable pattern. On the other hand, experiments (including computer simulations) commit a complementary sin. Experiments take their circumscribed ability to produce alternative results, by changing initial conditions, as a sign of the malleable and even “artificial” character of the norms that are currently in force outside the lab. If exclusive reliance on the historical method engenders a conservative politics of science, a similar reliance on controlled experimentation should issue in an impracticably radical politics of science: Mannheim's (1936) ideology and utopia revisited!

However, something more positive may be said as well. Recall Collins' studies of experimental replication: Instead of concluding that replication is either an unfalsifiable norm, or a falsified hypothesis about scientific practice, the policymaker may reason that replication seems to be, in principle, an effective way to ensure quality control in the scientific enterprise. If so, then she should ask *not* whether individual scientists do it or even whether they can do it. Rather she should ask: *At what level or unit of the scientific enterprise does or can replication occur?* One way to look at this new question is as a version of the (X-,Y+) interpretation of Collins' cases. Even if Collins is right that individual scientists do not replicate experiments, that may show only that replication is not the sort of thing that *individual scientists* do. For example, replication may be a collective unintended consequence. Priority concerns typically make scientists quite secretive in their dealings with colleagues, and the resulting lack of communication may be the main source of multiple discoveries (Brannigan and Wanner 1983). From a policy standpoint, selfish considerations may apparently lead to a wasteful duplication of scientific effort. Yet this “wasteful duplication” enables an unwitting replication of discoveries.

This take on the issue recalls our previous discussion of the original context in which “ought implies can” was made. Currently, the slogan implies that it is unreasonable to compel people to do something that is not within their power to do. In that case, a would-be norm may be invalidated simply by showing that the norm is not humanly realizable. However, Kant

first argued “ought implies can” to quite different effect. Kant wanted to show that if we have principled grounds for believing that a certain course of action is the one we ought to pursue, then there must be some faculty (indeed one we may have yet to discover) that enables us to do it. On this basis, Kant claimed that there must be a special “noumenal” aspect to our being—a “rational will”—that is subject to the moral order since, clearly, the ordinary physical aspect of our being is swayed amorally by the passions. While perhaps striking the modern reader as perverse, Kant's reasoning nevertheless serves to underscore the inherent ambiguity in using “human realizability” as a constraint on the acceptability of norms. Yet the ambiguity is not an unhappy one. Experimental psychologists, for instance, have shown that *individuals* are cognitively ill disposed to follow virtually every norm that has been proposed for rational inference in economic and scientific matters. Taking a cue from Kant, instead of scrapping all the proposed norms as just so many falsified hypotheses, and thereby concluding that “man [*sic*] is an irrational animal” (Stich 1985), we may need to turn from the individual to other “units of rationality.” For example, norms may have more bite as sketches for computer programs or as blueprints for the organization of cognitive labor (Fuller 1989: Chaps. 2-3). Likewise, the policymaker needs to broaden her imagination as to what might count as humanly realizable.

Some Kant-intoxicated philosophers claim the existence of “unconditional” norms—norms that bind people in all situations, no matter their ends, and even if the immediate consequences are not particularly salutary. These norms are *categorical* imperatives as opposed to the condition-bound *hypothetical* imperatives we have been discussing so far. But do such things exist? If norms govern the activities of real people, then shouldn't the norms reflect differences in people's situations, which means that all norms will be hypothetical imperatives? No, at least if there is more to a norm than merely a strategy that regularly gets you what you want. As an act of legislative will, a norm is designed to govern an entire community such that one's status in the community does not affect the norm's efficacy. This idea is the signature modern method for deriving principles of justice, immortalized by John Rawls (1972) as the “veil of ignorance” from which one operates in the “original position” of constitution-making. Apparently, then, an important goal of any normative inquiry is to sort out the categorical from the hypothetical imperatives: Which courses of action can be recommended to anyone no matter what others do? Which can be recommended only after a survey of what others are doing?

But wouldn't the prior exercise involve more than merely sorting imperatives? Wouldn't the normative inquirer be compelled to issue norms of her own? These questions are especially controversial when applied to science. Most of the hypothetical imperatives that philosophers invoke as “rational criteria for theory choice” emerged without legislation as individual scientists took advantage of situations that they realized would

remain unexplored by most of their fellows. Surprisingly, historians of science have said little about this self-selection process. One possible reason for the self-selection has to do with the nature of theorizing itself—at least the sort of theorizing discussed here, which Popper believes would engulf science in a “permanent revolution” if often practiced. Revolutionary theorizing of this sort is defended in this book. Such theorizing can reconfigure entire fields of inquiry by dialectically overcoming existing disciplinary differences. The import of successful theorizing in this sense—as in the cases of Newton, Darwin, Marx, and Freud—is to reorient the research of one's colleagues, and perhaps even to threaten their livelihoods altogether, if they are unable to adapt to the proposed change in milieu.

Philosophers often forget that scientists are generally taught to “theorize” only in the Platonic sense of constructing abstract mathematical models, but *not* in the more Hegelian sense of attempting a dialectical synthesis. Moreover, the typical context in which a scientist encounters a theory is the textbook. Textbooks present theory not as a challenge to the current disciplinary order, but as a safeguard against posing such a challenge. Theory appears as a glorified mnemonic device for keeping seemingly disparate notions related in the student's mind. In short, the incentive to theorize—in the synoptic sense that philosophers have traditionally thought to be essential for the growth of knowledge—has never been explicitly built into the normative structure of science. Theorizing is, of course, not prohibited, but it is definitely a risky venture professionally: The payoffs of success are big (for both the science and the scientist), but few succeed, and hence few try. But what would be the benefit of eliminating this risk by elevating the search for explanatory theories to a categorical imperative of science?

MACHIAVELLI REDUX?

Can all this talk of legislating and experimenting with the normative structure of science ultimately avoid the charge of manipulation? Manipulation typically presupposes a world in which the manipulable have well-defined interests against which the manipulator then imperceptibly acts. However, as indicated at the start of this chapter, I do not believe that science has any such “internally” or “autonomously” defined interests. Therefore, I deny the presupposition that underlies the morally repugnant sense of manipulation. Moreover, manipulation generally requires that knowledge be asymmetrically distributed across society. Hence, a specific group can always alter the structure of knowledge production, whereas everyone else is a passive recipient of its products. Social epistemology aims to break down the distinction between production and distribution that enables the morally repugnant sense of manipulation to take root in society.

Lest the reader find my denial of Machiavellianism too glib, let me now take up the charge in more detail.

Suppose that each hypothetical imperative associated with the history of science were shown to capture an effect that is emergent on the scientific labor being divided and organized in a certain way. Thus, no particular scientist would be explicitly guided by the imperative. However, the imperative offers the best explanation of what implicitly governs their collective behavior. If the policymaker is interested in maintaining the production of such effects, then she will be forced to gauge the advice she gives to individual scientists in terms of the likelihood that their subsequent actions will contribute to producing the desired effects. In Machiavellian short form, the ends will justify the means that the policymaker selects. Is this line of reasoning objectionably manipulative?

In rough-and-ready terms, manipulation occurs when one person knowingly gets another person to do something unknowingly that goes against her own interest, but benefits the first person's interests (Goodin 1980). Clearly, the knowledge policy strategy advanced in this chapter satisfies some of these criteria: The policymaker's bird's-eye view of the scientific process gives her an advantage over the average scientist in determining the overall significance of the scientist's work. Given the broader scope of societal aims within which science policy must be made, the policymaker's interest is arguably somewhat different from that of the average scientist. Indeed, the scientist is being made to serve the policymaker's interests. Conspicuously absent, however, is the idea that the policymaker wants the scientist to do something not merely different from, but demonstrably *against*, the scientist's own interests.

Now admittedly a virulent antipolicy tradition exists within the scientific community (Polanyi 1957). This tradition would blame all the deleterious consequences of scientific research on meddling policymakers who force scientists to act against their better judgment by making funding hostage to the production of ideologically sanctioned knowledge. Although such cases of deleterious consequences are all too familiar (e.g. Lysenkoism, Nazi genetics, the atomic bomb project), what remains unclear is whether an alternative research trajectory had been inhibited that the scientific community would have pursued left to their own devices. More likely is that some other policy imperative would have given direction to scientific research. If the policy were socially beneficial or neutral, the result would be credited to the "autonomy" of science. But if equally deleterious, the result would be laid at the doorstep of meddling policymakers. These frankly counterfactual speculations make one wonder whether the scientist has any specifically *scientific* interests that are different from those of the policymaker or whether differences in interests are entirely nonscientific in nature (e.g. a scientist's interest in receiving a bigger cut of the available funds). As a natural trajectory to scientific research,

independent of policy considerations, becomes harder to identify, the potential diminishes for policymakers to be objectionably manipulative.

But let's reverse the burden of proof: Why would anyone have thought that scientists had a distinct set of interests that could be disentangled from broader social interests and, especially, from the policymaker's interests? After all the idea of "interest" is an anthropomorphism that implies that events in the world neatly correspond to outcomes having different values for rival groups. The world, of course, is not so willing to oblige our efforts at totemism. According to the Doctrine of Double Effect, a self-interested course of action will be received by others who do not share our interests. Yet science policy enables scientists to ignore this elementary point by indulging a deep-seated psychological bias that would not normally be tolerated in other less esteemed groups. The most striking case of this bias is what social psychologists call *the fundamental attribution error*, which explains the asymmetry in the stories we tell about ourselves vis-à-vis those we tell about others (Hewstone 1989: Chap. 3).

We tend to explain the good things that happen to us in terms of enduring ("internal") personality traits and the bad things in terms of ("external") situational accidents. In contrast, we tend to explain what happens to others in reverse (i.e. people fail because of fatal flaws in their character and succeed out of sheer luck). Internalism, then, seems to be integral to the construction of self-identity. In that case, a good way to identify the dominant perspective—the "hegemonic authority"—in a society may be by whose self-identity story is presumed by all. Clearly, if all classes of people are susceptible to the fundamental attribution error, then social coherence can be attained only by privileging some of the asymmetrical accounts of self versus others at the expense of other such accounts. Thus, much of contemporary science policy can be readily seen as privileging the scientific community's commission of the error.

The fundamental attribution error also fosters the illusion that one's self-interest can be discovered by finding a stable personality trace. Yet if referring to anything at all, "interests" refers to utilities that exist outside oneself, cognitive access to which is likely to be no better than to any other external object (Goodin 1990). Indeed, even the interest groups that one identifies with at the beginning of a course of action may not be the interest groups with which one identifies later on, once some consequences of that action have been revealed. This point can then be used to get scientists to realize that as policy is projected into the indefinite future, one's own interests become less distinct from those of others. For example, as a nuclear physicist I may want unlimited funding for my field. But my endorsement of this policy assumes that, at the end of the funding, I still plan to be in nuclear physics and continue to identify with the community that will be receiving the funds at that time. As the quickening pace of scientific change forces a perpetual turnover of specialties, the odds that this will be the case—even without any government directive—diminishes.

Hence, from a purely “self-interested” standpoint, a more rational way to act is not to harm others in the course of benefiting those who are hypothesized as one's own successors (Parfit 1984).

With this in mind, policymakers can be interpreted as manipulative in a way that benefits the scientific enterprise—namely, by counteracting two sorts of nonscientific interests that scientists *themselves* possess:

1. The tendency to see one's own research as the very center of all that is worthwhile in science;
2. The tendency to satisfy the norms of science with as little effort as possible.

Hobbes would have recognized these two interests in the inhabitants of the state of nature and rightly diagnosed them as being born equally of ignorance as desire. In the case of (1), the policymaker arranges funding patterns so as to force scientists to think of their research as parts of larger projects, the realization of which may exceed the cognitive grasp of any of the participating scientists. This policy scenario is most prevalent in attempts to bring multidisciplinary perspectives to bear on pressing but ill-defined social problems. More modestly, the strategy may also be applied to consolidate the knowledge base of a discipline whose research has become highly fragmented through specialization (Fuller 1988a: Chap. 12). In the case of (2), the policymaker designs accountability procedures that force scientists to endure various probative burdens before being licensed to claim a cognitive achievement as their own.

Depending on how much others are expected to rely on the putative achievement, the probative burdens may be as light as simply reporting that one has carried out the appropriate procedures. The burdens may be as heavy as sustaining the scrutiny of other researchers with a vested interest in debunking the achievement or claiming it for themselves (Fuller 1988a: Chap. 4). If policy intervention ensures, so to speak, the productivity levels of science in (1), it ensures quality control in (2).

The trickiest area where STS may have policy relevance concerns the image that scientists have of themselves and their pursuits. STS research has earned its scandalous reputation by revealing discrepancies between scientists' words and deeds. Thus, scientists may appear to be laboring under some sort of false consciousness. Although this belief could well motivate the STS researcher to intervene in scientific practice, imputations of false consciousness to scientists are unlikely to motivate a change in behavior. Indeed, if the large-scale success of science is granted, then at least some forms of false consciousness would seem to have much to recommend them. Consider two hypothetical imperatives in this vein:

- A. If science is to enable prediction and control over larger portions of the environment, then scientists had best think of what they are doing

as probing ever deeper levels of reality (and not simply as applying craftier techniques).

B. If science is to produce ever more policy-relevant consequences, then scientists had best think of themselves as autonomous inquirers (and not as high-paid civil servants).

Suppose that these two imperatives were shown to work. Would that license a redoubled effort to educate fledgling scientists—and maybe even the public—in the myths of the profession? Here the issue of science policy as *ideological* manipulation is raised with a vengeance. The social psychology of creativity offers some clues as to how to treat this matter (Amabile 1983). Creativity is tied to a strong sense of one's work as “intrinsically motivated.” Interestingly, this finding is typically evidenced in the subjects' accounts for their activities. What exactly the expression “intrinsically motivated” picks out in a person's behavior remains open as each person demarcates “internal” from “external” factors differently. Still the suggestion is that the criterion for demarcating these factors can be renegotiated as scientists come to “internalize” political economy and social accountability as part of their motivational structure. However, this process will not be easy, especially as long as scientific language remains autonomous from the greater society.

A RECAP ON VALUES AS A PRELUDE TO POLITICS

How does the social epistemologist propose to mobilize STS research to alert policymakers to alternative strategies for funding and evaluating research? In a nutshell, by shaking policymakers from their unreflective stance of presuming that the “is” and the “ought,” facts and values, are fused together in some “implicit norms” or “natural trajectory” of knowledge production. The social epistemologist would aim for policymakers to reject equation of *statistically normal behavior* and *normatively desirable action*. The policymaker would have come to realize that the social construction of facts and that of values pull in opposing directions. Thus, any perceived sense of “normalcy” is only a temporary resolution of this tension.

No doubt the image that my answer initially evokes is that of each policymaker negotiating in her own mind (or for her own jurisdiction) which claims will be treated empirically and which normatively. Such a process would lead to a multiplicity of independent decisions resulting in incommensurable sensibilities about where the fact-value distinction should be drawn. Admittedly, the localistic bias of much constructivist STS literature suggests such a conclusion. Any piece of research can probably figure in any sort of value scheme as disparately interested parties find use

for the research. In that regard, research, while not value-neutral, may be inherently *value-indiscriminate*.

Perhaps the best way to dispel this lingering image of value-neutrality is for the social epistemologist to observe that if *values* are generally defined as “the sphere of freedom” and *facts* as “the sphere of resistance,” then one person’s treatment of a claim as normative may turn out to cause another person to treat that same claim as empirical. The crucial difference is that a decision taken by the first person appears as a brute fact to the second person. An asymmetry of this sort would thus define a *power relation* between the two persons.

THOUGHT QUESTIONS

☞ What does Fuller mean in stating: “Indeed a refusal to steer the course of science policy is itself a very potent form of science policy”? Is science largely an autonomous, self-correcting enterprise that does not require outside management? How are scientists able to maintain the apparent internal and external boundaries of science?

☞ What is “knowledge policy”? What are the similarities and differences between knowledge policy and science policy? How might social epistemologists convince policymakers that they do not know “enough about the production and distribution of knowledge to make intelligent decisions”?

☞ What is the knowledge society? What is the role of policymakers in the knowledge society? What is the folk wisdom that guides science policymakers? What are the problems that arise from this folk wisdom?

☞ What is “Fast Science”? How does Fast Science reward research and devalue teaching? What is responsible for the “inertial character” of science policy? How does science policy support or counter the aims of Fast Science?

☞ How are the aims of journalism akin to or different from constructivism? What role might science journalism play in a social epistemology? How does journalism feed the public’s appetite for Fast Science? How might this public attitude affect the processes comprising a social epistemology?

☞ Can science be managed as a business is managed? What is the difference between “basic” and “applied” research? What is the rhetorical dilemma for managing science if we hold this distinction? How do scientists take rhetorical advantage of the basic-applied distinction? What rhetoric is used to distinguish Big Science from Big Business?

☞ Is there such a thing as “scientific creativity”? Is there such a thing as “scientific genius”? If so, can creativity and genius in this sense be managed? If not, then is creativity or genius a socially conferred appellation?

☞ How are the consequences of basic and applied research posed rhetorically? What are the aims of this rhetoric? How might one judge and change this rhetoric? Can scientists be held accountable for the intended and unintended consequences of their work? How might one trace ultimate accountability?

☞ How might a “social construction of facts and values” lend direction to science policymaking? To what scripts do scientists, philosophers, and sociologists refer in describing scientific processes? How do social constructivists tend to interpret these scripts? What is indexicality? What difficulties confront social constructivists in moving from an observed local stage of science to a global stage on which policy can be made? How do Fuller’s explanations of knowledge production that attempts to “respect both local and global sensibilities” offer insights into scientific decision-making processes that can be used to set policy?

☞ In determining science policy, why is it important to distinguish between a scientist’s beliefs and desires? How might this distinction serve to direct policy based on norms of science such as experimental replication? What is the fact-value distinction? How does this distinction compare rhetorically to the distinction between beliefs and desires? How can the fact-value distinction be used in a grid-group analysis of policymaking, generally and science policymaking specifically?

☞ What does Fuller mean by the “fact-laden character of value commitments” as opposed to the “value-laden character of facts”? How is the normative view that Fuller proposes “logically prior” to pragmatism? Which aspects of the scientific enterprise does the pragmatist miss and normative theorist recover? What is the difference between a “hypothetical imperative” and “categorical imperative”?

☞ What role does theorizing play in science? How is theorizing a different activity in the social science? What limits might a social epistemologist, in crafting a knowledge policy, place on scientific theorizing?

☞ How is a normative approach to legislating the production of knowledge immune from charges of gross manipulation? What rhetorical difficulties does a knowledge policymaker face in invoking a normative stance? What are scientists’ attitudes themselves and their pursuits? What are scientists’ attitudes toward policymaking? What are policymakers’ attitudes toward

themselves and their pursuits? What are policymakers' attitudes toward science? How can these attitudes be resolved in to construct a policy leading to normatively acceptable action?

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