15 Sites of Scientific Practice: The Enduring Importance of Place

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Science in the twenty-first century is seemingly a world of perpetual motion. Scientists, specimens, instruments, and inscriptions race around the world on jets and through digitized communications, largely unfettered by the drag of distance or physical location. In an era when the globalization of science has never been more apparent, it seems almost anachronistic for us to suggest that "place" continues to matter a great deal for the practices and accomplishments of science. Our task in this chapter is to show that globalized science is at the same time emplaced science: research happens at identifiable geographic locations amid special architectural and material circumstances, in places that acquire distinctive cultural meanings. We seek to go beyond a mere listing of the various (and sometimes surprising) places where science happens, in an attempt to theorize *how* the material and geographic situations of research are sociologically consequential for institutionalized activities that appear, at a glance, to depend so little on them. In fact, the global standardization of research facilities shows how both the brick-and-mortar of material infrastructure as well as the symbolic understandings that privilege some places as authoritative sites for knowledge-construction actually *enable* the mobility of science all around. Place, ironically, achieves the appearance of placelessness.

Whether or not place matters for science—and how—has long been debated in STS.[1] These discussions have moved through four waves, and we suggest the need for a fifth. In the first wave, positivist and rationalist philosophers of science found little cause to examine the specific places where science occurs (Reichenbach, 1938; Popper, 1959; Hempel, 1966). However situated the actual practices of scientists might be, what mattered most from this perspective was the abstract, universal, and placeless character of scientific truth at the end of the day. The laws of gravity worked the same everywhere; even if scientists in different locations disagreed for a time about the content of those laws, persuasive evidence and compelling theory would eventually rub out geographical differences in belief. In wave one, science epitomized a "view from nowhere" (Nagel, 1989), disciplined into a single eye by method, instrumentation, techniques, and logic.

The second wave began with a recognition that this supposed "God trick" (Haraway, 1991) was a philosophical conceit rather than an adequate empirical account of how scientists construct legitimate knowledge. Beginning in the 1970s, STS ethnographers moved into the laboratory, discovering context-specific contingencies that shaped how scientists differently interpreted data, used machines, conducted experiments, and judged validity (Collins, 1974; Latour & Woolgar, 1979; Knorr Cetina, 1981; Lynch, 1985). The supposed placeless and transcendent character of scientific claims was no longer seen as a philosophical necessity but as a discursive accomplishment. Wave two discourse analysts showed how scientists routinely excise circumstantial "modalities" of specific places from their texts, leaving the appearance that the facts came straight from Nature (Latour & Woolgar, 1979; Gilbert & Mulkay, 1984). Although laboratory ethnographers established the irreducibly local character of scientific knowledge-making, conceptual interest in the laboratory as a place was minimal. The lab became an analytical resource—a means for deconstructing the "view from nowhere"—rather than a topic of interest in its own right.

By the 1990s, a third wave of research was well under way, in which STS scholars produced case studies of historically-changing sites of science, revealing the different geographic and material preconditions of making legitimate knowledge. By comparing the various settings where science happened, it was possible to discern how distinctive epistemic regimes were constituted in and through the situated, material conditions of inquiry. For example, the ancient agora in Athens was a place where privileged males could decide truth and virtue through public argumentation (Sennett, 1994)—in stark contrast to cloistered monasteries (Noble, 1992) and the secluded Renaissance studio (Thornton, 1997; Ophir, 1991), where solitude and contemplation were seen as necessary for scholarly pursuits. In the early modern period, the growing epistemic significance attached to "witnessing" collections of specimens accompanied the rise of museums, which were initially located in wealthy households and then in more accessible stand-alone buildings (Findlen, 1994). Similarly, the later importance of witnessing experimental apparatuses moved from the "gentleman's house" (Shapin & Schaffer, 1985; Shapin, 1988) to specialized laboratories in the nineteenth century (Gooday, 1991; Schaffer, 1998). By analyzing the shifting links between the place deemed appropriate for science and the creation of legitimate knowledge, studies from wave three provide rich materials for answering a signal question in STS: what must the construction of legitimate natural knowledge be like such that these kinds of places—located at this spot, built to these designs—fit the bill?

At about the same time, actor-network theory (ANT) offered conceptual perspectives that—in an emerging wave four—could suggest a diminished role for place in STS. To be sure, ANT directs attention to the nonhuman materialities at "centers of calculation" such as Pasteur's Parisian laboratory and the public arenas where he demonstrated his anthrax vaccine (Latour, 1983, 1988). And yet, it is the *transit* of Pasteur (and his research materials) from farms to labs to sites of public display that carries the most explanatory weight in Latour's explanation of the pasteurization of France. This insight has led some to give greater attention to "immutable mobiles" (and, more recently, "mutable mobiles") than to the seemingly static and emplanted centers of calculation. Emphasis is placed on the mobility or "flows" of heterogeneous actants through networks and, in particular, on the fluidity or malleability of substances as they move about—thereby diminishing the apparent significance of the specific geographical places where the actants pass through or end up. For Callon and Law, "circulation has become more important than fixed positions" (2004: 9), and this idea finds further support in social and cultural theory more generally, as in Manuel Castells' "network society" (2000) or David Harvey's (1990) arguments for the compression of space (and time) in postmodernity. As Frederic Jameson puts it, "the truth of experience no longer coincides with the place in which it takes place" (1988: 349).

We have no quarrel with recent STS attention to mobilities and fluidities, but these properties of technoscientific actants do not warrant abandoning the investigation of materially-situated and symbolically-encrusted "nodes," the places that serve as endpoints for the links comprising heterogeneous networks in the ANT approach. There is still a great deal to be learned about laboratories, field-sites, and museums as places of science—however unmoving they might now seem to be—and we argue that the initiative to fold places into non-geographic networks actually overlooks important features useful for explaining *how* science travels. Our fifth wave seeks to be more theoretical than wave three, as it tries to identify precisely how place has consequence for scientific knowledge and practices, and why a focus on geographic location and situated materialities can enlarge our understanding of science in society. We discuss (1) why science clumps geographically in discrete spots, (2) how the material architecture of laboratories resolves certain tensions inherent in the juxtaposition of the ordinary practice of science and its imagery or public understanding, and (3) how the emplacement of science creates opportunities for resistance to its cultural authority.

LOCATING SCIENCE

The stuff of science circulates swiftly and globally, but not unendingly. For all its obvious mobilities and fluidities (Mol & Law, 1994; Callon & Law, 2004), science alights at universities, laboratories, field stations, libraries, and other centers of calculation (Latour, 1987). And when scientific practices stay put for a while, an interesting geographical pattern emerges: science is not randomly or evenly distributed all over the skin of the earth. Rather, the activities and wherewithal of scientists are clustered together in discrete locations recognizable as centers where most science happens. It is provocative to say that the whole world must become a laboratory in order for it to be known scientifically (Latour, 1999: 43), but it is also sloppy. The map looks more like an archipelago, islands of science vastly different from the surrounding sea.[2] "Natural knowledge is constructed in specifically designed and enclosed space" (Golinski, 1998: 98).

Why does science disperse geographically into clumps? In this respect, science is much like any large-scale productive activity, such as making cars or making money: having certain people, machines, archives, and raw materials reliably close at hand is simply a more effective way to do business. Economists have described "agglomeration efficiencies" (Marshall, 1890)—gains in productivity that result from gathering together at a common geographic location the diverse constituent elements of an activity. At first glance, however, capitalism today does not evince agglomeration: corporate moguls jet everywhere, representing clients and investors from all over the world; transactions involving millions of dollars or Euros are made in the flick of a keystroke by currency traders "in fields of interaction that stretch across all time zones" (Knorr Cetina & Bruegger, 2002: 909); core assumptions about the economic theories underlying markets are understood more or less in the same way here and everywhere; factories, offices, and outsourced jobs flow from country to country, seeking greater profitability. What could be more "global" or "mobile?" And yet Saskia Sassen (2001: 5) finds that this globalization of economic activities generates "global cities" (New York, London, and Tokyo), specific places where corporate headquarters huddle together around the geographically centralized financial and specialized service functions on which they depend—lawyers, accountants, programmers, telecommunications experts, and public relations specialists. The "extremely dense and intense information loop" afforded by "being in a city" "still cannot be replicated fully in electronic space" (2001: xx). It is premature, Sassen suggests, to conclude that innovations in information, communication, and transportation technologies have the capacity "to neutralize distance and place" (2001: xxii)—and that is as much the case for science as for corporate capitalism.

Science clusters at discrete places because geographic proximity is vital for the production of scientific knowledge and for the authorization of that knowledge as credible (Livingstone, 2003: 27). "Place" enables copresence among people, instruments, specimens, and inscriptions (Bennett, 1998: 29). Particle accelerators, colliders, and detectors in high-energy physics illustrate the necessity—but also the difficulties—in gathering up scientific instruments at a common location (Galison, 1997; Knorr Cetina, 1999). Pieces of a detector may be built at scattered sites, just as the scientists involved with an experiment may corporeally reside at CERN, SLAC, or Fermilab only intermittently and for short durations. To cast experimental high-energy physics, therefore, as transient science misses the significance of the destination toward which the machines (and their tenders) eventually move. New particles could not be found without the precise temporary commingling of accelerators, detectors, and computers on site (no matter how much analysis of the data subsequently happens at universities often far away from the accelerator). Still, success at melding sophisticated machines is rarely automatic and typically hard-won for social and technical reasons: what happens at the destination laboratory in high-energy physics is described as "breaking components out of other ontologies and of configuring, with them, a new structural form" (Knorr Cetina, 1999: 214).

The "magnet" attracting science to a discrete place may also be a collection of specimens unrivaled in the world. Linnaeus's botanical taxonomy appears, curiously, as an eighteenth-century achievement of an already globalized science. Linnaeus himself traveled from Uppsala to Lapland (for collecting), and more consequentially to Holland, where an immense number of plant species had been gathered from around the world at botanical gardens in and around Leiden. For some historians, this movement of plants and scientists is key: Linnaeus's achievement "does not depend solely on the cascades of inscriptions produced, gathered, and reproduced within any one particular 'center of calculation'" because "the very possibility of that taxonomy presupposed the formation of a worldwide system of plant circulation mediating a plurality of sites of knowledge production, both peripheral and central, in which 'stable' and 'variable' features could fall apart" (Müller-Wille, 2003: 484). So much analytic attention is given to this "vast network of translation and exchange" that the locus of arrival becomes a trivial after-effect. Without a doubt, historical studies of collecting and transporting specimens have enriched our understanding of field sciences by expanding the cast of characters involved in science and by showing the mutability of research materials as they move from periphery to center (Drayton, 2000; Schiebinger, 2004; Schiebinger & Swan, 2005; Star & Griesemer, 1989). Still, Linnaeus did not need to travel to China or the Americas—just Leiden, because that is where the plants converged. He was as dependent, for example, on George Clifford's careful gardeners and passion for collecting as he was on the traders and sailors who procured the plants and got them safely to Holland, and there is little merit in diminishing the consequentiality of the former just to raise curiosity about the latter. Leiden mattered (Stearn, 1962) because Linnaeus's taxonomic efforts depended on the affordances of the Dutch gardens: "spaces in which things are juxtaposed," making them "already virtually analyzed" (Foucault, 1970: 131). With the concentration of so many botanical species at Leiden, and with their classificatory plantings, Linnaeus's gaze was impossible to achieve almost anywhere else in the world.

On other occasions, the accumulation of people at a place serves as its own magnet—attracting still more scientists to that spot. Even in sciences without much need for unique massive instruments or an incomparable collection of specimens, geographical clustering occurs. Folk wisdom depicts mathematicians as an especially peripatetic bunch of scientists—always scurrying from university to university to share ideas up-close and personally, a pattern of work and "flow" that reaches back to the late nineteenth century. Between 1900 and 1933, Göttingen was the place to be for cutting-edge mathematics. Felix Klein and David Hilbert were there, and "what made Göttingen probably the most eminent center of mathematics in the word—until 1933—was the unrivaled inspiring atmosphere among the numerous young mathematicians who flocked to Göttingen from everywhere" (Schappacher, 1991: 16). The place was a "cauldron of activity" with a "highly competitive atmosphere" where "even budding geniuses, like Norbert Wiener and Max Born, could be scarred by the daunting experience of facing the hypercritical audiences that gathered at the weekly meetings of the Göttingen Mathematical Society" (Rowe, 2004: 97). For early twentieth-century mathematicians, if you could make it in Göttingen, you could make it anywhere. The city assembled the most formidable audience that fresh mathematical ideas might ever face—and those that survived carried a widely respected geographic seal of approval (Warwick, 2003). Thus, some *places* ratify scientific claims.

The clumping of mathematics in centers like Göttingen is explained in part by the "thick" interactions enabled uniquely by face-to-face proximity. Boden and Molotch

(1994) suggest that the rich contextual information accompanying talk and gesture in close-up encounters is important for judging the reliability and authenticity of what others are saying (or implying). This, in turn, is vital for the development of trust on which scientific practices significantly depend (Shapin, 1994: xxvi, 21). Indeed, that sense of trust seems especially difficult to achieve among collaborators in the absence of face-to-face interaction (Handy, 1995; Olson & Olson, 2000: 27; Finholt, 2002; Cummings & Kiesler, 2005; Duque et al., 2005). In his analysis of physicists who study gravitational waves, Harry Collins (2004: 450–51) writes:

> As the Internet expands, more and more people are saying that it is time to put an end to these expensive little holidays for scientists in pleasant places. But conferences are vital. The chat in the bars and corridors is what matters. Little groups talk animatedly about their current work and potential collaborations. Face-to-face communication is extraordinarily efficient—so much can be transmitted with the proper eye contact, body movement, hand contact, and so forth. This is where tokens of trust are exchanged, the trust that holds the whole scientific community together.

Copresence at a place is also vital for the transfer of tacit knowledge: "experiments are matters of the transfer of skills among the members of a community," so that "the knowledge and skill . . . [are] embodied in their practices and discourse and [can]not be . . . 'read off' from what could be found in print, but [are] located in the uniqueness and extent of their experience" (Collins, 2004: 388, 608). Collins's "enculturation model" fits the Göttingen mathematicians: David Rowe contends that developments at Göttingen began to institutionalize an "oral culture" among mathematicians, in which "to keep abreast of it one must attend conferences or workshops or, better yet, be associated with a leading research center where the latest developments from near and far are constantly being discussed." Echoing Collins, Rowe suggests that it is "probably impossible to understand" print versions of the latest proof "without the aid of an 'interpreter' who already knows the thrust of the argument through an oral source" (Rowe, 1986: 444; Merz, 1998).

But what if Klein, Hilbert, and the Göttingen Mathematical Society had had access to video teleconferencing, which would seem to capture much of the contextual thickness of copresence? Göttingen might then have become just a node on a network of hook-ups, with no geographical location of any special significance (being there would matter less). Or maybe not: the coagulation of mathematicians at Göttingen also afforded a high probability of chance encounters with other experts, unexpected meetings that sometimes yield creative solutions or, at least, previously unimagined problems (Allen, 1977; Boden & Molotch, 1994: 274). Unplanned meetings sometimes take place in "trading zones," which Peter Galison (1997) has described (in his history of high-energy physics) as physical sites where theorists, experimentalists, and engineers run into each other—and, via emergent "contact languages" or "pidgins," collaboratively exchange ideas and information whose meaning may be different from one subculture to the next. Although Fermilab created a joint experimental-theoretical seminar every Friday, "More frequent are informal meetings 'in offices on the third floor of the Central Laboratory and at the Cafeteria, Lounge and airports'" (Galison, 1997: 829). At MIT's Radiation Lab, "engineers and physicists worked within sight of one another," and its "success was directly related to the creation of such common domains in which action could proceed . . ." (1997: 830). By contrast, video teleconferencing is an arranged and scheduled interaction: you need to plan in advance who is expected to phone in, and when. But in theoretical physics, Merz suggests that "interaction should not be forced, it should just happen . . . casual, non-final, provisory, informal" (1998: 318). Further research is needed to decide whether chance discoveries in science are as likely to emerge from video teleconferencing as from physical copresence in what Merton and Barber identify as "'serendipitous microenvironments' . . . where diverse scientific talents were brought together to engage in intensive sociocognitive interaction" (2004: 294).

MATERIALIZING SCIENCE

The point of Anne Secord's celebrated paper, "Science in the Pub: Artisan Botanists in Early Nineteenth-Century Lancashire" (1994), is to show that science *cannot* happen in a pub. Secord avoids contradiction by consistently using adjectives to modify botany or science: those who gathered at the pubs to talk about plants were "artisan" or "working-class" practitioners, and their societies were "local." It is surely the case, as Secord says, that these working men and women bought botanical treatises, tried to grow the best gooseberry, learned some Linnaean nomenclature, inspected plants on pub benches, and provided useful specimens to gentlemen who practiced "'scientific botany'" (1994: 276). Moreover, they saw themselves as doing botany and as contributing to botanical knowledge (and not just as collectors of specimens). Still, their "science" requires adjectives or scare-quotes. Secord is appropriately constructivist in seeking the contested meanings of such distinctions as professional versus popular science in the emerging practices of historically-situated people—she refrains from imposing timeless boundaries by analytic fiat (1994: 294; Gieryn, 1999). Whatever those working class Lancashire botanists thought they were up to, the evaluation of their activities by those who then (and later) had greater power to solidify the boundaries of science put them on the outside—not just because of their social class or lack of Latin and other refinements, but because of the places where they gathered: pubs.

Legitimate knowledge requires legitimizing places. The rising cultural authority of science through the nineteenth century (and beyond) depended in part on geographic and architectural distinctions between those places deemed appropriate for science and those that were not. The pub—along with other quotidian places where almost-science or pseudoscience occurred—was epistemically delegitimated, as Secord (1994: 297) suggests:

> [S]cientific practice became increasingly associated with specific sites from which "the people" were excluded. By defining the laboratory and the experimental station as the sites of legitimation of botany and zoology from the mid-nineteenth century (and thereby increasing their status), the place of science became strictly defined and popular science was marginalized.

By *materializing* scientific investigation in buildings distinctively different from other kinds of places, assumptions get made far and wide about the credibility of the results—and how that credibility may depend on real or imagined circumstances of production. Science elevates its cultural authority as the purveyor of legitimate natural knowledge by making its places of provenance into something *unlike* everywhere else. Putting science in the pub was an "exercise in denigration" (Ophir & Shapin, 1991: 4), and sometimes just having liquor nearby was sufficiently degrading. In 1852, Thomas Thomson reported that sharing a building for his new and excellent chemical laboratory with a "whiskey shop which occupies the ground floor does not accord with what one would expect from the University of Glasgow" (Fenby, 1989: 32).

But what kind of architecture now secures epistemic authority? A hint is found in Secord's story: a handloom weaver named John Martin gave a moss specimen to William Wilson, gent., who passed it along to his friend William Jackson Hooker, then professor of botany at Glasgow (1820–1841) and later the first director of the Botanical Garden at Kew. Hooker was pleased and asked Wilson to investigate the possibility of Martin's coming to work in Hooker's herbarium. Wilson had initially seen Martin as "addicted to neatness" (Secord, 1994: 288), but a visit to his working-class cottage convinced him otherwise (1994: 290):

> "I did not find that neatness which I expected," he reported to Hooker, and he was puzzled that there were few outward signs of "*order & arrangement*" when Martin's mind seemed to be "very well regulated" and he was "an original & patient thinker" (emphasis in original). Martin's plant specimens were "rather carelessly mixed in the leaves of a copy of Withering & in other Books, which are not so clean as I expected."

Pubs are also disorderly, not especially clean, and just as indicative of a material disposition unsuited for real science as Martin's messy cottage.

Order and *arrangement* have become markers of sites where genuine science occurs. The design of laboratories—through the material arrangement of its spaces and physical fixtures—achieves types of *control* not commonly found in other places. Foucault could easily have been thinking about the scientific laboratory when he wrote, about "heterotopias" in general: "their role is to create a space that is other, another real space, as perfect, as meticulous, as well arranged as ours is messy, ill-constructed and jumbled" (1986: 27). But Foucault may not have spent much time in actual labs: most give the appearance of being packed to the rafters with stuff, strewn about in disarray, giving off the impression that everywhere somebody is in the middle of something. Orderliness and cleanliness describe the laboratory as it exists in widely-shared cultural imageries—assumptions about what such places must be like in order to unlock the secrets of Nature. Emphatically, sites of science are both quotidian work places (not always meticulous) and authorizing spaces (purifying and logical), and, we suggest, the coexistence of these disparate states depends on architectural manipulations and stabilizations of three apparent antinomies: public and private, visible and invisible, standardized and differentiated. Laboratory sites simultaneously materialize both ends of these three polarities, in intricate ways that are consequential both for the productive efficiency of scientific knowledge and for the cultural authority of science as an institution and profession. How so?

Science is, at once, public *and* private (Gieryn, 1998). On one level, scientific work is an oscillation between intense communal interaction and solitude. Both the public and private aspects of inquiry have, at different times, been connected to the credibility and authenticity of resulting knowledge (Shapin, 1991). In Greek and Medieval thought, solitude was a means to prevent the corruption of thought by minimizing interference from others and enabling unmediated contact with the source of genuine wisdom—reclusive monks found God in the hermitage, Montaigne later found truth in the loneliness of his tower library (Ophir, 1991), Thoreau retreated to the "wilderness" of Walden Pond (Gieryn, 2002), and Darwin withdrew to Down House (Golinski, 1998: 83; Browne, 2003). Seclusion has its epistemic risks: delusion perhaps, parochialism, or secrecy (none contribute much to the pursuit of legitimate natural knowledge). So, starting from the early modern period, science also parades its public character: claims must be shared (Merton, 1973), experiments must be witnessed (Shapin, 1988), collaboration is increasingly required, and conferences become necessities. The scientific life these days is marked by intermittent solitude (for reflection, for creative bursts unfettered by the doubts of others) amid sustained collective efforts; the public side of science speeds the production of knowledge via efficient divisions of labor and, at the same time, secures credibility through the authentication of claims by informed audiences. This all gets *built-in*: the Salk Institute of Biological Studies in La Jolla, California, designed by modernist hero Louis Kahn in the early 1960s, has two dramatically different kinds of spaces. The architect Moshe Safdie (1999: 486) worked on the project:

> Kahn was obsessed with how he might create a space that would enhance the creative activity of scientists. He was impressed with the fact that scientific activity today requires solitude and collaboration. This led him to develop the basic scheme for the Salk: places for solitude reaching forward into a long courtyard from the places for collective work, the great, flexible laboratories.

Architecture manages the jointly public and private character of science work: "space . . . articulates exactly this double need for the individual and the collective aspects of research" (Hillier & Penn, 1991: 47).

Science is also "public" in its active engagement with constituencies outside the profession. Laboratories could not exist without financial support by corporate and government investors, creating an implicit *quid pro quo*: space for science yields knowledge and technologies vital for making profits, legitimating policy, and improving civil society. And yet the ability of science to deliver the goods is assumed to depend on its autonomy from direct interference by these constituencies—a different sense of "private." This ideology also gets materialized in the architecture of science buildings. The Cornell Biotechnology Building in Ithaca, New York, constructed in the mid-1980s, was designed to provide a welcoming space for diverse constituencies and beneficiaries while at the same time building-in a sequestration of research activities

(Gieryn, 1998). The place was built for a number of "publics": Cornell students, the taxpayers of New York State, and corporations with interests in biotechnology. During the design process, these publics were defined as a risk and a threat to the safe and autonomous pursuit of knowledge, even as they were acknowledged to be the *raison d'être* for the $34 million project. Architecture provided a solution to this social problem: a "beachhead" for the public is created in the atrium lobby, conference and seminar rooms, small cafés, and some administrative offices, giving constituencies a symbolic and material place in the building. In a "bubble diagram" drawn up early in the design process, a thick black horizontal bar separates PUBLIC from PRIVATE. Above the bar, the entering public is routed to conference rooms or administrative offices; below the bar is a list of research groups (drosophila, eukaryotes, prokaryotes, etc.) and support facilities (plant growth rooms, animal rooms, etc.). The bar on paper gets materialized as an inconspicuous door off the inviting lobby (with straw mats by Alexander Calder)—without way-finding signs—and leading to a hallway whose utilitarian finishes, unfamiliar machineries, and strange odors suggest a "backstage" (Goffman, 1959) where the uninvited are made to feel out of place. Jon Agar finds the same pattern with Britain's radio telescope at Jodrell Bank: "the spectacle needed spectators, but the public needed to be held back," and "a key tool in achieving this distancing was this discourse of interference: the identification of unwanted visitors as disturbing" (1998: 273).

Sites of science also manage juxtapositions of the visible and invisible. Laboratories create enhanced environments where it becomes possible to see things not visible elsewhere (Knorr Cetina, 1999). Accelerators and detectors enable high-energy physicists to see quarks (Pickering, 1984; Galison, 1997), arrays of centrifuges and PCR machines enable molecular biologists to see precise segments of DNA (Rabinow, 1996), a vat of dry-cleaning fluid in a mile-deep cave enables physicists to see massless solar neutrinos (Pinch, 1986), and astrophysicists on earth manipulate a space telescope to see stars as never before (Smith, 1989). "The laboratory is the locus of mechanisms and processes which can be taken to account for the success of science," accomplished by its "detachment of the objects from a natural environment and their installation in a new phenomenal field defined by social agents" (Knorr Cetina, 1992: 166, 117).

However, even as laboratories render natural objects visible, they make the observing practices of scientists invisible—or, at least, incomprehensible—to all but the few knowing experts. Visitors to the Cornell Biotechnology Building are steered away from research spaces by an environment coded as "public not welcome here." And yet, the "success of science" as a *privileged and authoritative* eye on nature depends on the transparency of the process of scientists' seeing—in principle, scientific practice is assumed to be open for all to view (secrecy pollutes credibility). Golinski (1998: 84) puts it this way:

> [T]he laboratory is a place where valuable instruments and materials are sequestered, where skilled personnel seek to work undisturbed, and where intrusion by outsiders is unwelcome . . . On the other hand, what is produced there is declaredly "public knowledge"; it is supposed to be valid universally and available to all.

For this reason, the Stanford Linear Accelerator hosts tour groups to make its activities visible to anybody. It is not apparent what those visitors actually see: "most visitors on these tours arrived wanting to be awed rather than informed . . . [and] often behaved as though they had been granted a special dispensation to see the inner sanctum of science and its most learned priests" (Traweek, 1988: 23). The James H. Clark Center, designed in 2003 for the Bio-X initiative at Stanford by noted British architect Norman Foster, opens up working laboratory spaces to full view through floor-to-ceiling external glass walls on three stories. The stunning new building has attracted tours and random visitors who confront signs, pasted all over the glass: "Experiments in progress—no public tours" and "Please do not ask to open the door!!!" The Clark Center suggests that what Ophir and Shapin found in the seventeenth-century house of experiment gets materialized still: the "site is at one and the same time a mechanism of social exclusion and a means of epistemically constituting conditions of visibility" (1991: 14).

Finally, the materialization of science in buildings plays both sides of yet another fence: standardization and differentiation. The Lewis Thomas Laboratory at Princeton University, completed in 1986, is very much like every other university molecular biology building of the same vintage and, at the same time, is architecturally unique. Almost nothing in the list of functional spaces (research labs, offices, support facilities, seminar rooms) or in the arrangements of benches, desks, sinks, and fume hoods within a research lab or in the infrastructural guts of the place (wiring, piping, conduits) makes the Lewis Thomas Lab stand out from its peers. It is as if the biotech building itself had been cloned, at universities all over (Gieryn, 2002). Neo-institutional theory from sociology predicts that *bureaucratic* structures in research organizations will become increasingly isomorphic (DiMaggio & Powell, 1991; Meyer & Rowan, 1991), but the same social processes may also cause a homogenization of the *physical spaces* that house such activities. Safety codes and requirements of the Americans with Disabilities Act coerce architects to conform to an approved legal standard. Professional trade associations such as Tradelines, Inc., bring architects and university facilities managers together at international conferences where design innovations are given either a "thumbs up" or a "thumbs down," creating a normative context in which few designers decide to go against the grain. Peripatetic scientists remember desirable features from a lab they visited recently and implore architects to design just the same thing for their proposed new building—a kind of mimesis. Moreover, a measure of institutional legitimacy is secured when a lab looks much like all the other successful labs elsewhere (indeed, the very presence of a laboratory legitimates some fields as genuinely scientific—like psychology [O'Donnell, 1985: 7] or physics [Aronovitch, 1989]—in their early days).

Importantly, these social processes responsible for the standardization of laboratory design are analytically distinct from their epistemic consequences. "The wide distribution of scientific knowledge flows from the success of certain cultures in creating and spreading standardized contexts for making and applying that knowledge" (Shapin, 1995: 7). With the rubbing out of idiosyncratic design elements, scientific laboratories become generic "placeless places" (Kohler, 2002), enabling scientists to presume that the "ambient" conditions in a laboratory here are equivalent to those anywhere else. This homogenization of space is vital to the flow of scientists, scientific instruments, specimens, and inscriptions from site to site: geographical location may change, but the mobile unit finds itself "at home" on arrival, in a set of circumstances not dramatically different from those where it started out. Ironically, the very "circulation" of scientific claims and objects is dependent on the materialization of equivalent standardized places where science settles down. For us, this signals the continuing *importance* of place for science, rather than its evisceration. Moreover, research on "situated activity" (Suchman, 1987, 1996, 2000; Lave, 1988)—paying attention "to the ways the body and local environment are literally built into the processing loops that result in intelligent action" (Clark, 1997: xii; Hutchins, 1995; Goodwin, 1994, 1995)—invites the possibility that standardized work spaces in laboratories could foster a routinization of bodily activities even as scientists migrate from one university to another. In this respect, STS interest in the importance of "embodied" or "tacit" knowledge is really only half the equation; practices get routinized in part by taking place in standardized spaces.

Still, "placeless" places are not necessarily "faceless" ones. Laboratories also materialize identities for *different* social categories, groups, or organizations, and so their designs seek to differentiate "us from them." The facade of one side of the Lewis Thomas Building shows a beige and white checkerboard—a signature feature of postmodern marquee architects Robert Venturi and Denise Scott-Brown, who were hired to provide Princeton with a building that would signal the University's commitment to molecular biology and elevate its national reputation in this field. A building "just like any other" would hardly have succeeded in luring top biologists to Princeton. MIT hired celebrity architect Frank Gehry to design its recently-completed Stata Center (2004). Gehry's "controlled chaos,"[3] a wonderful jumble of boxes tilted and askew, clad in brick and titanium, would seem to have little bearing on the very orderly artificial intelligence, logic, and computer science going on inside. But MIT now has "a Gehry," and when it comes to competition for scientific talent and institutional prestige, the difference is everything.[4] In the past, laboratories assumed different symbolic skins to announce other kinds of cultural significance. Nineteenth-century science buildings at British universities draped themselves in Gothic referents as a visible sign of the respectability of experimental research, semiotically aligning the activities inside with monkish purity and devotion while distinguishing them from the pursuit of lucre expected in factories (Forgan, 1998). These days, when the line between pure research and applied research for profit is difficult to locate, corporate labs (Knowles & Leslie, 2001) and university labs may be almost indistinguishable in their external appearances (or may even be co-located).

Even the insides of science buildings differentiate social groups and assert identities—more through location and restricted access than through ornament or infrastructure. At SLAC, the top floor is for theorists and directors while the basement is for instrument shops (Traweek, 1988); at the Lewis Thomas Laboratory, mouse people had space demarcated from that occupied by scientists using yeast or worms (Levine, 1999). Galison writes that "in the floor plans we are seeing far more than pragmatically situated air ducts; we are witnessing a physicalized architecture of knowledge" (1997: 785). From the nineteenth century to now, there has been a shift in laboratory architecture, from an emphasis on the unity of science to an emphasis on disciplinary differences. Earlier assertions calling for needed "juxtapositions" of all the sciences "[were] in the main supplanted by the vocabulary of separation and of specialization, which meant the creation of separate, purpose-built architectural spaces, with all the functional differentiation in plan, construction, and equipment which attended increasing specialization in scientific research and education" (Forgan, 1998: 213).

Other sites of science reproduce fundamental societal distinctions, such as gender. Eleanor Annie Lamson's contributions to the geophysical understanding of the Earth's density and structure were diminished as a result of *where* she did her research (Oreskes, 1996). Lamson, associate astronomer at the U.S. Naval Observatory, stayed on land to process data on marine gravity, data that had been collected—in part—during expeditions using submarines (mobile laboratories). "But only men went to sea. Only the men's work could be cast as a heroic voyage to 'conquer the earth's secrets.' Therefore, only men appeared in the public eye" (Oreskes, 1996: 100). This spatialized sexism has a long history: distinguished callers at Aldrovandi's sixteenth-century Italian museum were asked to record their presence at this privileged site for witnessing nature, but "he did not ask [women] to sign the visitors' book" (Findlen, 1999: 30), recalling an even more ancient pattern of female exclusion from monastic intellectual life (Noble, 1992). Findlen (1999: 50) believes that these gendered differentiations of space for knowledge-making had lasting consequence for the presence of women in science:

> [These configurations] established important preconditions for the public understanding of scientific space, as museums and laboratories emerged from the homes of aristocrats and gentlemen to enjoy a new autonomy. Such institutions, even when divested of their former location, continued to incorporate a host of assumptions about the appropriateness of women in sites of knowledge.

In turn, materiality also served as a boundary marker for cultural change when, at the Radium Institute of Vienna (1910s), "women working on radioactivity succeeded in acquiring 'a laboratory bench of their own,' indicating a shift in political importance of the role of women in science" (Rentetzi, 2005: 305).

CONTESTING SCIENCE

Spaces for science are a powerful blend of material infrastructure and cultural iconography that lend credibility to knowledge claims. And yet the situatedness of science in discrete geographical locations creates at the same time a certain vulnerability to challenge and contestation. Latour (1983) famously wrote: "Give me a laboratory and I will raise the world." But you can also throw a rock at a laboratory, break into it, and burn it down. Much as Foucault (1980) argues that the exercise of power always goes hand-in-hand with resistance, the very materiality of scientific sites makes them good targets, a kind of "contested terrain" (Edwards, 1979) where actors with divergent interests have something to dig in to and hold on to, in both the literal and figurative senses. The capacity of physical sites to authorize knowledge claims is never automatic or permanent; credibility emerges instead from a "negotiated order" (Maines, 1982; Fine, 1984), where scientific spaces become the loci for resistance and the negotiation of consent.

Scientists themselves assert that they have a unique and privileged way of seeing the places of knowledge-making, a view that is generally uncontested by nonscientists:

> The "doubling" of space in the places of knowledge means that two people looking at the same spot on the ground . . . might construe two different objects. And this "double vision" would flow from the fact that the one person is an officially competent and authorized inhabitant of the space while the other is a visitor or a support worker. Nor do modern sensibilities regard this phenomenon as anything out of the ordinary (Ophir & Shapin, 1991: 14).

On their own turf, the scientist's vision is hegemonic, trumping other ways of seeing. But scientists sometimes find themselves on other terrain, where their understanding of place is less privileged, and where nonscientists seek to establish their own authority over its representation. Recent STS studies of "field sciences" have found examples of this kind, where the boundaries between scientific and other ways of knowing places—as well as the boundary between laboratory and field itself—are blurred and contested (Bowker, 1994; Kuklick & Kohler, 1996; Henke, 2000; Kohler, 2002). The potential for conflict revolves around the materiality of places, and especially the place-bound interests that actors may have in particular sites—often quite different from scientists' interests in the same place.

Farmers, for example, have particular ways of growing crops that represent a kind of investment, a commitment to the interface of place and practice that structures their modes of production and colors their perceptions of new agricultural techniques. Henke (2000) has studied University of California "farm advisors"—scientists employed by the University but stationed in specific farm communities, charged with improving the production practices of local farmers. Farm advisors frequently use an experimental technique called a "field trial" to demonstrate to the local farm community the advantages of a new agricultural method or technology. These demonstration trials are often conducted on a farmer's own land because farmers simply do not accept "immutable mobiles"; they are more likely to trust results that take into account the local contingencies of their own place (climate, soil types, cultural practices, etc.). The overall objective of the field trial, then, is to adjust an experimental mode of knowing place to one that accords with farmers' ways of seeing their land. In effect, the field trial represents farm advisors' attempts to negotiate a compromise that will incorporate both the standardization of experimental practice and farmers' prejudice for place-bound data.

These kinds of negotiations trouble an easy attribution of epistemic authority in the field. When science seeks to shape places in the field, as in examples of applied science, other actors may be empowered through their own place-based knowledge. One way to explore these divergent "ways of knowing" place is through the study of environmental hazards. A canonical example in STS is Brian Wynne's (1989) study of negotiations between British government experts and sheep farmers jointly dealing with the effects of radioactive fallout from the Chernobyl power plant explosion. In the aftermath of the disaster, experts dispatched to the affected sheep farming area in Cumbria "assumed that scientific knowledge could be applied without adjusting to local circumstances," which greatly damaged their credibility with the sheep farmers (Wynne, 1989: 34). The story is similar to other conflicts over environmental risk, where the place-bound, experiential knowledge of local actors—variously described as "lay persons," "citizens," or "activists"—challenges the reductionist and supposedly universal techniques deployed by experts for assessing hazards (Martin, 1991; Tesh, 2000). Rejecting models of risk perception that posit a divide between fundamentally rational and irrational modes of perceiving risk,[5] many of these studies focus on the knowledge of place that comes from a long-term, bodily residence in a specific site. These "bodies in protest" (Kroll-Smith & Floyd, 1997; Beck, 1992) argue for the credibility of a more informal knowledge, one grounded in experience and place.

At the same time, many of these studies also show that communities responding to environmental hazards work to ally themselves with experts or to gain their own formal expertise in the methods of environmental risk assessment (Macnaghten & Urry, 1998; Fischer, 2000; Allen, 2003). The work of these "expert-activists" (Allen, 2003) makes an interesting comparison to Henke's case of applied agricultural science. On one hand, the University of California farm advisors tried to balance the formal and universalizing methods of science with an acquired knowledge of the specific geographical places where farmers grow their crops. On the other hand, communities that challenge expert assessments of local environmental risks sometimes choose to augment their own experiential and embodied understanding of place with more technical and institutionally-credentialed methods of measuring hazards. In each case, there is the potential for a fully "double vision" of place, drawing on both the scientific and the experiential—indeed, for this reason, some STS scholars have begun to deconstruct the very divide between "expert" and "lay" understandings (Tesh, 2000; Frickel, 2004; Henke, 2006).

Interestingly, scientists engaged specifically in field studies have historically faced their own problems of credibility, brought into high relief when their research was contrasted (often unfavorably) to laboratory experiments in which the relevant variables may be far easier to control. Laboratories and field sites have their distinctive epistemic virtues as places where legitimate natural knowledge gets made, leading to contestations between the rival "truth-spots" in disciplines as varied as biology (Kohler, 2002) and urban studies (Gieryn, 2006): labs maximize precision and control, but the field seems less of a contrivance and closer to the way Nature (or Society) really is. However, just as the distinctions between expert and lay understandings have been obscured almost beyond recognition, so too is the figure of "laboratory vs. field-sites" something more than a simple opposition of cultural practices and epistemic legitimations. Gieryn (2006) finds that members of the Chicago School of urban studies (1900–1930) constructed the city as *both* a laboratory *and* a field site. They oscillate (in their texts) between making Chicago into a specimen sliced and diced for statistical analysis and making Chicago into a found place best understood ethnographically through patient and absorbing long walks. Kohler suggests that by the 1950s, a variety of borderland or hybrid sciences had emerged in biology that drew variously on the epistemic virtues of both lab and field: "Traffic between laboratory and field no longer necessarily involved passage across a cultural frontier, or even physical movement from field to laboratory or vice versa" (2002: 293). Contestations over those places most suitable for making scientific knowledge need not persist forever.

A more graphic kind of emplaced contestation over the cultural authority of science will probably be more difficult to resolve. Whether science is located in the laboratory or in the field, the materiality and geographic specificity of places where research is conducted gives protesters a concrete target to attack, as in the case of break-ins, vandalism, and outright destruction of experimental places. However dramatic such assaults on science might be, they have received little systematic attention in STS. One well-known example happened on August 24, 1970, when activists opposed to U.S. involvement in the Vietnam War set off a bomb at Sterling Hall, on the University of Wisconsin campus in Madison. The building housed the Army Math Research Center, and the attack was designed to disrupt research allegedly focused on the development of new weapons. The explosion killed graduate student Robert Fassnacht and, prior to the Oklahoma City bombing in 1995, represented the largest bomb blast set off as a form of domestic protest in the United States (Bates, 1992; Durhams & Maller, 2000). More recently, animal rights groups have destroyed laboratory equipment as they liberated mice and monkeys from what is, for them, inhumane experimental handling (Lutherer & Simon, 1992). These attacks on the places of science show how impossible it is to sequester research from political turmoil, although cage rooms at animal labs are routinely shrouded in security and surveillance systems worthy of a bank vault.

Field sites for testing transgenic crops have also been frequent sites of protest and vandalism, at least since the technology was field-tested and became commercially available in the 1990s. Environmental activists concerned about possible hazards in the transfer of genes from transgenic to non-transgenic organisms—or potential disruption of ecosystems more broadly—have opposed the release of genetically engineered organisms "into the wild." At field-trial sites across the world, protesters have destroyed transgenic crops in an attempt to prevent the spread of genetic materials beyond the borders of the site (Cooper, 2000; Anon., 2002). These sometimes violent interventions center political attention on the boundary between a supposedly controlled space inside the laboratory and the unpredictability of placing research in the field. A Greenpeace press release condemning New Zealand's decision to allow field trials of transgenic crops asserts, "The only safe place for genetic research is a properly contained laboratory" (Greenpeace, 2001).[6] There is much irony in that assumption: laboratories and experimental field sites are designed and built to bring wild nature under control, to render specimens docile and compliant with the instruments and theoretical ambitions of the scientist. And yet, by putting science in a place—by giving an available and material home to the process of knowledge-making—sites are created that cannot render docile and compliant those human specimens who have cause to challenge the means, aims, and authority of science.

CONCLUSION

Martin Rudwick's map of 1840 London begins to suggest how the places of science have changed. With labels pointing to major scientific institutions and to residences of scientists important for the Great Devonian Controversy, the map plainly indicates the "small scale of scientific London" (Rudwick, 1985: 35). Everybody that one needed to talk to, every book or specimen to consult, every association meeting to attend, was (almost literally) just around the corner. How different things are today: relevant experts and major research centers are now scattered throughout the world, scientists collaborate with those on another continent (but not always face to face), and they analyze data from places they have never been themselves—sometimes gathered by remote sensing devices and then digitized and stored in a computer whose location does not really seem to matter. Do these changes signal that place itself has become less vital for an understanding of science in society? We think not.

Paradoxically, these historical changes in the siting of scientific inquiry could make the production of new knowledge easier and faster but make it more difficult to trust the received results. So much confidence in the credibility of scientific claims stems from widely shared assumptions about *where* processes of discovery and justification take place, and about the people, instruments, specimens, inscriptions and infrastructure assembled right *there*. As distal observations are increasingly mechanized and as data are increasingly standardized and made instantly (and anonymously) available to scientists everywhere, legitimate concerns about the "chain of custody" arise: exactly where did these data come from, who was present at their initial construction or later manipulation, and who ultimately is accountable for their validity? As scientists disperse themselves globally and replicate laboratories hither and yon, questions of credibility will grow (not cease): was the experiment done in architectural circumstances that enabled the collective witnessing and scrutiny that is (for some) the touchstone of scientific objectivity? Ironically, place was once thought to pollute the credibility of science—merely local knowledge was parochial and idiosyncratic and thus untrustworthy. Now that the production of scientific knowledge has gone global with a vengeance (the view today is from Everywhere), place will reassert its significance for science as ratifier of authenticity and trust.

Notes

1. For reviews of this literature, see Ophir & Shapin (1991) and Livingstone (2003). Two recent special issues of journals in the history of science have focused on geographical topics; cf. Dierig et al. (2003) and Naylor (2005). Our depiction of this research as comprising a series of "waves" borrows from Law and Mol (2001). Gieryn (2000) reviews the interdisciplinary literature on "place."

2. Andrew Barry has usefully distinguished "sites of calculation" from more encompassing but discontinuous "zones of circulation," where artifacts, technologies, and practices are "comparable and connectable" (2001: 203). But before STS researchers rush to collapse sites into zones (or worse, networks), we suggest the need for a better understanding of why, how, and when those discrete sites are consequential for science.

3. Gehry is quoted in Joyce (2004: xiii).

4. Raiding other universities for scientific talent is hardly new: "Thomson's Glasgow personified and incorporated the solution to these puzzles, and several dons decided the obvious course would be to hire him for Cambridge. Thomson turned down the offer. Space and resources were what counted: 'the great advantages I have here with the new College, the apparatus and the assistance provided, the convenience of Glasgow for getting mechanical work done, give me means of action which I could not have in any other place'" (Schaffer, 1998: 157).

5. As in, for example, Douglas and Wildavsky (1983) and Margolis (1996).

6. Even laboratory-based research on transgenic crops has been targeted by activists. The best-known example is probably the fire set at the office of Michigan State University researcher Catherine Ives in 1999 by members of the Earth Liberation Front (Earth Liberation Front, n.d.; Cooper, 2000).

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