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Laboratory Studies: The Cultural Approach to the Study of Science

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Chapter 7: Laboratory Studies: The Cultural Approach to the Study of Science

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The Origin of Laboratory Studies

This chapter is about a perspective in recent science and technology studies (STS) that has come to be called “laboratory studies”—the study of science and technology through direct observation and discourse analysis at the root where knowledge is produced, in modern science typically the scientific laboratory. Laboratory studies became feasible in STS when, in the 1970s, the field became more possessive of its subject and more inclusionary—when analysts began to readdress¹ not only the surrounding institutional circumstances of scientific work but the “hard core” itself: its technical content and the production of knowledge. Touching the hard core, however, required a special methodological handle. It was not only necessary to give up the belief that science was the very paradigm of rationality—a belief put into question by philosophers/historians of science such as Kuhn (1962/1970) and Feyerabend (1975). One also needed to gain access to the technical content of science through channels other than those of accepted scientific “facts” and theories—for once knowledge has “set” (once it is accepted as true), it is as hard to unravel as concrete. The methodological handles deployed were the study of scientific controversies and the study of unfinished knowledge.

[p. 141 ↓] The study of controversies became the methodological focus of a sociology of scientific knowledge, which developed in the early 1970s and resulted in a thoroughgoing sociological contextualization of science (see Bloor, 1976);² it examined, for example, how internal scientific standards and experimental evidence

fail to provide for scientists' beliefs (e.g., H. Collins, 1975, 1981a) and how the beliefs and knowledge claims of scientists are influenced by their social context (e.g., Barnes, 1977; MacKenzie, 1981; Pickering, 1984).³ Unfinished knowledge—the knowledge that is yet in the process of being constituted—on the other hand, became the province of laboratory studies.⁴ The real-time processes through which scientists, one of the most powerful and esoteric tribes in the modern world, arrive at the goods that continuously change and enhance our “scientific” and “technological” society are still hardly understood. In fact, these processes had not been systematically investigated by social analysts until the mid-1970s, when the first students of laboratories began their investigations.⁵ Philosophers of science, who until then were the authority on matters of scientific procedure and content, showed a preference for “the context of justification” and treated the context of knowledge production, which they called “the context of discovery,” with neglect and disdain.⁶ Historians often defined issues of scientific content as questions in the history of ideas detached from local settings. To be sure, both groups also enriched their accounts with studies of crucial experiments, but experiments, as we shall see, are not laboratories. The whole process of knowledge production in the contemporary fact factory of the natural sciences, and the role of the fact factory itself, were, in the 1970s, untrodden territory in social studies of science.

Laboratory studies have turned this territory into a new field of exploration. The method used in laboratory studies, ethnography (participant observation) with discourse analysis components, has become something of a contemporary equivalent of the historical case study method that became popular in the wake of Kuhn. Ethnography furnished the optics for viewing the process of knowledge production as “constructive” rather than descriptive; in other words, for viewing it as constitutive of the reality knowledge was said to “represent.” Constructionism is one of the major, perhaps the major, outcome of laboratory studies (see Sismondo, 1993); the origin of its emphatic use in STS lies in the attempt of students of laboratories to come to grips with their observational record of the “made” and accomplished character of technical effects. Another outgrowth of laboratory studies is the increased usage of ethnography in STS. Laboratory studies have stimulated studies of scientific work that are based on participant observation and now blend with the wider field of “ethnography of science and technology,” which extends the ethnographic approach to the study of significant

developments in whole fields and even to science policy (Cambrosio, Limoges, & Pronovost, [p. 142 ↓] 1990). Finally, there is one component of laboratory studies that needs to be mentioned specifically, and this is the notion of a laboratory itself. The laboratory is not just, I shall argue, a long-underexplored site of investigation newly “conquered by” students of science and technology. It is also a theoretical notion in an emergent theory of the types of productive locales for which laboratories stand in science. In sociology in general, localizing concepts are often associated with the small scale and the weak. Laboratory studies shed light on the power of locales in modern institutions and raise questions about the status of “the local” in modern society in general.

What is the theoretical power of the notion of a lab? How is it different from the older notion of experiment on which historians and philosophers of science placed a premium? What do laboratory studies mean when they claim that scientific facts are “constructed” in these settings? In this chapter, I shall seek an answer to these questions by first spelling out a notion of the laboratory that captures the laboratory's theoretical significance—I shall argue that this significance is linked to the reconfiguration of the natural and social order that in my opinion constitutes a laboratory (section 3). My second focus in this chapter will be to address the meaning of constructionism in laboratory studies (section 4) and to extract, from these studies, those elements and processes that demonstrate how facts are constructed (section 5). In this context, I will also summarize studies of scientific work and ethnographies of science and technology. In the last part of the chapter, I shall discuss some criticism of laboratory studies and needs for further studies of contemporary and historical science (section 6).

Laboratories are Distinct from Experiments and Organizations

How is the study of laboratories different from the study of experiments or from the sociology of organizations applied to facilities in science and technology? Both the study of experiments and the sociology of organizations have long been established and have constituted something of a tradition in science studies. Consider first experiments, which

have until recently carried much of the epistemological burden in explaining the validity of scientific results and rational belief in science. They provided the frameworks within which “the scientific method” was deployed and bore fruit. They were the units in terms of which science proceeded empirically step by step, the rungs in a ladder of theory testing and empirical verification. Experiments were largely defined methodologically in earlier studies; notions like the testing of theories, experimental design, blind and double-blind procedure, control [p. 143 ↓] group, factor isolation, and replication are all linked to experiments. The advantages attributed to experiments include the fact that they disentangle variables and test each variable by itself, that they compare the results with those of a control group, that they avoid experimenter bias and subjective expectations, and that their results can be justified through replications that “anyone” can check or perform. With this methodological definition of experiments in place, the real-time processes of experimentation in different fields remained largely unexamined (Gooding, Pinch, & Schaffer, 1989).⁷

When the first laboratory studies turned to the notion of a laboratory, they opened up a new field of investigation not covered by the methodology of experimentation. For them the notion of a laboratory played a role that the notion of experiment, given its methodological entrenchment, could not fulfill; it shifted the focus away from methodology and toward the study of the *cultural* activity of science. In many ways, the notion of a scientific laboratory began to stand for what to the history and methodology of science has been the notion of experiment. The laboratory allowed social students of science to consider the technical activities of science within the wider context of equipment and symbolic practices within which they are embedded—without at the same time reverting to a perspective that ignores the technical content of scientific work. In other words, the study of laboratories has brought to the fore the full spectrum of activities involved in the production of knowledge. It showed that scientific objects are not only “technically” manufactured in laboratories but also inextricably *symbolically* and *politically construed*. For example, they are construed through literary techniques of persuasion that one finds embodied in scientific papers, through the political stratagems of scientists in forming alliances and mobilizing resources, or through the selections and decision translation that “build” scientific findings from within.⁸ An implication of this has been the awareness that, in reaching its goals, research *intervenes*, to use Hacking's term,⁹ not only in the natural world but also—and deeply—in the social world.

Another implication is that the products of science themselves have come to be seen as cultural entities rather than as natural givens “discovered” by science. If the practices observed in laboratories were “cultural” in the sense that they could not be reduced to the application of methodological rules, the “facts” that were the consequence of these practices also had to be seen as shaped by culture.

If we now compare laboratory studies with earlier studies in the sociology of scientific and technical organizations, a similar change in perspective is apparent. Unlike traditional studies of scientific experiment, studies in the sociology of scientific and technical organizations had not concerned themselves with the technical content of the disciplines involved. Their topics were the classical questions of organizational structure and performance as [p. 144 ↓] a precondition for, and as possibly conducive to, scientific and technical achievements (see, e.g., Kornhauser, 1962; Pelz & Andrews, 1966). They were not how scientific facts are themselves produced in these settings or how the major mechanisms in the process of knowledge production can be described. Laboratory studies, to be sure, do not exclude organizational variables; for example, questions of resources, of communication within settings, or of linkages between organizational units reoccur in almost all laboratory studies. However, these questions play a role not with respect to a formally defined “organizational output” but as part of the cultural apparatus of knowledge production that becomes visible in laboratories. The shift in focus is significant in two respects: It suggests that isolating organizational structure variables and performance variables and studying them across organizations may not be sufficient to learn about constitutive aspects of organizational work, when this work involves the deep processing of complex information.¹⁰ Second, it suggests that the power and productivity of organizations may reside not only with the general organizational structures they adopt but also with the specific differences they institute in organizational practice with respect to other organizations and with respect to their environment in general (see below).

The Laboratory as a Theoretical Notion: The Reconfiguration of Objects and Subjects

The laboratory has served as the place in which the separate concerns of methodology and other areas such as organizational sociology could be seen as dissolved in cultural practices that were neither methodological nor social organizational but something else that needed to be conceptualized and that encompassed an abundance of activities and aspects that social studies of science had not previously concerned themselves with. The significance of the notion of a laboratory lies not only in the fact that it has opened up this field of investigation and offered a cultural framework for plowing this field. It lies also in the fact that the laboratory itself has become a theoretical notion in our understanding of science. According to this perspective, the laboratory is itself an important agent of scientific development. In relevant studies, the laboratory is the locus of mechanisms and processes that can be taken to account for the “success” of science. Characteristically, these mechanisms and processes are nonmethodological and mundane. They appear to have not much to do with a special scientific logic of procedure, with rationality, or with what is generally meant by “validation.” The hallmark of these mechanisms and processes is that they imply, to use Merleau-Ponty's terminology, [p. 145 ↓] a *reconfiguration of the system of “self-others-things,”* of the “*phenomenal field*” in which experience is made in science.¹¹ As a consequence of these reconfigurations, the structure of symmetrical relationships that obtains between the social order and a natural order, between actors and environments, is changed. To be sure, it is changed only temporarily and within the walls of the laboratory. But it appears to be changed in ways that yield epistemic profit for science.

What do I mean by the reconfiguration of the system of “self-others-things” and how does this reconfiguration come about? The system of “self-others-things” for Merleau-Ponty is not the objective world independent of human actors or the inner world of subjective impressions but the world-experienced-by or the world-related-to agents. What laboratory studies suggest is that the laboratory is a means of changing the

world-related-to-agents in ways that allow scientists to capitalize on their human constraints and sociocultural restrictions. The laboratory is an “enhanced” environment that “improves upon” the natural order as experienced in everyday life¹² in relation to the social order. How does this “improvement” come about? Laboratory studies suggest that it rests upon the *malleability* of natural objects. Laboratories use the phenomenon that objects are not fixed entities that have to be taken “as they are” or left to themselves. In fact, laboratories rarely work with objects as they occur in nature. Rather, they work with object images or with their visual, auditory, electrical, and so on traces, with their components, their extractions, their “purified” versions. Take the transition from agricultural science, a field science, to biotechnology described as a process of replacement by Busch, Lacy, Burkhardt, and Lacy (1991). Through the transition from whole plants grown in fields to cell cultures raised in the laboratory, the processes of interest become miniaturized and accelerated. Clearly, the growth of cells in a cell culture dish is faster than the growth of whole plants in the field. Moreover, these processes become independent of seasonal and weather conditions. As a consequence, natural order time scales are surrendered to social order time scales—they are sub- ject mainly to the limitations of work organization and technology. Astronomy provides another illustration: Through the switch to an imaging technology, to digitalization, and to computer networks (Lynch, 1991; Smith & Tatarewicz, 1985), astronomy has become a laboratory science though it is *not* an experimental science.

There are at least three features of natural objects that a laboratory science does not need to accommodate: First, it does not need to put up with the object *as it is*; it can substitute all of its less literal or partial versions, as illustrated above. Second, it does not need to accommodate the natural object *where it is*, anchored in a natural environment. Laboratory sciences bring objects “*home*” and manipulate them “on their own terms” in the laboratory.

[p. 146 ↓] Third, a laboratory science does not need to accommodate an event *when it happens*; it does not need to put up with natural cycles of occurrence but can try to make them happen frequently enough for continuous study. Laboratories allow for some kind of “homing in” of natural processes; the processes are “brought home” and made subject only to the local conditions of the social order. The power of the laboratory (but of course also its restrictions) resides precisely in its exclusion of nature as it is

independent of laboratories and in its “enculturation” of natural objects. The laboratory subjects natural conditions to a “*social overhaul*” and derives epistemic effects from the new situation.

But laboratories not only “improve upon” the natural order; they also “*upgrade*” the *social order* in the laboratory, in a sense that has been neglected in the literature on laboratories. Traditionally, the social has been seen as external to the conduct of science—something to be brought into the picture only to explain incorrect scientific results (Bloor, 1976, p. 14). Laboratory studies and other approaches in the new sociology of science have eliminated this asymmetry—they have found it surprisingly easy to explain much of what goes on in knowledge production in terms of social factors, and stressed the inextricable linkages between the objects produced in science and the social world. Yet my point here is different; it refers to the fact that the reconfiguration model also extends to the social order. If we see laboratory processes as processes that “align” the natural order with the social order by creating reconfigured, “workable” objects in relation to agents of a given time and place, we also have to see how laboratories install “reconfigured” scientists who become “workable” (feasible) in relation to these objects. In the laboratory, it is not “the scientist” who is the counterpart of these objects. Rather, it is agents enhanced in various ways so as to “fit” a particular emerging order of self-other-things, a particular “ethnomethodology” of a phenomenal field. Not only objects but also scientists are malleable with respect to a spectrum of behavioral possibilities. Moreover, it is not at all clear that these scientists must remain stable individual entities that are separated out from other objects in the laboratory. Certain of their features may be coextensive with those of objects; they may be construed as “coupled” to objects and machines, or they may “disappear,” as individual players, in epistemic collectives that match the objects in the laboratory. In the laboratory, scientists are, on the one hand, “methods” of going about inquiry; they are part of a field's research strategy and a technical device in the production of knowledge. But they are also, on the other hand, human materials *structured into* ongoing activities in conjunction with other materials with which they form new kinds of entities and agents.¹³ Recently, Latour has asked that we consider not only how scientists construe nature but also how they “co-construe” society as part of their enterprise—for example, by inserting [p. 147 ↓] into it the products of their work (1988) and by defining the nature, ontology, and limits of social actors (1989). Laboratory studies show how the

“constructors” themselves are reconfigured, not as a result of the political strategies of specific agents but as the outgrowth of specific forms of practice.

Constructionism and Laboratory Studies

In the above, I have drawn upon bits and pieces from existing laboratory studies—the idea of the laboratory as an entity that brings epistemic dividends can also be found in Latour (e.g., 1987); the reconfiguration idea comes from Knorr Cetina (1992a, in press); the interest in symbolic construction is best represented in Traweek (1988). Nonetheless, I have so far neglected the diversity of existing laboratory studies, which do not share a single model of science. In fact, the five major book-length monographs on laboratories available in print display as many viewpoints and approaches to science studies as there are authors: Latour's and Woolgar's *Laboratory Life* of 1979 has evolved, in Latour's collaboration with Michel Callon, into a semiotically inspired actor-network approach (e.g., Callon, 1986b; Latour, 1988); Knorr Cetina presents, in *The Manufacture of Knowledge* (1981), a constructivist approach oriented toward the sociology of knowledge that is extended into a model of epistemic cultures (1991, in press); Michael Lynch's work *Art and Artifact in Laboratory Science* (1985a) can stand for the ethnomethodological orientation; and Traweek's monograph *Beamtimes and Lifetimes* (1988) represents the analysis of a symbolic anthropologist who enters the world of high-energy physics. Nonetheless, some threads run through more than one study; some orientations, like that of constructionism, have raised most comments from the outside¹⁴ and have developed into fully fledged perspectives in their own right. Others, like the inspiration some laboratory studies drew from semiotics, rhetoric, and the metaphor of society as behavioral text, have led to specific literary models of how facts are constructed; still others, like the ethnomethodological concern with the fine-grained analysis of daily practices, have reinforced the interest in detailed description of scientific work. Consider first constructionism in laboratory studies.

What is Constructionism?

Constructionism holds reality not to be given but constructed: It sees the whole as assembled, the uniform as heterogenous, the smooth and even surfaced as covering an internal structure. There are, for constructionism, no [p. 148 ↓] initial, undissimulatable “facts”: neither the domination of workers by capitalists, nor scientific objectivity, nor reality itself. What generates this lack of deference for the solid entities in our systems of belief? Within laboratory studies, the insistence on direct observation and detailed description has consistently served as a device that calls forth and sustains the constructionist attitude. One of the first laboratory studies used as an epigraph a sentence from Dorothy L. Sayers: “My Lord, facts are like cows. If you look them in the face hard enough, they generally run away” (Knorr Cetina, 1981, p. 1). Detailed description deconstructs—not out of an interest in critique but because it cannot but observe the intricate labor that goes into the creation of a solid entity, the countless nonsolid ingredients from which it derives, the confusion and negotiation that often lie at its origin, and the continued necessity of stabilizing and congealing. Constructionist studies have revealed the ordinary working of things that are black-boxed as “objective” facts and “given” entities, and they have uncovered the mundane processes behind systems that appear monolithic, awe inspiring, inevitable. The deconstruction performed by constructionist studies is neither negative nor a turn toward “mere description”; rather, it is a turn away from the method of intuiting reasons for the so-called progress of science and toward the method of observing the real-time mechanisms at work in knowledge production. If these mechanisms are considered in sufficient detail, some form of constructionism ensues, whether one wishes for it or not. Constructionism was the answer laboratory studies gave to the microprocesses they observed in realtime episodes of scientific work.

Constructionist studies disassemble by multiplying—they multiply the players, the events, and the mechanisms associated with sustaining entities such as scientific facts. For example, they go from the fact of TRF (thyrotropin-releasing factor) to the agents implicated, the alliances mobilized, and the strategies involved in the making of this fact (Latour & Woolgar, 1979). Do they also disassemble the material world to which scientific “findings” refer? Yes, if we mean by this the real-world entities represented by

scientific descriptions. No, if we mean the existence of a material reality, or the realtime intervention in and causal interaction with this world. Constructionism as exemplified in the first laboratory studies is neither nihilism nor skepticism, nor a doctrine that reduces objects to something like imputed and subjective meanings. Constructionist studies have recognized that the material world offers resistances; that facts are not made by pronouncing them to be facts but by being intricately constructed against the resistances of the natural (and social!) order. What constructionism departed from, however, is the idea that the laws and propositions of science provide literal descriptions of material reality, *and hence can be accounted for in terms of this reality* rather than in terms of the mechanisms and processes of construction.

[p. 149 ↓] Constructionism did not argue the absence of material reality from scientific activities; it just asked that “reality,” or “nature,” be considered as entities continually retranscribed from within scientific and other activities. The focus of interest, for constructionism, is the process of transcription.

Distinctive Features of Constructionism in Laboratory Studies

One root of constructionism surely lies in the idea that the world of our experience is structured in terms of human categories and concepts; for Kant, the basic categories of the human mind did the structuring; for Whorf and ethnoscientists, it was language and culture seen through language; and for recent microanalysts, it is often meanings infused in negotiations and in definitions of situations. A second root of constructionism can be seen in the idea that the world is created through human labor. Applied to human institutions, this notion is encapsulated in Marx's famous phrase that “men make their own history,” even though, as Marx added, they do not make it free of constraints. Constructionism's recent history in sociology in general lies more with the phenomenological tradition; it is brought into focus in Berger and Luckmann's book *The Social Construction of Reality* (1967), which rekindled the interest of sociologists in constructionist ideas. Berger and Luckmann's main question—how it is that we experience social institutions as “natural” and unchangeable when they are, after all, created by society and consist of social action and social knowledge—reoccurs in

STS, most succinctly perhaps in inquiries into the solid, monolithic, and awe-inspiring character of technological systems (MacKenzie, 1990a). But on the whole, the answers given and the thrust of constructionist arguments in STS are different.¹⁵ Constructionist ideas were reinvented in studies of scientific laboratories rather than imported into them from phenomenology. There are several distinct uses and implications of these arguments that I take to be the following:

First, the construction metaphor takes on a much stronger flavor when it is applied to natural reality and to science. To say that kinship relations or gender relations are socially constructed and hence cannot be attributed to nature manifesting itself in these relations today hardly raises an eyebrow. But to make the same claim for scientific facts is still strongly contentious (Cole, 1991; Giere, 1988) and leads to endless arguments of the kind raised above about the constructionist understanding of the material world (e.g., Sismondo, 1993). When Pinch and Bijker (1984) asked that the same principles and methods that guide constructionist studies of science be applied to technology, little controversy ensued about whether technological artifacts could be seen as constructed. It seemed clear that they could; the term **[p. 150 ↓] construction**, Hamlin (1992) says in a recent discussion of technology studies, “would seem more at home in technology rather than in science” (p. 513). Thus constructionist ideas appear radical only with respect to natural reality, and with respect to our difficulty of granting two things simultaneously: the material world's independent existence and structure, and our locally successful¹⁶ implementation of a world retranscribed from within science that may not be coextensive with the former. Constructionist arguments in studies of science do not settle such issues philosophically. It is rather that they carry them from one arena of discussion to another, from the arena of philosophical argument to that of empirical examination. A second characteristic of the respective studies surely lies in the specific project that results from this move, the project of an empirical epistemology of science and the social world. Such a project considers, for example, which relations to “nature” and material reality are implemented in different locations, what “material reality” means in the context of the respective work, how particular epistemic regimes implicate and manifest themselves in visual, literary, and other technologies, how technological complexes like detectors “mediate” nature and objectivity, and so on. For constructionist perspectives in the study of scientific work, unlike for constructionism in recent sociology, philosophical questions about the nature of knowing provide a

resource. But the answer to the questions is sought elsewhere, in the study of the real-time processes of knowledge production.

The third characteristic of constructionism as it emerged from laboratory studies is the emphasis placed in the respective studies upon the phenomenon that knowledge is worked out, accomplished, and implemented through practical activities that transform material entities and potentially also features of the social world. This makes these studies continuous with the above conception of construction as the creation of the world through “labor” (Marx), and suggests a notion of practice that includes, but is not coextensive with, representations. One needs to emphasize that, in the face of recent, postmodernist epistemologies that see the world as constituted in terms of signifier chains and the deconstruction of science a part of a more general critique of representations (e.g., P. M. Rosenau, 1992, chap. 7).¹⁷ Construction has not been specified in laboratory studies primarily in terms of linguistic, cognitive, or conceptual events. Finally, constructionist studies introduced, with the notion of a laboratory, a concept that suggests that, in modern society, at least some world constructions, those of natural science, occur localized in specific settings, in the “workshops” (Heidegger) and “topical contextures” (Lynch, 1991) described in these studies. Relocating the idea of construction into particular physical and epistemic spaces meant shifting the focus away from particular actors, the prominent individual scientists of the past and present who continue to populate historical and contemporary accounts of [p. 151 ↓] science.¹⁸ In many areas of empirical natural science, the individual scientist is no longer the epistemic subject, and the role of individuals (or groups), and their status as human processors or workers fitted into a configuration of materials, needs to be reassessed from the vantage point of the settings in which they operate. The emphasis on the “bounded spaces” of world construction leads us back to what was said about laboratories earlier. But it also brings into focus a methodological principle that can guide laboratory studies but distinguishes itself from that of “following the actors” that Latour (1987) proposes for the actor-network approach in STS and that informs many historical and biographical studies of science. If construction is wrapped up in bounded locales, the ethnographer needs to “penetrate the spaces” and the stream of practices from which fact construction arises. In the next section, I want to address what the studies that have followed this principle have to say about how scientific results are constructed.

How are Facts Constructed?

Nothing Epistemically Special is Happening

The thrust of the original laboratory studies was that they attempted to show how natural scientific facts are constructed. They considered the making of scientific knowledge open to social science analysis and proceeded to observe the creation of knowledge at the workbench and in notebooks, in scientific shop talk, in the writing of scientific papers. One immediate result of all laboratory studies was that nothing epistemologically special was happening in these instances. In Rorty's (1985) formulation, "no interesting epistemological difference" could be identified between the pursuit of knowledge and, for example, the pursuit of power. The emphasis here is on the epistemological; laboratory studies did not show that there were no interesting *sociological* differences between molecular biology bench work and a courtroom trial, or between the building of a detector and trading at the stock exchange. The experience that nothing epistemologically special was happening wiped out the doubts that had accompanied analysts' first steps into the lab; the making of knowledge was indeed amenable to empirical analysis, and more so than anyone had expected before.

Construction as Negotiation and Interactional Accomplishment

What was the second major result? Presumably that almost everything is negotiable in the making of scientific knowledge: what is a microglia cell [p. 152 ↓] and what is an artifact (Lynch, 1982, 1985a, chaps. 4, 8), who is a good scientist and what is an appropriate method (Latour & Woolgar, 1979, pp. 161 ff.), whether one measurement is sufficient or whether one needs to have several replications (Knorr Cetina, 1981, chap. 2.2), what one sees on an autoradiograph film and what one does not see (Amann & Knorr Cetina, 1989), what is the best environment for good physics (Traweek, 1988, chap. 5) and what counts as a proper experimental replication (H. Collins, 1985, chaps.

2–3). As the last reference shows, not only laboratory studies but empirical studies of scientific work in general have demonstrated the negotiability of the elements, the outcomes, and the procedures in knowledge production. The possibility of negotiation in consensus formation is rendered plausible by the observation, on the part of these studies, that scientific outcomes are empirically “underdetermined” by the evidence in the sense that they frequently do not have univocal outcomes;¹⁹ in fact, experimental outcomes are often opaque, murky, ambiguous, and generally in need of interpretation and further experimentation. They are also made plausible by the discovery, in studies of scientific controversy and scientists’ discourse, that scientific findings and scientific accounts are frequently contentious and meet with more than one interpretation (H. Collins, 1981a; Gilbert & Mulkay, 1984). Uncertainty affects not only scientific outcomes and their interpretation but also the process of investigation itself—as shown, among other things, in a recent wave of studies of scientific work undertaken by symbolic interactionists (e.g., Clarke, 1987, 1990a; Fujimura, 1987, 1988, 1992b; Gerson, 1983; Star, 1983, 1985, 1986, 1989a; Star & Griesemer, 1989). For example, Fujimura (1987, 1988, p. 263) analyzes how “doable” problems are constructed in a process marked by uncertainty and ambiguity that scientists cope with through “articulation work,” for example, through negotiating tasks with funding agencies and others. Thus uncertainties and “interpretative flexibilities” open up the possibility for negotiation, and the resistance someone or something offers also creates and reinforces the openness of the process. Constructionist studies draw no line between these occasions. For them it is enough to show that the construction of facts includes recurrent elements of negotiation and that the straightforward process of putting “questions to nature” is interspersed with situations in which nature does not speak, or does not speak clearly and unambiguously enough to prevent contestation.

Who are the parties involved in these negotiations? Certainly they are scientists and groups of scientists, but also funding agencies, suppliers of equipment and materials,²⁰ clients, investors, Congress and ministries of science,²¹ and so on. From the beginning of laboratory studies, it has been clear that external agents play a role in these negotiations—hence the notion that the “decision impregnatedness” of scientific results is anchored in “trans” epistemic arenas of research or “trans” scientific fields (Knorr Cetina, 1981, [p. 153 ↓] chap. 4, 1982, 1988a). Later studies (Fujimura, 1987, 1988, 1992b) and especially studies of scientific technologies have also forcefully

argued that technical, social, economic, and political groups take part in the definition of scientific and technological developments, and thereby exemplify the flexibility in the way technologies are designed (e.g., Henderson, 1991a; Hughes, 1989b). Callon (1986b) and Latour (e.g., 1990), in their general argument for an analysis of the coproduction of society and nature, propose that we also include nonhuman actors as negotiating parties in our analysis. Nonhuman actors are, for example, the microbes, the scallops, or the acid rain investigated by science but also a door and an automatic door closer. Nonhuman agents include the world of things to which agency (“actant”ship²²) is attributed on behalf of the constraints they issue upon human behavior (a door allows us to walk through only in a particular place), which is itself a consequence of the work, and power, that we delegate to them.²³ Collins and Yearley (1992a) criticized this extension of agency to things and the consideration of nonhuman entities as full parties to processes of negotiation—for example, it can be questioned in what sense our understanding of the making of knowledge is improved at all if we simply call any interaction with nonhuman agents a negotiation. But my point here is different; extensions such as the above show the degree to which the idea of construction as negotiation has penetrated all parts of the analysis of science and technology, even that of instrumental action (human action oriented toward things), which is redefined to mean negotiation with the entities to which it is directed.

What does negotiation mean when it refers to actors in the social world? It is unfortunate that most constructionist studies to date have failed to analyze the patterns and processes that turn an ordinary interaction into a “negotiation.” We know that “what counts as a notable finding, a definitive anatomical entity, a thing's attributes, a procedure of measurement, an adequate display of data, and a plan of methodic action” may be asserted and modified in an interactionally sensitive manner (Lynch, 1985a, p. 264), but we know little about the rules and mechanisms that govern these modifications. An early attempt in this direction was made by the above author (1985a, chap. 7), who illustrates how scientists change their descriptions or accounts of scientific and technical objects in the face of expressions of disagreement by others. The “preference for agreement” Lynch discerns in the changes of descriptions accords well with ethnomethodological findings on conversations in general (e.g., Pomerantz, 1975), but their bearing is specific; conversational devices may be implicated in the production of scientific knowledge not just as passive vehicles of communication

through which features inherent in natural objects are made explicit but as coproducers of these features. This is also the thrust of a series of articles Amann and Knorr Cetina (1988a, 1989; Knorr Cetina & Amann, 1990) published on the patterns and [p. 154 ↓] uses of specific conversational routines when scientists “think through talk” and when they perform analyses of technical images through shoptalk. Accordingly, conversational routines are part of an interactional machinery that produces emergent outcomes not identical with the contributions of individual participants and not reducible to “objective” features of objects. In other contexts, for example, in Traweek's study of high-energy physics laboratories, *negotiation* refers to practitioners' bids for equipment and access to beam time, to conflicts over which experiment is to be accepted and performed or to how to set up the best decision structures for such matters (Traweek, 1988, chap. 5).

Most meanings of the concept of negotiation bring into focus the *interactional* element in episodes of knowledge production: They show how the process and outcome of inquiry are sensitive to the process and outcome of social interaction. In that sense, “negotiation,” more than other concepts, highlights the “*social*” character of the process of knowledge production. However, there are also other concepts with slightly different connotations that have been used to illuminate the idea of construction. One is the notion of *accomplishment* preferred by ethnomethodological studies of scientific work (e.g., Garfinkel, Lynch, & Livingston, 1981; Livingston, 1986; Lynch, 1985a). It includes the idea of shop work involving technical objects and instruments, and can be seen to encompass the kind of maneuverings practitioners engage in when dealing with anomalies and contradiction, for example, in the creation of a successful theory (Star, 1989a). Another is the notion of “*decision translation*” and of the “*decision impregnatedness*” of scientific results. This covers the results of negotiation but also includes selection processes that result from individual or structural and implicit selections (Knorr Cetina, 1981, 1982, 1988). How does a scientist decide to make a particular technical decision? By translating a choice into other choices. The point about these translations is that they often implicate nonepistemic²⁴ arguments and show how scientists continually crisscross the border between considerations that are in their view “scientific” and “nonscientific.”

Construction as Literary Construction and Representational Craft

Processes of construction are often rhetorical processes; they involve representational techniques of persuasion, which have been investigated with respect to scientific arguments and papers. Early statements of the question of the literary rhetoric of science were provided by Gusfield (1976) and Mullins (1977). They are succeeded today by an abundance of analyses of scientists' written discourse covered in detail elsewhere in this volume, and by a broadening of the notion of "style" as a means of expression (see [p. 155 ↓] Daston & Otte, 1991). Within laboratory studies, Latour and Woolgar (1979, chap. 6) described the larger process of stabilizing facts as an agonistic process in which "modalities" (modifiers of statements of fact that mark the degree of fact-likeness) are constantly added, dropped, inverted, or changed but in which the overall process is one of modality dropping. They also pointed out the significance of "inscription devices," "items of apparatus or particular configurations of such items which can transform a material substance into a figure or diagram" (Latour & Woolgar, 1979, p. 51). Following E. Eisenstein's (1979) arguments with respect to printed materials, one can identify the advantages of such inscriptions; inscriptions, especially printed inscriptions, can be more easily circulated, compared, and combined than the material objects of laboratory work. Inscriptions are, in Latour's terminology (1987, p. 227), "immutable and combinable mobiles,"²⁵ which, as Henderson (1992) claims with regard to visual representations, can also serve as a social glue between participants.

Other laboratory studies have pointed out a number of literary techniques of objectification that abstract away from interactional and other qualities of shop work in the laboratory. These include the use of the passive voice instead of the "I" or the "we" of the lab, the elimination of most if not all laboratory rationales for technical choices, strict sequencing techniques that reverse the sequence of events in the lab and make no reference to circular connections between stages of shop work, simplifications and extreme typifications of the experimental process that hide the idiosyncracies and the "know-how" of laboratory work, and the disembedding of the work with respect to its

strategic components and motivational dynamics and its reembedding in a context of “grand” scientific and practical questions from which the work appears to flow (Knorr Cetina, 1981, chaps. 5, 6).

Questions of representational techniques were also addressed with respect to image design and processing (Amann & Knorr Cetina, 1988a; Henderson, 1991a; Hirschauer, 1991; Knorr Cetina & Amann, 1990) and can be exemplified by Lynch's (1988) work on visualization in the life sciences and Lynch and Edgerton's (1988) study of representational craft in contemporary astronomy. The study finds that an “ancient aesthetic” of perfecting nature through a crafting of resemblances is part of routine image processing work. Lynch (1991) sees technologies such as “opticism” and “digitalism” in image processing embodied in “topical contextures,” by which he means the “spatiality of [a] situation bound to a technological complex.” This notion takes us back to the idea of a laboratory as a space within which certain epistemic possibilities are bound up, by suggesting that techniques of perfecting nature associated, for example, with optics may also be bound up and derive from certain local arrangements and contextual spaces.

[p. 156 ↓] Literary and rhetorical construction is construction through shifts of meanings and structure in texts similar to shifts of meaning in oral conversation. Inscription devices add to this by rendering the natural science laboratory into a workshop of text production. “Graphic” construction through the montaging of images elaborates further means of persuasion and shows how stylistic considerations inform the rendering of “realistic” images. However, the respective analyses to date have one drawback: They do not address the question of how scientific texts and technical images are decoded by an audience, and therefore cannot say how a given literary or graphic construction actually affects technical decisions. Because everyone within an audience of experts knows how statements, figures, and graphs are subject to techniques of representation, these entities are also easily and readily deconstructed, as discussions of scientific papers in laboratories demonstrate. While studies of scientific work occasionally make reference to such discussions, we are still lacking a systematic analysis of the deconstruction practitioners themselves routinely perform on images and texts. In other words, while we do have an elaborate picture of the scientist as author and writer, we lack a systematic analysis of the scientist as a reader.

Construction as Local Construction: The Reversals of Practice

There is another sense of construction that warrants special attention within the contexts of laboratory studies. This is that construction appears to be, always, local construction. As Rouse (1987) put it, paraphrasing Heidegger, "Science must be understood as a concerned dwelling in the midst of a work-world ready-to-hand, rather than a decontextualized cognition of isolated things" (p. 108). Reconfigurations of self-other things are local reconfigurations. The power of laboratories, as implied before, is the power of locales. But construction also means construction *with* local means and resources, with the equipment that stands around, the chemicals available, the technical skills and experience offered on the spot. There are numerous examples for this "opportunism" of inquiry, for its dependence and reliance upon local materials, the substitution of apparatus and chemicals for other apparatus and chemicals that are not in stock, the choice of one type of research animal rather than another depending upon the setting, the emergence of an "idea" suggested by local episodes or materials, and so on. Different analysts employ different terms to refer to the implied contingency; for example, Lynch, Livingston, and Garfinkel (1983, p. 212) report on the "embodied," "circumstantially contingent," and "unwitting" character of laboratory practices; Latour and Woolgar (1979, p. 239) refer to "circumstances" as that which stands around and becomes relevant in shop work; and Knorr (1977; Knorr Cetina, [p. 157 ↓] 1981, chap. 2) refers to the "indexicalities" manifest in "local idiosyncracies" and to emergent outcomes, variable rules and power, and the opportunism of research. Even philosophers who spend some time "watching" in a laboratory confirm this contingency (Giere, 1988, chap. 5). While there also are "standard" procedures that "work" successfully in many laboratories (see, e.g., the standardization of DNA technology described by Fujimura, 1988, 1992b), laboratory studies investigate how the successful working of a standard procedure is built out of painful processes of adaptation and learning that "fit" techniques to settings, and scientists to their methods. Furthermore, they examine how standard procedures appear to be standard only within specific contexts in which they are treated as a black box relative to other, "problematic" techniques, and how these black boxes nonetheless present persistent problems in the

laboratory associated with variations in apparatus and materials (Amann, 1990; Jordan & Lynch, 1992).

Results on the *local* character of shop work fly in the face of received interpretations according to which claims and procedures in science are standardized and universal, and according to which the local environment is merely incidental to the generation of particular results. On the other hand, they are not really surprising in the face of the *reversals* of rules and general characteristics through the definition and dynamics of situations that we know from other settings²⁶ and through the reconfigurations laboratories breed and rely on. With respect to science, the finding is complicated by the fact that, within the old logic, if the particular specifications of a laboratory must be taken into account in interpreting its results, then it becomes possible to challenge these results as artifacts: Its properties are qualities of the setting rather than of the natural objects themselves (Rouse, 1987, p. 71). Because it would be difficult to argue that all the local choices implicated in fact construction are “nothing but” insignificant variations without impact on the properties of the results obtained, a new logic is needed. Constructionist studies suggest that results do not become weaker but more solid and interesting through local specifications.²⁷ Local configurations breed the specific advantages and opportunities that, when they are structured into a scientific object, may make it more successful in the wider context. In this sense laboratories are like environmental niches (Vinck, 1991).

Construction Machineries and Cultures of Fact Construction

Studies of the constructionist work of science have not until recently used a comparative approach; they ignored the potential *disunity* of science with respect to its epistemic strategies as well as the *cultural structure* of scientific methodology. On the other hand, the comparative approach has been used [p. 158 ↓] very fruitfully by Traweek (1988, 1992) to investigate cultural differences between nations (the United States and Japan) with respect to laboratory organization, approaches to detector design and building, leadership style, and models of good working conditions for the science she studied—

high-energy physics. Thus the American “sports team” approach in which the leader is like a “coach” who has learned to locate highly skilled players and to design strategies for winning is contrasted with the Japanese “household” approach in which it is the responsibility of each member to keep the household and its resources intact and in which status is not determined by competition but by age (Traweek, 1988, p. 149, chap. 5). Other differences refer to the strong dependence of Japanese physicists on industry for building their detector, compared with the American physicists' preference for arranging (and later dismantling) their pieces of equipment on their own. Traweek (1988, chap. 3) also analyzes eminent physicists' stories about themselves, which define the virtues that are, in the different career stages of a physicist's life, associated with success.

Whereas Traweek's object of interest is less the making of physics than the making of (male) physics cultures, Knorr Cetina (1991, in press) recently used the same method in a first attempt to look not at the construction of facts but at the construction of the machineries deployed in fact construction. The machineries of fact construction include the skilful scientist, as investigated, for example, in studies of skills as social relational properties acquired and used in interaction (Amann, 1990; Hétu, 1989; compare Pinch & Collins, 1992). But they also include ontologies of organisms and machines that result from the reconfiguration of self-other-things implemented in different fields, the use of “liminal” and referent epistemologies in dealing with natural objects and their resistances, strategies of putting sociality to work through the erasure of the individual epistemic subject and the creation of social “superorganisms” in its place, or the use of equipment as “transitional” objects (Knorr Cetina, in press). A comparison between experimental high-energy physics and molecular biology shows that the meaning of the empirical changes as these machineries are implemented in different fields. In other words, it shows the *disunity* of the sciences with respect to construction mechanisms and the existence of *epistemic cultures*.

The containment of construction processes within epistemic (and national) cultures has consequences for the kind of results constructionist studies produced. Do we really find the same kind of negotiation in all experimental sciences? Are the basic categories we have developed (e.g., “contingencies,” “translation,” “decision impregnatedness,” “actors,” “networks”) and the processes we have described equally salient in all fields? From a micro-empirical perspective, it is necessary to pay more attention to how these

categories and processes are themselves dependent upon local practices. Models [p. 159 ↓] that generalize across all fields and disciplines are premature at best. The move toward considering construction machineries rather than single practices can point out a variety of models instantiated in different contexts; it multiplies the stories about how facts are constructed.

Construction “with” Laboratories and the Construction of Accepted Knowledge

Up to this point we have considered mainly construction processes within the laboratory. However, some of the concepts introduced to characterize inquiry in these settings such as negotiation and translation can also be deployed to relationships between entities in larger settings, for example, to relationships between laboratories or between entities that operate in different “social worlds” (e.g., Clarke, 1990a; Fujimura, 1992b; Star & Griesemer, 1989; Zeldenrust, 1985). Such larger settings are explored in Downey's (in press-a) ethnography of computer-aided design technologies, which, as Downey argues, reposition people and groups with vested interests in these technologies and renegotiate the institutional boundaries between universities, industry, and government. Cambrioso and Keating's (1991) ethnographic study of the establishment of the international classification of white blood cells displays a similar concern with nonlocal issues. With respect to such issues, construction often means the construction of an audience willing to believe in the knowledge produced by laboratories, willing to implement its results, and willing to link up with scientific “findings” by, in one way or another, reproducing them in further research and discussion. Latour (1983, 1988) and Callon (1980b, 1991; Callon & Law, 1982) have used the concept of translation to refer to this process. How do I persuade someone to accept my proposal, my method, my invention? By convincing them that it is in their interest to adopt my offer, by redefining (“translating”) their interests such that they converge with mine, and thereby by “enrolling” a heterogeneous crowd of “actors” (in the sense of actants; Greimas & Courtés, 1979, p. 3) into a network of associations that stabilize the technical object (Latour, 1983). The notion of translation has been redefined over time to mean not just interest translation but the “definition” of an agent.

“A translates B,” according to Callon (1991, p. 143), “is to say that A defines B.” With decision translations (see section 5.2 above), construction points at the selections, the local contingencies, and the “nonepistemic” considerations that are *structured into* a scientific object. With Callon and Latour’s translations, the strong interpretation is that the establishment of a scientific or technical object depends not on the *inherent* usefulness or “truthfulness” of the result but on whether one succeeds in building a structure of associations between parties enrolled through mutual definitions that hold up.²⁸ However, the network [p. 160 ↓] includes the scientific object and associated technical components, and these objects must not give way if the whole system of associations is not to break down. Thus construction through network building by means of translation already presupposes and relies on “cooperating” technical objects such as microbes.

In an interesting twist to the argument, Latour (1983, 1988) in his work on Pasteur has claimed that the process of translation may also involve changing society through colonizing the world with extramural laboratories. In other words, recruiting an audience may mean reconstituting, in relevant places, the conditions that obtain in the laboratory and that sustain the “reproducibility” of scientific results. According to Latour, Pasteur changed the conditions at French farms in such a way that his vaccination procedure became reproducible in these settings. In addition, he also changed French society in a more general way—by adding to it the microbe he had discovered in the laboratory as a mediating agent in food processes and in the transmission of diseases and as a mediating agent in social relations in which processes of hygiene and infection are embedded. Thus the view upon the laboratory afforded in such studies is a view from the outside; the laboratory is itself seen as an agent of change, a device in the shaping and construction of society. It serves as a locus for the “definition” of society, for its dissolution into elements and contributing features. This view of construction as the social construction of society *with* the help of laboratories would seem to be most suitable for examining technology or scientific objects oriented toward practical applications. As Gooday (1992, p. 1) has argued using historical data, the extramural laboratory thesis has only limited explanatory power when it is applied to other disciplines practiced contemporaneously with Pasteur’s work.

Criticisms of Laboratory Studies and Prospects for Further Research

Laboratory studies and studies of scientific work have met with several criticisms. One issue consistently raised with respect to studies that make strong constructionist claims is the question of what status these studies accord to material objects. Many students of laboratory have claimed that scientific reality is “constituted by” the process of inquiry, that it is a “consequence rather than a cause” of scientific descriptions, or that scientific facts are “inseparable from the courses of inquiry which produce them” (Knorr Cetina, 1981, p. 3; Latour & Woolgar, 1979, pp. 180–183; Lynch, 1985a, pp. 1 ff.; Woolgar, 1988b). These claims are frequently objected to on the [p. 161 ↓] grounds that they suggest that material objects are snapped into existence through the accounts produced by science, which most readers find “wildly implausible” (e.g., Giere, 1988, pp. 56 ff.; Sismondo, 1993).

However, there is a more sympathetic reading of these claims according to which what does indeed come into existence, within, usually, a longer term process, when science “discovers” a microbe or a subatomic particle, is a specific entity distinguished from other entities (other microbes, other particles) and furnished with a name, a set of descriptors, and a set of techniques in terms of which it can be produced and handled. In other words, some part of a preexisting material world becomes specified and thereby real as something to be reckoned with, accounted for, and inserted in manifold ways into scientific and everyday life. This does not preclude the possibility that some physical correlate of this entity existed, unidentified, tangled up with other materials, before scientists turned their attention to this object. But what is *not* suggested is that science merely furnishes the conception for preexisting physical objects marked off in exactly the ways science later describes from other objects. Perhaps one could say that the “facts” produced by science provide for the encounterability of material objects within forms of life. But this encounterability of course remains problematic, sensitive to circumstances, and potentially dependent upon the reconfiguration of the rest of a form of life as the difficulties with knowledge transfer and the “diffusion of innovation” demonstrate. It should also be noticed that this encounterability, when it is projected

into the past, requires work—for example, the work of historians who rewrite history.²⁹ Thus the question of how a physical entity comes into existence is indeed an interesting one raised by constructionist studies. But it is not a question that will be answered productively by an ontological sleight of hand, such as by assuming that specified material entities reside outside science and outside human experience.

There are other criticisms—most notably, perhaps, the criticism that laboratory studies, while well suited for the microstudy of scientists' day-to-day practices, are limited when it comes to the study of consensus formation (e.g., Pinch, 1986, p. 30).³⁰ This assessment is correct. Laboratory studies to date have mostly focused upon single laboratories or a few work situations. Many processes of consensus formation would seem to involve more than one laboratory, and potentially a whole scientific field. On the other hand, laboratory studies can shed light on the typical responses new knowledge claims encounter in scientific practice and on the circumstances under which such claims appear to be incorporated into further research. They can also shed light on how closure of debates over the interpretation of experimental results is reached within the laboratory, before scientific knowledge is published. If interpretative flexibility and the negotiation of experimental outcomes are an irremediable part of scientific work, as laboratory studies [p. 162 ↓] claim, the question of how negotiations end ought to be answerable by studies of this work. However, few students of laboratories have addressed these issues to date,³¹ and those that have, such as Latour (1988) in his reformulation of the question as one of the “stabilization” of facts, have resorted to historical data.

Consider a third major criticism that is often voiced against laboratory studies. This is that these studies and studies of scientific work narrow their focus to the intramural lifeworlds encountered in laboratories but ignore the societal context in which laboratories operate as well as the political aspects of science (e.g., Chubin, 1992; Fuller, 1992). This critique, too, is warranted; but it would seem to be important mainly with respect to those aspects of the wider context and those relational properties of laboratories that recur in scientific work and influence the construction of knowledge. Laboratory studies profess to be interested in the making of knowledge, and one can hardly blame them if they do not, at the same time, profess an interest in matters such

as science policy—except of course where science policy penetrates into laboratory culture or in some other way orients the outcome of knowledge production.

The above criticism nonetheless suggests areas of research into which laboratory studies need to be extended, and indeed are beginning to be extended—consider, for example, work that applies the laboratory studies' ethnographic approach to such politically sensitive issues as Canadian bio-safety regulations (Charvolin, Limoges, & Cambrosio, 1991) or to the travel of government “dossiers” along a path of institutional modifications within the framework of the Quebec government devising policy measures (Cambrosio et al., 1990, p. 206). Or consider Proctor's (in press) usage of the construction metaphor to study the social construction of ignorance with respect to the causes of cancer.

In addition, there is another area into which an extension of laboratory studies is sorely needed, and that is the area of the history of laboratories. Here, too, studies are beginning to emerge (e.g., Gooday, 1991, 1992; Hessenbruch, 1992; Kingsland, 1992; Shapin, 1988b), and historians have become aware of the need—and the methodological possibilities—for an investigation of the emergence, architecture, internal procedures, and different definitions of laboratories.³² Historical studies of laboratories are joined by historical case studies of experimentation (e.g., Galison & Assmus, 1989; Gooding, 1989; MacKenzie, 1989; Pickering, 1989; see Gooding et al., 1989). These studies manifest a strong recognition of the need to investigate the real-time processes of experimental work and continue and reinforce a trend initiated when earlier controversy studies turned to examining experimental episodes (e.g., H. Collins, 1975; Collins & Pinch, 1982b; Pinch, 1986; Shapin & Schaffer, 1985).

[p. 163 ↓] Finally, a most interesting area of future extension would be one in which we move from science and technology studies to sociology in general and consider the possibility that laboratories exemplify features also present in organized settings such as the clinic, the factory, the garden, the government agency. Such questions would bring laboratory studies in contact with the “new institutionalism” in sociology (e.g., Douglas, 1986a; Powell & DiMaggio, 1991), a direction of research that unpacks institutional arrangements in modern society by adopting a cultural and symbolic approach. Work beginning in the area³³ suggests that processes of laboratorization

can be found in a broad variety of settings and that it might be useful to distinguish between types of laboratories and patterns of laboratory formation. Modern society, after all, is a society of locales. What we learned from laboratory studies about the “situatedness” of knowledge (see also Haraway, 1991b) may be applicable to larger questions about the localization of experience in multiply embedded and varied sites. What different patterns emerge when we consider these sites in regard to their epistemic relations to an environment? How is the power of locales embodied in the scientific laboratory reflected in the power of other demarcated spaces? Localizing concepts have been of considerable importance to microsociological research in the last decades. Yet theoretical formulations of the relevance of locales are still sorely missing. The laboratory as studied within STS might help in focusing the many issues implied when we talk about situatedness and localization, and aid in producing the above theoretical formulation.

Notes

1. I say “readdress” because there have been more inclusionary studies before, for example, Merton's (1938/1970) early work on science in seventeenth-century England.
2. During controversies, knowledge is deconstructed by practitioners themselves, and, as it comes apart, analysts can examine the functioning of the standards that are normally thought to hold it together and the contextual influences that inform the opponents and their work.
3. At the time, this influence was specified in terms of an interest model from which some sociologists of knowledge such as Pickering and Shapin have moved away in recent years. See, for example, Pickering and Stephanides (e.g., 1992) and Shapin and Schaffer (1985). For a critique of the notion of interests in science studies, see Woolgar (1981).
4. There is of course overlap between the two developments in that studies of controversies usually also concern knowledge claims that are “unsettled” and in this sense unfinished knowledge. The overlap is most apparent in studies that examine

controversial experimental evidence and standards such as the aforementioned studies by Collins or the ones by Collins and Pinch (1982a, 1982b) and Pinch (1986).

5. I know of only one study from before 1977 that can qualify as a laboratory study. This is a study by the physicist and theologian Thill (1972) whose work was financed by a group of progressive Catholics not involved in academia. This work gives valuable technical insights in [p. 164 ↓] the methods of physicists but does not address the issues central to social studies of science. The first “sociological” ethnographer of science to begin a study of scientific practice seems to have been Michael Lynch, who started his work in 1974 (see Lynch, 1985a, 1991, p. 51). The studies of Latour and Knorr Cetina were conducted between 1975 and 1977, like Lynch's study in California (Knorr, 1977; Knorr Cetina, 1981; Latour & Woolgar, 1979). Traweek also started her work in the late 1970s (see Traweek, 1988).

6. See Nickles (1988) for comments on these tendencies and for more recent changes in the philosophy of science.

7. This holds until the late 1970s and early 1980s, when the above-mentioned controversy studies turned to the investigation of the role of experimental evidence and of standards of experimentation; see Collins (1975), Travis (1981), Harvey (1981), and Collins and Pinch (1982b). The situation has changed in the wake of laboratory studies in the sense that there is now a new interest in all matters of relevance in real-time processes of experimentation (see, e.g., the studies collected in Gooding et al., 1989). See also the historical study by Shapin and Schaffer (1985) and section 6 of this chapter.

8. The laboratory studies that argued these points most forcefully are by Latour and Woolgar (1979), Knorr (1977), Knorr Cetina (1981), Zenzen and Restivo (1982), and Lynch (1985a). For an illustration of the political nature of science, see also Shapin (1979) and Wade (1981). For a more anthropological study of scientific laboratories, see Traweek (1988).

9. Hacking (1983) draws a distinction between experiments that “intervene” and scientific theories that “represent.” This distinction, however, does not give adequate

weight to the instrumental usage of theories in experimentation or to the fact that some experiments, as we shall see later, focus upon representation rather than intervention.

10. It may be enough when the content of the work is limited to digitalizable routine manipulations of the kind described by H. Collins (1990/1992).

11. Merleau-Ponty's (1945/1962) original notion in the French version of his book is "le system 'Moi-Autruil-les choses'" (p. 69). For the English translation and the exposition of this concept, see Merleau-Ponty (1945/1962, chap. 5 and p. 57). For a different implementation of the notion of a phenomenal field in recent ethnomethodological work, see the articles by Garfinkel, Bjelic, and Lynch in Watson and Seiler (1992).

12. This point needs to be distinguished from the ordinary language of scientists, which often preserves a distinction between the more or less "natural"—for example, between the "wild type" mouse that represents the "natural" mouse—and other types. However, the "wild type" mouse is as much a product of special breeding laboratories that supply mice to scientists as other laboratory animals; not only could it not survive in the wild, but the lab could not use real mice captured in the woods because of the "nonstandardized" biological variations and diseases these animals carry. Similarly, in vitro and in vivo are distinctions of the laboratory that refer to differences in degrees of intervention or to manipulations performed on plants and animals versus manipulations of test tube substances, and so on. In no case we encountered can "in vivo" be seen as an enclave of nature preserved in the lab. However, my point here is not that such enclaves cannot exist in laboratories or, phrased differently, that everything must be transformed at all times when it enters a lab. It is rather that the notion of a laboratory can be linked to the pursuit and dispute of central transformations and that the efficacy of the laboratory in modern science can be clarified if we consider these transformations. Note also that this notion does not furnish the laboratory with a strict boundary or require that the laboratory is a physical place.

13. For illustrations of such reconfigurations in experimental high-energy physics and molecular biology, see Knorr Cetina (in press, chaps. 3–5); for the production of reconfigurations in surgery, see Hirschauer (1991); for medicine in general, see Lachmund (1994).

14. Commentators who stand for many others are Giere (1988) and Cole (1991). For a general review, see Zuckerman (1988b).

[p. 165 ↓] 15. Berger and Luckmann (1967) referred to processes of institutionalization, typification through language, and legitimation to account for the experience of social institutions as solid and “natural.”

16. Whether the project of science is globally successful in the long run is at best contentious today.

17. Within science studies, deconstructionism informs perspectives such as “reflexivity” (Woolgar & Ashmore, 1988) and discourse analysis (Fuhrman & Oehler, 1986; Gilbert & Mulkay, 1984; Mulkay, Potter, & Yearley, 1983). See the respective chapter on discourse analysis in this volume.

18. Some students of laboratories, in particular Latour, retained a strong interest in actors, an interest that is codified in the “actor-network” approach (e.g., Latour, 1987).

19. This experience of empirical underdetermination is to be distinguished from logical underdetermination (e.g., Grünbaum, 1960).

20. This would seem to be more so in fields like experimental high-energy physics that depend heavily on external supplies of parts for the building of their detectors. The way a detector is built is influenced by what parts become available at what cost. The detector, on the other hand, determines together with other elements in the experiment (e.g., the energy and luminosity supplied by a collider) what “physics” will be seen as a result of the experiment.

21. Again, this is more relevant to areas of “big” science such as space research or experimental high-energy physics, where the equipment depends on the amount of financing negotiated with Congress and similar government agencies, and where these negotiations proceed without intermediaries such as funding agencies.

22. The term *actant* is borrowed from semiotics, where it includes nonhuman actors, only requiring that these “participate in a process” (see Greimas & Courtés, 1979, p. 3).

23. For example, the work people would have to perform if there were no doors, according to Latour, is the work of breaking a hole in a wall and then rebuilding the wall (Johnson [Latour], 1988).

24. I consider “epistemic” arguments those that, in the eyes of participants, promote the “truth” of the results.

25. Inscriptions, of course, are not really immutable. As with any statement or instruction, what matters is the meaning of these entities and how they are handled in practice. Because meanings change and are reconstructed between contexts, it is only the physical appearance of inscriptions that remains stable.

26. For a summary of these effects of local practices, see Knorr Cetina (1987).

27. In fact, from a Wittgensteinian perspective, it is hardly surprising that these specifications are needed if technical effects are to be made to work at all. I am referring here to Wittgenstein's treatment of rules and other instructions as receiving their meaning from a constant flow of practice and from the specifications embedded in this practice (1976).

28. The actual picture Callon (1991, p. 143) presents is more complicated because, to avoid idealism, definitions are seen to be inscribed in “intermediaries,” which may be material objects.

29. Consider, for example, the various attempts to explain the cause of a historical person's death, which may reflect new potential causes of diseases “discovered” by science and then projected onto the past.

30. Models of consensus formation in science have been somewhat of a deferred agenda of many controversy studies—deferred because the first wave of these studies focused upon demonstrating how consensus in science cannot be explained, that is, by simply seeing it as an automatic consequence of the persuasiveness of experimental evidence or of the rationality of scientists (e.g., H. Collins, 1981a).

31. One paper that distinguished several types of consensus formation was published in German (Knorr Cetina & Amann, 1992).

[p. 166 ↓] 32. As an example for this trend, consider the 1992 joint meetings of the BSHS, HSS, and CSHS societies in Toronto (July 26–28), which for the first time addressed the topic of laboratories from a historical perspective.

33. See also studies that begin to look at “laboratories” in fields like sexual research (Hirschauer, 1992) or astronomy (Gauthier, 1991), in which the notion of an intramural laboratory has to be replaced by an arrangement of participating localities. See also the applicability of the present notion of a laboratory to the development of the clinic in medical fields (Lachmund, 1994).

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