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**ABSTRACT** This paper is a history of the promotion and use of models of technical knowledge production over a seven-year period at a modern physics laboratory. In this analysis, models are seen as reflexively performed in the course of laboratory practice and intertwined with issues of authority and control in the laboratory. Attempts to associate or disassociate different models of knowledge production with different identities at play in the laboratory, in particular those of 'scientist' and 'operator', are traced. The concepts of 'epistemic politics' and 'identity work' are explicated. To account for the relationship between technical knowledge claims, laboratory identities, modes of accountability, and the control of labor both inside and outside of the laboratory. Changes in this relationship over time at the laboratory are considered. In combining sensibilities from labor relations with insights from science and technology studies (STS) writings on expertise, the paper provides an example of a new kind of laboratory study in which accepted modes of authority and control are seen as implicated in the day to day production of technical knowledge in the laboratory, and performances of 'legitimate' means of technical knowledge production, and 'legitimate' technical identities, are likewise implicated in the ongoing negotiation of laboratory organization and order. Through this kind of analysis, issues of gender, race, and labor are engaged with the detailed world of technical knowledge production. The status of the account itself as a performance and intervention is highlighted through the intertwined narratives of experience on the part of the author of both the laboratory and of different strands of STS literature.

**Keywords** epistemology, expertise, gender, identity, laboratory, practice

# 'Lab Hands' and the 'Scarlet O':

## Epistemic Politics and (Scientific) Labor

*Park Doing*

> On a certain occasion when father Nicanor brought a checker set to the chestnut tree and invited him into a game, José Arcadio Buendía would not accept, because according to him he could never understand the sense of a contest in which the two adversaries have agreed upon the rules. (García-Márquez, 1970: 91)

## Introduction to a Laboratory: An Operator's Journey Begins

The elevator jolted to a stop and its doors slid methodically open. I was confronted with a wall of lights and numbers, electronic traces, hums, clicks, and buzzes. This was it. This was what I had come for. Science! I

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lingered in front of the control room before I made my way down the hall to the reception area. Interview days have a peculiar energy. Your senses are heightened. A potential world is opening before you, yet you see only glimpses, flashes of the future. You look for clues in the surroundings, the conversations, and people's demeanors. My friend had told me about this job. He said it was great. There were lots of chances to do interesting things and lots of smart people. He told me that the receptionist was very nice and he specifically told me to wear jeans, not slacks. Coming from a year of corporate life I compromised – black jeans. The interviews were fairly succinct. My questioners held their cards closely to their chests. I couldn't tell if they liked me. My master's degree work was in space plasma physics, not synchrotron radiation. Was that relevant? Had I taken mostly math classes or did I work with the radar equipment? I told them about the circuit that I had designed and built for a radar receiver. I sensed disappointment when I revealed that I didn't work on my own car. I quickly noted that it was more efficient to have someone do it who does it all the time, and that I was very busy. When prompted, I said of course I did the little things like change my own oil (although in fact I never had). The offices were messy. The hallways were chaotic and cluttered. I didn't know aluminum foil could be put to so many uses.

I was escorted past a flamboyant Georgia O'Keefe painting hanging on the wall and stopped at a kind of portal surrounded by lights and signs: 'Radiation Badge Must Be Worn Beyond This Point'. 'Synchrotron On'. With some trepidation, I clipped on a radiation badge. Why did I need it? What was the danger? If there was no danger, why have a badge? Did the badge protect somehow? My questions were answered politely by my guide as we made our way through the entrance to the experimental floor. The badge was a recording device, required by law. No person in the history of the laboratory (lab) had ever recorded an unhealthy dose of radiation. In fact, the ambient radiation underground was less than one usually encounters in the course of a day. I was feeling a bit sheepish for asking such questions when we walked into a cavernous hall whose ceiling must have been 50-feet high. I was dwarfed by the giant gritty lead slabs, huge chrome tanks, and gleaming steel pipes. Doors of experimental rooms slid open and shut, beeping alarms sounded and then were quiet. A gentle bell chimed continually. A large electronic billboard near the top of the wall at the end of the 'floor' indicated that the magnets were powered up and that the storage ring was operating. I looked up and scanned across the ceiling to the far side of the hall. A gouge in the concrete 40 feet above the floor was annotated with a sign that read, 'Top Quark Went Through Here'. I didn't get it. My tour guide said that he loved that sign.

In the interviews they had told me that the operator job was a good way to participate in front-line research, but that certain tasks and procedures needed to be done to keep the lab running, and these were the responsibility of the operator. Well, for me the point was to have a chance to get into some real science. What were a few tasks? I would be called an operator. I didn't much like the sound of it. What did I 'operate'? I had a

BSc in Electrical Engineering from an Ivy League school and was a project away from a master's degree. I had already worked as an engineer with the suit and the tie and the whole bit. Now I was some kind of technician? Well, operator anyway. 'Oh well', I thought, 'What's in a name?'. The main thing was that I could taste the science, the research, the action. Finally, I thought to myself, looking back on my life, something interesting.

## Introduction Number Two

OK, well . . . there is a bit more to the story. You see, at this time, I had also begun an introduction to something else. I had started taking some classes in a nearby Science and Technology Studies (STS) Department and I was becoming intrigued by different ideas about the nature of science. What was it? Where do you find it? How do you know when it is happening? In my classes, I read work in the sociology of scientific knowledge that took the view that the supposedly straightforward scientific method of formulating hypotheses and subjecting them to tests wasn't so simple after all; that Merton's norms and Popper's falsification did not account for the richness and subtlety of scientific practice.[1] According to this work, the way to find out about science was to leave the philosopher's armchair and go into the lab to experience science in all of its messy, confusing, and complicated glory. These authors referred to something called the 'social construction' of facts. I wasn't quite sure what this meant. How was the construction carried out? What materials were used? What forces held them together? I sensed that this topic was the area where my new work was to be done. In fact, I took the job at the lab in part because I had a vague notion that I would somehow have a chance to study science itself. I wasn't sure how I would do it or what I would do, but somehow I knew that something might happen if I could just get into the mix, get involved with some science. If there was more to the production of technical knowledge than how it is presented afterward in journals, talks, tours, and lectures, I was going to go and find it!

In previous accounts of lab practice, I had read of rituals and shop-talk, taken-for-granted instruments and procedures, and inarticulatable tacit knowledge and skills. It seems that some of these ethnographers of labs had degrees in physics but had 'switched over' to the humanities and social sciences. They said that their understanding of the content of physics was what distinguished them from the previous generation of sociologists. That the sociology of science wasn't just about how science was organized or funded, but also about what science produced. Sociology of science needed a sociology of knowledge to go with it. Being versed in physics, these researchers saw themselves as able to undertake such an exploration.[2] Others were humanities types through and through. They proposed that it was precisely their naiveté that would allow them to be open to the social and political dynamics of scientific practice that were taken for granted by the people they studied. Their technical innocence was their methodological hook.[3] I could see both sides. It seemed to me that, somehow, you needed to be versed and awkward at the same time. I wondered if that was possible. I couldn't help but notice that aside from one writer who was a former tour guide, none of the lab studies analysts had *worked* in a lab.

I had worked on an engineering design team for about a year before I joined the lab. With that experience and my university studies, I certainly wasn't starting from scratch. Nevertheless, I was new to this equipment, to this place. In order to begin to make my way, I pushed people at the lab to talk about how they really came to understand the equipment there. At that time, the people with whom I had the most contact happened to be operators. As I asked questions and tried to learn more, I noticed that when I asked the operators about their technical knowledge, they seemed always to talk about the scientists. Their descriptions of themselves were always referenced against depictions of the scientists at the lab. I became aware of a tension in the air. The feelings were palpable. As my introduction to the lab continued, I explored this tension. I had an idea that it would add up to something even if I wasn't sure exactly what.

## Learning By Doing: An Operator's Initiation

At the start of my first day on the job I was shown to the central control area of the lab where several operators were waiting to begin their day. I went with one of the operators to change a tank of compressed helium. I was attentive as he told me not to tighten the connection of the new tank too forcefully. It only needed to be snug, not locked down. My mind was alert. The buzz and clamor of the lab was beginning. There was a life to it. Announcements blared over the loudspeaker: 'Positron filling is finished', 'Injection is complete', 'Tuning is complete, experimenters please acknowledge', 'Irving Johnson line 8 please, Irving Johnson line 8'. Metallic chimes tolled: regular, repetitive. It seemed to me like layers of activity. If you listened carefully to the exchanges on the lab-wide intercom, you could keep up with what was going on in all the different sub-functions of the lab. That, I was to learn in time, was in fact an important part of the operators' job. Coming to that bit of knowledge, however, was not straightforward. There were many members of the lab, and many opinions as to what was important and what wasn't, what was true and what wasn't, what was real and what wasn't. My first lesson in this respect came as the operator and I returned to the control area. As my first tutor walked away, a second operator came up to me and asked what I had just done. When I told him who I had gone with and what we had done, I remember that he leaned over to me as if he was telling me a secret and told me not to listen to a word that that guy had said. I remember thinking at that point that learning about the lab wasn't going to be so easy.

I liked working out on the 'floor': it was a cavernous room the size of a football field arranged with a labyrinth of machines and equipment. I had a small desk in a room right off the 'floor', but I spent almost no time at it. All of the operators' desks were there, up against all four walls in a circle. Some of the desks had computers on them and some didn't. As the weeks went on, I relaxed my dress code. I went with blue jeans and a T-shirt and I started to let my hair grow out (although I had a long way to go to match some of the other operators). The one thing I could not do was ride a motorcycle, which put me in the distinct minority among my new peers.

After a few months, I became responsible for several routine tasks. There were pieces of equipment throughout the lab that had to be continually cooled with liquid nitrogen to avoid catastrophic failure. It was the operator's job to keep them constantly supplied. Several tanks of a variety of gasses had to be replaced on a regular basis. During my shifts, I would make rounds with a clipboard and record the various pressures and levels, and note when I filled a liquid nitrogen Dewar or changed a tank. The liquid nitrogen dispenser made a harsh screaming sound that was terrible to hear late at night. To have to go out in the freezing cold and get a new tank of oxygen was awful. I hated this part of the job, so ritualistic and mechanical. It felt like some kind of punishment.

My initiation continued as the operators, the operations manager, and the assistant operations manager told me about different aspects and systems of the lab. It was complicated. There wasn't much written down. I learned how to give a safety tour, how and when to check and mark down important readouts. I was learning what the beam line equipment was and how and when to talk to the storage ring operator over the lab-wide communication system. Amid my excitement at being in this new and strange world, I began to explore depictions of the relationship between the operators and the scientists at the lab. There was a certain disconnect. The operators and the scientists, it seemed, inhabited different worlds. One operator told me that he liked to work the night shift because fewer scientists were around and he could just 'set up and run the operation' without having to be embroiled in consternating conversations about everything. That was when he could get things done. Another operator who had been in the military told me that, much to his disappointment, he found the relationship similar to that of the officers and the enlisted men in his former life.[4] I was given the password to an early form of a chat room where operators wrote their impressions of the lab, their work, the scientists, and lab management. The scientists did not know of this log (or, as I was told, of this capability of the lab's computing network). The remarks were for the most part derogatory and pointedly critical.[5] I wondered why the operators felt this way about the scientists, and why they felt the need to hide their thoughts.

In light of this apparent separation from the scientists, I asked some of the operators about how they came to their knowledge of the lab's workings. The key, they told me, was their experience with the equipment at the lab and their history with equipment in general. One operator told me that as a kid he used to take apart the various machines around his house, such as clocks and sewing machines, in order to see if he could put them back together. When I asked if he was encouraged to do this by his parents, he assured me with a laugh that it was quite the opposite. When another operator told me how he also took apart equipment as a child, I asked him who taught him how to understand equipment. He said to me, 'Yeah, well, the equipment teaches you. It bothers you that you can't figure out why it works, so you take it apart and, over time, the more you know how to take it apart, the more you know how it goes back together. You break a few things and as time goes on you get better at it'. He then informed me that, 'I think it's the same reason someone has a desire to know about words and how they are put together. I think it is along those lines'. When I asked him if he was saying that he was using a kind of scientific method to gain knowledge about equipment, he paused. He then told me,

> It really depends on whether you have a mind of looking at the process of getting to the end of what you want or whether you just want to get to the end and you're more concerned with the science output or whatever it's going to be . . . there are people who just don't really concentrate on the fact that each individual component of any kind of system could be considered a weak link if it's not done right. I don't know if that is some kind of character trait, I just see that in a lot of people.

When I asked him if he had some particular people in mind, he told me, 'Well, if you ask me, a lot of the scientists around here don't have any scientific method about them at all'. When I asked him whether the scientists had taught him about how the equipment at the lab works, he told me curtly, 'I taught them'.[6]

As I worked my way into the life of the lab, I myself began to inhabit and perform the operators' 'just do it' kind of approach with regard to equipment. I felt uncomfortable asking too many questions. There weren't many manuals for the equipment and even those that existed were kept in a fairly unorganized filing cabinet. I remembered what I had heard, that the equipment teaches you. I was starting to understand how to act, what to do. I remember my first real project. I was told that a particular set of circuit boxes had been malfunctioning and that I should fix them. After being given the basics of what the boxes were supposed to do, I was left alone. Even though the designer of the circuits worked at the lab and was only a few doors away from me, I remember feeling that it wouldn't be right to go talk to him. At times I was fairly frustrated because I didn't really know what to do. I knew though, that I could refer to the model of understanding equipment that I had heard about. It was just me and the equipment. I simply tried things even if I couldn't exactly explain why. I was prepared, if I broke something, to say, 'well, I was investigating the circuit and I'm learning and hey, you have to break some eggs to make an omelet' or something like that. That was better than asking questions. As it turned out, I fried a few chips, kept the damages to myself, and got the boxes working. I knew that I could justify my actions later to my boss and to the other operators, and that I would be seen as doing what operators do.

## The 'Scarlet O': Knowledge Invalidated

The operators' antagonism toward the scientists with regard to understanding equipment was clear. The operators did not take things that the scientists said about lab equipment at face value. One operator told me that that scientific training only teaches a person how to get locked into concocting an argument for whatever point one wants to make.[7] In time, I also came to understand better how the operators felt that their knowledge was being discounted, but at the same time appropriated, by the scientists at the lab. The lab was about to undergo a major upgrade and the talk among the operators was about what was going to be done and who would be involved. I remember the tension in the air when one of the senior operators announced to the group that the upgrade was going to go in the way that it usually goes – that the operators would come in at the end, not the beginning. They would have to make everything work. The operators resented the fact that they would not be granted time to participate in designing and discussing the upgrade, but that they would be called upon, after the fact so to speak, to get everything going. They saw themselves as ignored in the design stage but called upon to save the day at crunch time.

Many operators understood the tag of 'operator' as a taint, a stereotype imposed upon them that scientists used to ignore their input into lab matters. Once you were cast in the role of operator, according to this line of reasoning, you were stripped of the credibility necessary to be a valid knowledge contributor in the lab. They spoke of 'the scarlet O' that they thought they must have on their chests that invalidated their knowledge according to the scientific staff because their arguments were obviously logical and compelling if taken on merit. They also referred to the 'glass ceiling' that they felt kept non-PhD holders from having their opinions weighed on merit.[8] The operators saw the exclusion of their voices from discussions of lab development as a sign of hypocrisy on the part of those claiming to seek scientific knowledge of experimental equipment and an intrusion of politics into the lab. In line with this reasoning, the operators saw an asymmetry to the way mistakes were defined at the lab. When operators made mistakes, it was because they didn't understand, or didn't pay attention, or didn't care, and the mistakes were never forgiven. When scientists made mistakes it was because they were confronted with difficult situations and these episodes were quickly forgotten.[9] To the operators, the criterion for whether something was considered a mistake was whether or not you were an operator. They were the underclass – invalid. One operator who had published a paper in a scientific journal while obtaining his Master's degree in Mechanical Engineering told me in the context of a discussion about a paper I was working on, 'I got my article published for my Master's. I just kept a low profile (about it here). Never talk to anyone about it because they don't want you to have a brain – just lug lead bricks'.[10]

I was not used to this. Coming from an Ivy League school and a stint in the corporate world, I was used to being unselfconsciously valid. Now here I was, an operator, schlepping gas tanks and sweeping floors, with no voice and, apparently, no brain. I would cringe when a scientist would introduce me to someone as an operator. And the pay. Although most of the operators were single, a few of us had kids to support on a low salary and the sting of that was very real. Many times I would check in at midnight for the beginning of my shift and wonder what the hell had I gotten myself into. It's hard to describe if you haven't felt it – stigma. It's not always pushed on you, but the occasional reminders serve to point out that it's always there in the background and can be brought out at any time. It feels heavy. It doesn't go away. I was an operator.

Why were the operators so disgruntled? Why weren't they listened to? What was going on? What should I do? What does this have to do with Popper, Merton, or social construction? To hear the operators tell it, they were not being recognized as the valid knowledge producers they felt that they were. Were they right? Were they wrong? What could be done? By now I was a full-fledged graduate student in an STS department. I was still working full time at the lab, but I was taking more and more classes and meeting more people in STS circles. I was more than intrigued at this point and as I delved into more STS literature I hoped for some answers about the situation that I was in. In my searching, I started to come across an interesting notion about what science was. I became familiar with variations on a model of science that was well suited for including technicians as valid producers of knowledge. It seemed that in bringing out the complexity of scientific and engineering practice, many historians and sociologists had found cases where 'technologists' did not simply apply already discovered scientific principles. Rather, new knowledge about how to proceed was derived from interactions *with the equipment itself*! In this vein, the historian of technology John Staudenmaier (1985: 120) argues for considering 'technological praxis as a *form* of knowledge rather than an *application* of knowledge'. In his study of cases in aeronautical engineering, Walter Vincenti (1990: 90) notes that full technical knowledge 'can only come from individual experiences'. Coming at it from the scientific 'side', in her study of Nobel-prize-winning scientist Barbara McClintock, Evelyn Fox-Keller (1983: 198) promotes the fact that McClintock did better science because she was patient enough and willing to learn from the material of her study and to take seriously each component in its own right.[11] To Fox-Keller, McClintock was innovative precisely because this valuable component of the scientific method was, as the operators at this lab have claimed, lost on most scientists. Stephen Barley and Beth Bechky (1993: 11, 28) pick up on this notion in their study of lab technicians when they note that 'the lab staff took pride in their ability to see intelligible codes where novices [*even scientists*] saw no information at all': they propose a new category of technical worker, the 'scientist-technician'. Edwin Layton (1977: 210) echoes this theme when he refers to 'technologists doing science'. The operators at my lab, it seemed, were articulating a kind of knowledge production capability that was also promoted by these STS analysts; one where experience with equipment or the material at hand is integral and necessary to the doing of science.[12]

That's it. I had found it! The lab just needed to understand that the operators were working in a valid scientific way and then there would be less animosity and better mutual understanding. In fact, Barley and Bechky (1993: 28) have argued that not recognizing that science has this aspect to it and that technicians are well placed to contribute in this regard is a prevalent form of lab mismanagement. Whalley and Barley (1997) have even said that technicians should leverage their importance and expertise by unionizing and demanding better compensation and more autonomy in their working lives.

But, somehow, something wasn't right. Something nagged at me. It seemed too convenient, too simple. The operators were right and STS was on their side? What if I hadn't come into the lab as an operator? Why did the scientists not see it this way? For some reason, I wasn't ready simply to accept what I had found. Then something happened to me. Just as I was wrestling with this issue, I underwent an important shift in my trajectory at the lab: to my surprise I was promoted. After my promotion, to assistant operations manager, I came to engage more intimately with these scientists against whom the operators seemed to measure their identity, capabilities, and worth. As I did so, my understanding of how knowledge claims and their attendant identities are used at the lab underwent an important shift.

## 'Lab Hands': Introduced by Scientists

It had come time to tear down and rebuild half of the lab's experimental stations. This was a big job. I was approached and asked if I would be willing to take charge of the other half of the stations to make sure that those operations remained intact while many of the operators concentrated on the rebuild. I felt pretty intimidated, because almost all of the other operators had been there longer than I had and I was still in the midst of trying to figure everything out, but I took the assignment. Working with the experiments and the experimenters seemed to be my forte, and I liked the idea that I would be closer to the research front. Getting an experiment working, getting it humming along. I liked that. I was good at it. After about a year in this mode I was made an assistant operations manager at the lab and began spending more and more time interacting with the scientific staff. As the assistant operations manager, I attended the weekly senior staff meeting, where no operators were invited, was involved in hiring decisions, and attended meetings with other laboratories as a representative of our lab. I also spent time discussing the operations, problems, and the future of the lab with scientists in their offices and on the experimental floor. As I moved into these different forums, I came to understand the scientists' view of who operators were and what they could know and do.

The scientists' offices were a level above the experimental floor, up a staircase and through a door. The hallway in which they were located resembled any bland academic hallway. The only signs of the experimental floor one flight of stairs below were the television 'scoreboards' that the scientists kept in their offices in order to monitor the status of storage ring operation. In contrast to the operators, the scientists usually wore some kind of collared shirt and slacks or khakis. None rode motorcycles, and none had long hair. It was interesting to me that, in contrast to my experience with the operators, when talking to the scientists I did not pick up feelings of animosity toward the operators. Instead, I got the impression that the scientists, in general, valued what they saw as the operators' important role at the lab. They often said that no project really gets anywhere without operators working on it. They even referred to the operators as the 'life blood' of the lab. The lab director was very proud that the operator job, as he conceived of it, was envied and copied at other laboratories. But, I wondered, who were these operators that the scientists so valued? Were they the same ones that I knew?

It was then that the scientists introduced me to an entity that I had not previously heard of: 'lab hands'. You see, operators had these. An operator with good lab hands, it seemed, could work with equipment in an intuitive and comfortable way. An operator with good lab hands had a feel for equipment that enabled him or her to sense problems and get equipment working. In my discussions with the scientists, it became clear that this was the essential quality that an operator must possess. Scientists would ask me which operators had good lab hands and which did not. There was not much talk about training operators to have lab hands, and everyone agreed that it was impossible to discern them through conversation. Instead there were slightly anxious periods while the scientific staff waited to see whether newly hired operators had good hands or not. It was a great relief to find that they did.

The scientists were proud that the operators had this innate ability. In a magazine article about the lab, one scientist boasted that 'our operators really know their jobs . . . they have an intuition for the machine that I haven't seen in any other facility. Some of the operators can tell if the magnets are working properly just by laying their hands on them to check the temperature' (Saulnier, 1996). Job postings for the operator position from this period noted that 'lab skills' were very important but experience, after all, was not necessary.[13] Here, experience was separated from an applicant's ability to do the job. What was needed was a certain kind of skill, an innate sense for how to proceed. One measure for whether an applicant possessed this kind of feel for equipment was to ask the candidates, when interviewing for the position, whether and how they went about repairing and maintaining their cars.

Well, this view of the operators seemed to be based on quite a different model of understanding equipment than the one to which I had been introduced by the operators themselves. According to this new view, ability was apparently embodied, inherent, located in the hands (not the head) of the operators, and was brought to bear in interactions with equipment, not developed through such interactions. When it came to lab hands, it seemed, you either had them or not. I was struck by the naturalizing aspect of these characterizations. The scientists were doing identity work. They were portraying the operators as a group with particular characteristics. The operators simply were a certain way. They were a certain type of people. Why were these characterizations being promoted by the scientists? What was this identity work about? In considering this, it occurred to me that these were the kinds of characteristics that have been historically attributed to women in many situations. The operators were intuitive creatures. They felt things in a holistic kind of way that couldn't really be formulated or explained. They did handwork. Their hands worked while their heads were involved in other activities (like socializing and gossip). I remember thinking how odd it was that these tough-talking motorcycle-riding men were having attributes assigned to them that typically were associated with women. There was also a primitivism to it. These traits of a 'natural' feel and the idea that the operators were closely identified with the equipment painted the operators as lab natives. The operators couldn't explain their 'knowledge', rather, they inhabited an almost mystical world where the laying on of the hands solved problems.

As I came to understand this new version of who the operators were and what they could do, I looked back on my time at the lab and recalled how I myself had been placed into this kind of identity. One time, when I was working with some of the other operators on a piece of vacuum equipment, a scientist came over and handed me an assembly that consisted of a silicon crystal clamped by its sides onto a base with a spring-loaded mechanism. The scientist told me that the crystal was not diffracting X-rays properly and then asked me to feel it in its assembly and tell him whether it was too tight. He knew I had worked with these assemblies before. Why wouldn't he loosen it and see if it performed better? Why come to me? In a way I was flattered to play a role in diagnosing a scientist's problem. In another way, I felt taken advantage of somehow. I would now be part of the scientist's account of his actions to other lab members. Would I be implicated in blame if the system continued to not work properly even after the clamp was loosened? I put my finger on the crystal and tried to slide it in the clamp. When these assemblies had worked for me, the crystals could slide back and forth in their clamp. This one was held fast. 'It's way too tight,' I told him, and he nodded and walked away. The scientist did not replace the crystal, but rather loosened the clamps. When I saw him an hour later, he held his thumb up to me and told me that it had worked. Another time, I had used sandpaper to 'rough up' the surface of a block of crystal silicon that was being also used to diffract X-rays. With a rough surface, this crystal performed differently and in certain ways better than a 'smooth' crystal. From that point on, whenever a crystal needed to be roughened, it was brought to me and I did it. When they brought the crystals, the scientists would talk about my calibrated fingers (and saliva) as necessary for the job.[14] Another time, a scientist came up to me when a vacuum pump didn't seem to be working and asked me if any of the operators had, 'you know, done the laying on of the hands thing' to assess whether there was in fact a problem.[15] I informed him that I hadn't but perhaps someone else had and he then pressed on to seek out other operators. Another time, a member of the scientific staff emphasized the essentiality of lab hands by explaining to a new summer intern, 'This is Park. He's interested in monochromators [devices that use crystals to diffract out X-rays of a particular wavelength from X-rays of multiple wavelengths, this is, to produce monochromatic X-ray light] too, but he's more hands on, interested in getting something working. I more just sit in my office and design things'.[16]

What had happened when I touched the crystal? What did it mean for operators to be 'hands on' or to do the 'laying on of hands'? In the case of my interaction with the scientist, was I using knowledge based on experience or an innate feel? I didn't know. I felt flattered that the scientist would stake future actions on a declaration from me, but I also felt the sting. Did he know how many times I had been in his situation? Did he know how I had learned what I did? Was he treating me as a colleague? A servant? A woman? A native? I did know that what I did was the fastest and easiest way to deal with an interruption in my busy day. This was the dissonance. While the operators heard praise offered for their services, they felt they were being praised for being something that they actually weren't, and for things that they actually weren't doing. And, what they really were doing was ignored, or worse, co-opted. To the operators, the compliments from the scientists were backhanded slights to their real abilities.

## Epistemic Politics and Laboratory Labor

The operators' abilities were interpreted differently by each group. Where the scientists saw interactions between the operators and equipment as tests of innate, instrumental ability channeled through the direct connection of their lab hands to the equipment, the operators saw such interactions as indications of the knowledge that they had gained through previous interactions with equipment. Where the scientists saw operators working with other kinds of equipment besides lab equipment as markers of general skill, the operators saw such interactions as markers of an ability to learn. When I asked a scientist about the difference between a scientist and an operator, he said that of course any good experimentalist needed the same kind of skill as an operator, but that a scientist also needed the creative ability to sort through scientific ideas and produce experiments that would be interesting to the field.[17] A scientist could do what an operator did, but not the other way around. Scientists, according to this view, had heads and hands while operators had only hands. The operators, on the other hand, felt that the problem with the lab was precisely the fact that their heads were procedurally ignored.

The scientists seemed unaware of the operator's views of them. They didn't realize that the operators saw them as the types of people who were unwilling or unable to learn from equipment in the proper way; that they were 'educated fools' who didn't understand the real world of scientific instrumentation.[18] To the operators, the scientists' adherence to the textbook and their narcissism about their own knowledge was, ironically, what kept them from being legitimate knowledge producers. The operators didn't know why the scientists were like this, they just knew that 'some people were that way'.[19] As time went on, I wondered, 'why was this identity work pursued so fervently at the lab? What were these characterizations, with their implications for knowledge production, really used for?'

It seemed evident to me to that neither the scientists nor the operators were 'right'. After all, by what means would one group come to have a more realistic view of the lab than the other? Rather, they each promoted and performed their own idea of who can produce proper technical knowledge and by what means. As I read more in STS, I found that various writers held the view that there is no one model of technical knowledge production per se, but that the production of models of knowledge production is itself the proper topic of study. A call for this kind of approach was voiced compellingly by the historian Otto Mayr (1976) and several works incorporating this approach with regard to the relationship between science and technology have been produced [Cowan (1996), barnes (1982), Layton (1976), Kline (1955)]. Warwick Anderson (1992) can be seen as using this appproach in his study of negotiations over automated hospital diagnoses. I believe that it is also the lesson of Steven Shapin's (1989) work on Robert Boyle's 'technicians'. It is not simply that the technicians in Boyle's 'lab' were not properly credited, but rather that proper consideration of scientific practice calls for a mode of analysis, whereby the nature of a 'scientist' or a 'technician' is not taken as self-evident and prior to political relations. I also found that the appeal to experience as a specific basis for knowledge production has been analyzed in this light, albeit in a significantly different cultural milieu, by Peter Dear (1995).

This sensibility resonated with my experience at the lab. I knew that neither side was right and that there was something more going on. But what? It was compelling to me that the distinction between 'operator' and 'scientist' was so salient in the performances of lab members, but what could this mean? As I continued my exploration into the field of STS, I became steeped in work that seemed to address just this kind of distinction. In studies of larger-scale public technical and political controversies, from global warming to nuclear waste 'disposal' to genetic engineering, views of who counted as an expert on any particular topic and who did not seemed to be related to the position and status of different groups as they vied for a voice in different decision-making processes. In the analyses of these arguments and contestations, a particular division was often brought into play, namely lay versus expert. It seemed to me that there was an analogy between the tensions between the scientists and operators at the lab and contestations between 'lay people' and 'experts' in these larger controversies. Indeed, I think the operators at the lab can be considered in the same light as Brian Wynne's (1989) Cumbrian sheep-farmers or Steven Epstein's (1995, 1996) AIDS patients/activists.[20] The operators were (at least according to them and backed up by a significant amount of STS literature) experts who had developed and could draw on a body of knowledge that was misunderstood and ignored by the institutionally accredited scientific establishment. At the lab, considerations of who was right or wrong in technical matters relied on differing and conflicting notions of expertise.

Yes, these were negotiations over expertise, but what does that really mean? The arguments between the operators and the scientists were about who could know what, but they were also about who should do what. They were arguments about expertise, to be sure, but they were also arguments about control, about who should be in charge: about work. After all, the scientists were the operators' bosses. It seemed to me that there was something to be learned from the interactions between the operators and the scientists that could help to show how intertwined are notions of expertise and relations of labor, and also to shed light on the identity work that was such a fundamental aspect of lab life. I looked again at writings on expertise, this time with an orientation to issues of management and control. Then I looked at writings on labor, with an eye toward how different notions of expertise were negotiated, all the while considering the episodes with the operators and the scientists.

While it is not present so explicitly, one can read in STS studies of expertise an implicit orientation to issues of management and control. The decisions at stake are about what actions to take, and who should take them. In Wynne's (1989) study, for example, he emphasizes how the sheep-farmers and the government scientists each discounted each other's methods and conclusions: to the scientists, the sheep-farmers were unscientific lay-people and to the sheep-farmers, the scientists were bureaucrats with no way to understand the actual experience of farming sheep. But where did this animosity come from? In an important sense, the sheep-farmers saw the government scientists as managers with authority that they themselves lacked. As the government scientists worked to control, by virtue of their scientific assessment, the practices and procedures of the sheep-farmers, the sheep-farmers rejected the control precisely by rejecting the 'science'. In discarding the scientists' knowledge claims, the sheep-farmers were also resisting the scientists' role as government-sanctioned managers.

In Epstein's (1995, 1996) analysis of AIDS activism, there is a similar dynamic at play. The people who were the 'subjects' of 'treatments' by the medical establishment did not hold the same definitions of disease and proper intervention as did the doctors who were the acknowledged experts. To the AIDS activists, the doctors were not just treating a disease, but by imposing their definitions of disease they were also imposing control over AIDS patients' lives. As these misinformed treatments imposed a definition and a regimen over AIDS patients' lives, the very terms of AIDS research were resisted by the patients and the activists who spoke on their behalf. Like the sheep-farmers, the AIDS activists rejected the science, and therefore the control, of the scientific establishment. In contrast to the sheep-farmers, however, the AIDS activists had access to money, had other institutional connections, and understood how to organize and coordinate a resistive movement such that they were able to bring about a redefinition of the terms of AIDS research and a restructuring of the institutional frameworks that supported and authorized those terms. The ideas of the AIDS activists came to be seen as a viable component of technical knowledge about AIDS. Indeed, some of the activists themselves came to be seen as recognized 'experts' who should be properly involved in research decisions. I propose that the AIDS activists can be seen as breaking into the ranks of management.

Of course, any discussion of the assertion of control by one group over another in the name of science should, I think, involve a discussion of Frederick Winslow Taylor and his movement of scientific management.[21] While there is an important connection between what I have been discussing so far and the advent of Taylorism, it is not, I believe, what it might initially appear to be. In critical discussions of Taylorism, the approach usually taken is to describe how the 'scientific method' was employed by Taylor, and taken up by management, to break down, keep track of, and control what once were complex, holistic, autonomous accomplishments of workers. From the perspective of this paper, however, it is interesting to look at how Taylor, who started as a lathe operator, was able to obtain the necessary credibility with management to implement his organizational schema in the first place. It wasn't simply that he was 'scientific', but rather that he and others could perform his direct experience with the equipment as the touchstone of his expertise.

While it does not state the assertion so directly, Harry Braverman's (1974) classic discussion of the rise of Frederick Taylor and his method intuits why Taylor's nascent attempts at managerial assertion were successful.[22] Just after his promotion from the ranks of the lathe-working crew of which he had been a member for several years, Taylor explains to the crew that although he had been a member of their ranks, he was now the manager and he was going to expect a different rate of work from them. He points out to them that he had never broken ranks with the rate of production while he was a lathe-operator, that he had been 'straight up' with them, but that he was now going to put an end to the practice of 'soldiering' that had limited the rate of production (Braverman, 1974: 93). And, in the end, that is what he did. How was Taylor able to exert control in this first and formative episode of 'scientific' management? How could he succeed in changing modes of production that had existed for many years, in redefining a 'fair day's work' (p. 95)? The answer lies, ironically, not in Taylor's reputation as a scientist, but rather in his appeal to his experience as a lathe-operator.

Taylor explained to the managers who wanted to promote him that he knew that as he asserted managerial control and increased the rate of production of the shop, the status of his knowledge, his expertise, would be challenged. He made the managers pledge to stand by him in the face of such a challenge. Taylor put it to them explicitly:

> Now, these men will show you, and show you conclusively, that, in the first place, I know nothing about *my business*; and that in the second place, I am a liar and you are being fooled, and they will bring any amount of evidence to prove these facts beyond a shadow of a doubt. The only thing I ask you, and I must have your firm promise, is that when I say a thing is so you will take my word against the word of any twenty or any fifty men in the shop. If you won't do that, I won't lift a finger toward increasing the output of this shop. (Braverman, 1974: 94, emphasis added)

Taylor understood the value of being a credible expert, and banked on the value of his experience toward that end. Indeed, in the course of an extended three-year battle to institute the new shop standards, Taylor's knowledge of 'his business' was duly challenged, as the lathe-workers blamed breakdowns of the machines on an unreasonable workload. Taylor asserted that the accidents were staged and that the men could produce significantly more work than they were producing. His history as a lathe operator allowed management – obviously an interested party when it came to redefining a 'fair day's work' – to stick by him for several years in a way that they had not, or could not have, with previous bosses (Braverman, 1974: 105). Had Taylor not been a lathe-worker and appealed to his direct experience as such, one might speculate as to whether or not his management scheme could have held up in the face of the lathe operators' resistance. It was not Taylor's reputation as a scientist, but rather his credibility as an experienced operator that was crucial in this pivotal episode of his career.

With the sheep-farmers, the AIDS activists, and the lathe-workers in Taylor's factory, antagonisms over expertise, over proper technical knowledge production, were also forms of resistance to assertions of control. In this light, what more can be said of the models of technical knowledge production and producers at play in the lab? I assert that the lab scientists' and operators' depictions of each other and their differing methods of technical knowledge production helped each group to justify actions when dealing with equipment, but also bolstered their respective claims for control of decision-making and authority over labor at the lab. The scientists' conception of operators and their abilities as based on a 'feel' for the equipment instrumentalizes the operators in two ways. First, the voice of the operators is treated as another scientific instrument, as in the case of the purported measuring of the temperature with hands. In accounting for their actions in the lab, scientists could justify basing moves on operators' accounts as they would justify decisions based on the readings of proper instruments. If the subsequent action didn't prove effective, the operator could be blamed in the same manner as a faulty instruments would. Second, the skilled 'lab hands' of the operators can be set to work in place of the scientists' own hands, leaving the scientists to operate in the creative realm of scientific ideas, technological innovation, and lab management. This kind of model implies that the scientists should be the ones who should control and direct the development of the lab: after all, they are the holders of technical knowledge.

In the operators' version of lab practice, knowledge of the lab equipment was derived through the experience of working with equipment. They paid attention to detail and learned as they worked with different machines. This knowledge came directly from the interactions with the equipment and did not depend on the accredited institutionalized knowledge of the scientists. In fact, the operators saw themselves as deriving knowledge in a way that the scientists couldn't even understand. Their model usurped knowledge-producing capabilities from the scientists who did not spend as much time around the equipment and thus implied that they, the operators, should rightfully be more in control of developments at the lab. Unlike Taylor, the operators used their claims of experience with the equipment, with a measure of success, to resist managerial control. One has to wonder how the situation would have been different if one or more of the scientists had risen from the ranks of operator and asserted that experience, as Taylor did, as a resource for management.

When the scientists talked about 'lab hands', they were asserting their 'rightful' control of the labor of the lab. When the operators talked about 'the scarlet O', they were resisting that control on the same grounds that the scientists were asserting it. Arguments over expertise were arguments over voice and control, and in the end, labor. This is epistemic politics. Negotiations and antagonisms/contests over who has what kind of access to different epistemic realms are also justifications for who should be in charge of whom and what. Importantly, this dynamic is recursive, as accepted justifications of authority are in turn used to cordon off and fortify epistemic realms that become the wellsprings of 'technical' knowledge. The operators claimed the epistemic realm of experience with the equipment. The scientists claimed the epistemic realm of the accredited institutional knowledge and the creativity to know what interactions with equipment meant. Importantly, the identity work that was such an ever-present part of life at the lab pervaded this dynamic.[23] In the case of the scientists' depiction of the operators, the 'feminine' attributes assigned to the operators (indicators of a lack of 'expertise') implied that their labor was by its very nature controllable and directable. The primitivism did the same kind of work. The operators, in turn, depicted the scientists as unconcerned with the facts on the ground, as educated fools who couldn't incorporate the complex details of the lab equipment into the proper understanding necessary to make proper decisions regarding development and operations. The operators were asserting to me that this kind of profiling at the lab was being used as a justification for their exploitation. The scientists were blissfully unaware of any possible counter-profiling with regard to their own status and expertise. This would certainly not be the first time in history that such a dynamic has transpired.

## Operators and Scientists: A Later Stage

As I came to carry with me into my work at the lab this new sensibility about the relationship between assertions of the proper means of knowledge production and assertions of control, I found myself pushing 'technical' arguments with a newfound zeal, for I knew what important work they were doing. I learned when to act 'technical', and when to talk about 'politics'. I learned when to play into different identities. Interestingly, my reputation as a serious lab member grew. As I was absorbed in this new mode of interaction, and with the life of the lab swirling around me, I couldn't help but notice that something had changed. The attitudes of the operators and the scientists seemed different than before. The animosity and contention apparent in my first few years at the lab had diminished. There was now less argument over what an operator or a scientist could know. There was even a certain calmness (lifelessness?) in the air. It wasn't just me. I began to ask people if they noticed a difference. It became an accepted part of lab discussions. The lab had changed. The culture of the lab, everyone agreed, was simply more stable now than it was before. I started to wonder why and how this was so.

From the beginning to the end of my working life at the lab, the number of experimental stations roughly doubled and there was a trend toward designing stations to specialize in one particular type of experiment rather than supporting a wide range of capabilities. During this period, the scientific staff remained basically intact, but the operator staff incurred significant turnover; while there seemed to have been some changes in institutionalized conceptions of the operator position that reflected the operators' views from the early phase, the predominant tone in the later phase was an acceptance of the division of labor implied in the operator-as-instrument model espoused by the scientists during the early part of my time at the lab. An indication that some of the views that the early operators had come to have an institutional validity can be seen in a job posting from this period. The advertisement reads:

> Job Opportunity: Research Support Specialist I. – Operator. Requirements: Good computer, mechanical, and some electronic skills required as well as experience with maintaining scientific equipment. Good communication and people skills absolutely necessary. Bachelor's in physics or engineering or 2–3 years equivalent experience required.

A striking feature of this listing is that the word 'Operator' only appears in a secondary way and the word 'Technician' does not appear. The substitution of the phrase 'Research Support Specialist' for the term 'Technician' in the actual job title was seen by the early operators as a marker of the recognition and status they felt they had not received previously. The taint of the term operator was being removed from the description of their work. In addition, experience is considered differently in this posting than in the earlier years. It seems that now 2–3 years of equivalent experience is seen as in some way equal to a Bachelor's degree. Here experience is seen as equivalent to institutionally certified knowledge in a way that it was not in the earlier listings. This again is in line with the operators' earlier conception of their work.

With these views of the operators from the early stage incorporated into management's view of the operator position, one might expect that operators in the later stage would more freely see themselves as doing properly scientific work and that their contribution would be openly valued by the scientists. But this just wasn't true. In my last couple of years at the lab, despite the official name change and altered job description, the operators were in fact seen as more remote than ever from the main business of the lab. The talk was no longer about the valuable 'life-blood of the lab', but rather about what was to be done about the operators' lack of knowledge and decision-making capability. In a switch from the first stage, the operators themselves shared the view that they were under-prepared. They themselves were frustrated at their lack of knowledge. Interestingly, the solution to this problem was seen by both groups as a matter of the scientists training the operators. The operators were upset because they felt that the scientists hadn't taken enough time to train them to do their jobs properly. At this point, the operators put the onus on the scientists to give them the knowledge they needed to function at the lab rather than claim that they needed time alone to work on and learn from the equipment.

The traditional division between scientist and technician was in fact more in place than before. The operators did not consider themselves to be scientists in their own right. When I asked one why not, he told me, 'Well, it's obvious, I'm not doing research of my own. I'm implementing designs and programs for other peoples' research'.[24] He then told me that the operators were not against the scientists, but that, the way he saw it, the operators and the scientists shared frustrations over what they saw as an increasingly bureaucratic work environment. At this time, operator training was seen as one of the most important lab priorities and conversations about what an operator and a scientist could know were far less frequent and increasingly seen as irrelevant. It was accepted that knowledge about the equipment derived from the scientists and needed to be passed on to the operators. Operators no longer claimed knowledge hard won from experience.

Why did the conversations and relations change? Why were the operators able to make such claims with such conviction during my early years? Why were the operators' and scientists' knowledge-producing capabilities seen as more straightforwardly understood in my later years at the lab? Why did the emphasis change from experience to training? The equipment had become more stable, more simply present. One aspect of this growth over time seems to be that the epistemic resource of experience had a different cachet, for the operators and the scientists, than it did at the beginning of my time at the lab. In the time of early development at the lab a good portion of the operators had taken part in building the equipment and could use appeals to this experience as a resource to bolster their claims for knowledge and control. As we have seen, experience is the cornerstone for the operators' claim to be knowledge producers, and the fact that they were in on the ground floor of developing the equipment at the lab make the appeal to experience in that situation a strong one. Some of the scientists, on the other hand, had arrived at the lab later, after a significant amount of the lab's equipment had been built up. Thus, they could not counter the operators' appeals to experience with credible references to their own experience.

As the lab developed, however, this situation changed. As new operators came in, they were in a very different position than the original operators who were leaving. The new arrivals came to a lab where most of the equipment already existed. They could not make the same credible claims to experience that the original operators could. The new operators were without this important resource and had no recourse but to defer to the scientists, who had not only greater accredited institutional knowledge but also now a history with the equipment at hand. The operators had little choice but to revert to performances of the predominantly accepted 'lab hands' model and publicly point out that their knowledge was dependent on the scientists. In an important sense, the dynamic was similar to the episode with Taylor and the lathe-workers in that the scientists could now lay claim to the old operators' epistemic territory of experience with equipment (that had previously been the realm of the original operators involved in the initial build up of the lab), not because they had actually been operators, but because they had now worked with the lab equipment for a longer time and could claim a more intimate connection than the newly hired incoming operators. Amid this new epistemic orchestration, it was more difficult for the operators to challenge the model and its top-down assertion of authority and control.

## Conclusion

The lab had changed. My life was changing. The old battles, it seemed, were becoming irrelevant. I could feel myself disengaging from the fray. In the months leading up to my departure from the lab, I began to have the acute feeling that I was an interloper. On the day that I left the lab, the high-pressure equipment that was waiting to replace me and my desk in the prime real estate of my office right off of the experimental floor loitered outside my door as if embarrassed, in front of the voluptuous surveillance of the Georgia O'Keefe painting gazing out over the hallway. People with faces I didn't recognize walked through the halls and worked out on the floor. These unfamiliar characters were focused on jobs, sounds, conversations, and problems with which I was not familiar. I felt like a spirit among them. I shook hands. I smiled. I tried to explain about graduate school, philosophy, and history. I received accommodating smiles in return. Old arguments dissipated. Tensions faded. I walked around the experimental floor as if to say goodbye to my old equipment. I looked up at the cavernous ceiling and my eyes drifted over to the enormous gouge in the wall, marked by the sign 'Top Quark Went Through Here'. I looked at the giant girders jutting out of the ceiling crane aimed right at the gouge and I smiled . . . it was a funny joke. But as my mine lingered on the image my smile subsided. As I stared at the word 'quark' above the broken concrete and rigid poised steeel rails, it occurred to me that the scene was more than a joke, it was actually much more serious, more important.

The primary point of this paper has been to show that the claims of the operators and scientists regarding their knowledge-producing capabilities were performances that linked the technical and the organizational modes of the lab in ways that privileged each group's status and authority over the other, and that these performances were contested differently from an earlier to a later stage of lab development. The epistemic resource of direct experience with equipment was drawn on by earlier operators in order to resist the operator-as-instrument model performed by the scientists in a way that later operators could not. In tracing this history, I am giving one answer to Sheila Jasanoff's (1996: 397) call to let the world back into the lab by showing how an STS research interest in expertise and labor can be brought into a lab study, and how episodes of lab life can be understood in terms of the epistemic politics and identity work at play in the wider culture as well. I do not, however, see the lab as a site that is simply as rich 'as any' for studying the production of technical and social order.[25] In the early 21st century the lab has a special role, as does the factory. For expertise and control of labor are ubiquitous, and everywhere intertwined. The lab and the factory are the functional inverse of each other, each hiding what the other makes explicit, and their combination is a parable for the dynamics of manipulation and control at play throughout social life. Control over the means of knowledge production is implicated in control over the means of material production (and vice versa). In presenting this particular account and analysis of such dynamics, I hope that (despite my own limits of imagination and moral awareness) I have provided the kind of 'flash of carmine' that has a chance to alter the reader's 'sense of the rightness of things' with regard to our present, technopolitical, lives.[26]

## Notes

A version of this paper was presented at the annual meeting of the Society for Social Studies of Science (4S). I would like to thank the session organizers for the opportunity to present my research, and the members of the audiences for their challenging questions. In particular, I would like to thank the awards committees of the 4S and the American Sociological Association (ASA) Science, Knowledge, and Technology Section, for inspiring my confidence in this research by awarding the 4S Nicholas Mullins Prize, and the ASA Sally Hacker/Nicholas Mullins Prize for an earlier version of this paper. I would like to thank Trevor Pinch, Sheila Jasanoff, Peter Dear, Ron Kline, and Michael Lynch for their insights and help with early versions of this paper. I also would like to thank the anonymous reviewers for *Social Studies of Science*, for their metaphors, patience, and guidance in pushing the paper to shift a gear, so to speak, if not change vehicles. Special thanks to the members of the lab for being so articulate, opinionated, and devoted to their work.

1. For an introduction to falsification as a demarcation criterion, see Popper (1963: 228). For a discussion of norms, or 'institutional imperatives', see Merton (1957: 309). For an introduction to an alternative to falsification as a response to the problem of induction, see Collins (1985: 6). For a refutation of norms as demarcation criteria, see Mulkay (1976: 637).

2. In this category I put Trevor Pinch (1986), Harry Collins (1985), and Andrew Pickering (1984). Of course, this was also the general spirit of Thomas Kuhn's work and on occasion he specifically argued that the realm of practice contributed to the essence of what science is. See Kuhn (1962: 59): 'There are instrumental as well as theoretical expectations'.
3. In this category I put Michael Lynch (1985; a good overview to the constructivist turn in the sociology of science starts on p. 3), Sharon Traweek (1988), and Bruno Latour and Steve Woolgar (1979).
4. At the lab, everyone carried a notebook in which they wrote technical notes to themselves about different projects. It was easy, then, for me then to record quotes from conversations that I would have with people in the notebook that I myself always carried. As I took notes on my projects at the lab, I simply also recorded quotes or conversations or documented episodes that I found interesting with regard to this study. In this paper, such notes are referenced to these notebooks as 'Field Notes' with the field notebook number and the date. This quote is from Field Notes, Book 2, 2/19/93.
5. This log was known as the 'oplog'. It was started by an operator who worked closely with the computer systems of the lab (he was later put in charge of those systems). When this person left the lab, the oplog files left with him.
6. These statements were transcribed from an audio recording that was made in November 1992. At that point I was taking a graduate seminar that was an overview of the field of STS. I told the operators involved that I was taking the course and that I needed to do a project for it. I asked them if they would be willing to allow me to record a conversation about their work. They agreed and the excerpts quoted are from that conversation.
7. Field Notes, Book 6, 12/6/93.
8. Field Notes, Book 3, 10/13/98.
9. Field Notes, Book 5, 11/18/93.
10. Field Notes, Book 2, 2/19/93.
11. See Fox-Keller (1983: 198) 'one must have the time to look, the patience to "hear what the material has to say to you"'; and (mirroring the operators' adage), 'every component of an organism is as much of an organism as every other part' (p. 200).
12. For other descriptions of this kind of model, see Kline (1992), Bucciarelli (1994), Cardwell (1976), Faulkner (1994), Gamber (1995), Hughes (1976), Jansen (1995), Konig (1996), Lelas (1993), and Molella & Reingold (1991).
13. The 1993 listing reads, 'A two year commitment to the position is requested. Experience is not needed but mechanical and lab skills along with a BS or equivalent in technical/scientific fields such as physics or engineering are desired'.
14. Field Notes, Book 2, 4/23/97.
15. Field Notes, Book 2, 11/20/96.
16. Field Notes, Book 4, 5/31/94.
17. This quote was from an interview I did with one of the scientists in April 1994. As I had done with some of the operators, I had told this scientist that I was writing a paper for a course I was taking and had asked him if he would do an interview with me. We spoke over lunch and had a general discussion of the topic of the difference between scientists and operators.
18. 'Educated fools' was a term coined by Walt Protas, a machinist at the lab, to describe his view of the nature of a scientist.
19. This quote is from the interview with the operators (Field Notes, Book 6, 12/6/93).
20. In particular, see Wynne (1989: 10–15, 33–36) and Epstein (1996: 249). ('Activists, as the research subjects' representatives or as subjects themselves, possessed grounded knowledge that many researchers found invaluable in the design of trials'.)
21. Much has been written about this controversial figure, from vastly diverging perspectives. For the work of Taylor's official biographer, see Copley (1969). For a more critical recent view, see Spender & Kijne (1996). For an argument relating Taylor's theories to his psychological make-up, see Kakar (1970).

22. In Braverman (1974), see Ch. 4, 'Scientific Management': 85–123.
23. I would like to point out that I see Sharon Traweek (1988: 107–125) and Peter Galison (1997: especially Ch. 4) as bringing out 'identity work' on the part of particle physicists, even if they don't analyze it specifically as such or explore the work that it is doing. While Traweek does relate such work to hierarchical relations within and between research groups, I hope my analysis would push both types of analysis further to explicitly consider how resources and labor are controlled and used both by these groups with respect to other groups and within the groups themselves.
24. This is from a recorded conversation in May 1996 with an operator at his apartment. This operator was aware of my graduate studies and I proposed the interview as part of my ongoing research.
25. Jasanoff (1996: 397) states: 'as some recent work in science studies has begun to show, the lab, if only we let the world back into it, can be as rich a site as any for examining the conjoint production of knowledge and social order'.
26. Jasanoff (1996: 413) argues: 'reflexivity and contingency are part and parcel of our critical enterprise. Yet it is not too much to expect from science studies, as from gifted ethnographic or historical accounts, the insights that help us refashion the rules we live by. A sudden glint of gold here or a brilliant flash of carmine there can irrevocably alter the viewer's sense of the rightness of things, even if parts of the picture remain forever inaccessible, obscured by the limits of imagination and moral awareness'.

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