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Remember the classic science studies parlor game "Awkward Student" (Collins, 1992)? One player pretends to be a teacher, the other a pupil. The teacher provides some basic instruction, then the student makes things awkward by stubbornly finding "correct" but non-common-sensical ways to follow that instruction. The teacher then adds more rules to the basic instruction to try to make it impossible for the student to provide awkward answers. As a thought experiment, Awkward Student demonstrates the interpretive flexibility inherent in experimental practice. No description of an experimental setup can ever be complete enough that it will be safe from "awkward" misreadings by replicators. There is always room for disagreement about whether one experimenter has awkwardly or faithfully replicated the technique of another.

The Awkward Student was a heuristic cornerstone of studies of scientific controversies in that it illustrated that both sides of a controversy could reasonably believe they were correctly following directions: hence, no associational criterion could adjudicate between them. The other students in the thought experiment rely on social determinants (the authority of the teacher, the Awkward Student's status as friend, peer, etc.) to decide whom to believe; similarly, scientists in a controversy must use social cues such as trust, class, nationality, gender, and age to help them decide who has done an experiment correctly. Almost all the early controversy studies, though focused not on student-scientists, but on disputes between well-established peers, researchers in the prime of their careers (Collins, 1975, 1998; Pickering, 1984; Finch, 1985; Mackenzie, 1990; Shapin & Schaffer, 1985). This made sense at the time in that controversies between peers were the "hard case" in which both sides could equally command authority, respect, and resources (hence, peer controversies were a more reliable test of the ideas of the new sociology of science).1

We suggest, though, that the pedagogical setting of the Awkward Student should be taken seriously. The student is "awkward" because he or she defies both common sense and the norms for the behavior of respectful pupils. Yet the student is also creating a kind of knowledge by pointing out alternative interpretations. Thus, in the classroom, like the laboratory, knowledge is simultaneously taught and created. Indeed, this interpretation of the Awkward Student is perhaps more faithful to its Wittgensteinian roots than to the uses to which its creator (Harry Collins) and other exponents of
controversy studies have put it. Even a quick review of Wittgenstein's life and work (Wittgenstein, 1953; Monk, 1996; Cavell, 1990) reveals the centrality of practices of pedagogy, training, schooling, and upbringing in his ethical and philosophical thought. Wittgenstein saw education as a site where meanings and values are generated, not just conferred.

Nor is Wittgenstein alone among the forebears of STS for having articulated new, overlooked insights about pedagogy. Most prominently, Thomas Kuhn and Michel Foucault both accorded pedagogy an important place in their analyses of science. Science for Kuhn and Foucault was not merely a positive, cumulative body of facts, but a web of practices, tools, and relationships that must be learned in order to be lived. Foucault, of course, focused on the architecture and bureaucracy of pedagogy, on the physicality and ubiquity of regimes of surveillance that co-produce knowledge along with subjects who know and are known (Foucault, 1977, 1994). Kuhn (1962), meanwhile, drew attention to the tools and time scales of training, to the ways textbooks, problem sets, and the succession of student cohorts generate "normal science" as a whole.

Building on the work of Wittgenstein, Kuhn, and Foucault, we interpret pedagogy broadly in this essay, not merely as formalized classroom teaching techniques—although these are certainly important—but rather as the entire constellation of training exercises through which novices become working scientists and engineers. This pedagogical dimension has been an important but understudied ingredient in many STS narratives. The classic stories of STS often unfold in modern research universities or other settings where teaching and training are overt, even primary, institutional motivations (e.g., Collins, 1974; Galsan, 1987; Woolgar, 1990; Lynch, 1983). Yet knowledge-making is the primary focus of these studies, and the protagonists' roles as teachers and/or students are subordinated to (or invisible beside) their roles as researchers.

At the same time a more Meritian-institutionalist strand of history and sociology of science has analyzed the mechanics and evolution of scientific training (Rosset, 1982, 1986; Kohler, 1987; Owens, 1985). These latter studies have artfully shown how pedagogical institutions can mirror and drive wider cultural change, and how training regimes structure and organize the colleges (invisible to us, and otherwise) of science. Knowledge, though, is usually taken as an unproblematic product in these Meritian stories. These authors acknowledge that new understandings of the world emerge from pedagogical settings, but how institutional structure and pedagogical imperatives shape the content of scientific knowledge is left unexplored.

We will make a stronger claim that synthesizes these two literatures. It is no accident that modern scientific knowledge is tied to teaching and training. If science and technology studies return to Wittgenstein, Kuhn, and Foucault and makes pedagogical a central analytic category, this coincidence emerges from the fact that training in focus, we can see that, even in ostensibly nonpedagogical settings, teaching and research activities are mutually reliant. The exigencies of one activity strongly inform the practice and content of the other. What scientists know of the world is a product of culturally driven decisions about whom to teach, what knowledge to validate by passing on, how to use pedagogy in the pursuit of social interests, and how to organize education. The tools of science are closely bound to the tools of pedagogy. Since nature is equivocal about its representation, the question of which instrument, image, or equation to use is often answered by the question, "which tool most facilitates pedagogy? Which representation is most easily passed on, or most adequately manufactures a new generation that adheres to the vision and values of current practitioners?"

We follow on a growing literature that makes explicit these connections between research and pedagogy (Olesen, 1991; Leslie, 1993; Kohler, 1994; Dennis, 1994; Warlick, 2003; Kaiser, 2005a,b). Historians and sociologists of education have also offered important insights into the connections between training practices and social values in science (Geiger, 1986; 1993; Solomon, 1985; Hofstätter, 1973; Clark, 1991, 1995). Here, as elsewhere, science studies faces the paradox—science and technology are cultural activities and thus share features with other human endeavors, yet they also have (or have been accorded) a distinctive domain of practice and knowledge that presents analytical peculiarities. This essay charts a course between these alternatives, showing where ideas about pedagogy can be imported into science studies and where we must forge our own vocabulary. For brevity and coherence, we focus on the modern period, although pedagogical issues certainly were not absent from earlier periods. Likewise, we concentrate on science and technology disciplines rather than on medicine although some path-breaking work on medical education should still inspire new work on the topic (Starr, 1982; Ludmerer, 1985; Bosk, 1979; Rosenberg, 1979).

REPRODUCTION

Two principal questions lurk behind all decisions regarding scientific training: why and how? Why should a society expend so much capital and effort to train new generations of scientific workforce, and how should their training proceed? Neither question has an automatic answer rising above the vagaries of time and place. In this section, we take up some prominent responses to the "why" question, as gleaned from recent studies. We turn to the "how" in the sections that follow.

Since at least the middle of the nineteenth century, nearly all practicing scientists and engineers have gone through some kind of formal training, for the center and a half has seen the decline of the "gentlemanly amateur" of science. Naturally the form of training have varied across time and place, as well as across the evolving disciplinary map (Kaiser, 2005). Yet the necessity of some form of training has emerged as the one constant across many of these distinct settings. As Sharon Traweek has emphasized, scientists and engineers must always work to reproduce new generations of practitioners, reestablishing the scientific workforce (Traweek, 1988, 2005).

The historically identifiable periods take place in all kinds of institutions, including some that are not overtly "educational." Throughout the twentieth century, for example, universities have partnered with many types of off-campus spaces to train new recruits: from exchange programs with industrial laboratories (Lowen, 1997; Slaughter et al.,
2002) to the citadels of “big science” at national laboratories (Galison, 1987, 1997; Traweek, 1988; Galison & Healy, 1992; Westrick, 2003) to top-secret weapons laboratories ( Gusstern, 1996, 2005; McNamara, 2001). In all these kinds of places, scientists and engineers work to train new members of their fields, mixing formal courses with more hands-on means of apprenticeship.

The reasons for undertaking this training are embedded within larger sociopolitical discussions. Reproduction of scientists and engineers is always a response to reproduction for: for national sovereignty or security, for economic well-being, for technological spin-offs, and so on. At the height of British imperial rule, for example, consensus emerged that Britain needed large cadres of “disciplined minds” who could staff the expanding civil service positions throughout the empire. This seemed to call for a certain kind of reproduction—one based on intense mathematical training and grueling written examinations (Warwick, 2003). Early in the nineteenth century, policymakers throughout the German states used similar arguments to encourage technical training, to build up a stock of efficient administrators (Turner, 1987). By the closing decades of the century, however, the rationale had shifted: education and industry leaders in the newly united Germany decided that the country needed large numbers of technically trained personnel to help manage the country’s late-blooming industrialization. This called for a new type of pupil to undergo a new type of training, weakening the hold of the classically oriented Gymnasien and encouraging the rapid growth of Realschulen and Technische Hochschulen, with their emphases on precision measurement and the sophisticated management of error (Pynson, 1977, 1979; Stichweh, 1984; Cahan, 1985; Fox & Guagnini, 1993; Olesko, 1991, 2005; Shinn, 2003). During the Cold War, politicians and educators in the United States, Western Europe, and the Soviet Union decided that “standing armies” of physical and social scientists were needed; the ideological battle between East and West would be fought in the classroom, a race to create the largest “manpower” reserves in nuclear physics and allied disciplines (Ailes & Rushing, 1982; Mukerji, 1989; Krige, 2000; Kaiser, 2002; Rudolph, 2002). In all these ways, scientific training has often assumed center stage in larger debates over political economy, domestic policy, and international relations.

At the level of institutions, decisions over which equipment to build and which lines of research to support are also intertwined with decisions about which types of training to foster. Should the new recruits learn individual initiative, focused around small-scale apparatus, or team sensibilities, using factory-sized equipment (Hellbrun, 1992; Kaiser, 2004; Traweek, 2005)? Should new instruments and practitioners be gauged by how well they fit into a long-established academic field, or by how well they cross boundaries, merging ideas and techniques across a wide range of specialties (Mody, 2005)?

Pedagogical institutions also serve as powerful filters. They can either encourage or impede the flow of certain types of students—such as women and minorities—into the professional pipeline. Although some seminal work has been done on women’s (often fraught) participation in modern science and technology (Keller, 1977; Rossiter, 1980, 1982, 1995; Murray, 2000; Oldenziel, 2000; Etkowitz et al., chapter 17 in this volume) and on that of minorities and non-Westerners (Manning, 1983; Williams, 2001; Sloton, 2004; Iti, 2004; Sur, 1991; Anderson & Adams, chapter 8 in this volume), much remains to be done. Beyond narrowly demographic studies, intergenerational work has examined various methodological impulses and their relationship with pedagogical infrastructure, such as the movement toward standardization and testing in the United States during the middle of the twentieth century (Lehm, 1997).

Economists and sociologists have likewise turned to this topic with zeal of late, demonstrating in detail the persistent gaps in enrollment, retention, and advancement of women and minorities in the technical workforce (Levin & Stephan, 1998; Stephan & Levin, 2005; Hargens & Long, 2002; Preston, 1994; Pearson & Fechter, 1994; Ross, 2004).

As Pierre Bourdieu and other historians and sociologists of education have long emphasized, therefore, generational reproduction is always based on a series of active choices and political-cultural decisions; training is never a neutral or passive activity (Bourdieu & Passeron, 1977; Bourdieu, 1988; Spring, 1989; Kliebard, 1999). Scientists and engineers mold their disciplines by pedagogically fashioning their disciples.

MORAL ECONOMIES

Historians and sociologists of education often talk of a "hidden curriculum," a series of values or norms—about proper behavior, civic duty, patriotism, and the like—that are embedded within schools' more explicit pedagogical operations (Arum & Beattie, 2000). So too is scientific and technical training shot through with decisions about values. Training is the central arena within which various communities craft and then reinforce their “moral economies”—often tacit conventions that regulate how members of their discipline should interact and behave, allocating resources, research programs, and credit (Shapin, 1991; Kohler, 1991). By learning these rules, year- long recruits learn these rules for behavior as part of their formative training; they learn what it means to be a scientist or engineer as they learn how to wield the tools of their trade. Just as responses to the “why” question—why undertake the labor-intensive task of replenishing a technical workforce?—these “how” questions show revealing variation across time and place. At stake are older generations’ aspirations and expectations for the new recruits’ behavior, as well as up-and-coming trainees’ own evolving self-image, including what they deem appropriate in their new roles (Daston & Shum, 2003).

One thing that technical training imparts is a set of expectations or guidelines for acceptable behavior. For example, the students in Franz Neumann’s nineteenth-century physics seminar in Königsberg, whom Kathryn Olesko (1991, 1995) analyzed, internalized a specific lesson about proper comportment. Neumann’s students cultivated an “ethos of exactitude,” learning to value rigorous error analysis above theoretical speculation. Calculating least-squares deviations for discrete data points, rather than relying on graphical interpolation (which, they feared, mixed data of different degrees of quality), was more than a mathematical exercise—it became a badge of
integrity. The Victorian undergraduates at Cambridge University whom Andrew Warwick (2003) has studied internalized different lessons about the scientist's proper role: success was bred from strict discipline. Unwavering mental concentration, only be achieved, they came to believe, by maintaining rigid schedules, interspersely competitive athletics (such as rowing) with coaching sessions with their mathematic professor and several hours each day of solitary study. The reliance on individual mathematical virtuosity that this regime fostered meshed poorly with other types of training in the late nineteenth century, such as group-based on-site engineering apprenticeship, leading to bitter conflicts over what type of training—and hence what type of person—would best command the new terrain of electrical engineering (Goodyear, 2004, 2005).

Often the pedagogically reinforced moral economies are deeply gendered. The postdocs in high-energy physics whom Sharon Traweek followed during the 1970s and 1980s, for example, internalized the lesson that they needed to brashly display their independence—it was no longer sufficient to complete their assigned tasks competently, as might have been expected of them as graduate students. They learned not to ask questions in front of certain people and to roundly disparage certain types of remarks from their peers (Traweek, 1988). But even the resources for research can be imbued with symbolic meaning. For example, dozens of leading American physicists worried that the rapid influx of federal funding after World War II was spoiling the value of the new generation. They cast a suspicious eye on the hordes of graduate students flooding their departments, complaining that the new recruits treated physics like a 9-to-5 job, a mere career rather than a calling. Many of the new students, meanwhile, daydreamed of parlaying their scientific training into a comfortable middle-class lifestyle. As their tools of training shifted from one-to-one interactions with faculty to ever-larger group projects, their self-identity tended more and more to the practical teamwork rather than the individual Kulturträger (Kaiser, 2004; Hermannowicz, 1998).

Of course, the proper behavior of "practical teamworkers" has not been constant across time, place, or field. As Robert Kohler (1994) has shown, the young "drosophilists" who flourished during the early decades of the twentieth century forged a distinctive pattern of behavior, centered around the exchange of fruit fly stocks. These exchanges should always be represented as lavish, never for cash; always swapped with "full disclosure" on both sides about research plans and know-how; and while research problems could be "owned," tools and materials could not be. To be a functioning member of the fruit fly genetics community meant adopting these customs and shaping one's behavior and practices accordingly. Many concerns have been voiced more recently, meanwhile, about the purported threats to long-standing scientific values by corporate interests on university campuses. Should graduate students postdoc, and faculty learn to chase the bottom line (in the form of proprietary information controlled by industrial sponsors) or labor for the free exchange of scientific and technical information? The heat and light such questions can elicit reveals a contemporary moral economy in transition (Hackett, 1990; Mody, 2006).

Scientific and technical training thus forges communities of practitioners who share broadly similar values, norms, and self-understandings. Students must learn what it means to be a scientist or engineer—not (or not only) in the abstract, but as enacted through daily interactions within specific settings. Throughout their training they internalize these lessons, acculturating to their discipline's moral economy.

PRACTICES AND SKILLS

Studies such as Kohler's shed light on more than one aspect of the "how" question: not only how a distinct moral economy is forged within a scientific community but also how new members of that community craft research practices and pass them along to new recruits. As Kohler (1994) demonstrates, Drosophila melanogaster was never a research tool outside of a specific community of drosophilists and a specific set of social, political, and economic ties that these researchers forged and shared. It took a lot of work to domesticate the nascent community of fruit fly investigators to share their stocks of mutant fly varieties, communicate their findings, and regulate intellectual property claims. All the while, these same drosophilists had to work hard to domesticate a particular variation of the fly into a useful and interpretable tool, just as moral economies are substantiated through pedagogy, so too are the tools and techniques that make up everyday scientific life.

Such a focus on scientific practices and embodied skills represents a return to a previously forgotten Kuhnian legacy. As Joseph Bourse (1878) has analyzed so clearly, two distinct visions of science reside within Thomas Kuhn's Structure of Scientific Revolutions. The dominant interpretation, which so exercised historians and philosophers on the book's publication, centers around conceptual worldviews, incommensurable paradigms, and the complexity of observations. Yet another, also by Kuhn, is that the reliance on science practice as a test of paradigms, and the incorporation of distinctive methods within a reigning paradigm. During the past two decades, scholars in science and technology studies have capitalized on this second Kuhnian motif, developing sophisticated means of analyzing the percolation of local practices in daily scientific work (Lynch, 1985a, 1993; Collins, 1992; Shapin & Schaffer, 1985; Galison, 1987, 1997; Pickering, 1995; Fujimura, 1996; Cocking, 2002; Warwick, 2003; Mody, 2005; Kaiser, 2005a). This burgeoning literature on scientific practice can be pushed further still by incorporating Kuhn's famous focus on scientific training. "Practices," after all, must be practiced.

Sometimes scientific practices are inculcated via explicit means, such as the circulation of texts. Education scholars, for example, have scrutinized how formal curricula get forged and promulgated. Major initiatives, such as the quintessential Cold War "Physical Sciences Study Commission" (PSSC) in the United States, have provided spaces for new teaching materials, ranging from textbooks to exercise workbooks, films, and lecture demonstrations. The challenge always remains, of course, how to align the specific goals of their authors with those of the teachers and students who encounter these texts in the classroom (Rudolph, 2002, 2005; Donahue, 1993).
Historians of chemistry have also been at the forefront of studying elements of explicit instruction such as textbooks. Contrary to the dog view of scientific textbooks (propounded by Kuhn, among many others), these books are often much more creative than usually thought. Scientific textbooks are rarely state repositories of finished work, or even logical reconstructions of reigning theories. Rather, for more than two centuries textbooks have provided authors, publishers, teachers, and students a forum for intellectual and pedagogical improvisation. Several prominent chemists, such as Antoine Lavoisier, Dmitri Mendeleev, and Linus Pauling used their textbooks to formulate—not just disseminate—their new visions of chemical knowledge and practice. Scores of other textbook authors, most of whose names have long survived with the same prominence to the present day, would experiment with their chemical textbooks, figuring out novel ways of treating such complicated topics as atomism, classification, and valence, along with their preferred protocols for investigating them (Hannaway, 1975; Lundgren & Rensaude-Vincent, 2000; Gordijn, 2005; Garcia-Belmar et al., 2005; Park, 2005). Scientists in other physical sciences have likewise fashioned their textbooks as instruments in on-going intellectual debates, delightfully assembling collections of tools and techniques for ready cultivation (Olesko, 1993; Kaiser, 1998, 2005a; Warwick, 2003; Hall, 2005).

Drawing on a long line of research, leading from Michael Polanyi (1962, 1966) through Harry Collins (1974, 1992) and beyond, several STS scholars have also interrogated nontexual means by which scientists have sought to transfer research practices and skills. Scientists and engineers have fashioned several distinct methods for trying to instill in their students the "tact knowledge" needed to become competent practitioners. Early in nineteenth-century Germany, for example, physicists like Franz Neumann and Friedrich Kohlrausch taught a new type of seminar, coordinating the seminar's curriculum with more formal lectures and creating new sets of hands-on teaching exercises that the students could work on together (Olesko, 1991, 2005). Cambridge University, meanwhile, underwent a major pedagogical realignment around the same time, shifting away from a culture of Latin oral disputations and catechetical lectures on authoritative texts to paper-based examinations. Central to these changes became the Mathematical Tripos, a grueling nine-day written examination that capped students' undergraduate studies. The text-based Tripos set in motion several further changes in the instructional paradigm. Students would work in small teams with ten or so free-paying students at a time, training them to tackle progressively difficult problems. Texts materialized to the new Tripos regime but only within an elaborate framework for instilling the local coaches' tact knowledge (Warwick, 2003).

Meanwhile new traditions of laboratory-based instruction took root throughout the United States and Great Britain, emphasizing hands-on techniques rather than sole book-learning as the key to pedagogical success (Hannaway, 1976; Owens, 1985; Kohler, 1990; Gooday, 1990; Hentschel, 2002; Cudahy, 2003). Even further removed from text-based training was the apprenticeship model adopted by Victorian engineers, which often set itself in explicit contrast with the Cambridge Tripos tradition (Gooday, 2004, 2005). Research schools have flourished throughout the nineteenth and twentieth centuries across Europe and North America, fostering the inculcation of in-house research techniques (Servos, 1990; Gerson & Holme, 1995).

In the twentieth century, scientists and engineers in several disciplines have turned more and more to postdoctoral training. Although today the "postdoc" stage often functions primarily as a "holding pattern" for young researchers—stuck waiting for a more permanent position, carrying the largest burden of day-to-day tasks in the laboratory, often without receiving full credit for their labors (Davis, 2005)—it has not always been that way. Postdoctoral training was originally developed with several goals in mind: it was meant to allow young scientists and engineers to develop the storehouse of tacit knowledge and practical skills that they would need to launch their careers, supplementing the formal course work that had filled an increasing proportion of their graduate training. Postdoctoral training, in other words, was designed to cultivate non-text-based practices and skills. Moreover, postdoctoral appointments often drive the circulation of these tacit skills. They last only a few years, and students usually conduct their postdoctoral research neither at the institutions at which they earned their doctorates nor at the institutions in which they will establish their careers (Thewes, 1998; Asmussen, 1993; Delamont & Atkinson, 2001). Hence, postdocs have been custom-designed to cultivate tacit knowledge and spread it across separate communities of practitioners, leading to "postdoc cascades" driving the transfer of skills (Kaiser, 2005a; Mody, 2005).

The result: only after extensive practice, drawing on a combination of text-based and tacit routines, do research skills become second nature for new technical trainees. Only after intense pedagogical inculcation do new recruits develop the "disciplined seeing" or "hands" of accomplished practitioners (Goodwin, 1994, 1997; Doane, 2004; Mody, 2005).

**DISCIPLINES, POWER, AND INSTITUTIONS**

Training thus generates scientific knowledge by creating the tacit skills that are an inalienable part of scientific understanding and by acclimating researchers to the tools, questions, exemplars, and outlooks that constitute scientific disciplines. Controlling the levers of education can, therefore, be a powerful tool in promoting one paradigm over another, one technical culture over another. Promoters of worldview are, at heart, seeking to realize their picture of the ideal cultural agent, the scientist who know and lives in a world consistent with a particular paradigm. Training and pedagogy are imbued with the politics of competing images of the ideal practitioner; survivors of this competition determine which inherited traditions will be seen as appropriate for pedagogical propagation. Foucault (1970, 1977), for one, made this point clear in his study of "discipline" and disciplines. Pedagogy is a variety of social control; not only is knowledge power, but so is the ability to decide which social and academic discourses are suitable recipients of what institutionally enshrined knowledge. Moreover, education is not merely the transmission of knowledge; it is a license to bend its subjects
to an authority's view of the world, to make them move, talk, and eventually think like "normal" citizens."

Arguments about training methods are, therefore, integral to the formation of scientific disciplines and the maintenance of boundaries between them (Geyrn, 1983; 1999). Examination of the institutions of technical training, then, offers dramatic examples of how disciplinary jockeying over "jurisdictions" (Abbott, 1988) or terrains of work and expertise intersects with institutional jockeying for power and resources. For instance, as the engineering disciplines were professionalizing in the United States (and elsewhere) from the 1880s to the 1920s, many of the questions at stake in professionalization (e.g., does the engineer serve a client or the public?) Who should count as an engineer? Are engineers technical experts or executives? were played out among the faculties of schools like the Massachusetts Institute of Technology (Layton, 1977; Noble, 1977; Servos, 1980; Carlson, 1988).

Importantly, both these discourses have both a disciplinary and a local aspect. As Christophe Lecuyer (1995) has shown, MIT's future was contested in these years by factions of faculty and local elites who organized around different notions of the relationships between science, engineering, and political reform. Some faculty members saw engineering education as a populist alternative to Harvard and other bastions of "classical" studies; and constructed MIT as a "school of industrial science." Others viewed engineering as (more) "applied science" and attempted to implement that definition by turning MIT into a research school, training its students in basic science and sending them out to apply that knowledge as engineering. Somewhat later, Dugald Jackson and his allies promoted a vision of engineering as a branch of management and pushed for an MIT that would train engineers to serve the giant research-oriented firms of emerging corporate America. The MIT that emerged was defined by a later generation of professors who had trained under (and hence were loyal to) the early populist faction, yet whose careers depended on the "applied science" and "engineering as management" factions—that is, training and career came full circle as determinants of MIT's organization.

Thus, institutional and disciplinary politics can, through standards and curricula, make real the different visions of what a discipline is and how it relates to its competitors—technical communities compete in institutional terms and are recruited by those who will make credible the disciplines' claims to work jurisdictions. Yet, as Foucault pointed out, pedagogy is not just standards and curricula; it is a process that unfolds within specific places and architectures (not limited to universities, of course) via the maintenance of specific relationships of power. Education brings its subjects within the reach of power; students are not merely taught but also watched, graded, measured, tested, punished, and otherwise surveilled and disciplined on their way to becoming full-fledged members of society and practitioners of their field. These observations have slowly filtered into STS, but often in the context of emphasis on pedagogy. Much of the past decade's interest in the architecture of science (Galsly & Thompson, 1999; Geyrn, 1998; Lynch, 1991; Thompson, 2002; Hannaway, 1986; Henke & Geyrn, chapter 15 in this volume), for instance, derives from Foucault's focus on built environment, yet few STS scholars have analyzed the experimental workplace as a pedagogical site that simultaneously fosters knowledge creation and the training of students of knowledge and power are often most closely linked when they are most asymmetrically distributed, in particular when knowledge becomes a tool for advancing state power. For instance, in the heat of the Cold War, nuclear weapons designers at America's national laboratories gradually instituted a rich system of training in which young designers were apprenticed to their elders, with novices slowly demonstrating to their overseers that they had learned the necessary tacit skills through participation in the ritual cycle of nuclear tests (Gusterson, 1996; McNamara, 2001). Out of this system emerged fully mature designers who were enculturated to the national strategy of deterrence (a strategy) many outside the labs found unfathomable) and who saw weapons science as a tool for world peace and security.

Of course, when nuclear weapons testing ended in 1992 and the pool of master designers to whom novices could be apprenticed shrank, the mesh of pedagogy and power began to unravel. Today, the U.S. nuclear establishment is obsessed with the problem of "knowledge loss" and has moved away from the informal apprenticeship model and toward classroom instruction, archival, oral histories, and even ethnography to formalize the tacit knowledge thought to reside with older designers. Yet one result is that designers who grew up in the testing era now feel like "dinosaurs" and mourn the loss of a once vibrant training culture (Gusterson, 2003).4 Systems of pedagogy, then, can offer a rare window on the microdomains of international politics, the emotional attachments of scientists and engineers to their epistemic cultures, and the complex phenomenon of knowledge determinism.

In other cases, pedagogical regimes put in place to reinforce national objectives and asymmetries of power can be redeployed by those whom training is meant to discipline. For instance, in colonial India, Western science was promoted by British administrators and seized on by local elites as a way to enculturate Indians to British values and practices and to preserve those elites' position in Indian society (Prakash, 1992; Ila & Rabb, 2004; Chakrabarti, 2004). Institutions for training Indians in Western science—museums, schools, agricultural extension—sprang up (Dominiak, 1998). Through these institutions, many Indians accepted Western knowledge as a yardstick of progress and took on the cultural models of a progressive, enunciated colonial subject through which that knowledge was exported.

Yet Indians also came to use the infrastructure of scientific training as a means of pushing the colonial state to take greater responsibility for its subjects and for building networks that subtly subverted the imperial relationship. For instance, as Ian Petrie (2004) has shown, the state's response to a series of late nineteenth century famines in India was to try to structure rural life using the latest in Western science, with the agents of these changes were, in many cases, to be young Indians sent abroad for study. After 1905, though, these men increasingly traveled to land-grant colleges in the United States, both to learn about crops (sugarcane, cotton, rice) that they would
be less likely to study in Britain and to absorb Progressive models of education and culture that many Indian intellectuals found "purer and healthier" than British analogues. That is, the infrastructure of technical education allowed Indian intellectuals to construct an idea of the United States as a more wholesome alternative to the Raj, an alternative that both put pressure for reform on the British administration and forged pathways of international cooperation that widened after independence.

**IMPURE PEDAGOGIES**

Power relations, of course, rarely run in only one direction. As the example of colonial India shows, pedagogy's use in maintaining discipline and order is never wholly successful. This is perhaps even more the case in the hybrid, impure world of modern research. Laboratories are (and have long been) diverse sites containing participants from a variety of disciplines, at different stages in their careers, and positioned in different parts of the lab hierarchy. STS scholars have recently become fascinated by such "trading zones" (Gallison, 1997) as a synecdoche for larger changes in the disciplines and the creation of a global knowledge economy. It is an obvious, though underemphasized, point that pedagogy is a continuous and pervasive aspect of such trading zones. With representatives of so many different disciplines and so many "novices" and "experts" in one place, modern research organizations are rife with pedagogy; their habits must teach each other their skills and knowledge in order to forge even the most temporary working language. In such a situation, power relations are continually reconstructed through pedagogy. As Sally Jacoby and Patrick Gonzales (1991) have pointed out, modern research is so complex that no one can understand the entirety of even a single project. As often as not, "novices" (graduate and undergraduate students) can be seen teaching the "experts" (their advisors, who may be more senior but may have lost touch with lab work and not understand the particularities of research).

Moreover, numerous studies, particularly of the development of scientific instruments (Rasmussen, 1997; Bromberg, 1991; Mody, 2004) have shown that the informal training of awkward newcomers at a diverse research site can foster revolutionary insights. As has been well documented, the circulation of postdocs and other junior researchers allows research labs to both adopt and reinterpret innovations developed in the postdocs' home institution, as well as to re-export postdocs to market their adopted home's practices, knowledge, technologies, and worldview (Mody, 2005)—the so-called postdoc cascade (Kaiser, 2005a). The continual exchange of graduate students and postdocs among clusters of academic, commercial, and government researchers helps knit together instrumental communities (Slaughter et al., 2002), and the back-and-forth of students across national borders co-constructs knowledge and foreign policy (Gordin, 2005; Ito, 2005; Martin-Rover, 1995; Martin-Rover & Carlson, 1985). Instead, since September 11, 2001, the global trade in postdocs and graduate students has sparked major policy debates, as visa restrictions in the United States have encouraged foreign students to seek training in other countries, and as the booming economies of China and India have allowed those nations to import postdocs to staff their fast-growing educational infrastructure (Anon., 2005).

We argue, then, that the heterogeneity of modern research drives discovery best when it is coupled to pervasively pedagogy. As the communities of practice literature has illustrated, organizations innovate when they contain people who need to be taught (Wenger, 2000). "Trading zones" have been successful sites of research at least in part because they always contain people who are "awkward"—newcomers who elicit instruction and who provide an insider/outsider perspective. Teaching, training, and learning push researchers to reconsider their practices and introduce mutations that advance discovery. When practices are released and replicated, they are (as the Auditor Student shows) never replicated in the same way. In much of STS, this is taken to be a problem in need of explanation, a site of disagreement and controversy. Often this is the case—teaching and learning are rarely free of disagreement—but as often as not replication offers a new way to do things, a chance to unlearn old habits while teaching new ones and to generate (not just pass on) knowledge through the interaction of novices and experts.

**PAYOFFS**

**Methodological**

For ethnographers of science and technology, attention to the pedagogical dimensions of technical work can have several methodological benefits. Most ethnographers of lab and field find that aspects of the social position of the ethnographer are fruitfully shared with the social position of students and trainees. Apart from the sociologist or anthropologist, students and trainees are usually the newest entrants to the laboratory setting. Often, they have the same awkward questions as the ethnographer and many of the same difficulties in adjusting to local practices, even the same insider/outsider's critical perspective on local mores. Thus, there is room for building significant rapport with students and trainees through these commonalities of position. Although the notion of rapport has come under criticism among anthropologists in the past two decades, the building of solidarities can still be a fruitful tool in coming to understand local technical cultures. Students and trainees will often have concerns or interpretations of sub rosa practices that they are unwilling to exhibit to their supervisors. Since, in many labs, students and trainees perform most of the day-to-day experimental or observational work, most ethnographers will find it worthwhile to participate in the distinct subculture of student life in and around the laboratory—through, for example, intramural softball teams and departmental picnics and holiday parties (Collins, 2004; Kaiser, 2004).

In finding similarities between ethnography and pedagogy, sociologists and anthropologists not only calibrate their relationships with members of the laboratory, they can also legitimate their presence and their method. As the communities of practice literature has noted, newcomers are often inducted into technical practices through "legitimate peripheral participation" (Lave & Wenger, 1991)—i.e., a kind of
participant-observational status at the margins of the community’s activities. Ethnographers should recognize this kind of pedagogy—so-called sitting with Nelly—as a central tool of their own practice.12 That is, the lab and the field already contain non-formal methods for generating and passing on something like ethnographic knowledge. Ethnographers should locate these practices and incorporate them into their studies. Often, when actors’ methods resonate with analysts’, something interesting is at stake. In this case, the similarity of pedagogy and ethnography can be used to pry open the inevitability and universality of scientific knowledge claims. This removal of ships from bottles (Collins, 1992) has traditionally been accomplished through controvery studies—i.e., through analysis of turbulent times in which actors’ disagreements beg the harmony of scientific knowledge. Yet the same turbulence can be seen more routinely and less disruptively in the continual education of newcomers in the practices of technical work.14

Institutional

Historians as well as sociologists and anthropologists stand to gain by elevating pedagogy to a central analytic category. In particular, a close scrutiny of pedagogy offers a means of merging insights from the quantitative Meritorian tradition in sociology of science with more recent work in a constructivist vein. Institutions and infrastructure—features that are obsessively quantified in the tradition of “scientometrics”—matter deeply to the modern sciences. Trends that often extend beyond an isolated laboratory or two can easily be missed if the focus remains exclusively on the hyper-local. Yet these institutional trends themselves are rarely the whole story—budget lines and enrollment patterns never interpret themselves; structural changes always undetermine scientists’ reactions to them. Hence the challenge: how to interrogate what gets deemed “appropriate” for pedagogical protocols in a given setting—and who gets to decide? How do the exigencies of training—with all its dependence on political economy and institutional momentum—help condition what will be deemed “teachable” and most fitting for new recruits to practice and master (Kaiser, 2002, 2004, 2006)?

Moreover, training—as a practice to which large, important institutions (universities) are dedicated, and which all institutions must do in part—is an analytic category STS should share with historians and sociologists of organizations. In particular, the notion institutionalism in sociology—with its wide-ranging exploration of “institutional isomorphism”—resonates strongly with science studies (DiMaggio & Powell, 1983; DiMaggio, 1991). Clearly, the question of how and why technical knowledge spreads and becomes standardized can mutually cast light on why and how different organizations come to resemble each other. For instance, as Annalisa Saloainus (forthcoming) has shown, the norm for biomedical labs in the 1960s in much of North America was “small science”—lab groups of three or four people. In the 1980s, when competitive pressures on research institutions (more competitive grants, rising costs in total funding, the rise of “big biology” typified by the Human Genome Project) caused a new norm for much larger labs (20 or more people) to spread. This institutional isomorphism was associated with a certain kind of “knowledge isomorphism”—biomedicine moved toward questions that could be answered by larger groups and questions that alleviated new funding and personnel pressures. Yet, perhaps more importantly, the pedagogy of larger labs triggered a more complex kind of knowledge dispersion—extended and/or multiple postdoctoral stints, once rare in the field, became more common, and young researchers spent much more of their careers moving from one institution to another, bringing with them (and often demanding) the values and practices they had learned elsewhere. Biomedicine became an epistemic community founded on mobility of people, practices, and knowledge.

Science Education and Policy

We conclude by noting that, while the study of pedagogy is only recently (re)gaining ground in science and technology studies, the insights of STS have been percolating into science education circles for some time. Primary and secondary science educators find themselves in the middle of practical conundrums about the nature of science that most STS scholars experience only second- or third-hand. For decades, the prevailing model of precollege science education was a more or less positivist (or perhaps Hegelian) one. Students learned (in many cases still learn) an abstract, all-purpose “scientific method” involving the advancement and testing of hypotheses and the unproblematic transmission and replicability of experimental methods; the stories of a few exemplary scientific heroes, usually with little attention to the paradoxs, practices, and wider social contexts associated with those heroes; and scientific content cleaned up and dehistorized. In the past two decades, though, science educators have begun to use science studies to challenge this model and replace it with a more ambivalent, less triumphalist view of science.

The classroom is, after all, a messy place, and some science education scholars such as Bill Carlson and Gregory Kelly (Kelly et al., 1993; Crawford et al., 2000), Reed Stevens (Stevens & Hall, 1997, 1998; Stevens, 2000), and Wolff-Michael Roth (Roth & McGinn, 1998) have used STS to validate and enrich that messiness in ways that may make science more transparent, more publicly accountable, and less polarizing. Students will, after all, be.Baskan, whether intentionally or otherwise; laboratory exercises will be irreproducible, no matter how canned the procedures; and, as the debates over creationism and “intelligent design” continue to show (Numbers, 1992; Tourney, 1991; Eason, 2004), students’ locally constructed knowledge of the world will be at odds with ostensibly universal scientific knowledge handed down by technical elites. By offering a picture of science as a human, temporally and culturally situated endeavor, science and technology studies can make awkwardness in the classroom a more positive experience and can prepare students better to judge the civic contributions of science once they graduate. A properly designed science curriculum could use STS to present a wide spectrum of society appropriate science, from the re-envisioned science and engineering more attractive to women and minorities (Cunningham & Helms, 1998)—or, at the very least, prepare science students more adequately for the highly social (even political) world of technical work.
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10. See also the other work of the team of Ochs, Jacoby, and Gonzalez: Ochs & Jacoby (1997), Ochs et al. (1994), and Ochs et al. (1996).

11. Dinges (2004) nicely illustrates this by using stories from the author's own training as a synchronist operator to politicize the notion of tacit skills. See also Latour and Woolgar's (1986) use of the perspective of an awkward technician-in-training.


13. M. Love and Wenger explain, "sitting with Nelly" is shorthand for a kind of training common in college industries and the early industrial revolution. Newcomers to an organization received little or no formal instruction; instead, they sat next to an experienced practitioner ("Nelly")—most piece-workers were women, observing and asking questions until they could go and replicate the work themselves. The similarity to many kinds of ethnography should be obvious.

14. Goodwin (1994, 1996, 1997) offers excellent examples by showing how the student-ethnographer is similarly unable to see and live in the same world as the adept until they have been through an extended, embodied process of perceptual realignment.

References


STs in the classroom is not, of course, an unproblematic match. As we have tried to demonstrate, education both reflects and drives cultural values; thus schools and universities have been hotly contested battlegrounds of various culture wars, including the so-called "science wars" of the 1990s. Science educators and education scholars have furiously debated the worth of "postivist" versus "postmodern" models of science (Allchin, 2004; Turner & Sullenger, 1999). Some worry that a curriculum borrowing from STS will be unteachable or even dangerous. These debates are healthy; indeed, we encourage STS scholars to reach out and engage with the pedagogical literature more closely. After twenty years of trying, STS may find the best place for "applied science and technology studies" is in education. STS's self-image as an interdisciplinary field has so far overlooked the potential ties between STS units and educational departments; we encourage this to change. Finally, looking beyond primary and secondary schools, it is already apparent that science and technology studies can influence debates about higher education. University administrators and national grant officers are starting to read the STS literature, and STS scholars are starting to contribute to long-standing arguments about the role of the university, the corporatization of pedagogy, and the commercialization of knowledge (Croissant & Smith-Doerr, chapter 27 in this volume; Mirowski & Sent, chapter 26 in this volume). As we have tried to show, the view from science and technology studies on the pedagogy of science and engineering is now sophisticated and complex and potentially of importance to educators and students alike.

Notes

1. Remember that some early controversy studies also explicitly focused on physics and mathematics as the "hard cases" that would prove the feasibility of social analysis of scientific practice.

2. Collins's articulation of the Awkward Student follows immediately on, and derives from, his discussion of Wittgenstein's views on rule-following.

3. For a recent discussion, see Warwick and Kaiser (2005).

4. See, for example, Gingerich and Weinstein (1988); Dear (1993), and Alder (1997).


6. We take care to note, though, that this disciplining license is neither consistently used nor successful. Many teachers offer up idiosyncratic views of the world, and many students reject what they are taught and remain "awkward." Such moments of pedagogical subversion or resistance deserve study in science, as in other realms of social practice.

7. For an exception, see Ritter's (2001) study of early American science lecture halls.

8. Though as McNamara points out, this culture may not be dying so much as reconstituting to new tools and new ways of making connections between different knowledge domains. See also Collins (1998).

9. The trading zone is a "place" where different kinds of practitioners meet and collaborate, construct local interlanguages for mediating those collaborations, and exchange artifacts, technologies, ideas, stock, personnel, and other cultural material, and knowledge.
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17 The Coming Gender Revolution in Science

Henry Etzkowitz, Stefan Fuchs, Namrata Gupta, Carol Kemelgor, and Marina Ranga

when one considers woman’s possibilities and her future... It is especially interesting to make a close study of their situation...

SEXUAL SEPARATION OF SCIENTIFIC LABOR

Why is science, the quintessentially rational profession, pervaded by seemingly irrational, gendered social arrangements (Glazer, 1964; Dix, 1987; Osborne, 1994; McIver & Robinson, 1992; Volland, 1999; Tri-national Conference, 2003)? Commission on Professionals in Science and Technology, 2004; Rosser, 2004)? Paradoxically, an uneven co-evolution of science, gender, and society displaces universalistic norms of science with discriminatory social practices and invisibilizes these harms. (Merton, 1942; 1973; Biebel, 1991; Ferree et al., 1999; Fox, 2001). By the late nineteenth century, a few women broke through gender barriers and entered the laboratory as "honorary men" but had to accept subordinate status. Like Lee Merz, they were relegated to a basement lab, literally or figuratively (Simé, 1996). Marie Curie was putative junior partner to her husband, a fiction maintained after his death despite the award of successive Nobel prizes (Goldsmith, 2005). Nobelist Marie Goepert Meyer was a research associate in her husband’s university lab, reining an earlier household gendered structure of science, until the shortage of male scientists during World War II allowed her to emerge as a researcher in her own right. Nevertheless, she did not receive an appropriate academic appointment to match her achievements until just before being awarded the highest scientific honor.

Despite the fact that women have entered academic science in ever larger numbers in recent years, they also leave traditional fields, in larger numbers than men, at each "critical transition" (Etzkowitz et al., 1995; National Science Foundation, 1996). Although lost to academia, women reappear in science-related occupations in the media, law, research management, and technology transfer that have opened up as a result of the increasing economic and social relevance of science. A "coming gender revolution in science" also transcends the traditional "sexual separation of labor" in science. Thus, the seemingly ineluctable negative relationship between female gender and scientific status a subject to change under conditions where there is (1) pressure from female scientists