PART FOUR

ISSUES IN KNOWLEDGE POLICY-MAKING
TOWARD A REVIVAL OF THE NORMATIVE IN
THE SOCIOLOGY OF KNOWLEDGE

The disciplinary boundary separating the sociology of knowledge from epistemology has been suspiciously silent for quite some time. An implicit agreement seems to have been made to let the sociologists concern themselves only with what actually passes as knowledge in particular cases, while the epistemologists take care of what ought to pass as knowledge in general. Upon closer inspection, however, it becomes clear that the terms of the agreement have been set by the epistemologists, who typically define the normative standpoint, the realm of the "ought," as a cognitive utopia populated by individuals skilled in deciding between theories which are sufficiently defined and articulated to be translated into a common language for systematic comparison. This legacy of logical positivism remains as robust as ever in the perennial attempts by philosophers of science to design a "logic of justification." As for that other sense of the normative—the one suggested by Plato's Republic and Bacon's New Atlantis, namely, the ideal regulation of real knowledge systems, to which the sociologist's expertise would likely prove relevant—it has been consigned to the realm of "mere" policy-making and technical applications. How did the sociology of knowledge lose the right to call itself a normative discipline, and how might it regain that right? These are the issues raised by this chapter.

Some opening remarks are in order about what "normative" means in this context. There are three perspectives from which we may regard what one ought to do, which is to say, the domain of the normative: first person, second person, and third person (Fuller 1984). The first and third person have been the prevalent perspectives in Western moral theory. In the first person, I prescribe norms for my own actions, which invariably entails adopting a certain attitude toward the world. Moreover, the value of that attitude is usually established on "intrinsic" grounds and not in relation to the consequences that having the attitude has on the world. Kant is the clear exemplar here. In the third person, I act as a detached observer or critic of a community of agents, without any direct interest in changing it, but nevertheless with an interest in judging to what extent their actions facilitate their expressed or latent values. In the case of latent values, I would probably have a theory, which may draw on categories unavailable to the community yet can be justified in terms of my general normative theory. Hume is the exemplar in this case. In contrast, we adopt the second person perspective, in which I prescribe norms for my own actions, given that I know how others are likely to act under various conditions. My goal here is not to make myself as I ought to be (contra Kant), nor to judge whether others are as they are ought to be (contra Hume), but to judge whether I am as I ought to be, on the basis of whether I have made others as they ought to be. Plato's philosopher-king is the archetype of the second person...
perspective, whose pale copies are to be found in the normative posture of executive administrators.

1. Normativity Lost

Sociologists of knowledge generally believe that the epistemological status of a claim is relative to the social group(s) which must certify it before it passes as knowledge. In its first wave, the period between the two World Wars, this point was taken to imply that every knowledge claim had an ideological component, which could be revealed by identifying the social group(s) whose interests would be served by certifying the claim as knowledge. At that time, the sociology of knowledge was dominated by such Marx-oriented thinkers as Lukacs, Mannheim, Horkheimer, and Adorno, all of whom played some part in the development of what has since become known as "The Frankfurt School." The normative project served by this orientation may still be seen in the work of Juergen Habermas, whose "ideal speech situation" is an attempt to allow for the rational evaluation of knowledge claims by having all claimants lay bare their ideological biases for criticism (Geuss 1982). The first wave of the sociology of knowledge has not survived as a coherent and influential body of research. We have so far considered two general criticisms that it failed to address adequately, both of which point to how knowledge acquires its "independent" or "objective" quality:

(a) How does one account for the fact that the social group which first proposes a knowledge claim is not necessarily the one that benefits once the claim is certified? (See ch. 1.)

(b) Even if it is granted that all knowledge claims are proposed in the interest of serving specific groups, how does one account for scientific claims continuing to pass as knowledge long after their original interests have been served? (See ch. 10.)

On the normative front more specifically, the sociology of knowledge foundered on the problems that have generally beset Marxism as a theory of revolutionary practice. In essence, these problems focus on what Marxists would call "the ideality of the real," or the extent to which the way things are is the way they ought to be. Another way of looking at the issue is in terms of Marxism's failure as an objective social science to have any desirable practical payoffs. Its incompetence on this score may be illustrated by the ease with which Marxism has been used against its own interests. For example, while Marx documented capitalism's systemic disorders in order to spur the German workforce to revolt, Bismarck was able to prevent the revolution from ever happening in Germany by reading Das Kapital as
suggesting the kinds of social welfare programs that would appease the workers. Needless to say, Bismarck's ability to impede revolutionary practice was a direct result of the *truth*, not the falsity or "relativity," of Marxist theory (Heilbroner 1970). Although claims about the normative ineritance of the sociology of knowledge are usually confined to Mannheim and said to turn on the intelligibility of a science that treats all knowledge-systems as cognitive equals (see ch. 7), this historical tendency toward normative ineritance may be better seen as emerging from the development of Marxism in the years immediately following Marx's death.

During the period of the Second International (1889-1914), the heyday of "Orthodox" (Engelsian, scientific materialist) Marxism, the problem took a particular form. If Marxist social science aspires to lawlike regularities, on the model of the natural sciences, then the prediction of capitalism's demise is true, if true, in virtue of laws of economic change that operate independently of whether the relevant individuals know that their behavior is constrained by those laws. In that case, there is no justification for those individuals, once they have learned of the laws, trying to precipitate what is supposedly an historical inevitability. Indeed, such attempts may be premature to the point of preventing the desired revolutionary outcome. It would seem, then, that Marxism's status as a scientific theory impedes its status as a revolutionary practice. Certainly, this was the political consequence of Marxism prior to the Bolshevik Revolution of 1917. The leading Marxist party in Europe of that period, the Austrian Social Democrats, presumed that all events somehow contributed to the ultimate demise of capitalism—in effect, that the real is taking an ideal course—which led them to see the role of the workaday politician as one of letting history pursue its course and simply attending, via parliamentary means, to the immediate needs of his constituency (Kolakowski 1978, vol. 2).

After 1923, and the publication of Lukacs' (1971) *History and Class Consciousness*, the Frankfurt School began their attempt to overcome the quietism of Orthodox Marxism. They argued that Marxism discovers "dialectical" rather than "mechanical" laws of history, which implies that the relevant sense of "necessity" is not that of the chain of events inexorably resulting in capitalism's breakdown, but rather that of the persistent social relations of production without which capitalism would not be possible. Once the latter have been identified and publicized, especially to the working classes, capitalism may be overcome at any time by intentionally negating those necessary conditions. In that case, the Frankfurt conception of Marxist science as "critical" and not "positive" specifies what needs to be destroyed in order for revolutionary practice to succeed, but it does not identify the next social order, nor does it say how that order will be implemented (Feenberg 1986). The revolutionaries decide these issues for themselves, and hence "create the future," when the time comes. This, in turn, provides a gloss of the Hegelian thesis, invoked by Marx but left unanalyzed by Orthodox Marxists, that freedom requires "the recognition of necessity." In short, then, the real is rendered ideal by the active construction of ideal-creators.
Both Orthodox Marxism and the Frankfurt School were unable to provide a positive account of the future. But whereas the Orthodox Marxists failed because they never had sufficient evidence for predicting the exact moment of capitalism's demise, the Frankfurt School failed because they left any positive account of postcapitalist society entirely up to the collective judgment of revolutionaries. Still, the net effect of both versions of Marxism was to unwittingly support Max Weber's (1964) arguments for the value neutrality, and hence normative inertness, of social scientific theory with respect to social policy. On the one hand, Orthodox Marxism divorced theory of any practical consequences by depicting the laws of economic transformation as proceeding independently of the judgments of those governed by those laws. On the other hand, the Frankfurt School reached a similar conclusion by portraying the laws as open-ended after the fall of capitalism, thus permitting any number of (and hence, no determinate) practical extensions into the future.

A viewpoint that could have sustained the normative thrust of Marxism would negotiate a middle course between Orthodox Marxism and the Frankfurt School, starting with an alternative reading of "freedom is the recognition of necessity," to wit, that revolution is possible only once the proletariat learn how to remove the obstacles that currently prevent history from taking its natural course. In short, the real takes on the appearance of the ideal only through human intervention, but the ideal exists—albeit in a suppressed form—independently of such intervention. This is essentially the attitude that experimental physicists have toward Newton's Laws: the laws always hold, but they can be demonstrated only through specific interventions, the discovery of which is the epistemic goal of science. Roy Bhaskar (1980, 1987) has been preeminent in developing Marxism in this direction.

Over the past fifteen years, the sociology of knowledge has received a new lease on life from several, rather disparate quarters, which we have examined periodically throughout this book. However, we have yet to flesh out the new image of the scientist that has emerged, especially his rather amoral normative orientation, in marked contrast to the morally upright image projected by Robert Merton (1957, chs. 15-16). Admittedly, before Merton, the sociology of knowledge presented the scientist as someone wedded to a particular research program in virtue of the social interests he perceived it as supporting, even to the point of vitiating his scientific judgment. The New Wave, however, tends to present him as being politically less principled but more astute. The scientist now appears to be an accommodating creature, a Machiavellian who moves comfortably from one research program to the next, regardless of the social interests at stake, whenever it seems that he can maximize the use of certain technical skills, which under the right circumstances—say, the successful performance of an experiment—will gain him credibility in the eyes of his peers. Karin Knorr-Cetina (1981) has coined the expression "the logic of opportunism" to characterize the scientist's Machiavellian moves, ones which she believes are likely to
increase as it becomes more expensive, both in terms of time and money, to learn new technical skills. A team of sociologists under Michel Callon (1980) at the Paris School of Mines have detected a similar phenomenon as being responsible for successful scientific innovation, namely, the ability to analyze a problem into parts that will permit the most efficient mobilization of resources, especially people.

Perhaps the most interesting feature of this new twist to the politics of science is that, to a large extent, the Machiavellian's judgments simulate those of the fabled "rational" scientist, since in order for the Machiavellian to maximize his advantage he must be ready to switch research programs when he detects a change in the balance of credibility—which is, after all, what philosophers of science would typically have the rational scientist do. To put the point more strikingly, it would seem that as the scientist's motivation approximates total self-interestedness (such that he is always able to distance his own interests from those of any social group which supports what may turn out to be a research program with diminishing credibility), his behavior approximates total disinterestedness. And so, we can imagine the ultimate Machiavellian scientist pursuing a line of research frowned upon by most groups in the society—perhaps determining the racial component in intelligence is an example—simply because he knows of its potential for influencing the course of future research and hence for enhancing his credibility as a scientist.

If Machiavellianism simulates scientific objectivity at the level of the individual scientist, then what simulates objectivity at the level of the scientific community? Arie Rip (1984) of the Amsterdam Science Dynamics Institute has suggested the concept of robustness, which pertains to the survival value of particular positions through a series of controversies. Robustness is reminiscent of Kuhn's "Planck Principle" and Noelle-Neumann's "Spiral of Silence," both of which were examined in chapter ten.

2. Normativity Regained

The continuing failure of sociologists of knowledge to address normative issues may be seen in the discussion pages of the leading journal in the field, Social Studies of Science, edited at the University of Edinburgh. Originally designed as a general forum for both empirical and normative questions, over the years the empirical has outweighed the normative to the point that normative issues are now deliberately eschewed by the journal. In fact, this trend has probably been the most publicized and abrasive feature of the New Wave of the sociology of knowledge—at least to philosophers such as Larry Laudan who believe that even theories of the social nature of knowledge must face new versions of the normative questions that have traditionally interested epistemologists. As a result, Laudan has engaged in many, largely fruitless exchanges with philosophers who support the sociologists in their avoidance of the normative, such as Edinburgh's Barry Barnes and David
Bloor. Barnes and Bloor would confine the role of the normative in the sociology of knowledge to matters of methodological housecleaning. And by "methodological housecleaning" is simply meant advice on the general structure of research programs most likely to issue in valid and reliable knowledge of the social world. Thus, in order to distance themselves from philosophical questions, Barnes and Bloor will pronounce only on how to regulate the sociology of knowledge, but not on how to regulate any other cognitive enterprise.

However, in all fairness to Barnes and Bloor, the way in which Laudan (1977, ch. 7) poses the normative questions facing the sociology of knowledge leaves much to be desired. Laudan seems to believe, in effect, that the best theory choices made in the history of science—Copernicus, Newton, Darwin, Einstein—are the products of the best method for choosing theories, namely, some version of the cognitive utopia alluded to at the start of this paper. Since this single method can explain all these exemplary episodes, there is no further need to refer to the specific social circumstances in which each theory was chosen. In other words (and this is typical of philosophical responses in general), Laudan thinks that the main challenge to the sociology of knowledge is that the reasoning of the scientific community may be closer to an idealized standard of rationality than the sociologists are willing to admit.

Rather than dismissing Laudan's challenge with an "in principle" disdain for normative questions, sociologists would be better advised to start addressing such questions by demonstrating that the various cognitive utopias proposed by philosophers are not only absent from actual scientific practice but, more importantly, are generally unfeasible given the social organization of science. For if what makes a norm "normative" is its ability to be enforced, then a necessary ingredient in the rational selection of a scientific norm is that the scientific community has the resources for enforcing the norm. And, as it turns out, most of the cognitive utopias of the philosophers involve activities such as inspecting the logical structure of arguments and replicating the experiments of one's colleagues, which are simply impossible to enforce on a systematic basis in the world of Big Science (Collins 1985). Therefore, if we assume that "ought implies can" applies equally well to the rational selection of norms in science as elsewhere, then the sociologist of knowledge is in an ideal position to declare the normative pursuits of the philosopher irrational.

However, before the sociologist can celebrate in his newfound cognitive authority over the philosopher of science, he must face an objection from the philosopher of law. The philosopher of law wonders whether the "ought implies can" principle can really be used to show that most of the avowed norms of science are irrational. After all, most cases of normative force in the law are ones in which the society benefits in proportion to the norm being obeyed more often, even though no one expects that everyone will ever conform to the norm in all the relevant situations. Consequently, it is rational to pass laws against littering, even if they cannot be systematically
enforced and only half the population conforms to them, because even that low level of conformity makes the streets that much cleaner. Arguably, all laws have this character, and so the burden of proof is on the sociologist to explain why the norms of the scientific community cannot have it as well.

But, of course, the reason why the sociologist wants to treat science as a special case is that science is regulated mainly by what may be called norms of coordination: that is, scientific norms fail to have systemic force if even one part of the system fails to obey them. This feature of the norms reflects the interdependent nature of scientific research. A researcher who "cooks up" his results can vitiate the conclusions of honest researchers who unwittingly incorporate the fraud, largely because there are no obvious signs of the level of conformity to scientific norms. This point is, in turn, explained by the public forum of science being a purely verbal one, consisting in reports of what one has presumably done. The contrast with the forum in which civil society is conducted could not be more striking; the degree of conformity to laws against, say, litter can be gauged by a fairly clear behavioral indicator such as amount of garbage on the streets. The contrast here also shows that the relevant issue in designing scientific norms is not whether everyone can obey them, but whether violations can be detected before they infect the entire knowledge production system.

So let us now turn to a positive normative program for the sociology of knowledge. This would involve "sociologizing" traditional epistemological questions. For example, if cognitive progress demands that knowledge be divided into discrete disciplines, does this render obsolete the philosophical ideal of assenting only to claims which one has first tested for oneself? It would seem that growing specialization expands the region of incompetence for any given individual, such that he ends up being able to test fewer of the claims on which his own decisions are forced to rely. This can be readily seen in the increasing role that deference to expert opinion plays. Yet it also opens the possibility of a new version of the "Cartesian demon" (see ch. 2) entering the knowledge system, as individuals defer to experts whose opinions have no real bearing on the claim at hand or have not themselves been properly tested. This can perpetuate and compound errors that may go undetected for long periods of time and may be impossible to correct after a certain point. Just how real is this possibility? And are there appropriate and affordable policy measures for monitoring the flow of information so that such errors can be isolated in time? For example, we might imagine a government bureau designed to determine the standards for the competent use of, say, the authority of quantum mechanics (which suggests that causal determinism breaks down at the subatomic level) in making arguments for the existence of free will. In short, does it pay to centrally coordinate the activities of the various departments of knowledge, or should a laissez-faire attitude prevail, whereby the disciplines monitor their own activities, borrowing and barring where they please? These are the kinds of normative issues stemming from empirical considerations on the social nature of knowledge growth which have been deliberately avoided by the contributors.
to Social Studies of Science, and to which philosophers like Laudan have
themselves failed to contribute, since they have focused their efforts
exclusively on attacking the sociologists.

3. Freedom and the Administration of Knowledge Production

Despite their studied avoidance of classical philosophical problems, the
recent sociologists of knowledge have produced a body of research which
points ultimately to a radical critique of the epistemological tradition, one
which focuses on demystifying the philosopher's ultimate cognitive utopia:
the free pursuit of knowledge.

There has been a remarkable amount of agreement among Western
philosophers over both the desirability and the feasibility of an institution
protected from all other social concerns which would be devoted entirely to
the pursuit of knowledge. This utopia is as old as Plato's Republic and has
been recently reincarnated as Popper and von Hayek's vision of the "open
society," as well as Habermas' "ideal speech situation." In each version, the
utopia works by abstracting away all the factors which could lead to
irresoluble disputes among rational individuals—namely, the particular
social interests that would be served by one theory being chosen instead of
another—so as to leave at most a difference in evidence base, which can be
bridged by the free communication of findings, which will, in turn, be
evaluated in terms of a common logic of justification. As with any of these
cognitive utopias, one must, at the start, question the exact status of the
claim being made. Is it being alleged as a sociological fact that all cognitive
diversity can be resolved by eliminating differences in social interest? In
that case, we have an empirically falsifiable hypothesis, which suggests
certain sorts of experiments and historical comparisons. (Indeed, one such
falsification may be to show the unfeasibility of even performing those
experiments or comparisons.) But perhaps, as is so often the case, it is
simply a matter of philosophical definition that whatever prevents
individuals from agreeing on a common logic of justification will be called
"interest-motivated." In that case, all that the sociologist can do is to balk
at the philosopher's idiosyncratic usage.

But let us grant for the moment that the philosopher's cognitive utopia
has the status of sociological fact. There still needs to be an account of how
rational individuals decide that it is time to choose a theory; for while a
logic of justification may specify which theory to choose given certain
alternatives at a certain time, it does not specify when the time is right for
deploying the logic. As we saw in the last chapter, recent sociological
research suggests that the interesting question is not why one theory was
chosen rather than some other; instead, it is why was it thought that a
theory choice had to be made at that time rather than earlier or later.
Moreover, Barnes (1974) has argued that if Quine is correct and theory
choice is always underdetermined by the evidence base, then there will never
be a time, just given a logic of justification, when it will make optimal sense to decide between theories. One theory may currently appear the best supported only because certain evidence has yet to be provided which would undermine its credibility and tip the scales in favor of an alternative. And left to their own devices, scientists, like the rest of us, would no doubt entertain for an indefinite period of time many incompatible theories at once, since each would have its special virtue in saving the phenomena. But contrary to these idylls of autonomy, deliberations do eventually yield to decisions, and indeed become the centerpieces of philosophical theories of scientific rationality. The sociologist's point would then be that however similarly the scientists justify their theory choices (granting that point for the sake of argument), the decisions themselves turn on such extrascientific exigencies as grant application deadlines, which place the sort of pressure on scientists that could never be generated from within the cognitive utopia of the philosophers.

But should we even grant that the philosopher's cognitive utopia could have the status of sociological fact? A forum for the free pursuit of knowledge seems empirically feasible until we start inquiring into what exactly knowledge is, such that it can be "circulated," "extended," "produced," and "distributed." Not satisfied with the usual philosophical metaphors, sociologists take it upon themselves to do a kind of "field ontology" of the cognitive enterprise. They ask: What are the signs that knowledge is present? And although many answers are offered, they all part ways with philosophical approaches by defining knowledge in terms of coded materials which are localizable in space and time. This shift, however slight it may seem, is in fact significant; for no doubt the plausibility of the free pursuit of knowledge has rested, at least in part, on an image of knowledge as a set of propositions, whose content is, in principle, accessible to all and independent of any material embodiment—save the few puffs of air it takes to express a proposition as a sentence. Such an image explains why philosophers as otherwise diverse as Popper, Habermas, and Toulmin still take public debate in the Athenian agora as the utopian model of knowledge production in the world of Big Science. In that case, it would seem that knowledge flows freely in its natural state (which is essentially as conversation), and that some external force must be applied (say, from particular social interests) before the flow is impeded.

However, the story is quite different once knowledge is seen as suffering from the same problems of scarcity that befall other material goods: to make knowledge more available to one place and time is to make it less available to some other place and time (Machlup [1962] is the pioneering work in this area). To encode quantum mechanics so as to make it accessible to a physicist on the cutting edge of research is, at the same time, to remove it from the first-year physics student or the lay public (not to mention future historians of science). Even if these advanced quantum mechanics texts were readily found in popular bookstores, they would still remain "inaccessible" to lay and student readers, because in order to decode the
texts, the nonspecialist reader would need to gain a kind of knowledge—the kind associated with an advanced degree in physics—which requires a relatively long period of cloistered study. In essence, then, the advanced quantum mechanics text tells the nonspecialist how much more knowledge he needs to acquire before he is able to possess the knowledge contained in the text. The same applies vice versa: that is, to encode quantum mechanics so as to make it accessible to the lay reader is to remove it from the professional physicist. If this point does not seem obvious, then consider the prospect of a physicist trying to design a research program on the basis of one of the many available popularizations of quantum mechanics: How would he translate the various slogans, metaphors, and worldviews into empirical operations? He would probably have to do his own amateur history of the popularization, tracking down the texts on which the popularizer draws for his account. And if this example seems farfetched, then consider the more realistic case of someone acquainted with quantum mechanics solely as a program for doing research at the cutting edge of physics being asked about its "cultural implications." He would no doubt draw a blank at first and would start to speak confidently only after having studied the implicit procedures used by Bohr, Heisenberg, Bohm, and others to map quantum mechanics onto the mainstream of Western culture.

At this point, two sorts of objections may be raised. First, someone sympathetic with our general strategy of reinterpreting the problem of knowledge in economic terms may nevertheless wonder whether, in the long run, there is necessarily a trade-off between allocating resources for popular texts and for technical science texts. Instead, it would seem that scientists often assume the role of popularizer—especially in government forums—as a means of acquiring the time and money needed for conducting research which will ultimately yield a series of technical journal articles or books. Cases of this kind seem persuasive, however, only because they regard the knowledge enterprise from its end state: that is, scientists are presented as having already succeeded in acquiring the resources they needed for continuing research by initially taking the time to show the public the relevance of that research. But when these cases are regarded in terms of the actual sequence of decisions that must be made before that end state is reached, it becomes clear that the scientists indeed recognized the need for making a trade-off, which they did by temporarily redirecting time and effort from technical to popular texts. Admittedly, this strategy was meant by the scientists as a means of buying more time and effort for technical matters, but it was by no means guaranteed that such an attempt at popular support would succeed: for example, the government may decide to fund some other research team. Thus, the seemingly short-term decision to produce publicly accessible texts may turn out to involve an irreversible trade-off.

The second objection arises from the impatience of the classical epistemologist who, having witnessed what has just transpired, wonders whether the only normative questions raised by the above account concern
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the production and distribution of physics texts—questions better handled by someone familiar with the publishing market than by either a philosopher or a sociologist. But this impatience would reveal a failure to come to grips with a thoroughgoing materialist epistemology. The classical epistemologist clearly has an idealist bias, insofar as he would interpret the points we have been making as being simply about the different ways, popular and professional, in which the basic propositions of quantum mechanics can be embodied. Indeed, the fact that our society embodies quantum mechanics in many different ways, suited to the needs of many different groups of people, may strike the epistemologist as proof that quantum mechanics has been made universally accessible. Unfortunately, as our remarks were meant to suggest, the crucial epistemological differences occur at the level of the different textual embodiments, since a popularization of quantum mechanics offers the lay reader no more access to the work of the professional physicist than a state-of-the-art physics text offers the professional physicist access to the general cultural issues which interest the lay public. And here we may define "access" in strictly materialist terms, namely, A has access to B's work, if A has the capacity to causally influence B's work. The fact that the layman, through his reading of popularizations, cannot provide the sort of evidence which would either increase or decrease the probability of a standing hypothesis in quantum mechanics demonstrates his lack of access. Likewise, the fact that the physicist, through his professional training in quantum mechanics, cannot inform public opinion on whether the indeterminacy principle bears on the problem of free will demonstrates his own special lack of access.

From the above considerations, it should be clear that normative questions concerning the production and distribution of texts in a society are properly within the purview of an epistemologist who, like the sociologist of knowledge, is inclined to a materialist ontology. Among the most important normative questions that need to be answered involve decisions about the codification of various subject matters, especially taking into account who is likely to obtain access to whose work as a result of producing and distributing a specific codification. A society with few popularizations but many technical texts in quantum mechanics is likely to have a quite different epistemic profile from a society with many popularizations but few technical texts in that field. For example, history would suggest that in the latter society the heyday of quantum mechanics research had passed and that the time had come for the research to be integrated into the mainstream of the culture.

However, the relevant questions here are not simply ones of whether a society can produce and distribute enough technical and popular physics texts. Indeed, economically advanced societies are able to solve these problems with ease. But, clearly, this is not the whole story. Placing an advanced quantum mechanics text in every household in the United States is, by itself, unlikely to increase the average American's competence in technically rendered physics. If a society were interested in raising the
public's level of competence in physics to that of the specialist, then it
would have to make a major economic commitment to producing and
distributing the texts that would be needed to bridge the epistemic gap
implicit in the difference between popular and technical physics texts.
(Bridging texts could play a similar role in informing members of one
discipline of work in another discipline, and would have effects analogous
to the ones raised here for the public.) That gap is now filled, rather
unsystematically, by the sorts of texts used in teaching introductory and
intermediate college courses. I say "rather unsystematically" because, as any
college teacher knows, such texts are oriented more to the institutional
constraints of college teaching (for example, the text is designed to cover
one chapter per week, material is presented so as to serve as the exemplars
for self-contained exercises) than to refining and raising the reader's known
level of competence. Moreover, in capitalist societies such as the United
States, where textbooks are typically published by commercial rather than
academic houses, consumer demand is increasing the epistemic gap between
what students learn at the elementary and advanced levels. The student's
most vivid experience of this gap occurs when he finds that, say, the chapters
on sex and drugs that loomed large in his introductory psychology text are
not proportionally represented in the content of the upper division courses.
Systematically producing the right kind of "bridging texts" would, in the
first place, demand writers with a Piagetian sense of how to get the reader
to realize a deficiency in his knowledge which will, at the same time, lead
him to a more advanced understanding. Writers of this kind would
undoubtedly have a very versatile understanding of physics, one that could be
put to equally good use in philosophy, some of the other humanities, or even
in advanced theoretical physics itself. They would constitute a new breed of
popularizer, half journalist and half pedagogue, who would be contributing
to what E. D. Hirsch (1987) has recently called "cultural literacy." While a
full-blown commitment to raising the public's level of competence in
physics may well seriously divert resources, especially brainpower, from the
cutting edge of research, it would likely facilitate the exchange of
information among the various sectors of society, thereby distributing power
more equitably. Hirsch has stressed this point, which he thinks will lead to
a sort of "public science," analogous to Walter Lippmann's (1955, part 2)
"public philosophy," which would allow a society to identify itself in terms
of the knowledge that its members have in common.

In a period such as the eighteenth-century Enlightenment, when the
Newtonian world-system was commonly seen as having extended the
frontier of knowledge to its limit, the commitment would be easily made,
as many of the best minds believed that they had a choice between correcting
popular prejudices by producing bridging texts (the most important being
L'Encyclopédie) or simply solving the puzzles which remained in
subsuming chemical and biological phenomena under Newton's Laws. But it
should be recalled that this reallocation of cognitive resources did not
simply create a better-informed public. More significantly, once physics
was no longer seen as having much of a cutting edge, it was generally inferred that man had finally gained control over nature, and especially himself as part of nature, which, in turn, inspired the various political acts of self-determination culminating in the French Revolution of 1789. The case of the Enlightenment brings to the fore the major points that a sociologized epistemologist must consider when making policy suggestions for organizing knowledge in a society. A policy to make the knowledge possessed by the special sciences publicly accessible may not only give the impression that inquiry has slowed down, but also that the public may act with confidence on matters where it would not have previously, since—at least from the standpoint of resource allocation—the public's ignorance of the special sciences had been tied to the scientists' own residual ignorance of their domains of inquiry. Once the scientists' ignorance has been eliminated, the resources are made available to enlighten the public. The advisability of utilizing the time, money, and brainpower in this way is, of course, another matter.

In conclusion, we have seen that a key reason why the sociology of knowledge has not been usually regarded as a normative enterprise is that epistemologists have presumed an excessively restricted understanding of "normative" which manages to include the decisions that individual scientists ought to make for regulating their own research practices in idealized settings, yet exclude the decisions that policymakers ought to make for regulating the research practices of the scientific community as a whole in more realistic settings. I have argued that this excessively restricted notion of the normative can be traced to the idealist bias in classical epistemology, which does not take the material instantiation of a proposition—that is, the text which expresses the proposition—to be an essential epistemic property. However, once a more materialist perspective is admitted into epistemology, one in which the problem of knowledge is redefined in terms of the economics of text production, then it is possible to pose, once again, the realistic normative problems which concerned Plato and Bacon and which are tackled most effectively within the general framework of the sociology of knowledge.