Untangling Context: Understanding a University Laboratory in the Commercial World

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The past twenty years have been an incredibly productive period in science studies. Still, because recent work in science studies puts a spotlight on agency and enabling situations, many practitioners in the field ignore, underplay, or dismiss the possibility that historically established, structurally stable attributes of the world may systemically shape practice at the laboratory level. This article questions this general position. Drawing on data from a participant observation study of a university biology laboratory, it describes five features of the institutional landscape that shape this laboratory's practice.

The past twenty years have been an incredibly productive period in science and technology studies. Most fundamentally, recent research has helped challenge idealized images of science as a distinctive asocial realm in which strict adherence to formal rules allows scientists to "read" nature to explain the operation of physical and biological systems. Such an idealized image has been replaced by a picture of science-as-craft in which practical reasoners act in local contexts in often highly contingent ways.[1]

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Despite its vitality, much current work in science studies focuses on agency (cf. Latour 1987, 1988) and enabling situations (cf. Fujimura 1987, 1988, 1992; Star and Griesemer 1989), and as a result, it tends to ignore, underplay, or dismiss the possibility that historically established, structurally stable attributes of the world may systemically shape laboratory practice. To ascertain whether structural context within which laboratories are located is influential, we need to pay explicit conceptual and empirical attention to its possible efficacy.

In this article, I start where many previous science studies scholars have set their gaze—in the laboratory—and like many of them, I draw on the techniques of ethnography. However, unlike those working in the ethnomethodological (Lynch 1985), social interactionist (Fujimura 1987, 1988, 1992, 1996; Clarke 1991; Star and Griesemer 1989), and semiological (Latour 1983, 1987, 1988; Callon 1986, 1995) traditions, I am interested in the ways in which the structural or institutional context in which a lab is situated shapes the practices within it. Although such concerns have motivated a good deal of work in the history of science (cf. Kloppenburg 1988; Kohler 1990; Leslie 1993; Noble 1984), as contemporary university science becomes increasingly woven into a commercial web, it is crucial to carry such an approach to the level of the laboratory.

Theory and Method

The conceptual orientation adopted in this article is in many ways a reaction to two approaches to the social studies of science and technology that have had profound influences on the field: actor network theory and the social worlds approach. Actors—human and nonhuman—and boundaries, distinctions, and dichotomies are central to the actor network approach. We are advised to follow the actors wherever they lead and to see the world from their perspective. With this starting point, advocates of this orientation focus their attention on construction. They deem it inappropriate to make any a priori assumptions about the distinction between context and content, inside and outside, science and society, arenas and actors (Latour 1987). Similarly, actors—their identities and interests—are seen as the product of constituting processes (Cambrosio, Limoges, and Pronovost 1991; Wynne 1992). Generally, proponents of this approach present agent-centered analyses in which actors operate in highly manipulable worlds subject to minimal constraint (Moore 1995). Although the focus tends to be on actors' constitutive activities—their active building roles—analysts stress that causality (although they would likely not want to use the term) is not unidirectional. Instead, either the world is a seamless web (Shapin 1988) in which evaluating effects and causal direction is impossible or, as in Law and Callon's later work, elements of the world—in particular, networks and actors—mutually shape one another (Law and Callon 1992, 25). Finally, enrollment is a central concept in Latour's actor network analyses, and perhaps the best way to enroll others is to offer them something that will enable them to reach their goal in such a way that their actions will advance your goal. Enrollment involves the translation of the other's interest into your terms, and in the best case, you make yourself "indispensable" to the efforts of others (Latour 1987).

Law and Callon have modified and clarified this framework in recent years. In line with the prohibition against assuming distinctions, they claim that it is too simple "to say that context influences, and is simultaneously influenced by, content" (Law and Callon 1992, 21). They propose the notions of "global network" and "local network" as alternatives. A global network, as Law and Callon define it,

> is a set of relations between an actor and its neighbors on the one hand, and between those neighbors on the other. It is a network that is built up, deliberately or otherwise, and that generates a space, a period of time, and a set of resources in which innovation may take place. Within this space . . . the process of building a project may be treated as the elaboration of a *local network*—that is, the development of an array of the heterogeneous set of bits and pieces that is necessary to the successful production of any working device. (pp. 21-22, emphasis added)

Law and Callon (1992, 22, emphasis added) argue further that the notions of content and context may be transcended "if projects are treated as *balancing acts* in which heterogeneous elements from both 'inside' and 'outside' the project are juxtaposed."[2]

Law has devoted greater attention to conceptualizing power than other actor network theorists. According to Law, power is a function of the network of relations in which an actor is implicated. From this perspective, power can be treated as a condition or a set of conditions but must also be understood as an effect. Moreover, power is a product of "more or less precariously structured relations" (Law 1991b, 170), and power is itself precarious (Law 1991b, 177).

The social worlds approach has much in common with actor network theory. Like research in the actor network tradition, the work of social worlds proponents is, to a significant degree, agency centered and emphasizes construction. In her recent book, for example, Fujimura (1996, 2, emphasis added) describes her work as an exploration of the "representational, organizational, and rhetorical work done by researchers, students, sponsors, and audiences *to create* the 'world' of proto-oncogene research." In addition, analyses do not consider unidirectional causes but emphasize instead interactions between different worlds and "co-construction" (Fujimura 1996, 11). Like actor network approaches, social worlds analysts advise us against assuming distinctions between context and content, outside and inside. Clarke and Fujimura (1992) suggest that the realm of the laboratory is inseparably linked to the other "worlds." They call on us to think about all of the factors outside of the lab as part of the "situation itself" rather than as "context." In many instances, "the world is in the laboratory and the laboratory is in the world" (Fujimura 1996, 11).

Like some work in the actor network tradition, emphasis in the social worlds approach is placed on the mutual benefits of relations. Attention is focused on the ways in which "standardized packages" (Fujimura 1987, 1988, 1992, 1996) and "boundary objects" (Star and Griesemer 1989) make possible cooperation between social worlds, not on the ways in which some worlds are capable of shaping the practices of others.

In her recent work, Fujimura (1996) explicitly conceptualizes power. She suggests that power and authority are "distributed among different actors, objects, and social worlds" (p. 17). Citing Gramsci, Fujimura declares further that "the process of standardizing technologies is a process of establishing *hegemony* . . . over other ways of knowing" (p. 72). Hegemony and power are attributes of technologies, "paradigms, and ultimately standardized packages" (pp. 153).

Despite having enlivened the field and appropriately cautioned against reductionism, actor network and social worlds research suffer from related methodological and conceptual shortcomings. First and most basically, emphasis on agency has led analysts to ignore or underplay the constraints placed on agents in their efforts to act.[3] At a methodological level, restricting analysis to the world as seen by actors may, as Law (1991a, 11) notes, lead us to ignore "distributions [of resources, for example,] . . . that are of no concern to the actor being followed." We may, furthermore, overlook institutional constraints to which actors are not particularly attentive.

A second limitation of actor network and social worlds approaches is their virtually exclusive emphasis on processes of construction. Active agents are always constructing their worlds in this work. But emphasis on construction turns our gaze away from the effects of the already constructed attributes of the world in which science is practiced. Furthermore, focus on coconstruction, interactions, and dialectical relations suggests that it is analytically impossible to isolate unidirectional causal relations. Analysts may thereby overlook structural effects.

A final weakness in the actor network and social worlds traditions is rooted in their conceptions of power. In keeping with the centrality of agency, both approaches stress the mutuality of relations. Such emphasis, however, ignores the possibility that the benefits of relations may be asymmetrical. In the actor network approach, translation implies a kind of ultimate compatibility of interests in which benefits of alliance are mutual. Such a portrait ignores the possibility that enrolled actors may benefit less than the enrollers.[4] Indeed, careful work in the history of science and technology (Kloppenburg 1988; Leslie 1993; Noble 1984) has shown that power inequalities affect outcomes and that not all relations in the world of technoscience are mutually beneficial for all actors.

Fujimura's (1996) use of the term hegemony in her discussion of power is particularly inadequate. Gramsci (1971), of course, defined the term in many different ways. Common usage draws attention to the notion that domination may occur with the consent of subordinate actors. This may occur because the realization of "real" interests of subordinate groups becomes inextricably linked to the realization of dominant group interests, or the worldview of the dominant group may become the worldview of society as a whole. Indeed, central to Gramsci's formulation is a view of the world in which relations of domination and subordination are a pivotal feature. Such relations are largely absent from Fujimura's usage.

In my view, Law's (1991b) recent work marks an appropriate recasting of the concept of power in science and technology studies. However, his emphasis still diverts attention from the effects of power and inequality. Stressing the precarious character of power relations, Law is concerned with "*how it is that relations are stabilised for long enough to generate the effects and so the conditions of power*" (Law 1991b, 172). In other words, Law is interested in the constitution or construction of power, not in its effects. Finally, it is not clear to me that the assumption that power relations are always precarious is an appropriate position with which to begin an analysis. Across history, such relations seem reasonably stable over time.

In this article, I adopt a structural, organizational, or institutional perspective (cf. Epstein 1996; Kleinman 1995; Moore 1996; Webster 1994; Wright 1994). However they are named, structures can be seen to constitute specific formal and informal, explicit and implicit "rules of play," which establish distinctive resource distributions, capacities, and incapacities and define specific constraints and opportunities for actors depending on their structural location (Lindberg 1982). Power and its operation are then understood within this structural context. The rules of play that define structures give certain actors advantage over others by endowing them with valued resources or indeed by serving as resources themselves. In the latter case, enforcement of rules may privilege certain actors over others. Here, power can be said to exist in a number of senses. In the simplest, behaviorist sense, power can be said to have been exercised when the structural location of one actor enables him or her to shape the action(s) of another actor in ways in which the latter might not otherwise act. In the more complicated case, following Lukes (1974, 21, 22), "the bias of the system is not sustained simply by a series of individually chosen acts, but also, most importantly by the socially structured and culturally patterned behaviour of groups and practices of institutions." Such bias can "unevenly [distribute] influence, access, and control" (Wright 1994, 11). In the first instance, agents can be said to have power, although the basis of this power is organizational. In the second instance, power is directly an attribute of structures and need not be enacted to have effects.

Advocates of actor network theory and the social worlds approach might assert that the framework I present here reifies structures, portraying "these as pre-existing, fixed and sovereign—rather than themselves only partially formed, indeterminate and open to construction through processes of negotiation" (Wynne 1992, 577). Ironically, for analysts who wish not to assume distinctions, this kind of response sets up a dichotomy: structures, actors, interests, and identities must be viewed as either always in the process of construction and reconstruction, or analysts must accept them as already existing when actors arrive on the scene. In the latter case, analysts are accused of reifying the world they are studying. But this is not an either/or issue; a third position is both possible and cogent: structures, interests, actors, and identities are constructed historically, but at any given time, these phenomena have an established character, and this configuration has effects. In the case of structures, human and organizational actors are likely to confront them as "external" and sometimes "constraining."[5]

Indeed, I do not deny that these structures are constructed but argue only that at any given point, actors will confront structures that are already constructed and that these structures will shape practices. If one must always focus analysis on the construction process, then one can never explore the effects of already constituted structures. Furthermore, if we assume that entities such as "content" and "context" or the "micro" and the "macro" exist in dialectical relationship at every instant—if they are always in the process of co-construction—then we cannot explore how one might shape the other. I do not dispute that such relationships can be reciprocal over time, and I do not wish to imply a world in which certain actors always lack agency. However, in this article, I take a synchronic look at one laboratory and focus not on how the laboratory shapes the environment in which it is embedded but on how the environment shapes the laboratory.

If structures, although constructed, have histories, then it is legitimate to begin one's analysis assuming the existence of particular structures. This might be viewed as a pragmatic move (Wynne 1992, 579), offering a starting place and enabling one to initiate investigation. This is the approach I take here. I entered the laboratory knowing that this lab had relations with university administration, with for-profit companies with which the laboratory collaborates, and with commercial suppliers of research materials. I knew, furthermore, that matters of intellectual property were of concern to the lab leader and lab members. I did not know what these factors meant for lab practices or how they affected laboratory life. Finding this out was the aim of my project.

In this essay, I assume that university laboratories are situated within a complex institutional matrix. Although all aspects of this configuration are subject to change, at any given point these structures are stable and can put pressure on laboratory practice. Given this baseline assumption, my approach might be viewed as an ethnography in which my attention is focused on what Michael Burawoy (1991, 271) refers to as "the 'macro' determinations of everyday life." Like Burawoy, I am interested in understanding the ways in which "the situation is shaped from above rather than constructed from below" (Burawoy 1991, 276). I am attentive to the specificity of the laboratory I am studying, but I am more generally concerned with what this case can tell us about the world in which a particular lab and many other university laboratories are embedded.

The Site: The Handelsman Lab

Crammed on a portion of the sixth floor of Russell Laboratories at the University of Wisconsin–Madison is the laboratory complex of Professor Jo Handelsman. The lab includes four office spaces, a meeting room, and five laboratory work spaces depending on how one counts. The staff, including technicians, staff scientists, an administrative assistant, graduate students, and postdocs, varies from about ten to fifteen. On any given day, a visitor is confronted with a collage of sound—pipette tips rhythmically hitting plastic containers, the din of a local pop music station, arguments about university policy, and discussions about the unreliability of the polymerase chain reaction (PCR).[6] The walls tell visitors something about the institutional location of the lab: there are photos of fields of alfalfa and of lab workers beside a pickup truck; posters announce seminars in microbiology, molecular biology, and plant breeding. A press account tells the story of the lab's research and the promise lab inventions may hold for Wisconsin farmers.

Jo Handelsman came to the Department of Plant Pathology at the University of Wisconsin as an assistant professor in 1985 with an interest in nitrogen fixation. A discovery by an undergraduate doing work under Handelsman's supervision changed the direction of the lab and now, more than a decade later, Handelsman, a full professor, and her lab are leaders in the field known as biocontrol—in this case, the use of microorganisms to control plant disease. Soon after Handelsman's arrival, workers in her lab found that among 700 bacterial strains taken from Wisconsin soil, one—a *Bacillus cereus* strain the lab calls UW85—was virtually unique in its apparent ability to protect a wide range of agricultural crop plants from two diseases—damping off and root rot—caused by a microorganism called *Phytophthora* (Gallepp 1994; Handelsman et al. 1990).

Since making the initial determination of UW85's biocontrol properties, research in the lab has gone in a number of different directions. Researchers have devoted substantial energy and resources to understanding the "biological mechanisms" through which their *Bacillus cereus* strain works. As part of this investigation, they have reported finding that UW85 produces two antibiotics, one of which—zwittermicin A—is novel (Silo-Suh et al. 1994; Haiyin et al. 1994). In addition, they have hypothesized that UW85 may play a role in "root camouflage," a phenomenon in which similarities between microbial communities immediately surrounding plant roots and those in surrounding soil make roots less attractive to disease (Gilbert, Handelsman, and Parke 1994).

While some researchers in the Handelsman lab focus their attention on the biological mechanisms that explain UW85's effectiveness, others focus on the field potential of this biocontrol agent. This latter work has involved the application of UW85 to plant seeds and its placement directly in furrows to ascertain its effectiveness as a disease suppressant in contrast to the standard chemical application—metalaxyl. Concern here has not been limited to simple effectiveness tests; researchers are also examining the best methods for applying UW85 (Osburn et al. 1995). In addition, researchers working in a greenhouse have been attempting to breed alfalfa varieties that will enhance the effectiveness of UW85.

This is a university laboratory shaped by the demands of advanced scientific training. Students spend long hours in the laboratory, attend classes, and help to manage the laboratory, making certain that supplies are ordered, keeping equipment operational, and scheduling seminars and lab meetings. Handelsman seeks to teach her students to do "good biology," and their efforts to understand UW85 as a biological system have led to a slew of peer review articles. Much of the lab's research is supported by government and university grants, but the dual focus of the laboratory on so-called basic and applied research and Handelsman's commitment to research of social significance has led the lab into collaborations with agro-industry concerns, and successful collaboration has drawn laboratory workers into the realm of intellectual property protection.

Delineating the boundaries of the Handelsman lab is not only conceptually difficult but is practically problematic as well. Down the hall from Handelsman's office is the office and laboratory of Robert Goodman. Goodman has been a professor in the Department of Plant Pathology since 1991. Prior to joining the faculty at the University of Wisconsin, Goodman served for some nine years as the executive vice president for research and development at Calgene, a California biotechnology company. Handelsman and Goodman have several ongoing collaborations, including field experiments on UW85. Handelsman and Goodman share students. The labs share work space, supplies, and equipment, and Handelsman and Goodman regularly hold joint seminars for their students.

Between March and September 1995, I was a participant observer in the Handelsman lab. I was taught to screen soil samples from Costa Rica in search of *Bacillus cereus* strains that contain the novel antibiotic, zwittermicin A, which lab workers initially found in *Bacillus cereus* from Wisconsin soils. Over the summer, I was responsible for organizing meetings to discuss the field research jointly undertaken by the Handelsman and Goodman labs. I followed lab scientists to the field, the greenhouse, seminars, and meetings with industrial collaborators and the University of Wisconsin's patent agent, the Wisconsin Alumni Research Foundation (WARF). I interviewed lab workers on several occasions and was given complete access to all Handelsman laboratory documents. I continued as a part-time observer of the lab for a year after the end of my full-time stay.

A Laboratory and/in the World

I entered the Handelsman laboratory well aware of the existing literature on laboratory life, and my experience in the lab gave this work depth and reality. In learning basic laboratory techniques, I regularly confronted the craft character of lab bench work and the accommodations to local contingencies with which lab workers must deal each day. At the same time, if it is possible to think of laboratory science as existing on a continuum ranging from relative insulation from the effects of university-industry relations aimed at producing commercializable science to deep immersion in such relations, the most prominent laboratory studies (Knorr Cetina 1981; Latour and Woolgar 1979; Lynch 1985; Traweek 1988) focus on sites at the insulated end of the continuum. Beyond matters of conceptual orientation, this choice of sites must surely have affected what previous scholars have seen (see Whitley 1984; Fuchs 1992). Although the Handelsman lab certainly is not at the "immersed" end of the continuum, neither is it highly insulated. Thus, the Handelsman lab provides an opportunity to explore what it means for a university laboratory in the 1990s to exist in a commercial environment.

The Handelsman laboratory exists in a relatively stable institutional environment with specifiable characteristics. At the most general level, the practices of the laboratory are affected by the laboratory's location vis-à-vis the economy, the state, the university, and the research areas in which lab investigation takes place or on which lab workers draw. More specifically, laboratory practice is shaped by the character of agricultural pest control as this field has been defined by a history of industry dominance, the commercial research supply industry as it has developed following innovations in molecular biology and intellectual property protection, the formal and informal rules and norms that govern the U.S. intellectual property regime, and university-related practices and policies concerning intellectual property protection.

In the sections that follow, I will detail the nature of the Handelsman lab's relationships with these arenas. To contextualize the analysis, I turn my attention first to four closely related features of the lab's environment: developments in molecular biology, recently enacted federal laws, the emergence of commercially produced research supplies in molecular biology, and changes in the character of university-industry research relations.

The experimental work undertaken in 1973 by Cohen and Boyer that laid the foundation for recombinant DNA (rDNA) research is indirectly central to the Handelsman lab story. First, this was a pivotal development in the construction of boundary objects (Star and Griesemer 1989) and standardized packages (Fujimura 1987, 1988, 1992, 1996) in the field. The technique itself and related techniques that have followed have been fundamental to university biology in the period subsequent to this initial research. Furthermore, the technical power of rDNA and Cohen and Boyer's successful patent application for this fundamental technique, combined with a 1980 U.S. Supreme Court decision affirming the legitimacy of patenting life forms, are commonly viewed as the essential ingredients in initiating the commercial biotechnology revolution (Yoxen 1983; Kenney 1986; Krimsky, Ennis, and Weissman 1991; Teitelman 1989). The Handelsman lab operates in an environment constituted by this revolution. Lab members employ the patented techniques and materials of the biotechnology industry, they engage in patenting life forms, and their work is of interest to companies invested in commercial biology.

In this context, university-industry relations in the biological sciences have become increasingly common. Faculty relations with industry in the United States have a long history, dating at least to the advent of the graduate school as an organizational form in the United States (Slaughter and Rhodes 1990, 349). As far back as the 1920s, such relations were especially important in chemistry and engineering (Webster 1994, 123). Earlier still, in 1862, the U.S. land grant university system, of which the University of Wisconsin is a part, was established. The founding mission of the land grant system involved an explicit commitment to economic development and aiding the farm economy through university research (Kloppenburg 1988; Axt 1952).

Since the early 1980s, however, university science has been facing an increasingly "commercialized" environment (Slaughter and Rhodes 1990, 341). Two developments have been particularly influential. First, "shifts in the allocation of funds for academic research have forced investigators either to reduce their programmes of research, to redirect them or to find additional sources of support" (Etzkowitz and Peters 1991, 135). One source of this support has been industry, and the so-called revolution in molecular biology has attracted many companies to university biology laboratories (Kenney 1986). Although industry support for academic research in the United States has always been a small portion of overall university budgets, on average its portion has doubled in recent years from 4 percent in 1981 to 8 percent in 1994 (Webster 1994, 124). And indeed, the university researchers studied by Slaughter and Rhodes (1990, 343) "saw changes in the federal funding structure as pushing researchers toward more commercial activity." The second central development highlighted by investigators of the new university-industry relations is a change in federal law. In an effort to promote technology transfer, in the early 1980s, the U.S. Congress turned over to universities the rights to intellectual property that arose from federally sponsored research at those universities (Etzkowitz 1994, 412; see also Peters and Etzkowitz 1991, 432; Etzkowitz and Peters 1991, 135; Slaughter and Rhodes 1990, 341).

The 1980s also saw the standardization and commercialization of many tools central to molecular biology research. According to Fujimura (1996, 6), by the mid-1980s, the cancer researchers she studied viewed the technologies developed in their field not as objects of study "but as tools for asking and answering questions and for stimulating a flurry of work at the molecular level on questions previously approached at the cellular and organismal levels." Thus, biological research materials suppliers became part of the environment in which proto-oncogene research as well as other work using molecular hybridization, molecular cloning, nucleotide sequencing, gene transfer, and other technologies was undertaken. PCR technology and the related *Taq*, which are central to the discussion to follow, were not commercialized until the late 1980s.

Evaluating Experimental Results: UW85 Field Trials

Historians of science and technology have studied the effects of patronage on disciplinary contours (Kohler 1990), graduate curricula (Leslie 1993), and the trajectory of technological development (Noble 1984). This research shows that in cases where university scientists depend on a single patron for substantial portions of their research or related support, money gives patrons the capacity to shape the orientation of scientific fields, science education, and the focus of research projects. Concern about this type of influence also drives much recent work on university-industry relations (Webster 1994, 124; MacKenzie, Keating, and Cambrosio 1990).

The character of corporate influence in the case of UW85 is both more complicated and considerably simpler than the many histories of patron influence illustrate. It bears the imprint of the time and place of its discovery but in a much less direct way than, for example, cases in which university research agendas are shaped by the companies that pay the bill. The research priorities and concept development in the lab have not been directly shaped by an individual corporation's funding priorities. Instead, a particular actor in the lab's environment—the agrichemical industry—shapes research practices indirectly. Here, the lab's decision to work in this environment requires lab workers to "play by the rules" that characterize this context. At a commercial level, the very notion of what it means for UW85 to be an effective biocontrol agent is affected by the chemical(s) with which it would ultimately compete for farmers' loyalty. In terms of experimental design, this means that the existing chemical used to fight pathogens against which UW85 is aimed serves as a yardstick to measure the relative efficacy of UW85.[7]

The modern chemical industry is a product of World War II. From the 1930s until the war's end, sulfur, oil sprays, and inorganic chemicals were among the most popular means of fighting plant pests, and the production of inorganic pesticides increased dramatically throughout the 1930s (Kohn 1987, 162). Chemical warfare research provided the foundation for important developments in synthetic organic chemicals. In agriculture, these chemicals were substantially more target specific than earlier agrichemicals and generally were viewed as more effective. By 1950, organic chemical pesticides constituted 60 percent of the U.S. pesticide market, and this figure had reached 90 percent by the mid-1950s (Kohn 1987, 162, 163).

The move to synthetic chemicals was associated with massive increases in agricultural productivity. From the end of World War II through the early 1960s, corn production per acre nearly doubled from 36.5 bushels per acre to 68.1. Wheat productivity jumped dramatically too, from 16 bushels per acre to nearly 26. And the same can be said for many other crops (Kohn 1987).

Alfalfa is an important crop in the North Central United States. It is a feed source for Wisconsin's $3.8 billion dairy and beef industries and adds some $72 million to the state's hay crop. According to one lab funding proposal, alfalfa "underpins" some 50,000 jobs in areas as wide ranging as plant breeding and dairy farming (Goodman, Handelsman, and Grau n.d., 1). Alfalfa is threatened in certain regions by *Phytophthora* root rot, one of the crop's most destructive diseases.

In the mid-1980s, researchers determined that metalaxyl, combined with the use of disease-resistant varieties of alfalfa, reduced disease damage ("Researchers Recommend" 1987). Soybeans are also affected by root rot, and metalaxyl increases yield of this crop too ("Resistant Varieties" 1989). Metalaxyl is manufactured by Ciba-Geigy and was approved for use as a fungicide in 1983. The chemical is marketed for use on a wide range of crops, including apples, broccoli, cabbage, cauliflower, cucumbers, onions, spinach, and tomatoes (Marshall 1984, 10).

For the past several years, the Handelsman lab has taken UW85 to the field to test its disease-fighting capacities on alfalfa and soybeans in Wisconsin. In addition, the lab's industrial collaborator has contracted for additional field tests of the lab's biocontrol agent, and in general UW85 has been tested against metalaxyl or other chemical fungicides. In the lab's one published paper on UW85's effectiveness in the field with soybeans, lab workers report a carefully controlled experiment undertaken during five growing seasons in which their *Bacillus cereus* strain was tested against metalaxyl for its effectiveness in suppressing the fungi that cause root rot and damping off. The experiment involved different formulations of both the biocontrol agent and the chemical fungicide.

It would surely be the general consensus among farmers, pesticide retailers, and pesticide manufactures that, all other things being equal, UW85 will have a chance at commercial success only if its performance-cost ratio as a biocontrol agent is at least equal to the ratio for any product with which it might compete in the market.[8] But, in addition, metalaxyl is used as a yardstick against which to measure the efficacy of UW85. According to a peerreviewed scientific article published by lab workers and their industrial collaborators, "UW85 treatments provided a yield benefit each year that was *of similar magnitude* to the benefit provided by Ridomil"—one of the trade names under which metalaxyl is sold (Osburn et al. 1995, 555, emphasis added). In further highlighting the comparison between UW85 and metalaxyl, the report makes an additional important observation:

> Historically, yield enhancement by bacteria [like UW85] that suppress disease and promote plant growth have been plagued by *inconsistent performance* in the field. In the 5-year trial described here, we observed no *greater variability in performance* of UW85 than in that of a commercial fungicide. (Osburn et al. 1995, 555, emphasis added)

In addition to the role of metalaxyl as a yardstick in measuring the success of UW85, the target specificity of metalaxyl makes the chemical a vital research tool in the lab's work. Existing research indicates the specific diseases against which metalaxyl is effective. Consequently, in lab research, when seedling emergence is significantly higher with metalaxyl than in the no-treatment control, researchers can conclude with some confidence that the pathogen targeted by metalaxyl is affecting the control. By extension, of course, if the performance of UW85 is similar to the performance of metalaxyl as compared to the no-treatment control, researchers may legitimately suspect that UW85 is controlling the same diseases as metalaxyl.

In this instance, my research has uncovered no reciprocal influence, no "mutual shaping." Instead, a particular environment that developed over a particular period of time shapes the work of the laboratory in specific and documentable ways. What is more, it is possible to understand this situation in terms of a power asymmetry. If we grant that member companies in the agrichemical industry are, like the Handelsman lab, working to fight plant disease, we must acknowledge that agrichemical companies and the Handelsman lab are, at some level, working in common cause. But this should not lead us to ignore the fact that the agrichemical industry has defined the rules of the game in which the Handelsman lab plays. In this instance, power is exercised indirectly. The definition of what it means for a biocontrol agent to be effective or how the effectiveness of such an agent should be measured is not enforced here by the demands of an individual chemical firm. Instead, a history of past practices by a set of companies that collectively constitute an industry defines the terms of some of the Handelsman labs' current practices, and if the lab wishes to undertake work on UW85 as a biocontrol agent, it cannot escape these terms.

Commercial Research Materials and Experimental Practice

If the agrichemical industry defines one aspect of the institutional environment in which the Handelsman lab is situated, the research material supply industry and its relations to the lab constitute a second important feature of this environment. Current work in molecular biology depends in a large measure on commercially prepared research materials and equipment. The pervasiveness of such materials and equipment shapes everyday laboratory practices in obvious and mundane ways as well as in less obvious and more significant ways.

Scientists in the Handelsman lab are quite aware of the "contingency of the local"; they are acutely conscious that even when efforts are made strictly to control experimental practice, variability of the laboratory environment can never be entirely controlled for. However, perhaps because standardized materials are successful in the senses of Fujimura (1992) and Latour (1987), their workability is typically taken for granted. When an experiment goes awry, the local is likely to be scrutinized first.

This was the case in the use of the *Taq* polymerase in an endeavor distinct from the UW85 project and undertaken jointly between Handelsman's lab and the lab run by Robert Goodman. This project involves efforts to assess the diversity of bacteria in microbial communities in soil. Researchers are attempting to do this using what is termed a molecular phylogenetic approach. Such an approach involves isolating "total DNA" from soil samples. This strategy has the advantage of avoiding the necessity of culturing bacteria from soil to catalogue them. Culturing—growing bacteria on culture media in incubators—can only be accomplished successfully for a small portion of all soil bacteria. Although distinct from work on UW85, the microbial ecology project is related to the UW85 investigation, insofar as this assessment could lead to the discovery of additional novel antibiotics and conceivably new biocontrol agents.

Researchers working on the project bypass culturing by extracting genetic material directly from the soil and, using a technique called the polymerase chain reaction (PCR),[9] copy or "amplify" specific pieces of DNA known as 16S ribosomal RNA genes. This specific gene is useful because all organisms have "very high identity" in terms of it. As a result, small differences between these genes across organisms provide a signature for each organism and permit researchers to categorize previously undiscovered soil bacteria.

By heating the double-helix structure of a piece of DNA, PCR separates the twisted strands into two individual strands. The so-called primers—short, single-stranded molecules—bind to the ends of the now single strands of DNA, and nucleic acids fill in the remaining positions in the sequence, creating a new double helix. This process is repeated, permitting the geometrical amplification of the target DNA fragment. The amplified product of PCR can be used in a wide range of further experiments.

As with other work in the lab, researchers on the microbial ecology project rely on the *Taq* polymerase in their amplification efforts. In this project, assuming the appropriateness of commercially available *Taq* was a mistake. In initiating this work, Scott Bintrim, the researcher in charge of amplifying 16S genes from soil samples, set up a negative control—a tube containing the various chemicals used in PCR—including *Taq*—and water, but not including any DNA from soil. After amplification, he ran his DNA samples and his controls on an electrophoresis gel to assess the success of amplification. For several months, Bintrim successfully amplified DNA from his soil samples, but there was a discouraging problem as well: the negative control appeared to contain the 16S gene. Bintrim considered various sources of contamination, including the possibility that the water contained some microorganism with the 16S gene.

For some time, lab workers focused on eliminating contamination from the water, using materials that would rid the liquid of DNA. After several months, lab leader Goodman happened to talk to a researcher who also worked with the 16S gene and learned that contamination through *Taq* is a common problem in work on this particular gene. *Taq* is commercially "grown" in the common gut bacteria called *Escherichia coli*. *E. coli*, of course, contains a version of the 16S gene. For most researchers who use *Taq* in PCR, the presence of a small quantity of *E. coli* DNA is not a problem since their practices would not lead to the *E. coli* DNA being amplified.

To participate in the contemporary molecular biology field, university laboratories must rely on materials supplied by commercial concerns. These materials are standardized: they are created to suit a wide market of laboratories, not the local needs of individual labs. The configuration of these materials is generally taken for granted, but this state of what is perhaps necessary ignorance can have adverse effects on laboratory practice. Here again, the context in which a laboratory exists can be said to shape laboratory practice. The supplier-lab relation is beyond the control of individual laboratories.

Obviously, a noncommercially produced standardized material—a tool produced and standardized by another university lab or a nonprofit organization—could lead to the same types of problems the Handelsman and Goodman labs confronted with *Taq*. My aim is to illustrate different ways in which laboratory practices can be affected by structural factors beyond the confines and control of the laboratory. I focus on commercially produced research materials because such materials are increasingly at the heart of university biology. If the lab relied on a noncommercial source for a research material, although of a different variety, external constraints would still be operative.

This case illustrates the existence of a power dynamic between the research materials supply industry and laboratories, which can be understood on two levels. First, *Taq* comes in a limited number of variations from a limited number of supply houses, and to participate effectively in the world of molecular biological research, university labs are dependent on these companies for *Taq* and similar standardized resources (Pfeffer and Salancik 1978). Second, because to work effectively researchers must trust the efficacy (Shapin 1994) of externally supplied materials, they are not typically inclined to scrutinize the contents. They take these black boxes for granted and, in this way, the supply industry influences the practical mechanics of laboratory work.

Standardized Technologies as Embodiment of Power Relations

The commercial potential of developments in the biological sciences has made intellectual property protection and rights daily considerations in the lives of many university scientists. In debates over the implications of university-industry relations for the biological sciences, attention has been paid primarily to what patenting practices mean for the free flow of information in university science (cf. Kenney 1986; Blumenthal, Gluck, et al. 1986). However, the U.S. intellectual property regime itself can be viewed as an institution: it specifies a set of rules and of opportunities and constraints. In the next three sections of this article, I consider how the intellectual property regime directly and indirectly impinges on the practices of the Handelsman lab.

I turn my attention first to the polymerase, *Taq*. Beyond being a black box, *Taq* is intellectual property, and *Taq*'s proprietary character can potentially limit a university lab's freedom of movement. *Taq* is widely used in the Handelsman and Goodman laboratories and in molecular biology laboratories more generally. It was used in the two labs' joint microbial ecology project. In addition, I used the substance regularly in my efforts to locate in bacterial samples a gene that codes for resistance to the Handelsman lab's antibiotic, zwittermicin A.

The importance of the proprietary status of *Taq* became clear to me during a weekly meeting held jointly by the Handelsman and Goodman labs. At the meeting, Goodman suggested that perhaps the labs should cooperate and hire someone to make *Taq* "in-house," instead of relying on the commercial product. But PCR and *Taq* were developed in an environment that bares very little resemblance to the romantic vision of the scientific community as a world of Mertonian communism (Merton [1942] 1973) and perfect information. Both PCR and *Taq* are owned by the drug company Hoffmann LaRoche, and in May 1995, LaRoche filed a list of what it termed "patent infringers." Included in the list are more than forty university and government laboratories in the United States and two hundred individual scientists ("Market and Technology Updates" 1995). This filing amounts to a direct challenge to the tradition and letter of U.S. patent law, which includes an "experimental use exemption" permitting scientists engaged in "basic research" to use patented products and processes without securing a license.

While the field research done by the Handelsman lab has an ultimate commercial aim, generally the work done by the lab—including field experiments—is aimed at understanding the biological workings of UW85. This work and the molecular ecology project qualify as basic research and therefore are covered by the research exemption, as the law now stands.

Certainly we can view *Taq*, as Fujimura and others who draw from symbolic interactionism might, as a boundary object or standardized package that permits cooperation between diverse social worlds (e.g., universities, commercial research materials suppliers, the medical industry, the agrichemical industry). But this standardized package embodies a particular set of property relations. *Taq*, an essential research material, is the property of a company interested in making a profit. The company can use its monopoly on the polymerase to restrict production and shape price. For a university lab with finite resources, an expensive research material means that they can afford less of some other material.

But through the LaRoche patent on *Taq*, intellectual property law can affect practice as well. The proposal to manufacture *Taq* in-house may be a dangerous move unless the Handelsman and Goodman labs have the capacity to fight LaRoche's legal challenge to the experimental use exemption. In short, although university labs that oppose LaRoche's efforts to define the experimental use exemption narrowly and to restrict the use of *Taq* might desire to confront the company's legal position, to do so would demand an investment of time and money. LaRoche certainly has the legal expertise to promote its position, and the company is apparently willing and economically able to do so. Most university laboratories, by contrast, do not have lawyers on their staffs, and a legal challenge to the LaRoche position, even if economically possible, would take scarce resources from bench research. Under such circumstances, university laboratories are likely to acquiesce.[10]

The exercise of power here is rather straightforward. In his critique of pluralist castings of power, Dahl defines the "one-dimensional view" of power as "A has the power over B to the extent that he [sic] can get B to do something that B would not otherwise do" (quoted in Lukes 1974, 11, 12). Cases of power of this variety may be rare or politically or analytically less important than others, but clearly LaRoche's threat of lawsuit is likely to prompt university labs not to do something they might do under different circumstances: manufacture *Taq* themselves. Of course, LaRoche's success may narrow the meaning of the experimental use exemption in U.S. patent law, making the kinds of considerations Goodman raised in a lab meeting unthinkable. If so, we move to a more complicated power dynamic in which a set of rules governing intellectual property practices are altered to the advantage of companies like LaRoche, and these rules come to be taken for granted by all parties concerned.

Faith and the Efficacy of Patent Protection

Beyond issues raised when university scientists routinely work with patented technologies and research materials, an increasingly important consideration in many contemporary university biology laboratories is whether to patent inventions arising from laboratory research. Debates about the implications of the involvement of academic scientists in the creation of legally protected intellectual property have created a substantial din in science policy circles (Blumenthal, Gluck, et al. 1986; Buttel and Belsky 1987; Kleinman and Kloppenburg 1988; Nelkin and Nelson 1987). The problem of intellectual property also has been taken up by a number of scholars in science studies (Hilgartner and Brandt-Rauf 1994; Myers 1995; Cambrosio, Keating, and Mackenzie 1990; Bowker 1992, 54; Packer and Webster 1995, 1996), but researchers have not paid attention to the ways in which the demands of intellectual property regimes or, more specifically, of the widespread social faith in the efficacy of patent protection can affect scientists' decisions and practices.

Discussions of intellectual property protection and preparation of patent documents do not dominate the time of laboratory workers, but they are features of life in the Handelsman lab. The Handelsman lab consistently pursues patent protection. Handelsman herself seems to have an excellent sense of what is patentable, and, as of the middle of 1996, WARF had successfully applied for twenty-three patents on the lab's behalf in the United States and around the world. Among these is a five-claim U.S. patent on UW85 for its role in control of damping off and root rot, two diseases that affect alfalfa and other crop plants (Handelsman, Mester, and Wunderlich 1989).

As regularly as the lab applies for patents, I found no evidence to suggest that the direction of laboratory research has been shaped by patent possibilities. Moreover, although observers (Blumenthal, Gluck, et al. 1986; Kenney 1986; Kleinman and Kloppenburg 1988) of university-industry relations in the biological sciences have raised concerns about publication delays and increased secrecy that can result from university researchers' attention to intellectual property protection, Handelsman is careful to prevent patenting considerations from restricting the public presentation of laboratory results.[11]

But Handelsman and others in her lab are interested in the social usefulness of their research. To realize the market potential of their work, the lab must attract commercial interest. To do so, lab members believe it is necessary to patent their inventions and offer licenses to companies, which are in a position to develop and market these possible products. Indeed, this is precisely how universities throughout the United States promote the commercial potential of faculty-created, biology-based inventions: they offer interested companies patent rights. And judging by the kinds of relationships companies seek with universities, companies are interested in exclusive intellectual property rights (cf. Etzkowitz and Peters 1991; Kenney 1986). In this context, lab members' own attitudes toward patenting are irrelevant because if company representatives believe in the importance of patent protection, they are unlikely to take an interest in unpatented inventions.

Although there is little evidence that intellectual property protection promotes innovation and widespread use of inventions (Dworkin 1987), belief in such a causal relationship is widely held (Etzkowitz 1994, 392). Companies, like Utah-based Myriad, regularly speak of the importance of patent protection. According to Myriad's director of corporate affairs, Bill Hockett, his company would never have invested in the research that led to the discovery of the two genes associated with hereditary breast cancer if they had not been able to secure intellectual property protection. According to Hockett, "Without the protection that the patent affords, a company could not invest hundreds of millions of dollars in getting it to the marketplace." He asserts further that "if anyone could use the results of our research and our investment, it would not be worth much for the people who invested in our company" (quoted in "Where Should the Line Be Drawn?" 1997). Along these lines, Etzkowitz and Peters (1991, 163) suggest that small companies often do not believe they can "afford to take the risk of an unrestricted license, allowing others to follow up and easily copy them. They [believe they] need the protection of an exclusive license in order to take the risk of bringing a product to market."

As Handelsman suggests, this need to patent may be "more important as an illusion than as a reality." But in the end, Handelsman notes that "if an invention is unlikely to be developed in the public sector, if it is not something that can just be used the way it is, then we tend to patent because it's the only way to ensure that a company or somebody will really take it and run with it commercially." Indeed, even if Handelsman and her colleagues do not believe patents work the way many in industry do, if she and her lab members wish to attract commercial interest in their work, they must operate according to the accepted faith in the efficacy of intellectual property protection. As Samuelson (1987, 12) notes, "What matters is that most people in this society believe . . . [that the patent system promotes innovation], and that this faith guides people's actions."

In this case, the institutional context—here a formal and informal, explicit and implicit set of intellectual property rules—clearly shapes the practices of the Handelsman lab. From what she reads and hears, Handelsman has come to understand industry attitudes toward patent protection. She is interested in seeing her work commercialized, and she actively seeks financial support from industry for her lab's research. Thus, Handelsman uses patents to attract commercial interest in the lab's work. To do so depends on her abiding by informal corporate attitudes and implicit expectations. It also requires her to follow explicit formal rules for filing patent applications.

Whereas in the case of *Taq* and LaRoche, power was exerted explicitly, in this case it is not. Instead, a set of rules that is widely taken for granted and constitutes a social common sense (Gramsci 1971) shapes the practices of the Handelsman lab. It is beyond the capacity of individual labs to change these rules, and flaunting them can prompt sanctions: university labs are likely to lose corporate patronage.

University Administration and Intellectual Property

To acquire patent protection for one's inventions, it is not enough to accept the rules of the U.S. intellectual property regime. As Myers (1995) emphasizes, the process of applying for a patent and ultimately having patent claims accepted is time-consuming and expensive. Intellectual property law is a highly specialized and arcane aspect of the U.S. legal system, and it would be nearly impossible for an unassisted scientist to successfully file for patent protection. WARF, the University of Wisconsin's patent agent, has the legal expertise and financial wherewithal to file patent applications on behalf of university faculty, and here we see another instance in which the structure of the U.S. intellectual property regime indirectly shapes the practices of the Handelsman lab.

Established in 1925, WARF "administer[s] patents and licenses resulting from research discoveries brought to it by University of Wisconsin faculty members" (Blumenthal, Epstein, and Maxwell 1986, 1621). Entering into WARF's world means largely entering on the organization's terms. WARF will not pursue just any faculty disclosure but only those that agency staff believe will be successful and profitable. WARF is offered some two hundred faculty disclosures per year, and the organization converts fewer than half of these into patent applications. This means that barring independent resources (WARF spends about $17,000 to file, prosecute, and have issued each U.S. patent) or the interests of a company willing to pursue patent protection in collaboration with a university scientist, Wisconsin researchers are limited in their pursuit of intellectual property protection and associated licensing to only those inventions that are of interest to WARF.

Correspondence between WARF and Handelsman indicates that the relationship between the Handelsman lab and the organization sometimes has been vexed. Although WARF has been reasonably accommodating, documents make clear that decisions concerning invention licensing are up to WARF. In one instance, WARF agreed not to enforce patent protection on a lab invention if and when the invention was used in a restricted way by Wisconsin farmers with whom the Handelsman lab hoped to collaborate. According to a letter from a member of WARF's staff to Handelsman, "WARF will forbear from enforcing any patent it may obtain from this technology against individual Wisconsin farmers practicing the technology with respect to biocontrol agents for use on their own farms. WARF will similarly forbear such enforcement against farmers' cooperatives and other small businesses serving Wisconsin agriculture if they are located in Wisconsin and if their practice of the technology is limited to alfalfa and to the county in which their principal place of business is located and any contiguous county" (3/20/92). WARF made clear, however, that "WARF's keeping itself free to permit in-state alfalfa practice of the technology is not the same as guaranteeing to affirmatively *allow* such practice" (3/20/92).

In other correspondence, WARF asserts that a company licensing a lab invention need not provide the lab with information concerning the use to which the invention will be put. According to a letter from a WARF staff member to Handelsman, the company was "not willing to disclose what their particular needs were, only that it was outside the agricultural field of use" (11/27/91). This, of course, means that the lab loses control of its invention. Finally, the breadth of the patent drawn by WARF and the nature of the licensing agreement into which the organization enters with a commercial concern may restrict, as it appears to have done in one case, the freedom of movement Handelsman has in seeking further corporate support. As a WARF staff member acknowledged to Handelsman, "The exclusive rights to which . . . [the company] has an option clearly were logical to grant at the time they were granted. Just as clearly, those rights make for present problems in your attempts to secure funding for additional research projects where patents on inventions that might be made in that additional research are likely to be blocked by the existing patents and applications covering your past research" (11/21/91). If the patent claims are broad and a particular patent is licensed to a company, Handelsman may be prohibited from offering future related inventions to another company in her search for additional lab support. In a time of stagnating government support for research, this kind of limitation could be significant for a lab like Handelsman's, which has a budget of some $250,000 to $300,000 annually (cf. Etzkowitz and Peters 1991, 149).

At first glance, patent and licensing issues may seem to have very little to do with the kinds of epistemological concerns that have been the focus of a great deal of work in science studies; however, for a university laboratory at the boundary between basic and applied work and at a time when research budgets are uncertain, patent and licensing activity may shape research financially and practically. When patents and licensing agreements constrain what a lab leader can offer to potential corporate collaborators or patrons, this may affect the kind of research the lab can undertake. If a university patent agent decides lab collaboration with farmers (or anyone else, for that matter) is not in the interest of the university, the agent may be in the position to restrict this research cooperation.

In this regard, the Handelsman lab is not unique in its location. Intellectual property protection is becoming an increasingly important aspect of university science. Cost would typically prevent a university scientist from pursuing intellectual property protection independently. If they are interested in securing intellectual property protection, academic scientists must choose between either working directly with a company (in the event that their research is not supported by the federal government) or working with their university's patent agent. In either case, having decided to pursue intellectual property protection, the university scientist's role in determining how to pursue this protection will be constrained by the scientist's inability to seek patent protection independently. Here, we have a case of power of the simplest variety: the resources (money and legal expertise) at the disposal of WARF and similar patent agents can lead university labs to act in ways in which they might not if they had access to these resources on their own.

Conclusion

Nearly two decades of creative scholarly analyses have provided important new ways to understand science and technology and served to disrupt traditional notions of the autonomy of science from society. A good deal of work in science and technology studies has explicitly attended to what might be referred to as the social organization of power in the scientific field—exploring the relations between science and other social elites (cf. Kleinman 1995), the role of society's most powerful groups and institutions in shaping the trajectory of scientific development (cf. Kloppenburg 1988), and the use and abuse of expertise (cf. Brown and Mikkelsen 1990). Nevertheless, some of the most high visibility work in the area, especially lab-level analyses, has either entirely avoided explicitly conceptualizing a structural dynamic to power in science, underplayed it, or outright rejected its importance. More specifically, I have argued that research drawing on actor network theory and the social worlds approach has often ignored, discounted, or dismissed the centrality of historically established, structurally stable attributes of the world that systemically shape practice at the laboratory level. This seems at once surprising and distressing, given the character of the complex institutional web in which university science is undertaken.

The five examples I have discussed from the Handelsman lab suggest that structures that extend well beyond, but reach into, the laboratory can have profound effects on laboratory practice. Given the focus on agency in recent work in science and technology studies, it is important to note that the Handelsman lab was not forced into the commercial world. But having chosen to do research that might be commercially useful, having decided to seek commercial support for that research and to use commercially produced research materials and technologies, the Handelsman lab has been subjected, in many senses, to the "rules" that govern the world of commerce.

Although I have focused on power asymmetry and the structure side of the "structure-agency" equation, it is clear that structures are constructed and reconstructed in daily practices over time. Indeed, the practices of university "patent infringers" and the lawsuit in the *Taq* case point to one particular structure that may be in the process of (re)construction. But if we accept that structures are always in the process of (re)constitution, it is a matter of investigating specific historical cases to trace when stable structures are constraining and when and to what extent practices alter structures. We cannot assume either that the world is overwhelmingly constraining or that it is easily manipulable.

Notes

1. Among other recent work, I refer interested readers to Pickering (1992), Clarke and Fujimura (1992), and Jasanoff et al. (1995). Two volumes that, through investigation of empirical cases, highlight some of the most important observations made in science and technology studies are Collins and Pinch (1993) and Rabinow (1996).

2. It is not immediately obvious why we should need to "transcend" the context-content dichotomy. Indeed, it seems to me there may be times when looking at the effects of the world "outside" the lab on the practices "inside" is worthwhile. Furthermore, while we need not deny that projects or practices can result from "balancing acts" and juxtapositions, I see no reason to assume this either. Indeed, doing so might very well lead us to overlook cases of imbalancing/balancing acts in which there are asymmetries in the efficacy of various factors implicated in the construction of projects or shaping of practices.

3. Obviously, I am generalizing, and not every aspect of my critique applies to everything written in these two traditions. On the matter of constraint, for example, see Callon (1991).

4. Perceptive critiques of the work of Bruno Latour can be found in Amsterdamska (1990), Knorr Cetina (1985), and Scott (1991). Frickel (1996) challenges and engages actor network theory in interesting ways.

5. My work is not the first attempt to conceptually balance structure and agency. Among the most prominent theorists who seek to circumvent the structure-agency dichotomy is Anthony Giddens (1984). Giddens provides a thoughtful analysis of the ways in which structures are carried by knowledgeable actors and the ways in which structures are reproduced in and through everyday practices. Within the context of a general theory of the relationship between structure and agency, Giddens at once takes seriously the idea that structures are constructed and that at any given moment they can be stable and constraining. My goal in this article is far more modest than Giddens's project. How the balance of structure and agency work and how social order is reproduced are not my concerns here. Instead, I aim to take a synchronic look at several distinctive structures and to illustrate the ways in which these structures can constrain or shape actors' practices. Two interesting efforts to use Giddens's theory of structuration in science studies are Abir-Am (1987) and Hagendijk (1990). For a recent effort to grapple with the structure-agency problem in different empirical terrain, see Fine (1996).

6. For a rich rendering of the history of the development of polymerase chain reaction (PCR), see Rabinow (1996).

7. In his study of university-industry relations, Webster (1994, 138) found that industry definitions of success did not simply shape university definitions of success and quality. Instead, distinct definitions from the two "cultures" became fused.

8. This is not the case where there is no chemical treatment for a given plant disease and a biocontrol agent is the only means of disease suppression.

9. The PCR process was initially developed by Kary Mullis while working at the Cetus Corporation in California (Rabinow 1996). His findings were reported in *Science* in 1985. PCR was patented in 1987 and commercialized in 1988 by Cetus. Called "one of the most important and powerful tools in molecular biology," between its initial development and the fall of 1994, research for more than 25,000 scientific articles relied on PCR (Alexander 1994). According to a 1993 report, more than 70 percent of molecular biology researchers use PCR as a tool in basic research ("Hoffman-La Roche" 1993). Mullis won the 1993 Nobel Prize in chemistry for his work.

10. A similar situation arose a few years ago with the so-called cystic fibrosis gene. This gene and the principle mutation associated with cystic fibrosis were patented by the universities of Toronto and Michigan, and these institutions expressed an inclination to require researchers to pay royalties for using this genetic material ("U.S. Demands" 1993).

11. It is conceivable that in writing papers, lab workers are careful to limit open-ended speculations to prevent later patent applications from being rejected on the grounds that there is "prior art." However, I found no evidence of this.

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