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What Are Cultural Studies of Scientific Knowledge?

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Interdisciplinary studies of the sciences have been dramatically transformed over the past two decades by sociological studies of scientific knowledge. The postpositivist interdisciplinary formulation of "history and philosophy of science" has been fundamentally challenged by the sociological perspectives offered by the Edinburgh "Strong Programmed" the Bath constructivist-relativist approach, applications of discourse analysis to science, and ethnographic laboratory studies. Many features of scientific work that have been highlighted by these sociological traditions have become indispensable considerations for any subsequent interpretation of science. These "social constructivist" studies have brought renewed attention to the epistemic importance of laboratory practices and equipment, to the omnipresence of conflict and negotiation in shaping the outcome of scientific work, to the formation and dissolution of disciplinary boundaries, and to the permeability in practice of any demarcation of what is "internal" to science. Constructivist studies have also effectively highlighted the sheer difficulty of scientific work: getting equipment and experiments to work reliably, replicating their results, and achieving recognition of their success and significance.

Despite the significance of social constructivism, however, much of the subsequent work in science studies does not easily fit within the terms set by the disagreements between social constructivists and the proponents of internalist history and philosophy of science. **[End Page 1]** Among the central issues between social constructivists and internalists were the relative importance of social and rational (or external/internal) "factors" in explaining the content of scientific knowledge, the relations between empirical descriptions and epistemic evaluations of the methods and achievements of scientific research, and the coherence of either realist or relativist/constructivist accounts of how scientific knowledge is related to the world. Recent work in a variety of science studies disciplines has increasingly challenged the very terms of these debates. Concerns have been raised about the goal of explaining scientific knowledge, the presumed explanandum of the "content" of knowledge, the supposed opposition between descriptive and normative approaches, and the intelligibility of the question that realist or constructivist interpretations of knowledge are supposed to answer.

In this paper, I shall try to articulate and illustrate some important issues that mark the movement beyond the terms of the disputes between internalists and social constructivists. For convenience, I adopt the phrase "cultural studies of scientific knowledge" to refer to this quite heterogeneous body of scholarship in history, philosophy, sociology, anthropology, feminist theory, and literary criticism. In using such a term, it is crucial to keep in mind that it cuts across some very important theoretical differences, including some significant scholarly work taking place across the very boundaries I am articulating between cultural studies and the social constructivist tradition. My aim is not to reify cultural studies, but to highlight some important issues which might reshape the terms of interdisciplinary science studies.

So what are cultural studies of scientific knowledge? I use the term broadly to include various investigations of the practices through which scientific knowledge is articulated and maintained in specific cultural contexts, and translated and extended into new contexts. The term "culture" is deliberately chosen for both its heterogeneity (it can include "material culture" as well as social practices, linguistic traditions, or the constitution of identities, communities, and solidarities) and its

connotations of structures or fields of meaning. An extensional characterization might usefully help specify this still quite broad notion, at least for those familiar with the science studies literature. Among the practitioners of cultural studies of science I would include such diverse historians as Donna Haraway, Robert Marc Friedman, Simon Schaffer, Evelyn Fox Keller, Robert Proctor, and V. B. Smocovitis; sociologists and anthropologists such as Sharon Traweek, Bruno Latour, Paula Treichler, Leigh Star, Michael Lynch, and Karin Knorr-Cetina; philosophers like Ian Hacking, Helen **[End Page 2]** Longino, Arthur Fine, Sandra Harding, and myself; and literary theorists such as Gillian Beer and Ludmilla Jordanova.

Anyone who knows these scholars' work knows that that list is very far from comprising a monolithic group: it encompasses sharp and far-reaching theoretical, methodological, and political differences. Yet there are both historical and philosophical considerations that have narrowed this list substantially, and that provide its coherence. I shall begin with several historical vignettes that may help situate the differences between cultural studies as I conceive them, and the sociological and philosophical traditions to which they are responding. I should emphasize that these do not even constitute a sketch of a history of cultural studies, but only some possibly revealing fragments. In this context, I shall then discuss more systematically what I take to be the most important theoretical issues that demarcate cultural studies of science as a significant and distinctive field of inquiry.

Situating Cultural Studies of Scientific Knowledge

My first historical note fittingly recognizes the indebtedness of cultural studies of science to the social constructivist tradition. Cultural studies follow the lead of the Strong Programme and its sociological successors in refusing to require distinctive methods or categories to understand scientific *knowledge* as opposed to other cultural formations. Karl Mannheim's earlier sociology of knowledge notoriously *exempted* the natural sciences and mathematics from its purview. [1] Similarly, the tradition established by Robert Merton, which still largely dominates American sociology of science, did address the natural sciences, but insisted that its investigation of scientific institutions and norms largely took for granted the content of successful scientific work. [2] Mertonians have been concerned with how that work could be embodied institutionally and culturally, and how deviations from its established norms and methods might be appropriately explained. Much of the philosophy of science (and some historical work) have likewise been constituted by distinctions between the imagination, reasoning, and evidence "internal" to the establishment of scientific knowledge, and the biographical and social factors that at least ideally might be excluded from epistemological reflection. **[End Page 3]**

By contrast, cultural studies of scientific knowledge take as their object of investigation the traffic between the establishment of knowledge and those cultural practices and formations which philosophers of science have often regarded as "external" to knowledge. Scientific knowledge is taken to be a cultural formation that has to be understood through a detailed examination of the resources its articulation draws upon, the situations to which it responds, and the ways in which it transforms those situations and has an impact upon others. As I shall argue shortly, cultural studies do not try to replace internalist accounts of knowledge by relying upon a privileged alternative explanatory framework (e.g., social factors), but neither do they grant epistemic autonomy to what is currently accepted as scientific knowledge.

A second, more historically specific vignette may help locate some interesting differences between social constructivism and cultural studies of science. The culture and politics of scientific knowledge became a focal point of state politics in both the United States and Great Britain during and after World War II, as the state became more actively involved in the support and direction of scientific research. The issue broadly concerned how best to organize, support, and direct scientific inquiry in a democratic political culture. In Great Britain, crystallographer J. D. Bernal argued for the deliberate political management of science for socially beneficial ends. [3] Bernal was a committed socialist, who maintained that a capitalist society was incapable of developing or utilizing scientific knowledge

effectively or humanely. He emphasized that scientific inquiry was a social product of human labor, which required considerable resources, and promised great benefits, but also could create new resources for oppression. What was needed was a social transformation in which a humane science could flourish, but one that he also saw as implicitly called for by the aims of science itself: "science implies a unified and coordinated and, above all, conscious control of the whole of social life." [4]

"Bernalism" was prominently opposed by the physical chemist Michael Polanyi. [5] Polanyi's epistemology emphasized the importance of practical skills and nonverbal communication in what he **[End Page 4]** called the "personal knowledge" that shapes scientific work. But his position had important and conservative political consequences: science could not be deliberately directed to social ends without undermining its epistemic success; furthermore, since the basis of scientific knowledge was inarticulable, no one could understand how best to advance science who was not a practicing scientist. There was no alternative, on Polanyi's account, to unrestrained freedom of scientific inquiry, and administrative control of scientific resources by a scientific elite.

The social constructivist tradition has taken an ambivalent stance toward the Bernal/Polanyi debate. Constructivists have adopted a Bernalist interpretive stance toward scientific activity, emphasizing that research is a process of social production and certification, which must be understood in terms of social categories. The descriptions of scientific activity that they have developed from this stance, however, are deeply indebted to Polanyi. Polanyi's account of scientific knowledge as locally situated, tacit knowhow has directly influenced both relativist and ethnographic studies of scientific laboratories (Harry Collins, Trevor Pinch, Bruno Latour and Steve Woolgar, Karin Knorr-Cetina), which have been an important component of the constructivist tradition. Furthermore, despite their occasional rhetoric of antiscientism, the constructivists have predominantly shared Polanyi's antinormative stance, which forecloses the possibility of criticizing scientific practices and beliefs. [6] Constructivists initially seem to preclude criticism of scientific practices on different grounds than Polanyi did: they espouse a far-reaching epistemic relativism, instead of an elitist defense of the unquestionable authority of scientific communities. Yet in practice, these two positions converge in their defense of community authority. Thus, constructivists Harry Collins and Steven Yearley offer this Polanyiesque objection to Michel Callon's account of the fate of a French research project on scallop cultivation: "There is only one way we know of measuring the complicity of scallops, and that is by appropriate scientific research. If we are really to enter scallop behavior into our explanatory equations, then Callon must demonstrate his scientific credentials." [7] **[End Page 5]**

Where social constructivists thus find themselves drawn to both sides of the Polanyi/Bernal debate, proponents of cultural studies will typically be attracted to neither. The poststructuralist theoretical influence upon much of cultural studies of science is not congenial to the Marxist humanism that animated Bernal: Bernal's presumption of a common human interest and a shared project of liberation through the social appropriation of production is at odds with cultural studies' sensitivity to differences and contested meanings and identities. Yet Polanyi's vision of a self-managing scientific elite is still less attractive. Instead of sanctioning or relativizing scientific communities, cultural studies contest their boundaries and the authority established by marking and policing those boundaries. [8] A very different politics of knowledge must follow from this stance, neither Polanyi's scientific oligarchy nor constructivists' pluralism of epistemic communities.

Such an epistemic politics can neither allow the scientific community to speak authoritatively in a unified voice, nor can it colonize science in the name of a privileged vocabulary imposed upon science from a standpoint of epistemic sovereignty. [9] My final historical vignette thus appropriately emphasizes the indebtedness of cultural studies of scientific knowledge to the last half-century of political criticism of science from within the scientific community. Contemporary cultural studies of science owe much to the political ambivalence among physicists that led to the *Bulletin of the Atomic Scientists*, and the more widespread scientific opposition to militarized scientific research (especially during the Vietnam War), the formation of groups like Science for the People and the Radical

Science Journal Collective, the rise of a scientific environmentalism that included opposition to corporate and government domination of research on pesticides, low-level radiation, etc., up through the controversies over recombinant DNA research and then the Human Genome Project. The first wave of research on issues of science and gender, which emphasized the criticism of ideological treatments of gender in biology and psychology, was also largely the work of scientists, **[End Page 6]** and their work was probably a precondition for the more far-reaching discussion of science and gender in recent cultural studies. [10] Cultural studies of science belong not only to the history of the academy and its disciplined historical, philosophical, and sociological interpretations of science, but also to the history of science, the culture of science, and political struggles over scientific knowledge.

In situating cultural studies of scientific knowledge in these ways, I have tried to emphasize their continuity with important aspects of the twentieth-century culture of science. But now the time has come to say something about what their own distinctive contributions are to understanding science and scientific knowledge. Of course, given that cultural studies of scientific knowledge are both diverse and contested, there is something artificial about attributing to them a common picture of scientific work. Yet there are significant common themes, however diversely developed, which mark important contrasts to other ways of understanding the sciences. I shall mention six such themes: antiessentialism about science; a nonexplanatory engagement with scientific practices; an emphasis upon the materiality of scientific knowledge; an even greater emphasis upon the cultural openness of scientific practice; subversion of, rather than opposition to, scientific realism or conceptions of science as "value-neutral"; and a commitment to epistemic and political criticism from within the culture of science.

The Heterogeneity of Science

Cultural studies of scientific knowledge reject the idea that there is an essence of science, or a single essential aim that all genuinely scientific work must aspire to. In Richard Rorty's succinct formulation, "natural science is not a natural kind." [11] The practices of scientific investigation, its products, and its norms are historically variant. They also vary considerably both across and within scientific disciplines: high-energy physics, low-temperature physics, radioastronomy, synecology, molecular biology, taxonomy, paleontology, and meteorology are in many respects quite different **[End Page 7]** epistemic practices--and this list does not even encompass more directly "applied" scientific fields. Scientific work is also culturally variant even within the same field; there are often, for example, important national differences in the style, direction, standards, and goals of scientific work. This does not at all mean that different scientific cultures are self-enclosed or mutually uncomprehending, or that individual scientists or groups cannot navigate their borders quite effectively. Nor does it mean that the epistemically interesting differences in scientific cultures neatly map onto national, linguistic, or other cultural boundaries. I will say more about cultural difference later in this section.

For now I just want to emphasize that the variability within scientific practice involves many of its important features. It includes the scale, precision, technological sophistication, sensitivity, theoretical transparency, and theoretical independence of its instruments; the scale, location, mobility, and accessibility of its objects of inquiry; its social order (e.g., the size of its effective research groups, and their degree of heterogeneity in knowledge, skill, mutual understanding, status, etc.); its theoretical sophistication and the relations between theory and experimental or observational practice; its distance from specific "applications" of knowledge; the character and significance of its engagement with other cultural practices; the relative importance of description and explanation; and the institutional organization of its research and communication.

Insensitivity to the heterogeneity of the sciences is an important part of what cultural studies take to be wrong with global legitimations of the rationality of science, or its referential success, and equally wrong with those epistemic relativisms which place scientific communities (and their accepted results) on a par with others and with one another. Whether one is arguing that scientific knowledge

as such is superior to other epistemic practices, or that it is "no better than" others, or that it is somehow less adequate, the mistaken assumption is that scientific knowledge belongs to a single kind, similar or distinguishable *in kind* in any interesting way from other kinds. Similar problems are manifest in any attempt to distinguish natural science from social or human science. [12]

Against Explanation in Science Studies

Cultural studies' antiessentialism extends to my second theme: one of their most important differences from the social constructivist **[End Page 8]** tradition is their opposition to an explanatory stance toward scientific knowledge (or its "content"). Social constructivism typically presents itself as an explanatory social science, which can (potentially) account fully for the epistemic outcomes of scientific practices. In this case, the vocabulary of social interaction (interests, negotiations, etc.) is supposed to hold the key to an adequate understanding of scientific work. But as Nancy Cartwright has noted about physical explanation, "The aim [of an explanatory science] is to cover a wide variety of different phenomena with a small number of principles. The explanatory power of [a] theory comes from its ability to deploy a small number of well-understood [expressions] to cover a wide variety of cases. But this explanatory power has its price [which is] to constrain our abilities to represent situations realistically." [13] The need to account for the phenomena in terms of a theory's explanatory concepts suppresses differences among the phenomena being explained, whether those differences are susceptible to alternative explanatory frameworks or not. For example, a social explanation of the content of a scientific practice is not well situated to consider the variety of ways such a practice may be appropriated and used; cultural studies of science may well be concerned with the plasticity of what constructivist studies take as an unproblematic explanandum.

But there are two related difficulties with an explanatory stance, which are perhaps even more fundamental for cultural studies. First, cultural studies take exception to the ways in which an explanatory stance reifies the boundaries between the interpretation and what it interprets. This reification can take different forms. Latour and Woolgar, for example, adopt (at least rhetorically) the stance of the ethnographer as *stranger*; [14] while Collins and Yearley present themselves as disciplinary *antagonists* to the natural sciences: "We provide a prescription: stand on social things--be social realists--in order to explain natural things. The world is an agonistic field (to borrow a phrase from Latour); others will be standing on natural things to explain social things.... [SSK, then] wants to use science to weaken natural science in its relation to social science." [15] Cultural studies have instead been influenced by **[End Page 9]** that tradition in postcolonial anthropology which is suspicious of attempts to impose categories unilaterally upon the Other [16] --even when anthropology has been repatriated, science has been made into the Other, and the imperializing anthropologists present themselves as the "underdog" to the established cultural authority of the natural sciences. [17]

The second related problem with social explanations of scientific knowledge concerns the reification of the categories of the (social) explanans, which is self-consciously defended by Collins and Yearley in the passage I just quoted. Cultural studies focus upon the articulation and significance of meanings, and are reluctant to set the categories of social explanation outside of their purview. This reluctance increases wherever such explanations presume the *unity* of social identities or categories, which cultural studies frequently must deconstruct. Such an exception becomes further troubling given the widespread acknowledgment that the categories and practices of social explanation themselves belong to a scientific tradition. This issue has often been discussed by social constructivists under the heading of "reflexivity," although it has too often been misunderstood as leading to a general skepticism, or as a rhetorical problem in a sense that can be distinguished from the political positioning of the explanatory project. [18] Cultural studies instead take reflexive questions as an invitation to consider their own complex epistemic and political relations to the cultural practices and significations they study. [19] **[End Page 10]**

Local Knowledge

The third feature of cultural studies I want to emphasize is its insistence upon the local, material and discursive character of scientific practice. [20] Scientific knowledge is often discussed as if it were a body of free-floating ideas detachable from the material and instrumental practices through which they were established and connected to things. Cultural studies (along with other recent studies of experimental practice) emphasize, instead, the importance of specific complexes of instruments and specialized materials, and the skills and techniques needed to utilize them, in shaping the sense and significance of knowledge. They also emphasize the particularity of networks of scientific communication and exchange, which shape both what needs to be said and what vocabulary and technical resources can be appropriately utilized.

For example, cultural studies emphasize the ways in which disciplines can be created or transformed as much by new instruments and objects as by new concepts or theories (although we should be wary of distinguishing these categories too sharply, as if instruments and objects were somehow prediscursive). The transformation of classical cytology into modern cell biology was focused more by uses of the ultracentrifuge and the electron microscope than by any particular theoretical innovations, but it thereby changed what counted as a scientifically interesting question about cells, and as an adequate answer to it. [21] Peter Galison has argued as well that some basic concepts of particle physics were altered by **[End Page 11]** the use of counters in the 1930s; they transformed "electron," for example, in practice from an aggregate to an enumerable concept (without instantiating distinct *individuals*). [22]

Instruments belong ineluctably to local contexts within which there are the facilities, skills, and discursive practices that enable them to operate significantly. Philosophers in the 1960s and 1970s thought that the influence of instruments on scientific knowledge could be captured in terms of the theory-ladenness of observation; but that presumed that the crucial aspects of the instrument's functioning were theoretically understood. Almost invariably this is not the case, as sources of error and noise are regularly circumvented by practical engineering, which does not require full theoretical comprehension. [23] The locality of knowledge is also suggested by the importance of the exchange of actual materials to be used or investigated (particular cell cultures, plasmids, superconducting ceramics, etc.), which are not readily reproducible from a description.

Some scientists and philosophers may balk at this emphasis upon the irreducible locality of scientific knowledge, but they should be clear about what they are thereby doing: they are excluding from scientific knowledge most of what experimentalists, instrumentalists, and even phenomenologists within the sciences distinctively know. Cultural studies' emphasis upon the locality and materiality of knowledge must, however, be distinguished from the suggestion that such knowledge is either "tacit" (as Polanyi argued) or mute (as is perhaps implied by some recent studies of experimental practice, which may seem to suggest a materialist explanation of scientific knowledge as opposed to its cultural interpretation): in either case, material practice would be rendered *inarticulable*, and hence inaccessible to the interpretive practices of cultural studies.

The Openness of Science

My fourth theme from cultural studies, what I call the openness of scientific work, conflicts with a widespread sense of scientific communities as relatively self-enclosed, homogeneous, and unengaged with other social groups or cultural practices. Even such an **[End Page 12]** influential and informative precursor to cultural studies of science as Thomas Kuhn's *Structure of Scientific Revolutions* emphasizes the intellectual and normative autonomy and uniformity of scientific communities. The social constructivist tradition has often followed Kuhn in this respect, emphasizing either the social interests or the social interactions that constitute the shared beliefs, values, and concerns of scientific communities. [24] But cultural studies of scientific knowledge display a constant

traffic across the boundaries that allegedly divide scientific communities (and their language and norms) from the rest of the culture. Bruno Latour has provocatively expressed this sense of the openness of scientific work in saying that scientific work itself effectively destabilizes any distinctions between what is inside and outside of science, or between what is scientific and what is social. [25]

It is important to recognize that the traffic across the boundaries erected between science and society is always two-way. For now, I will emphasize the manner in which scientific work continually draws upon and is influenced by the culture "outside." The traffic in this direction involves, among other things, scientists seeking and acquiring material and financial resources, recruits, meaningful or significant questions and problems to investigate, a vocabulary and the metaphors and analogies it incorporates, allies, and mucht more. I want to present multiple examples to illustrate my point, to make plausible the range and depth of the claim that cultural studies make and their justification for it.

My initial case is taken from Robert Friedman. [26] Friedman has shown how important *theoretical* features of Vilhelm Bjerknes's evolving studies of atmospheric geophysics were shaped by specific **[End Page 13]** relationships cultivated with military and civil aviation, fisheries and agriculture. Bjerknes's group replaced the prevailing statistical climatological approach to meteorology with a three-dimensional modeling of atmospheric dynamics. These models emphasized the formation and movement of atmospheric discontinuities (or "fronts")--but this very conception depended initially upon both the needs of and the resources providable by aviation and shipping. Aviation needed much finer-grained and differently conceptualized atmospheric analyses than prevailing meteorological theory could discriminate; at the same time, airplanes and airships were indispensable for acquiring the data to enable a three-dimensional atmospheric geophysics that could reveal rapidly moving and sharply delineated discontinuities. These relationships were indispensable for the imposition throughout Europe and North America of a common instrumentarium and metrology, marked in physically rather than phenomenologically significant units, and temporally synchronized rather than timed for local convenience. Most previously practicing meteorologists did not even comprehend the new units of measurement; yet without these changes, there could be no knowledge of relevant atmospheric features.

High energy physics (HEP) may seem more remote from particular social interests or cultural practices than does meteorology. But cultural/political engagement can make considerable difference in what kind of knowledge can be produced. As Traweek has pointed out, the principal determinant of a HEP group's work is its detector. [27] All accelerator research groups take pulses of particles from the same beam, but what knowledge they produce depends upon the detector they put in its path. In the United States, detectors are short-lived, and they require continual tinkering to keep them at the very edge of the state of the art, without introducing irreducible noise into the data, or excessive expense and time into their work. Experimental physicists build detectors themselves (and rework them), both to minimize noise, and to achieve the precise data response desired. In Japan, by contrast, this approach is impossible: the funding for high-energy physics is tied to its corporations, and physicists only specify general design criteria for a detector, which is then built by industrial firms and cannot be altered on site. As Traweek notes, such highly expensive machines with the most sophisticated components must then be used for a long time. Whereas in the United States a physicist will typically **[End Page 14]** work with several generations of detectors, in Japan a detector will survive through several cohorts of physicists who spend their careers with one machine. These differences importantly affect the kinds of questions that can be asked, as well as the most important characteristics of good results.

My third example comes from historian Donna Haraway. She has documented a sharp transformation in the 1940s and 1950s in the metaphors that organized research and its interpretation in several fields of biology--notably evolutionary theory, genetics, developmental biology, and immunology. Haraway describes the change as "a transformation from a discourse on physiological

organisms, ordered by the hierarchical sexual division of labor and the principle of homeostasis, to a discourse on cybernetic technological systems, ordered by communications engineering principles." [28] Haraway's argument connects both the theoretical and economic resources for these transformations of core fields of biological science to war-related developments in operations research and labor management, and their intellectual plausibility in part to contemporary transformations in the economy and in cultural images of language and self. Such metaphorical structures in science are tremendously important epistemologically, especially because of the ways they shape the development of subsequent research. They help determine what are the interesting questions, and what would be intelligible as an answer to those questions.

The intertwining of scientific knowledge with cultural constructions of sex and gender should be especially emphasized, for it has been very influential in the formation of cultural studies of science. Some engagements of science and gender should by now be unsurprising (although they have certainly not been uncontested!). Could research into endocrinological influences on sex differences in behavior or ability, or evolutionary explanations of gender difference, be expected to escape the effects of cultural constructions of gender? Similarly, when one recognizes the epistemological importance and cultural complexity of researchers' credibility, it would be astonishing if gender were not significant there. I thus choose two more indirect examples to emphasize the theme of the openness of scientific work.

The first comes from Evelyn Keller. Keller's recent historical inquiries have concerned the cultural formation of molecular biology in its peculiarly central place within the biological sciences **[End Page 15]** today. From H. J. Muller's ecstatic analogies between his X-ray induced genetic mutations and Ernest Rutherford's bombardment of atomic nuclei with alpha particles ("Mutation and Transmutation--the two keystones of our rainbow bridges to power!"), [29] to molecular biologists' frequent identification of DNA molecules with "the secret of life," and "the displacement of flesh-and-blood reference that is [thereby] symbolically effected," [30] Keller argues that the representation of the *significance* of molecular biology has been powerfully gendered. She interestingly connects the ways scientists have attempted to legitimate the biological centrality of this work to powerful cultural narratives of male birthing and second birthing. What is at issue here is not the specific role that DNA molecules play in heredity, but the gendered significance of specific research programs in biology in relation to other elements of biological (and physical) science.

A very different sort of example is displayed in a recent discussion by Donna Haraway of the content of *Science*. [31] The meaningful content of this official journal of the AAAS is usually understood to reside in its scientific articles and its letters, news, and commentary--yet almost a quarter of the journal's actual pages, by my count, are typically devoted to advertisements. This fact alone suggests the economic significance of scientific instrumentation. What Haraway has done is to study the imagery developed and exploited in the advertisements to striking effect. From the rabbit at the computer keyboard staring at its graphically constructed image on the screen ("A few words about reproduction from an acknowledged leader in the field"), to the male scientist bottle-feeding a monkey in the lab at midnight, to DuPont's genetically engineered laboratory mouse with active oncogenes ("OncoMouse-TM"), the humor and imagery in the advertisements play subtle and not-so-subtle variations on cultural narratives of gender and birthing, origins and salvation, purity and pollution, nature and culture. These advertisements raise complicated issues about their intended audience and **[End Page 16]** the significance of the imagery they embody--and they remind us that scientific knowledge is more than just the carefully dry prose of the canonical journal report.

Realism and Value--Neutrality: Subverting the Questions

The final two points I want to make about cultural studies are closely connected. Cultural studies take a subversive rather than an vantagonistic stance toward some long-standing philosophical questions about science, such as realism and value-neutrality; they challenge the formulation of the

question rather than proposing an alternative to its traditional answers. This approach is, in turn, importantly connected to the place of epistemological and political criticism within cultural studies of science. Cultural studies endorse neither the global legitimations of science often put forward by philosophers, nor the attempt by many sociologists of science to describe science while bracketing or relativizing any critical assessment of it.

Realism is the view that science (often successfully) aims to provide theories that truthfully represent how the world is--independent of human categories, capacities, and interventions. Social constructivists typically reject realism on two counts: first, the world that science describes is itself socially constituted; and second, its aims in describing that world are socially specifiable (satisfying interests, sustaining institutions and practices, etc.). Cultural studies of science, on the other hand, are better understood as rejecting both realism and the various antirealisms, including social constructivism. [32] Both realists and antirealists propose to explain the content of scientific knowledge, either by its causal connections to real objects, or by the social interactions that fix its content; the shared presumption here is that there is a fixed "content" to be explained. Both scientific realists and antirealists presume *semantic* realism--that is, that there is an already determinate fact of the matter about what our theories, conceptual schemes, or forms of life "say" about the world. Interpretation must come to an end somewhere, they insist, if not in a world of independently real objects, then in a language, conceptual scheme, social context, or culture.

Cultural studies, instead, reject the dualism of scheme and content, **[End Page 17]** or context and content, altogether. There is no determinate scheme or context that can fix the content of utterances, and hence no way to get outside of language. How a theory or practice interprets the world is itself inescapably open to further interpretation, with no authority beyond what gets said by whom, when. [33] This position has at least two important consequences in comparison to social constructivism. First, cultural studies can readily speak of statements as true, for "truth" is a semantic concept that never takes us beyond language: to say that "p is true" says no more (but also no less) than saying "p." Second, this position dissolves the boundaries between cultural studies of science and the scientific practices they study. Cultural studies offer interpretations of scientific practices, including the texts and utterances that such practices frequently articulate--but scientific practices are themselves already engaged in such interpretations, in citing, reiterating, criticizing, or extending past practice. As Arthur Fine suggested,

> if science is a performance, then it is one where the audience and crew play as well. Directions for interpretation are also part of the act. If there are questions and conjectures about the meaning of this or that, or its purpose, then there is room for those in the production too. The script, moreover, is never finished, and no past dialogue can fix future action. Such a performance is not susceptible to a reading or interpretation in any global sense, and it picks out its own interpretations, locally, as it goes along. [34]

Cultural studies' interpretive readings are thus part of the culture of science, and not an explanation or interpretation of it from "outside." The boundaries between science's "inside" and "outside," its centers and its margins, are always themselves at issue in interpretive practice, and not something already fixed. The point is not to place all interpretations on a par, for some count as relevant, serious, and significant while others do not. Rather, it is to say that just which interpretations count in this way, and when, and where, is itself part of what is at stake in ongoing interpretation.

What I earlier called the "openness" of scientific practice is thus crucially relevant here. Internalist history and philosophy of science, and social constructivism, are thus both mistaken when they **[End Page 18]** try to establish once and for all what is relevant to the determination of truth, whether it be reasons and evidence narrowly construed, or "social factors." One cannot separate the determination of the truth of a scientific claim from the heterogeneous considerations that shape it as a truth claim at all, as a claim that is intelligible, significant, bearing a (variable) burden of proof, and relevant to various other practices and claims.

Cultural studies likewise try to subvert questions about whether science is (or should be) value-neutral. Traditional discussions of "the" question of value-neutrality reify the notion of 'value' just as the realism debates attempt to reify truth." [35] Questions about truth inevitably devolve into multiple questions about significance, relevance, intelligibility, or burden of proof. Similarly, Robert Proctor has recently argued, the question of value-neutrality is not one question but many. [36] Proctor's work thereby opens a significant topic for cultural studies of science--namely, to locate historically and culturally the very conception of scientific research and knowledge as value-free.

The prominence of the term "value-free" undoubtedly stems from the influence of Max Weber. Ironically, Proctor has shown us, Weber's principal concern was not to keep values from influencing science, but the reverse: his advocacy of *Wertfreiheit* was a critique of scientism. But other important concerns have been articulated under this same heading. Against the Nazis' advocacy of a racialized and nationalized science, or the Soviet Communist Party's rejection of Mendelian genetics, the notion of value-freedom has been timidly invoked to challenge the political censorship of scientific work (timidly, for it suggests that if science were not fully and rigidly value-free, it might be appropriately subject to censorship). Similarly timidly, the notion of value-freedom has been used to challenge the exclusion of scientists on grounds of gender, race, nationality, or political or religious affiliation. A very different use of the conception of "value-freedom" has been to draw problematic distinctions of pure from applied, or basic from "mission-oriented," **[End Page 19]** research. Of course, those scientists (and their employers) whose work is applied or mission-oriented by any intelligible criterion have not hesitated to appropriate the legitimating notion of value-freedom.

Value-freedom is also attributed to nature as well as science. Here we encounter the modern conception of the "disenchanted" universe, which rejects an ordered cosmos, and the criticisms of vitalism and teleology in biology. This usage is in direct conflict with the frequent use of scientific work to legitimate or discredit values (e.g., the controversies over sociobiology). But what is important for our purposes is that the various conceptions of nature as disenchanted and science as value-free are an important topic for cultural studies, with a rich and contradictory history, and not a framing of its investigations.

Cultural Studies as Politically and Epistemically Engaged

These discussions of the concepts of truth and value lead us to the final issue that I take to characterize cultural studies of science. Sociological constructivists frequently insist that they merely *describe* the ways in which scientific knowledge is socially produced, while bracketing any questions about its epistemic or political worth. In this respect, their work belongs to the tradition that posits value-freedom as a scientific ideal. By contrast, cultural studies of scientific knowledge have a stronger reflexive sense of their own cultural and political engagement, and typically do not eschew epistemic or political criticism. They find normative issues inevitably at stake in both science and cultural studies of science, but see them as arising both locally and reflexively. One cannot not be politically and epistemically engaged.

Two examples of how the burden of proof is determined in AIDS research will illustrate my point, and will reinforce the earlier claim that cultural studies of science are in the end continuous with the reflexive practice of science itself. Paula Treichler and Cindy Patton have both noted that retrovirologists confidently announced that a sequence of RNA that they had isolated was "the AIDS virus" or "the cause of AIDS," long before anything had been established about its detailed role in the clinical development of the disease or about the presence or absence of cofactors. [37] It seems that within **[End Page 20]** the present scientific climate, the burden of proof falls heavily upon the opponents of what Evelyn Keller has called "master molecule" explanations of biological phenomena; therefore, the kind and degree of evidence that they and the proponents of such explanations need to provide for their claims differ accordingly. [38] Similarly, the widespread scientific discussion of the "African origin" of AIDS has, for historically and politically significant

reasons, confronted looser standards of evidence than have other claims about its epidemiology. Treichler's and Patton's arguments in each case are neither uncritical descriptions of how the scientific burden of proof is assigned, nor part of a global relativizing of scientific argument; instead, they offer a detailed *criticism* both of how that burden falls, and of its consequences, via an interpretation of how it was historically constituted. Their argument is not that scientific claims should be rejected for extrascientific reasons, but instead that the local patterns of scientific reasoning and relevance relations need to be reconstructed at specific points.

The critical standpoint afforded by such cultural studies is not that of epistemic sovereignty as inscribed in a "narrative leviathan," [39] which would legislate for science and culture on the basis of its grasp of the right explanatory factors to account for scientific knowledge without residue. Rather, cultural studies are located within ongoing conflicts over knowledge, power, identity, and possibilities for action. Whatever critical insight and effectiveness they may have must result from their responsiveness to the resonances and tensions among what I have called the alignments and counteralignments shaping an epistemic situations. [40] An epistemic alignment is a dynamic and heterogeneous array of practices, objects, and communities or solidarities, which reinforce, appropriate, or extend one another, and thereby constitute knowledge. Cultural studies are *reflexive* attempts to strengthen, transform, or reconstitute existing alignments or counteralignments, by resituating them historically and geographically.

The crucial differences between the normative standpoints of social constructivism and cultural studies of science are succinctly **[End Page 21]** expressed by several of their most prominent practitioners. Trevor Pinch sees "the task for the sociologist [as] to try and recapture some of the 'life world' of the scientist--the taken-for-granted practices and interpretations which make available the natural world." [41] The goal of such a arecapture" is to rearrange the relations of authority among disciplines. As Collins and Yearley put it, "SSK wants to use science to weaken natural science in its relationship to social science.... We want all cultural endeavors to be seen as equal in their scientific potential." [42] It is instructive to contrast such accounts with Donna Haraway's articulation of a vision for cultural studies:

> Feminists have to insist on a better account of the world; it is not enough to show radical historical contingency and modes of construction for everything.... [Sol "our" problem is how to have *simultaneously* an account of radical historical contingency for all knowledge claims and knowing subjects, a critical practice for recognizing our own "semiotic technologies" for making meanings, *and* a no-nonsense commitment to faithful accounts of a "real" world, one that can be partially shared and friendly to earth-wide projects of finite freedom, adequate material abundance, modest meaning in suffering and limited happiness. [43]

To put the difference polemically, social constructivism is antagonistic to the cultural authority claimed by the natural sciences, but uncritical of scientific practices. Cultural studies reverse this stance, aiming to participate in constructing authoritative knowledge of the world by critically engaging with the sciences' practices of making meanings.

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Endnotes

1. Karl Mannheim, *Essays on the Sociology of Knowledge* (London: Routledge and Kegan Paul, 1952).

2. See, for example, Robert Merton, *The Sociology of Science: Theoretical and Empirical Investigations* (Chicago: University of Chicago Press, 1973).

3. J. D. Bernal, *The Social Function of Science* (Cambridge, Mass.: MIT Press, 1967); J.D. Bernal, *Science in History* (Cambridge, Mass.: MIT Press, 1954). For an informative discussion of Bernal's views, see Gary Wersky, *The Visible College* (New York: Holt, Rinehart, and Winston, 1978).

4. Bernal, *Social Function of Science*, p. 409.

5. Michael Polanyi, *Personal Knowledge* (Chicago: University of Chicago Press, 1958).

6. For a useful discussion of the continuity between the social constructivist tradition and Polanyi's antinormative account of scientific research, see Steve Fuller, "Social Epistemology and the Research Agenda of Science Studies," in *Science as Practice and Culture*, ed. Andrew Pickering (Chicago: University of Chicago Press, 1992), pp. 390-428.

7. Harry Collins and Steven Yearley, "Epistemological Chicken," in Pickering, *Science as Practice*, p. 316.

8. For critical discussions of the epistemic significance of policing epistemic borders, see Sharon Traweek, "Border Crossings: Narrative Strategies in Science Studies and among Physicists in Tsukuba Science City, Japan," in Pickering, *Science as Practice*, pp. 429-465; and Joseph Rouse, "Policing Knowledge: Disembodied Policy for Embodied Knowledge," *Inquiry* 34 (1991): 353-364.

9. For a discussion of the role of epistemic sovereignty within science studies, see Joseph Rouse, "Beyond Epistemic Sovereignty," in *Context and Disunity: New Directions in the Philosophy of Science Studies*, ed. Peter Galison and David Stump (Stanford: Stanford University Press, forthcoming).

10. See, for example, Ruth Hubbard, *The Politics of Women's Biology* (New Brunswick, N.J.: Rutgers University Press, 1990); Anne Fausto-Sterling, *Myths of Gender* (New York: Basic Books, 1985); Ruth Bleier, *Science and Gender: A Critique of Biology and Its Theories on Women* (New York: Pergamon Press, 1984); and Lynda Birke, *Women, Feminism, and Biology* (Brighton: Wheatsheaf, 1986).

11. Richard Rorty, "Is Natural Science a Natural Kind?" in idem, *Objectivity, Relativism, and Truth: Philosophical Papers, Vol. I* (Cambridge: Cambridge University Press, 1991), pp. 46-62.

12. Joseph Rouse, *Knowledge and Power: Toward a Political Philosophy of Science* (Ithaca, N.Y.: Cornell University Press, 1987), chap. 6.

13. Nancy Cartwright, *How the Laws of Physics Lie* (Oxford: Oxford University Press, 1983), p. 139.

14. Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of a Scientinc Fact*,

1st ed. (London: Sage, 1979).

15. Harry Collins and Steven Yearley, "Journey into Space," in Pickering, *Science as Practice* (above, n. 6), pp. 382-383.

16. For discussions of these issues, see George Marcus and Michael Fisher, *Anthropology as Cultural Critique* (Chicago: University of Chicago Press, 1986); James Clifford and George Marcus, eds., *Writing Culture* (Berkeley: University of California Press, 1986); and Renato Rosaldo, *Culture and Truth* (Boston: Beacon Press, 1989).

17. Collins and Yearley explicitly portray their own explanatory antagonism toward the cultural hegemony of the natural sciences as like "the underdog so familiar from romantic portrayals of science" ("Journey into Space," p. 382).

18. Such an interpretation of reflexivity as a version of epistemological skepticism and as an apolitical question of rhetoric is exemplified in Steve Woolgar's recent work, both in *Science: The Very Idea* (London: Tavistock, 1988), and in most of the papers that Woolgar edited for *Knowledge and Reflexivity* (London: Sage, 1989). James Bohman, *New Philosophy of Social Science* (Cambridge, Mass.: MIT Press, 1991), recognizes the inadequacy of the skepticism adopted by Woolgar and *some* of the postcolonial anthropologists, but seems to agree with them that a serious reflexive attention to one's own interpretive position must lead to such skepticism.

19. An excellent example exploring how the categories of cultural interpretation of science are themselves deeply engaged with scientific traditions is Sally Humphries, "Serious Stories: Modifications of Narrative in Early Greek Prose," *Science in Context*, forthcoming. Donna Haraway, *Primate Visions* (New York: Routledge, 1989), and Traweek, "Border Crossings" (above, n. 8), both illustrate how reflexive attention to the construction of one's own text can engage the political significance of doing cultural studies of science, rather than just undertaking the futile rhetorical task of representing the supposed "ideology of representation." See also Woolgar, *Science*, chap. 7.

20. For a detailed discussion of the locality and materiality of scientific knowledge, see Rouse, *Knowledge and Power* (above, n. 12), chap. 4; and Rouse, "Policing Knowledge" (above, n. 8).

21. For detailed discussions, see Hans-Jorg Rheinberger, "Experimental Systems: Historiality, Deconstruction, and the 'Epistemic Thing,'" *Science in Context*, forthcoming; and William Bechtel, "Integrating Sciences by Creating New Disciplines: The Case of Cell Biology," *Biology and Philosophy*, forthcoming. The differences between these two papers are themselves salutary for the concerns of the present argument, since Rheinberger's account exemplifies the kind of interpretation of the articulation of meaning that I attribute to cultural studies, whereas Bechtel's paper attempts an interesting mediation between social constructivism and the more traditional history and philosophy of science.

22. Peter Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987), chaps. 2-3.

23. Ian Hacking, *Representing and Intervening* (Cambridge: Cambridge University Press, 1983); Sharon Traweek, *Beamtimes and Lifetimes* (Cambridge, Mass.: Harvard University Press, 1988), esp. chap. 2.

24. Thomas Kuhn, *The Structure of Scientific Revolutions*, 2nd ed., enlarged (Chicago: University of Chicago Press, 1970). Prominent examples of social constructivists whoi emphasize the role of relatively enclosed scientific communities or forms of life include Harry Collins, *Changing Order* (London: Sage, 1985), (especially his notion of a "core set"); and David Bloor, *Wittgenstein: A Social Theory of Knowledge* (New York: Columbia University Press, 1983). Karin Knorr-Cetina, *The Manufacture of Knowledge* (Oxford: Pergamon Press, 1981), with its emphasis upon "trans-

scientific fields," was remarkable for its early divergence from the focus upon scientific communities.

25. Bruno Latour, "Give Me a Laboratory and I Will Raise the World," in *Science Observed*, ed. Karin Knorr-Cetina and Michael Mulkay (Beverly Hills, Calif.: Sage, 1983), pp. 141-170; Bruno Latour, *Science in Action* (Cambridge, Mass.: Harvard University Press, 1987).

26. Robert Marc Friedman, *Appropriating the Weather: Vilhelm Bjerknes and the Construction of a Modern Meteorology* (Ithaca, N.Y.: Cornell University Press, 1989).

27. My discussion of this example is drawn from Traweek, *Beamtimes and Lifetimes* (above, n. 23).

28. Donna Haraway, "The High Cost of Information in Post-World War II Evolutionary Biology," *Philosophical Forum* 13 (1981-82): 245.

29. Quoted in Evelyn Fox Keller, Physics and the Emergence of Molecular Biology,"*Journal of the History of Biology* 23 (1990): 397.

30. Evelyn Fox Keller, "From Secrets of Life to Secrets of Death," in *Body/Politics: Women and the Discourses of Science*, ed. Mary Jacobus et al. (New York: Routledge, 1990), p. 187.

31. Donna Haraway, "The Promises of Monsters: A Regenerative Politics for Inappropriated Others," in *Cultural Studies Now and in the Future*, ed. Paula Treichler et al., forthcoming.

32. Rouse, *Knowledge and Power* (above, n. 12), chap. 5, develops an extended argument rejecting both realism and empiricist or constructivist antirealisms, based upon a minimalist or "deflationary" conception of truth.

33. For further discussion of this account of meaning and truth, see Samuel Wheeler, "Indeterminacy of French Interpretation: Derrida and Davidson," in *Truth and Interpretation*, ed. Ernest Lepore (Oxford: Blackweil, 1986), pp. 477-494; Samuel Wheeler, "True Figures," in *The Interpretive Turn*, ed. David Hiley et al. (Ithaca, N.Y.: Cornell University Press, 1991), pp. 197-217; and Rouse, *Knowledge and Power*, chap. 5.

34. Arthur Fine, *The Shaky Game* (Chicago: University of Chicago Press, 1987), p. 148.

35. Stephen Stich, *The Fragmentation of Reason* (Cambridge, Mass.: MIT Press, 1991), displays a stunning blindness to this parallel. Having powerfully argued that any attempt to fix the intension of "truth" potentially drives a wedge between "'*p*' is true" and the reasons for believing "*p*," Stich settles upon a cultural pluralism about reason by appealing to the values that fix the objects of an individual's or a culture's desires, without recognizing that an exactly parallel argument could be developed to fragment the concept of "value."

36. Robert Proctor, *Value-Free Science? Purity and Power in Modern Knowledge* (Cambridge, Mass.: Harvard University Press, 1991).

37. Paula Treichler, "AIDS, HIV, and the Cultural Construction of Reality," in *Social Analysis in the Time of AIDS: Theory, Method and Action*; ed. Gilbert Herdt and Shirley Lindenbaum (Newbury Park, Calif.: Sage, forthcoming); Cindy Patton, *Inventing AIDS* (New York: Routledge, 1990), esp. chaps. 3-4.

38. Evelyn Fox Keller, *Reflections on Gender and Science* (New Haven: Yale University Press, 1985), chap. 8.

39. I take this phrase from Traweek, "Border Crossings" (above, n. 8).

40. See Joseph Rouse, "The Dynamics of Power and Knowledge in the Sciences," *Journal of Philosophy* 88 (1991): 658-665; and Rouse, "Beyond Epistemic Sovereignty" (above, n. 9).

41. Trevor Pinch, *Confronting Nature* (Dordrecht: D. Reidel, 1986), p. 19.

42. Collins and Yearley, "Journey into Space" (above, n. 15), p. 383.

43. Donna Haraway, *Simians, Cyborgs, and Women* (New York: Routledge, 1991), p. 187.