PART ONE

ISSUES IN DEFINING THE FIELD OF SOCIAL EPISTEMOLOGY
CHAPTER ONE

AN OVERVIEW OF SOCIAL EPISTEMOLOGY

The fundamental question of the field of study I call social epistemology is: How should the pursuit of knowledge be organized, given that under normal circumstances knowledge is pursued by many human beings, each working on a more or less well-defined body of knowledge and each equipped with roughly the same imperfect cognitive capacities, albeit with varying degrees of access to one another’s activities?

Without knowing anything else about the nature of social epistemology, you can already tell that it has a normative interest, namely, in arriving at a kind of optimal division of cognitive labor. In other words, in words that only a Marxist or a positivist could truly love, the social epistemologist would like to be able to show how the products of our cognitive pursuits are affected by changing the social relations in which the knowledge producers stand to one another. As a result, the social epistemologist would be the ideal epistemic policy maker: if a certain kind of knowledge product is desired, then he could design a scheme for dividing up the labor that would likely (or efficiently) bring it about; or, if the society is already committed to a certain scheme for dividing up the cognitive labor, the social epistemologist could then indicate the knowledge products that are likely to flow from that scheme. I thus follow the lead of Plato’s Republic and Francis Bacon’s New Atlantis in conceiving of the “epistemology” in social epistemology as having an interest in describing our cognitive pursuits primarily as a means of prescribing for them.

Yet at the same time, social epistemology is not “utopian” in the pejorative sense that Marx used to distinguish his own “scientific” brand of socialism from those, such as Saint-Simon’s and Fourier’s, which were based on philosophical ideals that could never be implemented on a mass scale. I take the “normal circumstances” cited in the question to be universal, both historically and cross-culturally, a “brute fact” about the nature of our cognitive pursuits to which any normative epistemology must be held accountable. Moreover, I take this brute fact to be responsible not only for the variety of ways in which knowledge has been pursued, but also for the variety of products that have passed for knowledge itself. Consequently, in so aligning myself with naturalistic approaches to knowledge, I reject the Cartesian gesture of withdrawing from all social intercourse as a means of getting into the right frame of mind for posing foundational questions about the nature of knowledge. For even though the social world may appear to be a confusing place from which to deliver epistemic judgments—certainly more confusing than the privacy of one’s own study—it is nevertheless the normal (and probably the only) place in which such judgments are delivered. If you still doubt the wisdom of this move, just recall that only an old rationalist prejudice, one popularized by Descartes himself, ties the adequacy of knowing to the clarity and certainty of thinking.
Later in this book (ch. 7) I will argue that a crucial way in which a discipline maintains its status as "science" is by manipulating the historical record so that it appears to be the inevitable outcome of the course of inquiry up to that point. In the first part of this chapter, I attempt a similar legitimating move by showing that social epistemology is a natural development from the history of philosophy since Kant. However, since social epistemology as I conceive it will probably strike many readers as an offspring of rather dubious lineage, I shall then proceed, in the second part of the chapter, to write a different revisionist history of modern philosophy. Here social epistemology, in its incarnation as "the sociology of knowledge," constitutes a radical, if not wholly successful, break with all previous theories of knowledge. But an unwitting combination of philosophers and sociologists nowadays threatens to smother the revolutionary impulse in a spirit of accommodation. Finally, I offer some suggestions as to how the social epistemologist can remain both exciting and relevant to contemporary issues in the theory of knowledge.

1. Social Epistemology as the Goal of All Epistemology

You should now have a rough sense of the conceptual location of social epistemology. But let us proceed somewhat more systematically. First, in calling my field of study social epistemology I have identified it as a branch of philosophy, indeed perhaps the main branch of that discipline. Yet a common response that philosophers have made to sociology over the past two centuries is to invoke what Larry Laudan (1977) has called "the arationality assumption," namely, that sociological accounts of our cognitive pursuits are appropriate only when those pursuits fail by universally acceptable standards of rationality. Even Karl Mannheim (1936), who established the sociology of knowledge as a separate discipline in the twentieth century, invoked this assumption when he exempted mathematics and the natural sciences from his field of inquiry. For all their ideological differences, both Laudan and Mannheim portray the sociologist of knowledge as wanting to show that the domain for which a knowledge claim is valid is restricted by the social conditions under which that claim was first made. Thus, unlike the founder of sociology, Emile Durkheim (1961), who, in Kantian fashion, saw the universal features of cognition--space, time, number, cause--grounded in features shared by all societies, Laudan and Mannheim assume that sociological accounts of knowledge, if they have any grounding at all, are grounded in the features of particular societies, and hence are, in principle opposed to the philosophical accounts, which are based on appeals to universal rationality. Given the uneasy complementarity that has thus developed between philosophy as the study of the universal and sociology as the study of the particular in our cognitive pursuits, it would seem that "social epistemology" has become an oxymoron, a contradiction in terms.
Still, it is curious that for all its current centrality to philosophy, the discipline of epistemology has a distinctly post-Kantian origin. Before Kant philosophers typically understood the nature of knowledge and the nature of reality as two sides of the same coin. The generic philosophical question may thus have been posed: How is reality constituted such that we can know it (insofar as we do), and how are we constituted such that reality can manifest itself to us (insofar as it does)? The point of the Kantian critique--at least as it was taken by Kant's successors--was to detach the question about knowledge from the question about reality, largely by arguing that the question about reality makes sense only as a disguised version of the question about knowledge, and that the answer to the question about knowledge places no discernible constraints on what the answer to the question about reality might be. In this way, it became conceptually possible in the nineteenth century to practice epistemology as something distinct from metaphysics (Habermas 1971, Hacking 1975a).

However, Kant's critique alone was not sufficient to establish epistemology as a legitimate philosophical enterprise. After all, if Kant's predecessors had been convinced that knowing about the nature of knowledge told them nothing about the nature of reality, then what would be their motivation for studying knowledge? The nineteenth century provided an answer that was suited to the post-Kantian philosophical sensibility. For once disciplines started to proliferate, claims to knowledge began to be made which were justified solely on intradisciplinary grounds but which were clearly meant to have interdisciplinary cognitive import. This gave the internal structure of knowledge--quite independent of any link to reality--a new complexity that required study in its own right. The general term coined for these claims was "reductionist," the two most notable cases of which were the attempts to reduce chemical phenomena to atomic physics and the attempts to reduce mental phenomena to a kind of physiological mechanics. The point of studying knowledge, then, would be to arrive at rules for adjudicating the various reductionist claims, which would involve devising a metalanguage for rewriting all such claims so as to display the exact extent of their cognitive authority, often known as their evidential warrant. The normative import of this exercise may be seen in that once the atomic hypothesis was granted cognitive authority in the explanation of chemical phenomena, it was possible to judge the relative "progressiveness" of a research program in chemistry, either past or present, by the likelihood with which it would facilitate the reduction to atomic physics. The ultimate goal of the epistemologist would thus be to map out the structure of cognitive authority among all the disciplines as a means of providing direction for their research--which is precisely the goal of social epistemology.

And so, my short answer to the alleged self-contradictoriness of "social epistemology" is that epistemology has been a well-motivated, autonomous field of inquiry only insofar as it has been concerned with the social organization of knowledge. Such had clearly been the case with the first
epistemologists, Auguste Comte and John Stuart Mill, and it continued to be the case in the twentieth century with the logical positivists. The continuity of this concern is nowadays lost, however, mainly because logical positivism's legacy has been greater in the techniques it introduced for doing epistemology than in the actual project for which those techniques were introduced.

Consider the ease with which Kuhn, Feyerabend, and Hanson were able to show, in the late 1950s, that the positivist panoply of correspondence rules, equivalence relations, and subsumption strategies could not adequately account for the structure of cognitive authority in the sciences. Indeed, many positivists even granted the more radical critique that these creatures of formal logic had no place whatsoever in the epistemological project. Why? Because the offspring of logical positivism, the analytic philosophers, were coming to the ironic realization that the formal techniques which they had inherited were better suited for the very problems in metaphysics whose intelligibility the logical positivists had questioned. The focus of these problems was the nature of material and logical necessity, in which the work of Georg von Wright, Jaakko Hintikka, Nicholas Rescher, Saul Kripke, and David Lewis has figured prominently. In short, then, contemporary analytic philosophy has let its inquiries be dictated by the available means rather than the original ends.

In spite of the positivists' errant ways, Kuhn and the Popperians have managed to pick up the historical thread and continue the epistemological project into the present day. Although many Popperians would deny it, a constant reminder that this project is still about the social organization of knowledge is the frequent allusions to political theory that one finds in contemporary philosophy of science: Popper's self-styled "open society" vision of the scientific community marks him as a classical liberal, while Feyerabend's emphasis on the "open" and Lakatos' on the "society" aspects of the Popperian vision marks them as, respectively, an anarchist (or libertarian) and a social democrat. And Kuhn, with his talk of normal science being dominated by a single paradigm which can be replaced only by "revolution," is, by all accounts, a totalitarian. These ideological labels should not be taken as merely suggestive metaphors, but rather as literal statements of what the various "methodologies" become, once the epistemologist is transferred from the context of appraising already existent products of knowledge to the context of recommending the scheme by which knowledge ought to be produced (Krige 1980). Indeed, it would not be farfetched to say that, when done properly (that is, when done self-consciously as social epistemology), the philosophy of science is nothing other than the application of political philosophy to a segment of society, the class of scientists, who have special capacities and special status but also make special demands on each other and the rest of society in the course of conducting their activities.

Returning to the positivists, it is well known that their chief ideologue, Otto Neurath (1962), saw the Unified Science movement as, in part, a way
of driving out the politically conservative and elitist tendencies of hermeneutical thinking in the "human sciences" (which defined the task of interpretation to be the situating of texts in a clearly defined tradition of readers and writers who saw themselves largely as communicating only with one another) and driving in the more radical and egalitarian, specifically Marxist, politics associated with a naturalistic approach of the "social sciences." Less well known is how Neurath's preoccupation with the status of "protocol statements," those fundamental building blocks of evidential warrant in the natural sciences, contributed to his overall project.

In light of what I have so far said about social epistemology, a clue to the desired link may be found in the work of the historian in residence at the Vienna Circle, Edgar Zilsel (1945), another Marxist who proposed that the decisive factor in the rise of the Scientific Revolution was a shift in the structure of cognitive authority, such that the pronouncements of the scholar class were now held accountable to experimental standards which throughout the Middle Ages had certified expertise in the artisan class. Being experimental standards, they were indexed for such publicly observable features as time and place, which served to open up the knowledge production process to people from all walks of life, even to those who were not trained in the reading of esoteric texts. This change had the effect of not only democratizing the very act of observation, but also of creating a system for efficiently sorting through the various speculations that had been advanced in the past. Neurath's concern with protocol statements, along with other positivist attempts at formulating a principle of verification, may perhaps be seen as raising to self-consciousness the values of equality (of the individual knowers) and progress (of the collective body of knowers) which were first asserted in the Scientific Revolution.

If, as I have been maintaining, all epistemology worthy of the name has been motivated by essentially sociological considerations, then my thesis should equally apply to the very attempts by epistemologists, such as Laudan, to dispense with the social character of knowledge. Our first exposure to these attempts was in terms of the arationality assumption. Underlying it is a distinction, popularized by the positivist Hans Reichenbach (1938), between a knowledge claim's "context of discovery" and its "context of justification" (sometimes called "context of validation"). By distinguishing these two contexts, epistemologists can dissolve the apparent paradox in saying, for instance, that given the appropriate tests, a belief in Newton's Laws could have been justified at any point in history, even though that fact could not itself have been known before Newton's time. On the other hand, by conflating these two contexts, sociologists fall victim to "the genetic fallacy," which leads them to say that the validity of Newton's Laws is, in some way, affected by their origins in seventeenth-century England. It is nowadays popular to soften the blow just dealt to the sociologist by arguing that the nature of the discovery/justification distinction is little more than verbal. Thus, "discovery" captures the
novelty normally felt about the most recently justified claim in one's research program, while "justification" describes the logical status of the discovery, once divested of its psychological trappings (Nickles 1980). No doubt this is an astute observation, but it merely neutralizes the distinction without explaining how it too is sociologically motivated. Let us now turn to one such explanation.

As the above examples indicated, the discovery/justification distinction is normally invoked by epistemologists in order to prevent the sociologist from unduly restricting a knowledge claim's domain of validity: Newton's Laws were just as valid before Newton discovered them as afterward, and the fact that we are three centuries beyond Newton does not, in any way, diminish our justification for believing in his laws. The intuitive soundness of these claims rests on conceiving of justification as an idealized discovery procedure--in other words, as a sort of scientific competence that can be abstracted from historical performances of scientific reasoning. Once Newton's Laws are stripped of the socio-historical baggage that might make him hostile to them, even Aristotle could come to have a justified belief in them.

Now consider the epistemologist's strategy itself from a sociological viewpoint: How does it make a difference to the domain of people who are eligible to validate Newton's Laws? Taking our cue from Zilsel, the strategy clearly opens up the domain of eligible people by reducing the amount of esoteric knowledge required of the potential validator. In particular, he need not have participated in the cultural milieu of seventeenth-century England, which implies, among other things, that he need not bring to his observations the specialized training that only scientists living at that time would have. Instead, the potential validator would require skills--for example, the ability to perform certain calculations and to focus attention on certain phenomena--that any intelligent and interested human being could be taught at any time or place. Thus, one of the ways in which epistemologists have argued for the universal nature of validity claims is by appealing to the intuition that Galileo, say, could have convinced Aristotle that his account of local motion was in error by conducting free-fall experiments in his presence.

One consequence of regarding the discovery/justification distinction in this manner is that it turns out not to be as ideologically pernicious as philosophers and sociologists have often suggested. A source of the distinction's perniciousness was thought to be that the concept of justification presupposes a Whiggish, or absolutist, conception of epistemic change. After all, while it is clear that Galileo could convince Aristotle of some things, could Aristotle convince Galileo of anything? However, as we have seen, even if Aristotle's and Galileo's persuasive powers turn out to be asymmetrical, that has happened only after both have been limited to justification procedures that are, in principle, equally accessible to all intelligent individuals. In other words, this apparently absolutist end has been reached by strictly egalitarian means.
Even if all the preceding considerations have been enough to persuade you that epistemology is an inherently sociological activity, you may still wonder why epistemologists have been so hostile to the idea. My own diagnosis of the situation points to a rhetorical strategy that epistemologists regularly deploy—and sociologists unfortunately fall for. It involves treating cognitive pursuits and their social organization as if they were two independent entities and then asking how does knowing about the social organization of a particular cognitive pursuit add to our knowledge of the pursuit as a cognitive pursuit. Of course, the typical answer to this question is that it adds nothing to our knowledge of the pursuit as a cognitive pursuit, which leads the epistemologist to conclude that sociology is irrelevant to questions of epistemic status. The sociologist tacitly assents to this conclusion by concentrating his efforts on those features of cognitive pursuits which he himself recognizes as noncognitive. To get a clear sense of the fallaciousness of this strategy, compare the analogous, and historically more familiar, case of someone (perhaps a medieval scholastic) maintaining that knowledge of physiology is irrelevant for knowing about the human being as a human being. How would he show this? By arguing that since every creature has a physiology, there is nothing distinctively human about having a physiology, and, therefore, nothing about the humanness of human beings can be learned by studying their physiology.

What the arguments against sociology and physiology have in common may be described as either a logical fallacy or a rhetorical strategy. The logical fallacy they jointly commit is to confuse the essential features of an object with the features that distinguish it from other objects. As Duns Scotus would put it, the arguers have mixed matters of quidditas with matters of haecceitas. Most pointedly: several essentially different objects can share some of the same essential properties. The "essential differences" refer to distinctions in species of the shared essential properties. And so, just because human beings are not the only creatures with a physiology, it does not follow that human beings would be what they are without their physiology. Indeed, the science of taxonomy was founded on the idea that a sufficiently fine-grained understanding of physiology would enable one to make distinctions amongst the animal species so as to show that human beings have a unique physiology. Thus, rather than disqualifying them from being distinguishing properties, shared essential properties may provide the basis for making the relevant species distinctions. Likewise, just because cognitive pursuits are not the only activities that are socially organized, it does not follow that cognitive pursuits would be what they are without their social organization. Indeed, the sociology of knowledge was founded on the idea that a sufficiently fine-grained understanding of social organization would enable one to make distinctions among the various human pursuits so as to show that particular cognitive pursuits have unique patterns of organization.

As for the rhetorical strategy deployed by the opponents of sociology and physiology, I will call it, for lack of a better name, negative reification. It
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consists of a two-step move: (1) Q, the defining structural property of P, is distinguished from P and made into a separate entity; hence, the social organization of knowledge is distinguished from knowledge "as such," and the human physiology is distinguished from the human being "as such." This is the reifying move. (2) Even though Q has now been formally distinguished from P, the content of Q remains in P, thereby rendering Q devoid of content. This is the negative move. It leaves the impression that one can give an adequate account of knowledge or the human being without referring, respectively, to its social organization or its physiology. However, upon closer scrutiny, it can be shown that the allegedly unnecessary entity is covertly presupposed in the adequate account. Thus, in dualist accounts of the human being, the mind, which supposedly defines the human being as such, is characterized as having a paraphysiology of its own, namely, a system of interdependent functions which regulates the body. Likewise, as we saw when examining the context of justification which supposedly defines a cognitive pursuit as such, a parasociology is presupposed, namely, a normative account of the terms under which one is eligible to participate in the cognitive pursuit. Why hasn't negative reification been more often recognized for what it is? Largely because accounts of both the mind and the context of justification are never discussed with enough specificity to broach the issue of instantiation: When has one identified instances of mental activity? Of a justificatory context? Once these questions are raised, one is forced to introduce considerations of, respectively, physiology and sociology.

2. Social Epistemology as the Pursuit of Scandal and Extravagance

With the founding of the Institute for Social Research at Frankfurt in the 1920s and the publication of Karl Mannheim's *Ideology and Utopia* in 1936, the sociology of knowledge was at first notorious for maintaining that the best way to inquire into the nature of knowledge is by questioning the motives (or "interests") of its producers. Whatever else one might want to say about this program, it was certainly meant as a radical critique and replacement of the epistemological enterprise, especially of its classical task of laying down interest-invariant foundations for knowledge.

Indeed, the sociology of knowledge was conceived as an irreducibly normative discipline, integrally tied to social policy-making (Mannheim 1940). Its central thesis was that the social acceptance of a knowledge claim always serves to benefit certain interest groups in the society and to disadvantage others. As a piece of knowledge policy, the implications were clear: if granting epistemic warrant involves, among other things, social acceptance, and a key benefit of being granted such a warrant is the power to make authoritative pronouncements, then granting epistemic warrant is a covert form of distributing power.

Putting aside, for the moment, the deliberateness with which this policy
normally is (or even can be) carried out, the thesis appears most plausible when considering how disciplinary specialization (in law, medicine, business, and the sciences) has removed an increasing number of issues from public debate to the testimony of "experts." These disciplines exercise "power," in the sense that all epistemically warranted opinion in their respective domains requires their certification, which, in turn, forces the warrant-seeker either to undergo the arduous training of becoming such an expert or simply to conserve effort and defer to the experts already in place. Not surprisingly, then, the normative issue of most concern to these sociologists of knowledge, especially their most recent exemplar Habermas (1975), is how to prevent the republican ideal of "civic culture" from totally dissolving, in modern democracies, into a "mass culture" whose members uncritically submit to the authority of experts.

The original scandal created by the sociology of knowledge, then, was to claim that any answer to "What are the sources of knowledge?" presupposes an answer to "How should society be organized?" Classical epistemology appeared to be a viable pursuit precisely because there were thought to be certain knowledge claims whose social acceptance had equal benefit for all—at least for all rational beings—and hence had no net effect on the distribution of power. This is simply a vivid way of expressing the "value-neutrality" of scientific knowledge: that is, while such knowledge may be used to promote a wide variety of values (as in the different policy ends to which economics may be applied), the knowledge itself is not biased toward or against the realization of any particular values. The source of the equality of benefit afforded by these privileged knowledge claims was the equality of access alleged of them—at least when reduced to the ultimate warrants for their assertion. Unlike such traditionally hermetic forms of knowledge as magic and cabalistic theology, the efficacy of natural and social scientific knowledge would seem not to rest on its access being restricted to only a few specially trained individuals; rather, access to the natural and social sciences has always been advertised as (in principle) open to anyone, since its epistemic warrants ultimately rested on the sorts of logical calculations and empirical observations that any rational individual, with a modicum of training, could perform. Indeed, in the revamped version of the classical position defended by Mill, Peirce, Dewey, and Popper, increasing the accessibility to the scientific process was thought to increase the quality of the knowledge produced, since it would increase the level of mutual criticism of knowledge claims, which would, in turn, increase the chance that creeping value biases would be purged from the process. And so, if a "cult of expertise" has developed in modern times, as the early sociologists of knowledge were inclined to think, then the classical epistemologist would interpret that simply as a case in which certain social interests (perhaps of the knowledge producers themselves, in the case of experts) have perverted for their own ends the natural development of knowledge, which promotes equality of benefit and access.

In its first incarnation, the sociology of knowledge remained little more
than a scandal. It failed to launch a full-scale conceptual revolution--let alone undermine the project of classical epistemology--because of conceptual confusions at its own foundations. These confusions made knowledge production seem much too contrived, as if the dominant class interest could simply dictate what passes for knowledge in the society. After all, if granting epistemic warrant is indeed a covert form of distributing power, does it follow either that granting epistemic warrant is identical with distributing power or even that epistemic warrant is granted in order to distribute power? The former possibility can be read as a semantic thesis of the sort that emotivists typically make about ethical utterances, transferred to epistemological ones; the latter possibility can be read as describing the motives or intentions of those who grant epistemic warrant, which, in an extreme form, would constitute a conspiracy theory of knowledge. Neither of these possible conclusions necessarily follows from the premise, though the original sociologists of knowledge certainly made it seem otherwise. One omitted possibility is a more indirect and interesting conclusion, namely, that granting epistemic warrant simply has the effect of distributing power--thereby leaving open such questions as whether the groups benefitting from this distribution of power are the ones who were originally motivated to propose the particular knowledge claim, whether either the motivators or the benefitters are the ones who make the most use of the claim in proposing other knowledge claims, and so forth. A persuasive defense of this conclusion would be enough to undermine the project of classical epistemology as characterized above, since it would show that there is no method for granting epistemic warrant, including the methods of the natural and social sciences, which does not have an effect on the distribution of power in a society. Yet, at the same time, such a defense would not be forced to assume the extravagant thesis that knowledge is nothing but a myth that the powerful concoct to maintain their power.

At the source of the conceptual confusions that undermined the Old Wave sociology of knowledge was an equivocal reading of the claim that all knowledge is "value-laden" or "interest-laden." Three sorts of groups may be said to have an "interest" in the social acceptance of a knowledge claim:

(a) those who were motivated to propose the claim in the hope that they might benefit from its acceptance;
(b) those who actually benefit from the claim's acceptance;
(c) those who make use of the claim in the course of proposing other knowledge claims.

Let us call (a) motivators, (b) benefitters, and (c) users. The classical epistemologists erred in failing to see that, given the interest-ladenness of all knowledge claims, such groups always exist, and therefore must be taken into account by any normative theory of knowledge production. However, the early sociologists of knowledge were extravagant to suppose that in
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many (perhaps most) cases of knowledge production, the motivators, benefiters, and users are the same group. If that were the case, then not only would the production of knowledge be reduced to the dissemination of ideology, but the dissemination would prove to be incredibly effective; for the original ideologues would then be shown to have tight enough control over the use of their ideology that only they and their allies benefit.

This piece of extravagance has been revealed by the current crop of empirically-minded sociologists of knowledge to be little more than a rhetorical illusion. The New Wavers (Latour & Woolgar 1979, Knorr-Cetina 1981, Gilbert & Mulkay 1984, Collins 1985), who describe themselves as "social constructivists" or "anthropologists of knowledge cultures," argue that all previous inquiries into the nature of knowledge production--both philosophical and sociological--have erred in concluding that there must be some sort of tight control on the use of knowledge simply because the practitioners of a particular discipline justify their knowledge claims in similar ways (by drawing on the same body of knowledge, employing the same inferential techniques, and so forth). Stated this baldly, the naivete is clear: Why should it be presumed that an account of knowledge production, as might appear in a book or a journal article, represents how knowledge is actually produced? After all, the diagnostic tools available to manuscript referees are fairly limited and rarely extend to a comprehensive testing of the knowledge claim under review. Not surprisingly, then, knowledge producers tend to take care in gathering evidence and testing claims only in proportion to the likelihood that the referees will check them. Moreover, an essential part of what makes an account of knowledge production something more than a report of the author's beliefs is that it describes what ought to have happened, given the avowed norms of the discipline. Even mistakes and accidents must be accounted for in the right way. Thus, the process by which knowledge is typically disseminated and integrated serves to insure a uniformity in the expression and justification of claims without insuring a similar uniformity in the activities leading up to these moments of textualization.

However, one can be more or less naive about the relation of words to deeds in knowledge production. More naive is the classical epistemologist, who takes the expressed justifications literally as referring to a common method with a track record of getting at an extrasocial reality. A little more astute were the original sociologists of knowledge, who nevertheless continued to think that behind similar forms of expression must lie similar forms of constraint, even if they turn out to be nothing more than the ideological force exerted by a discipline's dominant interest group. To counteract this naive spirit of determinism, the New Wave sociologists like to say that knowledge production is "contingent" or "context-dependent" or "open-ended." Unfortunately, these expressions mask rather than remedy the shortcomings of the earlier accounts, especially that of the Old Wavers. In particular, each of these expressions can imply either
(i) that no knowledge producer can fully predict and/or control how his research will be used by others in their research, or

(ii) that any knowledge producer is relatively free to tailor other knowledge claims to his specific research situation.

The error that the New Wavers make is to interpret (i) as if it were conclusive evidence for (ii), which only serves to make knowledge production seem, once again, too much under the direct control of the producers—the twist being that instead of knowledge production being determined by large corporate wills such as disciplines and other interest groups, it is now said to be determined by somewhat smaller corporate wills such as research teams and even individuals; hence, extravagance returns through the back door.

Sometimes the sociologists (Bloor 1983, ch. 6) try to mitigate these extravagant claims of scientific self-determination by saying that the norms of scientific practice function as a tacit civil code. In that case, the freedom attributed to the scientist in (ii) is constrained by the fact that there are only a certain number of legal ways in which he can appropriate knowledge for his own research. But even this legalistic gloss does not impose quite the right sense of "constraint" on the scientist's activities, since it continues to allow for a charlatan to be successful at the science game. In other words, it is still possible for someone to bring about whatever effect he wishes on the scientific audience by couching his claims in a legally prescribed manner. However, another feature of the legal analogy can be used to block the charlatan's success, namely, that while a law regulates some social activities, one activity that it does not regulate is its own application. Likewise, the would-be charlatan may know all the scientific norms without thereby knowing which are the appropriate ones to apply in his case: to know the right things to say is not necessarily to know the right times to say them.

For example, he may competently write up a (fraudulent) experiment which purports to disconfirm some standing hypothesis, but if other members of the scientific community are writing up (genuine) experiments which provide support for the hypothesis, then the charlatan's ruse will probably either be ignored or suspected (as would any other deviant claim) and perhaps subsequently unmasked. This uncertainty about how norms are to be applied in future cases have led followers of Wittgenstein to speak of the "open-textured" nature of language games. It is an uncertainty that is grounded on the inability of any given social agent to dictate the manner in which his fellow agents will conform to the existing norms; hence, the invalidity of inferring the benefitters from the motivators. Notice also that throughout this discussion, the constraints and ultimate failure of the scientific charlatan have been explained entirely in sociological terms, such that he is undermined primarily because he is unable to track the cognitive movements of his colleagues and only secondarily because his experiments
are actually fraudulent.

What we have just attempted here is a *sociological simulation* of the classical epistemic ideal of objectivity. It goes some of the way toward answering scientific realists such as Boyd (1984) who argue that the best explanation for the "success" of science is its access to extrasocial, "objective" reality. In contrast, the method of sociological simulation rests on the assumption that objectivity and the other virtues of knowledge production can be *exhaustively* explained by sociological principles. Notice that this position is the exact mirror image of scientific realism: on the one hand, scientific realism typically does not deny the social rootedness of knowledge claims, only the relevance of that fact to an explanation of the truth or falsity of those claims; on the other hand, sociological simulation need not deny the "real" truth or falsity of knowledge claims, only the relevance of that fact to an explanation of the social rootedness of those claims. The point of contention between the two sides concerns whether claims that exemplify epistemic virtues such as objectivity are best explained as "truth-enhancers" or as "institution-maintainers."

The relevant test case for the scientific realist and the sociological simulator is a situation, perhaps a thought experiment, in which a culture accepts a knowledge claim that we take to be substantially correct but for reasons, or through a method, that we consider highly suspect. In other words, by our lights, the culture has stumbled upon the truth "by accident." A good example here would be the ancient atomist belief in a principle of inertia. Needless to say, Democritus never conducted anything like Galileo's experiments, but his metaphysical picture was conducive, in many respects, to thinking along "proto-Galilean" lines. However, as far as we know, the atomists did not try to verify experimentally their metaphysics, and indeed, given their basic beliefs about the radical contingency of nature, they probably would have balked at the very idea of conducting such experiments.

Scientific realists like to point to examples like this, where a culture gets it right "in spite of itself," as simultaneously illustrating that our inquiries are, in some way or other, directed at fathoming the one reality which we all inhabit and that inquirers fathom that reality with varying degrees of success. The sociological simulator would challenge this conclusion by questioning whether it would be possible to convince the atomists to conduct experiments and thereby get it right by "the appropriate means." For if it turns out that the atomists cannot be so persuaded, then their social practices—especially, the pursuit of pure speculation—will have been shown essential to their belief in an inertia principle, which, in turn, casts doubt on whether it really is like the principle that we endorse.

The realist normally is thought to have the upper hand in the debate because the more cross-culturally and cross-temporally accepted a knowledge claim is, the more difficulty the sociologist has in explaining it simply in terms of the claim's rootedness in several, otherwise quite different, social environments. However, the case of the scientific charlatan shows that the realist's dialectical advantage can be undercut, if the
sociologist admits that neither the motivators nor the benefitters of a knowledge claim—nor even the two groups combined—have full control over how the claim is used. In other words, the three sorts of interest groups identified earlier overlap much less than either the Old or New Wave sociologists have been inclined to think. Given a wide enough expanse of history, examples are plentiful. Consider the fate of intelligence testing since 1895. Starting with Binet, the motivators in French pedagogy treated the tests only as diagnostic tools for identifying students in need of remedial education. The users have since included the whole gamut of philosophers of science and social science methodologists who have looked to the tests as evidence for the scientific status of psychology. And since the tests were introduced into Anglo-American psychology through Spearman, the benefitters have ironically been those who believe that IQ refers to an innate intelligence capacity that can be little changed by education (Gould 1983). Therefore, the best sociological explanation for the objectivity of intelligence testing is its versatility in contexts quite unanticipated and even unintended by its originators.

Interestingly, an early step in the direction toward sociological simulation was taken when Popper (1972) introduced World Three, the realm of objective knowledge, whose independent existence emerged as an unanticipated (and usually unintended) consequence of later theorizing about cognitive instruments, such as counting systems, which were originally designed for quite specific practical purposes. For example, the ancients may have developed the study of mathematics in the course of trying to simplify the many measurement tasks performed in everyday life. A general strategy emerged to separate formally the measuring instruments from the measurable things, which led to the invention of a system for representing the natural numbers and, subsequently, to the discovery of properties that the system has independently of how it is used. These properties became the source of problems, such as the nature of irrational numbers, which were sufficiently distant from matters of applied arithmetic to constitute an autonomous domain of knowledge. Yet, at the same time, since this autonomous domain was still thought to underlie all measuring tasks, mathematical experts gained the power to certify the competence of engineers and other professional measurers. At this point, the simulation ends, as the sociologist of knowledge reappears to examine the specific institutional and rhetorical means by which mathematics has maintained this power over the years, which, as Bacon would have it, is expressed as knowledge of the underlying structure, or "essence," of a widespread social practice like measuring.

The above discussion suggests the following division of explanatory labor between the psychology and sociology of knowledge. Psychology enters to study the cognitive limitations on one's ability to anticipate the long term consequences following from one's own interactions and artifact productions, as well as to backtrack those consequences once they have occurred. These limitations are the hidden liability behind the mind's
capacity to "economize," that is, to condense and stereotype information so that less of it needs to be stored. We shall later return to this theme. The various performance errors in memory, reasoning, and attribution identified by cognitive psychologists in the laboratory (Nisbett & Ross 1980) can serve as the basis for these studies, keeping in mind that outside the lab the "errors" are rarely caught once they are committed and may be socially rationalized through reifications, such as Popper's World Three. The significance of such cognitive limitations will vary according to the number of individuals and groups whose activities have causal relevance for one another, their distance in time and space from each other, and the technology available for circulating the relevant cognitive artifacts, especially texts. And it is the sociologist's task to study these variables. However, this division of labor is easier said than done, as we shall see in the next section.

3. Nonnormative Social Epistemology and Other Accommodating Banalities

The New Wave sociology of knowledge has not only inherited the scandalous ways of the Old Wavers, but it has also had its brush with banality. The banality comes from recent attempts, within both sociology and philosophy, to divest epistemology of any normative force. Sociologists have long suspected that philosophical talk about how knowledge "ought" to be produced is motivated by a desire to speak with an authority that lies beyond the check of the empirical disciplines. To safeguard against empirical critique, philosophers since Plato and Descartes have typically supplemented their accounts of the idealized rational knower with a story about how he is continually undermined by his own deep-seated passions (Dawes 1976). The same move can be detected in post-Popperian philosophy of science, with Lakatos blaming the actual scientists of the past for being so swayed by special interests and mob psychology that they rarely conform to his rationally reconstructed history. As the sociologists see it, the philosophers have cleverly turned a weakness into a strength: instead of taking the empirical remoteness of the philosophical ideal to mean that the ideal is false, philosophers take it to mean that real knowers are prevented from realizing the ideal by some part of their psychology which they have yet to discipline properly. Thus, the more remote the ideal, the greater the need for Method (capitalized to indicate its epistemically privileged status). And the greater the need for Method, the greater the authority of philosophers, the experts on Method. As these remarks suggest, the banal response to this philosophical ruse is to treat normative epistemology, at best, as an expression of sour grapes that knowledge is not produced as the philosopher would like or, at worst, as an excuse for the philosopher to ignore altogether empirical inquiries into the nature of knowledge production. In both cases, the prescription is clear: the epistemologist should end his normative ways and thereby dissolve the boundaries that
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currently exist between his work and that of the historian, psychologist, and sociologist.

The philosophical route to banality is quite different in that it portrays the epistemologist as more deceived than deceiver. The *locus classicus* is Quine's (1969, ch. 3) "Epistemology Naturalized," though Rorty's (1979) *Philosophy and the Mirror of Nature* has probably done the most to popularize this picture. The basic idea is that when philosophers from Descartes to Kant proposed a general Method for "justifying" knowledge claims, they were confusing two rather different enterprises: on the one hand, there is the issue of *legitimating* knowledge claims, which is decided by the conventions of a particular culture and will depend on the interests that the culture has in acquiring knowledge; on the other hand, there is the issue of *explaining* knowledge claims, which involves studying their causal origins, a task that Quine, for one, takes to be within the strict purview of behavioral psychology and neurophysiology. By dividing the labor of justification in this manner, the need for a special discipline of epistemology is eliminated: legitimation is best handled by the humanistic disciplines traditionally devoted to cultural criticism, while explanation is a task for which the natural sciences and their emulators in the social studies are best suited. Moreover, once justification has been so divided, the deepest epistemological problem is conquered. This problem, according to Quine and Rorty, is how to account for our ability to generate an indefinite number of theories from the impoverished evidence base afforded by our senses. The twofold way to a solution is, first, to treat how one gets from the evidence to *at least one* theory as a matter of psychological explanation, and then, to treat how one gets from many theories to *only one* as a matter of cultural legitimation. In neither case is there any need for someone equipped with a universally applicable normative theory of knowledge.

There are several problems with these retreats to banality and the spirit of interdisciplinary accommodation that they breed. First of all, the most that either the sociologists or the philosophers have shown is that the methods by which epistemologists justify knowledge claims are not uniquely theirs, though epistemological discourse does its best to obscure any resemblance to the methods of the special disciplines. Suppose no case for methodological uniqueness could be made. It still would not follow that epistemologists are not in a particularly good position to make normative judgments about knowledge claims. After all, to speak of "the division of cognitive labor" is to talk not only about differences in the *techniques* used by the laborers but also about differences in the *materials* to which they apply those techniques. And so, even if the secret of the epistemologist's success amounts to nothing more than an idiosyncratic application of the same deductive and inductive canons used by scientists and humanists, the epistemologist would remain distinctive in that he attends to the interrelations among the knowledge claims made by the special disciplines, while scientists and humanists are restricted to the operations of their own disciplines. Another way of making the same point, which echoes themes raised both earlier and later in
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this chapter, is to say that the critics of normative epistemology have a blurred image of the two strands of positivism which have defined the problem of knowledge since the early nineteenth century: on the one hand, the critics recognize the logical positivist strand and its emphasis on the unity of method, which reduces the task of epistemology to the sort of conceptual housecleaning that practitioners of the special sciences could themselves do, were they not occupied with more important empirical assignments; on the other hand, the critics neglect the Comtean strand and its emphasis on philosophy's unique role of applying the scientific method to the sciences themselves for the purpose of regulating their development.

Still skeptical of the epistemologist's normative powers, the critics may wonder why the special disciplines cannot simply regulate their own activities. Indeed, this laissez-faire attitude is shared by both naturalistically inclined philosophers of science (Laudan 1981) and sociologically inclined critics of epistemology (Bloor 1981), who would normally find themselves at loggerheads. The attitude comes through most clearly in what would seem to be an innocently "pluralistic," even "ecumenical," attitude that the likes of Laudan and Bloor have toward the inclusion of history, psychology, and sociology in any naturalistic theory of knowledge. Presumably, they believe that the philosopher can learn much of value from empirical approaches to knowledge. However, they do not seem to notice that the knowledge claims made by these empirical disciplines are not obviously compatible with one another. Indeed, some of the more interesting claims may be incompatible in a way—call it "incommensurable" if you will—that does not suggest any easy resolution within a particular discipline. As prima facie evidence for this incommensurability, consider that, from examining the citation patterns in their relevant literatures, one would be forced to conclude that psychologists and sociologists of science seldom draw on each other's work, even though they are normally lumped together as part of the "science studies" disciplinary cluster (a limited exception is De Mey [1982]). Upon further consideration, this finding should not come as a surprise, since all that ultimately unites the practitioners of "science studies" is a common whipping boy, namely, classical epistemology. Consequently, they have not devoted much effort to challenging each other's claims. Let us now, then, turn to some latent points of confrontation as a means of showing that there is still room for the sort of normative-yet-naturalistic epistemology envisioned by Comte.

One key point of confrontation concerns the degree of psychologism that needs to be incorporated in a sociological account of scientific activity. In general, a sociologist is "antipsychologistic" if his account of social interaction does not require that social agents have any private mental contents, such as particular desires or beliefs, distinct from their publicly defined role-expectations. In that case, among sociologists of science, the Edinburgh School (Barnes 1977, Shapin 1982) must be counted as amenable to psychologism, since it typically postulates that social agents have relatively well-defined "interests" which they try to promote by
manipulating the course of certain legitimating institutions, such as science. Consequently, the Edinburgh School has tended to present scientific debates as a "superstructure" whose "infrastructure" consists of competing political, economic, and/or cultural interests.

In contrast, the strongest case of antipsychologism may be found among the social constructivists. On their view, derived in part from Wittgenstein and Foucault, an agent's mental contents are themselves socially constructed through institutionalized mechanisms of attribution. If social agents have a specific "interest" on this view, it is simply the interest in maintaining or enhancing one's position in the scientific game; hence, the emphasis that the social constructivists place on the persuasive elements of scientific texts. Indeed, the social constructivists break with both the Old Wave Frankfurt School and the New Wave Edinburgh School in maintaining that the production of scientific knowledge does not function primarily as an arena for competing special interests; rather, they believe that "there is no ultimate objective to scientific investment other than the continual redeployment of accumulated resources." (Latour & Woolgar 1979, p. 198)

As we shall subsequently see, the capitalist model of knowledge production provides a new slant on the idea that knowledge is pursued "as an end in itself."

Moving from psychologism to psychology, precedent may be found in the history of social psychology (especially in Gestalt and attribution theory) for the incorporation of sociological research. These efforts have focused on the finding that, in an experimental situation open to many interpretations, the presence of several people can enforce closure on how a subject perceives or reasons about it. If we then take "antisociologism" to mean an account of the individual mind that does not include a mechanism for registering the effects that the presence of others might have on his beliefs, desires, or course of action, then the psychology of science experiments conducted in the Tversky and Kahneman (1981) paradigm, which currently dominates the discipline, would have to be called antisociologistic. These experiments (see Tweney, Doherty & Mynatt 1982) typically show that real scientists do not implicitly reason about evidence, hypothesis selection, and so forth according to some norm explicitly upheld by both philosophers of science and the scientific community. The psychologists in this paradigm sometimes codify the implicit reasoning procedures but rarely explain why they diverge so greatly—and so systematically—from the explicit norm. The nearest that they come to a general explanatory principle for the errors is to show that subjects' responses are sensitive to the protocols in which the experimenter frames the problem: frame the same problem in two different ways, and you get two different responses. Not surprisingly, then, scientists are most likely to reason according to the explicit norm when they are asked to solve a problem framed in the "canonical form" usually found in textbooks. This would seem to open the door to some sort of sociologism, perhaps pointing toward the psychological limits of disciplinary socialization: At what point do experimental protocols countervail scientific training in determining the
scientist's response? However, antisociologism dies hard among the psychologists of science, who tend to believe that the errors are built into the individual's cognitive mechanism. The result is a picture of human beings as inherently defective computers who, at least when doing science, are subject to external validation checks (Faust 1985).

Interdisciplinary infiltration aside, the incompatibility of psychologism and sociologism also emerges in the form of rival historical hypotheses about the origin and maintenance of disciplines. Though it has been customary to treat disciplines as the point of contact between "the social" and "the cognitive" (roughly, the moment when methodology becomes institutionalized), the most common strategy for explaining their presence has been broadly psychologistic. To get a sense of the difference between a psychologistic and a sociologistic approach (whose exemplar is Foucault) to the history of disciplines, consider the following alternative hypotheses for the rise of biology as a special science in the nineteenth century:

(d) **Historical Psychologism**--Biology began essentially as an ideological movement by a group of individuals, call them "vitalists," who shared an interest in gaining social recognition for their belief that the phenomena of life could not be exhaustively studied by the methods of the physical sciences.

(e) **Historical Sociologism**--Biology began essentially as an "opportunity structure," namely, a collection of procedures for observing, describing, and organizing (both people and things) that had proved effective in quite disparate domains but were now made available for use as an ensemble. Individuals with different, and perhaps even radically opposed, agendas realized that it was in their own interest to contest their claims in a forum bound by these procedures.

Given these alternatives, the received wisdom in both the history and the philosophy of science would recommend hypothesis (d). After examining why in each case, we shall consider the change in strategy that an acceptance of (e) would involve.

While the historian would not have a principled reason for preferring (d), it is clear that histories of science tend to have (d)-like scenarios. Take the issue of interdisciplinary borrowing, as in the case of a physicist or economist making use of some mathematical technique. Arguably, a history of conceptual breakthroughs would be exhausted by a history of interdisciplinary borrowing. However, historians frequently understate the importance of such borrowing by focusing on the fact, say, that the mathematical technique contributed significantly to the solution of the physicist's problem rather than on the fact that the physicist had to refashion his problem in order to take advantage of the mathematical technique. Casting the borrowing episode in the former light makes the history of
science seem dictated by the intentions of the individual scientist, while casting the episode in the latter light makes the scientist appear constrained by the available resources. And while many histories of science have been written of the successes or failures of scientists' projects, few have considered the extent to which opportunity structures have or have not been utilized.

Philosophers have a characteristically principled reason for preferring a (d)-like to an (e)-like history of disciplines. It turns on a commonly held view about the goal of knowledge production, an issue about which we shall have more to say later. But for now, we need only to attend to the rudiments of this view, namely, that knowledge producers first stake out a domain about which they make claims, which they then test with special procedures to arrive at truths about the domain. Even in this simple account, a psychology is imputed to the knowledge producer which, in turn, tends to favor a psychologistic reading of his activities. The crux of this psychological account is that theory formation precedes method selection. Without such an account, it would be difficult to see the motivation for the dispute between realists and verificationists in epistemology. The plausibility of realism rests, in large part, on the alleged psychological fact that we make claims which we believe are true or false, even though we have no way of determining which value. Presumably, according to (d), such claims are often made in the early days of a discipline, when the group has certain beliefs about a domain but no commonly accepted means of testing them. Likewise, verificationism makes sense as an antidote to realism insofar as people do indeed tend to theorize first and choose methods later; for then, the verificationist wants to show that the theory becomes cognitively significant (and hence its claims acquire truth values) only once a method has been selected for testing it. In contrast, an (e)-like historiography of science turns the preceding psychological account on its head: domains and theories about them—the very contents of disciplines—are constructed ex post facto to justify the appropriation and successful deployment of certain techniques by a body of individuals (Abir-Am 1985).

So far we have looked at some instances within the history, psychology, and sociology of science which indicate interdisciplinary incompatibility. However, inside the philosopher's own camp, there have also been epistemological forms of psychologism and sociologism. Here the incompatibility is overlooked, not out of a spirit of ecumenicity, but simply because there have been no clear rules for conducting philosophical arguments about the regulative ideals of knowledge production, the category under which these forms of psychologism and sociologism normally fall. "Regulative ideal," of course, is a Kantian term for what, in this case, is better called a "cognitive utopia," the optimal social organization of knowledge production—the chief aim of social epistemology. If we use "Truth" as a placeholder for whatever is taken to be the goal of producing knowledge, then the psychologistic and sociologistic utopias, modeled on
Kant and Popper, respectively, may be distinguished in the following manner:

(f) Epistemological Psychologism--The best way for the knowledge process to produce Truth requires that all producers share the same attitude toward the process, namely, they should all intend to produce Truth.

(g) Epistemological Sociologism--The best way for the knowledge process to produce Truth does not require that all producers share the same attitude toward the process, but rather that they evaluate each other's products in the same way.

One reason that the expression "knowledge claim" has been appearing where "belief" normally does is to remain neutral between (f) and (g) as to what the knowledge process is supposed to produce. Putting aside accounts of belief as a disposition to act in a certain way, the production of justified (or reliable) true belief is normally taken to involve a change in the producer's psychology (Goldman 1986). In contrast, a knowledge claim may be regarded psychologically as the outward expression of an attitude or sociologically as a move in a language game which may be defended or defeated in certain publicly observed ways. The reader may also notice a resemblance between how the distinction between (f) and (g) has been drawn and how the distinction between the contexts of discovery and justification is normally drawn. In the most general terms, psychologism, like the context of discovery, involves evaluating the frame of mind which the producer brings to the knowledge process, while sociologism, like the context of justification, involves evaluating the consequences of the knowledge process--the actual knowledge products--regardless of the producers' frame of mind. As Durkheim (1938) was the first to realize in his debates with Tarde and LeBon, sociologism had to be distinguished from psychologism in this way in order to prevent sociologism from becoming nothing more than psychologism on a mass scale. To get a sense of the deep-seated differences suggested here between (f) and (g), let us look at how both would respond if the Edinburgh School turned out to be correct in claiming that the knowledge production process is little more than an arena for competing interests.

A proponent of (f) would expect the production of Truth to be thereby arrested, since the individuals are intending Truth only insofar as it serves their particular interests. In order to make the knowledge production process more "productive," the proponent of (f) would have the individual producers aim for the Truth regardless of its social consequences for them or their fellows. This would entail truly asserting one's own beliefs in the hope that they are true (and treating everyone else's assertions in the same way). One can readily see a role for Method in putting the knowledge producer in the right frame of mind for the arduous task of truly asserting the truth. In
contrast, the proponent of (g) is not nearly so Method-conscious, largely because he does not believe that the production of Truth is especially aided by the producers having a special attitude toward the knowledge process, such as "intending to produce Truth." Consequently, the proponent of (g) is more sanguine about the Edinburgh thesis, believing that as the knowledge producers try to maximize their own interests, they will try to undercut each other's claims in the most publicly acceptable way possible--namely, by appeals pertaining to the production of Truth--so as to cover up the fact that their criticisms are motivated exclusively by self-interest. If this process continues long enough, it will be the basis of an "invisible hand" or "cunning of reason" explanation for how the Truth can ultimately be produced by a collection of individuals whose main interests have little to do with producing Truth. Moreover, this explanation is sociologistic in that the individuals are rendered unwitting producers of Truth because the means by which they pursue their interests is dictated by the need to obtain a favorable response from their fellows.

4. Social Epistemology Rendered Normative and Epistemology Rendered Interesting

Following the example of ethics, there are two standpoints from which one can make normative judgments:

(m) before someone acts, so as to direct his action;

or

(n) after someone acts, so as to evaluate his action.

The New Wave sociological critique of epistemological practice assumes that philosophers make normative judgments only in sense (n), namely, as critics of an activity in which they themselves do not participate. However, philosophers can equally be read as speaking normatively in sense (m), which would show that, like the original sociologists of knowledge, they follow Marx's dictum that not only must they interpret the world but they must change it as well. Indeed, the truth of the matter may well be that normative judgments in sense (n) about past knowledge production are meant as the basis for issuing normative judgments in sense (m) about future knowledge production. Still, we need to give the philosopher's propensity for idealization some sociological credibility. A good strategy here may be to treat idealization as an elliptical form of social engineering. The ultimate source of this strategy is Francis Bacon, though its twentieth-century exemplar has been Gaston Bachelard's (1985) theory of scientific experimentation, several versions of which persist to this day (including von Wright [1971], Bhaskar [1980], Hacking [1983], Heelan [1983], and Apel [1984]). The basic idea is this: if we
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go by ordinary observation alone, nature does not appear to have a lawlike character. What we need to do, then, is to create an environment in which this lawlike character can be exhibited on demand. The instructions for creating such an environment, which are normally left implicit in a law's \textit{ceteris paribus} clause, consist of social practices that would probably have no place outside this context. For example, the social space needed for demonstrating the law of inertia nowadays requires that observations be taken of objects moving in a frictionless medium in a laboratory. As a rule, the social constructivist would want to say that when a scientist claims that some melange of phenomena is "essentially" governed by a particular natural law, all the scientist means is that he knows how to constrain the environment so as to make the phenomena behave in a regular manner. Indeed, attempts have been made (Rip 1982, Whitley 1986) to define disciplines according to the sorts of constraints they require, and perhaps many of the difficulties involved with integrating research from the social sciences are due to these disciplines operating with incompatible constraints (for more on this point, see ch. 8). And so, when philosophers speak of the ideal rational knower, they too may be suggesting that our judgments of knowledge production should be taken in a more restricted social setting, with the philosopher's preferred Method functioning as partial instructions for creating that setting. Thus, we should take seriously as a step toward ideal rationality Descartes' remark, early in the \textit{Meditations}, that his Method cannot be implemented until after one has withdrawn from normal social intercourse.

The only problem with interpreting the epistemologist's normative judgments in this way is that they provide only \textit{partial} instructions. The other part of the instructions, involving the rhetorical, technological, and generally administrative means by which the epistemologist causes his Method to be followed, has rarely been a topic of philosophical discussion. The most immediate sign of this failure has been the lack of interest that epistemologists have generally had in issues of \textit{education} (Dewey being the obvious exception). Instead, the epistemologist has all too often presumed (though some, like Descartes, have been explicit) that his Method is "self-certifying," which is to say, that, if presented with the Method, all rational beings would recognize it for what it is and follow it. This presumption would seem to be enough to convict the epistemologist, at least in the sociologist's eyes, of both arrogance and naivete. However, the epistemologist may try to vindicate himself by arguing that the sociologist has missed the point of his idealizations being clothed in counterfactuals rather than directives, namely, that his interest is restricted to \textit{thought experiments}, cases of methodical abstraction whose validity does not rest on their empirical realizability and, therefore, does not require that the social environment be restructured in any significant way. But even a response of this sort need not satisfy the sociologist, who can always subject the epistemologist to what I shall call \textit{the constructivist's regress}, namely, that as long as there are no self-certifying Methods, and even if what is at stake
is simply the conceptual necessity of the outcome to a thought experiment, there will always be a difference between the epistemologist's Method and the means by which the Method's acceptance is brought about, with the latter requiring some transformation of the social environment—even if only at the level of selecting language that will be persuasive to the intended audience.

Having recovered the normative and social element for the epistemologist, amidst the suspicions of the sociologist, let us now make sure that the naturalistic element remains. Once it is granted that the philosopher makes normative judgments in sense (n), we can then question how those judgments work. Consider two possibilities:

(n1) Every case of knowledge production is evaluated against the best possible case of knowledge production.

(n2) Every case of knowledge production is evaluated as if it were the best possible case of knowledge production.

Given its concern with constructing a method for reaching optimal knowledge production, epistemology has traditionally been normative in sense (n1). However, given what the sociologists have shown to be the suspect motives and inadequate means for following through on (n1), epistemologists might turn to being normative in sense (n2) and thereby practice panglossian epistemology, after the character in Voltaire's Candide who believed that "this is the best of all possible worlds." The idea would be for the epistemologist to treat what he would normally regard as "interference" in the smooth working of the knowledge production process (fraud, misunderstanding, and other errors) as indicating merely that he has yet to fathom where such "stray parts" fit in the process (Dennett 1971).

In focussing on knowledge as it is actually produced, I do not mean to suggest that the panglossian epistemologist should base his account simply on a snapshot of current practices. Rather, he should identify the historically invariant and regularly varying features of knowledge production which would lead someone to think that it was the sort of thing—a "process" or "system"—to which one may reasonably attribute an overall design. And so, a question in the panglossian vein would be, "Why would the process be designed so that knowledge producers usually explain their success in terms of a particular methodology, even though they rarely do anything rigorous enough to be described as conforming to that methodology?" On the panglossian view, one could grade the adequacy of a classical epistemology by how much of the actual knowledge production process turns out to be dysfunctional in relation to its idealized method. Whereas the classical epistemologist himself would expect a certain amount of dysfunctionality, the best epistemology would now be the one that finds nothing dysfunctional about the process.

At this point, instead of getting excited by our proposal, the classical
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epistemologist might argue that he too has taken a design stance toward knowledge production, the only difference being that he does not presume that the process is always (or perhaps ever) in perfect working order. Indeed, by presuming that the process is optimally functioning, the panglossian epistemologist seems to commit the naturalistic fallacy, since he not only infers from how knowledge production is to how it ought to be, but actually equates the two. This objection would, thus, charge the panglossian with an embarrassing amount of naivete. In countering the objection, let us start by considering two ways in which an epistemologist might take a design stance toward knowledge production:

(p) Assuming that he already knows the purpose of producing knowledge, he can then determine how and whether the parts of the knowledge production process function to realize that purpose.

(q) Assuming that he already knows that the parts of the knowledge production process function optimally to realize some purpose, he can then determine what that purpose could and could not be.

The classical epistemologist goes the way of (p), which explains his concern for "truth-enhancing methodologies": truth is the goal and (the epistemologist's) method is the means which may or may not be operating in actual cases of knowledge production. But in light of the difference between (p) and (q), how is the classical epistemologist able to assume that he knows the point of producing knowledge, especially since he is the first to admit that there have been few cases of optimal knowledge production? The answer normally given, of course, is that by definition knowledge is methodologically sound access to the truth. Consequently, if the classical epistemologist were to learn that most of what passes for "knowledge" was not produced in what he would consider a methodologically sound manner, he would withdraw use of the term in those cases rather than change the definition of knowledge to capture something else that the cases have in common.

Now, if the panglossian claimed to know the point of producing knowledge in the way that the classical epistemologist does, then he would truly be naive; for he would then be denying what both the classical epistemologist and the sociologist of knowledge readily admit, namely, that there are major discrepancies between philosophical idealizations of knowledge production and the actual cases. However, the panglossian takes a design stance, in sense (q), toward knowledge production. It follows that the knowledge production process works optimally toward some end, but it is a matter of empirical determination what that end is: What sorts of goals can be realized given the actual structural constraints on knowledge production? On this view, it may simply be a matter of empirical fact that actual knowledge production processes lack any clear indicators for such qualities as retention, accumulation, and convergence, which philosophers of
science since Charles Sanders Peirce (Rescher 1978) have associated with the alleged point of knowledge production, progress toward the Truth. (A stronger conclusion would show the unfeasibility of instituting the appropriate indicators.)

The idea that the trinity of retention, accumulation, and convergence might constitute yet another philosophical mythology seems farfetched until we start examining its foundations. All three Peircean qualities draw on the idea that knowledge is not merely transmitted, in a loose sense, from person to person but that some invariant content is preserved in the course of each successful transmission. The task of analytic philosophy in the twentieth century has largely been to arrive at a theory of this stricter sense of transmission, or translation. Yet, the historical record reveals only a series of controversial attempts to define the nature of translation in terms of what makes two sentences "synonymous" or "express the same proposition." And, no doubt, much of the inconclusiveness of this enterprise has been due to the lack of "real world" exemplars for any stricter sense of knowledge transmission, outside the standard cases of truth-functional equivalence in formal logic. On the basis of this record, the panglossian epistemologist concludes that whatever may be the point of knowledge production, it does not need to involve an activity as illusory as the analytic philosopher's sense of translation.

As a result, the panglossian is skeptical of two images of the knowledge product that presuppose a strong sense of translatability. First, he doubts that knowledge can be regarded as a storehouse whose contents are expanded and contracted at appropriate moments. This picture often informs the view that unequivocal judgments can be made about the relative size of two bodies of knowledge, and it is central to the belief that knowledge is essentially cumulative. The logical conception of translation assumed by the storehouse picture becomes especially clear in attempts to render scientific revolutions "rational." For example, Isaac Levi (1984) has argued that in the course of such revolutions, the knowledge base must contract before expanding, and not vice versa, because were a revolution to start by expanding the knowledge base, a contradiction would be harbored. The "must" in the argument seems to be invested with transcendental status—at least if the history of science is to be rendered rational. Thus, we should expect that the parts of Aristotelian physics in potential contradiction with Newtonian mechanics were fully excised from the early eighteenth-century knowledge base before Newtonian physics was added. But clearly this was not the case historically. This implies that the same knowledge production process can incorporate incompatible bodies of knowledge, which, in turn, suggests that real knowledge processes are, at best, imperfect monitors of their own logical coherence. We shall later return to this point, when looking more closely at the knowledge process as a "system."

The panglossian epistemologist also doubts that the knowledge product is realistically regarded as a deductively closed network of commitments. Whereas the storehouse picture tried to account for the expansive and
contractive properties of knowledge, the network picture attempts to capture some of knowledge's other alleged properties, such as the power of propositions to explain their logical consequences, and of those consequences to confirm the propositions from which they were derived. Logical positivism was dedicated to elaborating this picture. One telling feature is that it typically defines rationality in terms of the knower believing all the logical consequences of the propositions to which he is explicitly committed (Dennett 1978, part 2). The principle sources of irrationality, then, come from the knower either not making the right inferential moves from one proposition to the next or simply not having a comprehensive enough grasp of his own knowledge base to know what he knows. If historical reality undermined the first image, psychological reality undermines the second; for if knowledge were a deductively closed network, then rationality would be exclusively the property of *artificial* intelligences, since what passes for irrationality in the network picture is the norm for human beings and their knowledge production processes (as seen above in their tolerance for contradictions). This observation shows that the network picture violates the panglossian's methodological stricture that the point of knowledge (and rationality a fortiori) be empirically determined by studying its naturally occurring instances (Chemiak 1986).

Having just given the panglossian critique of the two images of the knowledge product that presuppose a strong, logician's sense of translatability, it must now be admitted that both have become very much bound up with ordinary conceptions of knowledge. Thus, the burden remains with the panglossian to come up with an alternative image that is true to the knowledge product in its "natural state." To meet the classical epistemologist's challenge, we shall provide the panglossian with a composite image of knowledge production, drawn mostly from the Marxism, Structuralism, and Systems Theory that is current among French philosophers and sociologists of science. It is a somewhat sanitized synthesis of proposals put forth by Pierre Bourdieu (1975), Michel Serres (1972), and Latour & Woolgar (1979). (Fuller [1984] places this image in the context of contemporary European social theory.) Yet, as the following theses make clear, the sociological simulation marks a radical break with how the classical epistemologist has handled such matters.

(A) It is misleading to regard knowledge as something that could, at least in principle, be accumulated indefinitely by all knowledge producers. Rather, knowledge production is an "economic" process, which means that the more knowledge had by one producer, the less had by another. Therefore, the key issue in regulating knowledge production is not how to accumulate more but how to redistribute more equitably.

(A1) The knowledge production process "economizes" at two levels: at the micro level, each new knowledge product (say, a journal article)
redistributes the overall balance of credibility (see [B]); at the macro level, a relatively constant amount of knowledge is circulating in the process, which means that relatively little of the knowledge produced is preserved in the long run; rather, it is "translated" (see [B1]).

(B) "Having knowledge" is not a matter of possession, as the having of a mental representation typically is in classical epistemology. Rather, it is a socially ascribed status that a knowledge producer can (and normally wants to) earn in the course of his participation in the knowledge process. A producer "has knowledge" if enough of his fellow producers either devote their resources to following up his research (even for purposes of refutation) or cite his research as background material for their own. The producer continues to "have knowledge" only as long as these investments by his fellows pay off for them. Thus, "having knowledge" is ultimately a matter of credibility. But given the numerous ways in which producers can draw on each other's work, the fact that there are centers of credibility in the knowledge production process does not necessarily imply that the producers agree on anything more than on who the credible knowledge producers are.

(B1) The sense of "translation" relevant to knowledge production is limited to the design of functionally equivalent texts, which facilitate the distribution of credibility in the knowledge process. Since more knowledge is produced than could ever be preserved (footnotes do not accumulate in proportion to the footnotable material available), a premium is placed on works which can render redundant much of what is already in circulation. Thus, what are normally called "interpretations," "synopses," and "glosses" pass as translations. These works accrue credibility for their producers and diminish, if not entirely subsume, the credibility of the producers whose works are replaced. Thus, the relevant sense of "translation" is that of substituting and eliminating texts, though without the presumption that the exact contents of those texts are retained along the way. That retention is really as spotty as this account suggests may be seen in the extent to which the contents of a text can be lost without ever having been definitively refuted, only to be recovered at some future date to revolutionize the particular knowledge production process.